CHEMICAL HERITAGE FOUNDATION

MARIO J. MOLINA

Transcript of an Interview
Conducted by

David J. Caruso and Jody A. Roberts

at

The Mario Molina Center
Mexico City, Mexico

on

6 and 7 May 2013

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
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Chemical Heritage Foundation
Center for Oral History
315 Chestnut Street
Philadelphia, Pennsylvania 19106

MARIO J. MOLINA

1943	Born in Mexico City, Mexico, on 19 March

Education

1965	BS, Universidad Nacional Autónoma de México, Chemical Engineering
1967	MS, University of Freiburg, Germany, Polymerization Kinetics
1972	PhD, University of California, Berkeley, Physical Chemistry

Professional Experience

	Universidad Nacional Autónoma de México
1967-1968	Assistant Professor
	University of California, Irvine
1975-1979	Assistant Professor
1979-1982	Associate Professor
	California Institute of Technology
1982-1984	Member of Technical Staff, Jet Propulsion Laboratory
1984-1989	Senior Research Scientist, Jet Propulsion Laboratory
	Massachusetts Institute of Technology
1989-2003	Professor, Department of Earth, Atmospheric and Planetary Sciences and Department of Chemistry
1992-1997	Lee and Geraldine Martin Professor of Environmental Studies
1997	Institute Professor
	University of California, San Diego
2003-present	Professor, Department of Chemistry and Biochemistry and Scripps Institution of Oceanography
2003-present	President, Mario Molina Center for Strategic Studies on Energy and the Environment

Selected Honors and Awards

1976	University of California, Irvine, Alumni Association Special Recognition for Contributions in Basic Research
1976-1978	Alfred P. Sloan Foundation Fellow
1978-1982	Camille and Henry Dreyfus Teacher-Scholar
1983	Tyler Ecology and Energy Prize
1983	Society of Hispanic Professional Engineers Award for Achievement in Science and Technology
1984	Council for Recognition of Hispanics, Science Honoree

1987	American Chemical Society Esselen Award
1987-1988	American Association for the Advancement of Science, Newcomb-Cleveland Prize
1989	NASA Medal for Exceptional Scientific Achievement
1989	United Nations Environment Program Global 500 Award
1990-1992	Pew Scholar on Conservation and the Environment
1993-present	Member, National Academy of Sciences
1993-present	Member, Institute of Medicine
1993-present	Member, Academia Mexicana de Ingenieria
1993-present	Member, Academia Mexicana de Ciencias
1993-present	Member, Pontifical Academy of Sciences
1993-present	Fellow, American Association for the Advancement of Science
1993-present	Associate Fellow, Third World Academy of Sciences
1994-1996	Max Planck Research Award
1995	Nobel Prize in Chemistry
1995	United Nations Environment Program Ozone Award
1996	Walker Prize, Boston Museum of Science
1996	Titular Member, European Academy of Arts, Sciences and Humanities
1997	Honorary Member, American Meterological Society
1997	Associate Fellow, Third World Academy of Sciences
1997	Honorary Degree, Doctor of Science, Yale University
1997	Honorary Degree, Doctor of Laws, University of Calgary, Canada
1998	American Chemical Society Award for Creative Advances in Environmental Science and Technology
1998	American Geophysical Union Fellow
1998	Doctor of Science, Honoris Causa, Connecticut College, New London, CT
1998	Honorary Degree, Doctor of Science, Occidental College, Los Angeles, CA
1998	Willard Gibbs Medal
1998	American Physical Society Fellow
1999	UNEP Sasakawa Prize
2000	Doctor Honoris Causa, Pontifica Universidad Católica del Perú
2000	Doctor Honoris Causa, Universidad Nacional Mayor de San Marcos, Perú
2000	Honorary Member, Sociedad Química del Perú
2000-present	Member of the Pontifical Academy of Sciences, Vatican
2001	Doctor Honoris Causa, Universidad de las Américas, Puebla, Mexico
2001	Honorary Degree, Doctor of Science, Trinity College, Connecticut
2001	Honorary Degree, Doctor of Science, University of Miami
2002	Fellow of the American Association for the Advancement of Science
2002	Honorary Degree, Doctor of Science, University of Waterloo, Canada
2002	Honorary Degree, Florida International University
2002	Honorary Degree, Utah State University
2002	Doctor Honoris Causa, Universidad de Pachuca, Mexico

2002	Medalla al Mérito Ciudadano, Legislature of the Mexico City Government
2002	Presea Ezequiel Montes Ledesma, Querétaro, Mexico
2002	John P. McGovern Medal, Sigma Xi
2003	Environment Award, Heinz Family Foundation
2003	National Hispanic Scientist of the Year, MOSI, Tampa, Florida
2003	Doctor of Science Honoris Causa, Tufts University, Massachusetts
2003-present	Member of the Mexican National College (Colegio Nacional de México)
2003	Member of the *International Council on Clean Transportation*
2004	Doctor Honoris Causa, Benemérita Universidad Autónoma de Puebla
2004	Doctor Honoris Causa, Universidad Autónoma Metropolitana, Mexico
2005	Doctor of Science Honorary Degree: University of South Florida
2005	Award for Leadership in Science and Education, Merage Foundation for the American Dream
2006	Doctor Honoris Causa, Universidad Autónoma del Estado de México
2006	Doctor Honoris Causa, Universidad de Chile
2007	Honorary Member, Society of Toxicology
2007	Premio Nacional a la Excelencia Jaime Torres Bodet, Mexico
2007	Premio Nacional Benito Juárez García al Mérito Ciudadano, Mexico
2007	Doctor Honoris Causa, Colegio de Postgraduados (Institución de Enseñanza e Investigación en Ciencias Agrícolas), Mexico
2007	Honorary Degree, The City College of New York
2008	Máster de Oro, Spain
2008	Gran Cruz de la Orden de Isabel la Católica, Spain
2008	Doctor Honoris Causa, Universidad de Santiago de Chile, Chile
2008	Presea Estado de México, "José María Luis Mora", Mexico
2008	Honorary Member of Instituto Mexicano de Ingenieros Químicos, Mexico
2009	Honorary Member of Fundación Carlos III, Spain
2009	Doctor Honoris Causa: Centro de Investigación y de Estudios Avanzados del Instituto Politécnico Nacional, Mexico
2009	Doctor Honoris Causa, Universidad Alfonso X El Sabio, Spain
2009	Honorary Degree, Duke University, USA
2009	Doctor Honoris Causa, Universidad Michoacana de San Nicolás de Hidalgo
2010	Doctor Honoris Causa, Universidad de Guadalajara, Mexico
2010	Doctor Honoris Causa, Univesité libre de Bruxelles, Belgique
2010	Doctor Honoris Causa, Universidad del Valle de México, Mexico
2011	Doctor Honoris Causa, Universidad Nacional de San Luis Potosí, Mexico
2011	Honorary Degree, Washington College, United States
2011	Honorary Degree, University of British Columbia, Canada
2011	Officer in the Order of Oranje-Nassau, Netherlands
2011	Doctor Honoris Causa, Washington College, Maryland
2011	Doctor Honoris Causa, University of British Columbia, Canada
2012	Doctor Honoris Causa, Whittier College, California

2012	Doctor of Science Honorary Degree, Harvard University, Cambridge, Massachusetts
2012	Doctor Honoris Causa, Universidad Complutense de Madrid, Spain
2013	Doctor Honoris Causa, The University of Manchester, UK
2013	Medal "San Ignacio de Loyola", Iberoamericana University, Mexico
2013	Gold Medal of the President of the Italian Republic
2013	Presidential Medal of Freedom, United States of America
2014	Knight of the Legion of Honour, France
2014	University of California San Diego Medal
2014	United Nations Champion of the Earth Award
2014	Doctor of Science (Honorary Degree), John Jay College of Criminal Justice, New York University, USA
2015	Doctor Honoris Causa, Williams College, Williamstown, Massachusetts, United States
2015	Doctor Honoris Causa, Texas A&M University, Texas, United States
2015	Award "Salvador de la Capa de Ozono", Instituto Mexicano del Aerosol A.C. / CANACINTRA, Mexico
2015	Award for his Professional Career, Cámara Nacional de Fabricantes de Envases Metálicos, Mexico
2015	Primer Ejemplar de la Moneda Conmemorativa del 45º Aniversario del Consejo Nacional de Ciencia y Tecnología, Mexico
2016	"Spirit Awards", Latino Caucus of the California State Legislature, USA
2016	Award "Corazón de León", Universidad de Guadalajara, Mexico
2016	Award "Global Quality Gold" Elite Category, Global Quality Foundation, Mexico
2016	Received the "Keys of the City" of Ensenada, Mexico
2016	Doctor Honoris Causa, National University of Cordoba, Argentina
2016	Member of the National Academy of Science of the Argentinian Republic
2017	Doctor Honoris Causa, Boston University
2017	Doctor Honoris Causa, King's College London, United Kingdom

ABSTRACT

Mario Molina grew up Mexico City, Mexico, one of eight children. His father was a lawyer and judge, his stepmother a teacher and housewife. Molina liked music and played the violin seriously. He also loved science, particularly chemistry; encouraged by a chemist aunt, he set up a home lab in a bathroom. He spent his middle-school years in Switzerland in order to learn German, and returned to Mexico for high school. He liked physics and math.

Molina studied chemical engineering at Universidad Nacional Autónoma de México (UNAM). On his own he developed a chemical catalyst to blow polyurethane foam; with friends he established a monopolistic business in a garage. He did a master's degree in polymer kinetics at the University of Freiburg, where he met Theodore Vermeulen and decided to apply to the University of California, Berkeley. Using lasers, he completed a PhD and did postdoctoral work in molecular dynamics in George Pimentel's lab. He also married during this time.

Molina next moved to the University of California, Irvine, to F. Sherwood Rowland's lab, becoming interested in certain industrial chemicals, chlorofluorocarbons (CFCs) and their movements in the atmosphere, discovering that the dissolution of CFC affected the ozone layer. This led to a publication in *Nature* of his ozone depletion theory and the recognition of ozone as chemically active. Molina moved to the California Institute of Technology's Jet Propulsion Laboratory (JPL) to do more hands-on experimentation on the Antarctic ozone hole, with particular attention to the relationship between chlorine and ozone. Reaction from the scientific community and the public was at first muted or even skeptical, but media publication and Congressional testimony eventually convinced everyone, including even E.I. du Pont de Nemours and Company, of the seriousness of the problem. CFC aerosols and coolants were banned; ultimately the Montreal Protocol, the first global attempt to limit harm to the atmosphere, was signed.

Wanting to return to academic life, to deal with policy issues, and to have more influence in the environmental chemistry field, Molina accepted a professorship at the Massachusetts Institute of Technology (MIT). His focus turned to the more complicated chemistry on the surface of the planet, especially in Mexico City. His work has improved the air quality in Mexico City considerably. He won the first Nobel Prize for environmental science and is one of only three Mexicans to have won the prize. He has established scholarships at MIT and in Mexico City. He was selected to the President's Council of Advisors on Science and Technology (PCAST) and the Union of Concerned Scientists.

Wanting to continue in PCAST, to open the Mario Molina Center for Strategic Studies on Energy and the Environment in Mexico City, and to do research – which he could not do while at MIT - Molina moved to the University of California, San Diego. There he collaborates on research into particles in the lower atmosphere, working on air quality with Mexican government, and contributing to policy ideas about climate change. He says the Montreal Protocol was relatively easy because it was focused on a small conclusion that all signatories could easily see and that was relatively inexpensive to alter, whereas climate change is much more complex and diffuse.

INTERVIEWERS

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

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

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ALSO PRESENT: Lorena González

INTERVIEWER: David J. Caruso
Jody A. Roberts

LOCATION: The Mario Molina Center
Mexico City, Mexico

DATE: 6 May 2013

CARUSO: Today is the sixth of May, 2013. I'm David Caruso. I'm here with Jody Roberts, my co-interviewer. This is our first interview session with Dr. Mario Molina, here at the Mario Molina Center in Mexico City, [Mexico]. Thank you again for agreeing to participate in this oral history interview. As I mentioned, I'd like to start at the beginning and hear about growing up in . . . I assume you grew up in Mexico City.

MOLINA: That's correct, yes.

CARUSO: You were born in 1943.

MOLINA: That's correct, yes. Where should I start? As a child, I had the normal interests children have, except perhaps at that time it was more common to read and not just to watch TV. And so I remember, I must have been eight, nine years old and liked to read pirate novels and things like that. Again, all normal.

But somehow or other [. . .] I ran across some biographies of scientists and that was extremely attractive for me. Thinking back, I think it was important and quite useful that at home we had lots of books. My father [Roberto Molina Pasquel] was a lawyer. Although he had a private practice initially, he became interested in academic work, and so he created an institute at the National University [Universidad Nacional Autónoma de México (UNAM)] for international law. So, he had academic interests, I guess, and as part of that we had a library with all sorts of [books] to choose from.

Anyhow, that's how I became interested in science. And as a result of that, I guess my parents did notice and gave me some toy chemistry sets or microscopes as presents, which I enjoyed tremendously at that time.

CARUSO: Can you tell me a little bit more about where your family lived? I know your father was a lawyer. What type of law was he practicing?

MOLINA: Well, his private practice was connected with standard [law] issues. [He worked] with several friends and, I think, running what here in Mexico is routine legal advice [. . .]. But his university activities were on the side; that was really just a hobby of his—this all happened in Mexico City.

I went to an elementary school, a very small one which had more personalized teaching, but in spite of that I remember getting bored, very bored, in school. I now understand very well what was wrong, and is still wrong, with many schools: [it's that] you only have to memorize things. I still remember a few things I was taught, such as the seasons are there because sometimes our planet is closer to the sun than others and that . . . . I was still very young when I realized that my teachers were not very good scientists. [laughter]

ROBERTS: So you realized that early, that they weren't very good at this science you were being taught?

MOLINA: Not while I was in elementary school. Of course, I started enjoying chemistry, [but not in connection to school]. Eventually it became part of school, but it [was after] secondary school. [. . .]

My **<T: 05 min>** mother Leonor Henríquez de Molina] passed away when I was three years old or so, but my father married again, and so I consider my mother—because I lived with her so many years—not really my biological mother but the second wife of my father, Luz Lara de Molina. She was actually also an elementary school teacher and didn't work anymore after being married. Both parents at home, they really supported [my interest in science].

Another point, perhaps, that was not all that common is that, again, for some reason, we had classical music at home [playing regularly], so I became a fan of classical music since I was very, very young.

ROBERTS: What did you like to listen to?

MOLINA: Beethoven and other composers. We still had the plastic records in our hands at that time. [. . .] I remember we were all very happy [that my father bought a high fidelity system at that time]. [laughter]

But I remember later [. . .], I was already in high school, I had to make a decision. I started playing the violin as well because I had one aunt [Maria Eckart] who was actually

German, but she came to live in Mexico, and she helped me learn to play the violin. But at some point in high school my parents asked a friend of theirs [for] advice since I liked the violin, how should I pursue it, and they got what I believe was terrible advice, which is—because I was so interested in music and in science— “Unless he plays at least eight hours a day, forget it.” [laughter]

I think that was terrible because I would have liked to [continue playing], and I played a little bit afterwards, but I should have played more just for my own pleasure and not necessarily to become a famous performer. But the reason I remember it is because I was sufficiently seriously considering between music and science that my parents might have asked me and I might have thought about it, but decided, no, I want to be a scientist. Because, by that time I had already further developed my scientific taste.

CARUSO: I'm familiar with what things were like in the United States in the late 1940s and 1950s, right: there was a heavy investment in science and technology. Was there anything parallel happening in Mexico at the time?

MOLINA: No, not really. I think one issue I would criticize, again, [about the culture] in Mexico is the lack of interest and emphasis on science here, and that’s understandable. There has been a tradition of putting a lot of weight on the arts and history, but it’s mostly the humanities. And for some reason, science was not something highly regarded, which obviously was a mistake at that time, given the importance that science has had historically, even for economic development and human well-being. [. . .]. It’s never been very relevant [in Mexico], except in recent years [. . .]. The current decision-makers in government are trying to change that. But, no, when I was a child that was certainly not the case.

But I can add a couple [more pieces] of information. [. . .] **<T: 10 min** In high school, there were physics and chemistry classes, but as is the case in the [United] States, teaching chemistry in high school has a very bad name. Most people hate it, because it’s terribly boring and it’s a matter [mostly] of memorizing things. [. . .]. Since my chemistry teachers knew I liked it, I had a special relation with them. But [. . .] the other two points: one is it was also quite normal for children not to like school and sciences, so I didn’t have friends with whom I shared this sort of affinity for science; except much later in high school, I had one or two friends that liked science as well, and, of course, much later [in college] the situation did change, so it was no longer a very lonely activity.

I did have one aunt, [Ester Molina], [. . .]—about the same age as my father—who was a chemist. I did not develop my interest in chemistry because of her, but after it was clear that I did like chemistry, she became interested and then helped me enjoy chemistry more because we did more sophisticated experiments together. She was familiar with chemistry experiments in college, so essentially we did many of the experiments that students do in first-year college, and I remember we had to go to special stores to buy the chemicals. I was no longer using toy sets.

Some of the chemicals were poisonous, so they wouldn't have been sold to children, but with her [help we were able to purchase them]. [laughter]

ROBERTS: Was she living in Mexico City as well?

MOLINA: Yes. Yes, that's what made it feasible. So at least once a week, we would get together and do experiments. At some stage I was able to borrow a bathroom at my house that was not in use, so I really converted that to a laboratory. Fortunately, [the bathroom] was not needed, and so that's where I did a lot of experiments.

ROBERTS: Did you have any famous incidents in that bathroom laboratory with your family?

MOLINA: Well, bad smells I guess or ... but nothing special.

CARUSO: No explosions?

MOLINA: No explosions. But anyhow, it was very fun because some of those were really, as I mentioned, more sophisticated experiments. [. . .] But fortunately I did not have any particular taste for explosions as some of my friends had ... [it is] the more glamorous part of it. But the other important thing that happened when I was still very young—in fact, just finishing elementary school—is that my parents had the idea of sending us abroad to learn another language—I'm the fourth in my family—so my oldest sisters, [Leonor Molina Henríquez and Marta Molina-Henríquez], they went to Canada, so they learned English. My older brother, [Roberto Molina Henríquez], who's also a lawyer, they sent him to the United States to a school near Boston, [Massachusetts]. And they thought, "Well, German is an important language for chemists." So they sent me to Switzerland, which was nice. In Switzerland, they have good boarding schools, except, of course, I was about eleven years old.

So at that age, it's not particularly nice to be abroad and be in a boarding school, but it was fine. And there I had even more, if you want, affinity [for chemistry]. I worked more closely with my chemistry and math teachers because they did realize that it was something I enjoyed and it was the reason I had gone there, so I did manage to do also **<T: 15 min>** unusual experiments, but particularly to work closely, I remember, in geometry and mathematics with [my math teacher]. I had a very good connection with him, because we did things outside the regular class, something that was feasible in a boarding school.

The thing, though, that was the same as here in Mexico is that none of my friends had any affinity for science at all in this school in Switzerland. Recently I've gone back to visit the school, [. . .], which is well-known [Institut auf dem Rosenberg] and is in St. Gallen

[Switzerland]. [It turns out that] one of the classrooms which they have for children [was] dedicated to me. This happened after winning the Nobel Prize; but it was a nice surprise for me. [. . .]

It was quite an experience, and I did learn German. I had learned a little bit before with this aunt I was telling you [about] who taught me a bit of violin. I did continue to play the violin. That was also nice in Europe. And then I just returned here to continue with high school. But between that and the help from my aunt, my chemist aunt, I had reinforced my love for science. I had remembered already . . . decided if at all possible, I certainly would like to be a scientist and do research. I wasn't quite sure whether it's something I could do. I remember when I was very young . . . I thought maybe I can do this at least as a hobby, but I might have to earn my living doing some real work, and so it was very [nice to learn] later that I could actually earn a living being a research scientist. So that's all as a young kid.

CARUSO: I have a few questions.

MOLINA: Sure. Sure.

CARUSO: Just returning to your father's legal work, were you ever exposed to what he was doing as a lawyer, either in private practice or in the academic setting? Did you know what was going on?

MOLINA: Not when I was very young. Later, after I returned from Europe, I did, because I remember . . . not in much detail, but it was international law and so on. I do remember celebrating [. . .] his doctorate. He received a PhD, which was very unusual for lawyers in Mexico at that time; [he did it] through his academic work at the National Autonomous University [of Mexico].

ROBERTS: What was his degree in?

MOLINA: His degree was in law. [. . .] And in his specialty, international law. [. . .] My father became the head of the institute that he created—and again, at that time it was still his hobby—but being the head, he was involved also in the university administration. [. . .] He had to go to some periodic meetings that [the] university has to manage itself. But there were also some student representatives and I became a student representative as well, [probably] because of my grades, but I do remember that was fun. [I] went together with my father to these gatherings with the rector of the university, which was quite unusual, [being] in very different fields. But other than that, I had really no direct involvement with [my father's profession].

CARUSO: When you say international law, I know there are lots of . . . are you talking about relationships between corporations internationally? Are you talking about . . . ?

MOLINA: No; **<T: 20 min>** [. . .] one of his specialties was international trust funds. I probably should know more about it, but [what I remember is that] these were legal issues, probably connected with financing international institutions. And to add a bit more, [. . .] he was still practicing as a lawyer when he was invited by the Mexican government [to become an ambassador], because of his interest in international law. He was by no means a career diplomat, and I was already in college. I´m the fourth child, and my younger brothers and sisters [Luis Molina-Lara, Javier Molina-Lara and Lucero Molina-Lara], [travelled] with the family and went to Ethiopia and to Australia and the Philippines. I was already in college, so I didn't join them. , but again, this was a good example for me of how something academic could lead to this sort of recognition. In a very different field, of course.

CARUSO: So, just so that way I have the numbers correct, you have two older sisters and one older brother?

MOLINA: That's right.

CARUSO: . . . and three younger brothers?

MOLINA: No. Two younger brothers and one younger sister.

CARUSO: Okay. And did the younger ones also do time abroad at different . . . ?

MOLINA: Well, they did, but that was traveling with the family, [not by themselves]. My father was an ambassador already, so there was probably no need to send them abroad specifically. But, yes, in some sense, that's what they did.

CARUSO: I know that your older brother is a lawyer. Did anyone else in your family pursue a specific profession?

MOLINA: The brother that is next to me, the younger one, is a physician, so he became a cardiologist and, ironically, just last year he became, I think the fourth or fifth cardiologist here in Mexico to receive a doctoral degree as well. Most physicians don't have a PhD, but he does research as well, though he still has a private practice—it's one of those things you can do here in Mexico—but in the mornings he works in a public hospital and that's where he carries out his research, and then in the afternoon he has his private practice as a cardiologist.

CARUSO: So it sounds like your family was very encouraging of children pursuing whatever they would like to do.

MOLINA: I guess so, yes.

CARUSO: Okay.

ROBERTS: What about your sisters?

MOLINA: The oldest sister passed away; she was devoted to her family. But the next one did study architecture, but eventually she married, and at that time, I guess, it was less common for women to pursue a professional career as it's possible now. At that time, with a family, it was quite unusual for a married woman to devote a lot of time to her job. Fortunately, these are the sort of things that have changed a great deal.

ROBERTS: And did they all stay in Mexico?

MOLINA: Yes, all of them stayed. My youngest sister went to college, and I guess she likes and works with psychology as an advisor, and she lives in Morelia, [Mexico], so she's the one member of the family that does not live in Mexico City.

CARUSO: So, I would assume then that . . . well, let me ask it this way: were your parents involved in your academic career directly? Were they sitting you down at night and making sure you did your homework? Or was it more up to you to get things done?

MOLINA: It was more up to me. They were involved in giving me support to follow [the] things that I didn't enjoy [doing], but, no, they never put any pressure in terms of doing well in school or in college. And again **<T: 25 min>**, what I do remember is just the presents, like the chemistry sets, microscopes, or books, occasionally. So they did pay attention to that. I also liked to build things, and so that was part of the idea. I remember we had Erector Sets [and similar toys], so I had very complicated things.

So that's the sort of thing that they, fortunately, did listen to. But not being scientists themselves, I had very little direct help from them. Even my aunt who was a chemist, it was for fun we did these experiments. She could be very professional, but she did not get involved with my studies in any way.

ROBERTS: I'm interested to know more about this, because you talked about the culture of Mexico being more focused on arts and humanities. Your father's a lawyer; it seems like a very traditional sort of professional family that way. But somehow they were supporting you in becoming this budding scientist. And I'm curious both how you discovered that idea and how you expressed that to them and where they found support for this? I mean, they found chemistry sets and things like that, but it doesn't seem like that's something that would have necessarily been readily available.

MOLINA: That's . . . yeah. I guess.

GONZALEZ: There were the toys, you know.

MOLINA: Yeah, there were toys. So that was not an uncommon toy, and I remember from my friends that these were not toys they particularly enjoyed. But it was not that difficult, because, after all, the university was there. There were a small number of well-known Mexican scientists that were appreciated by society, so it was not that science was completely neglected. It was acknowledged that it was something important, but it was not really part of the culture. So I would imagine that it's more through, perhaps, my father's academic interests, being connected to the university, and my mother, perhaps, being a teacher that they thought it was fine.

And my initial attraction for science, it might have been a little bit by chance, but I do remember it did have a lot to do with my fascination with these biographies. And not necessarily very well-known scientists, but [Antonie] van Leeuwenhoek, just discovering the microscope and how he found another world out there. Because I remember I lived that experience myself. I was sick. [I got an] infection most children get, and so I had to stay at home for days or weeks. Maybe because I was sick I got this toy microscope and I remember looking at the drop of very dirty water, which I had prepared just sort of letting some lettuce rot. And the enormous fascination I had with a very simple microscope looking at the drop and seeing it teeming with life. It's something you normally are not even aware of, or if you have to do it as part of school homework, maybe it's a way to remove your curiosity for things.

But so it had nothing to do with school and it must have resonated with the books I read. So those are things I remember, those types of experiences or playing with the chemistry sets, crystallizing things in beautiful colors and things, how things change. So it's just this natural curiosity that I now realize, because my involvement more recently with science education, that most children actually do have this curiosity, it's just that we manage in school, normally, to fight it in some way. But I was lucky; somehow or other, I had enough support to do it and then enjoy it.

CARUSO: So I still . . . I don't fully know why chemistry and not biology or physics? Why was it . . . ?

MOLINA: Yes. Yes, that's a good question. Chemistry was perhaps related to **<T: 30 min>** [perhaps] chemistry sets and being able to do these sophisticated experiments, but I was very interested in physics and mathematics as well; to the point that when I went to college, to the university, I was questioning whether I should actually study physics. Biology at that time was not modern biology that is more sophisticated and more physics—or chemistry—oriented. So it was nice and I tried [it], but it wasn't that challenging for me at that time because I did not know about this new world of biology, which, of course, is enormously important nowadays and it's very quantitative, very chemistry-oriented. When I was a child, I remember, however interesting it was, it was not mathematical enough for me.

And ironically, because of all these connections, then I finally decided to continue with chemistry; of all things, chemical engineering, because there were no physical chemists here. At that time physical chemistry was not an option. And I realized I had learned very clearly that that was the one topic where you could do physics, mathematics, and chemistry simultaneously, and, in fact, that was correct. I did the right thing there. And I could have studied just physics; but, again, I did want to do chemistry.

So, [I´ll give you] one more piece of information here connected with biology: I was also very interested in biology, but not to the extent of choosing that as a career. But one example: one of my friends, by the way, one with whom I then later shared some of the chemistry experiments at home—marginally: he wasn't that involved with it, but his father was part of this group of Spanish intellectuals that migrated to Mexico that turned out to be very influential. And so I went to a high school where I had a number of excellent teachers, [professionals who migrated from Spain as refugees and probably wouldn't have been teachers there], but they had a very important influence here in mathematics, as well. Anyhow, this friend's father was an entomologist. And so I was able—and I was very lucky—to participate several times, half a dozen times at least, in field trips that were college field trips because he was a professor at the [National] Polytechnic Institute. So those for me were very nice experiences, being able to make a professional field trip. And I realized, "Well, that's interesting," but what was most satisfying for the biologists that were in the field trips is to discover new species that they were able to name, and so on. So that was all very interesting to me, but I thought, no, I like more the mathematical, the physical part.

ROBERTS: How old were you when you went on these field trips?

MOLINA: I was in high school. That was after I returned from Switzerland, so maybe fourteen, fifteen, something around that age. But it was still very interesting, just the excitement of finding a new species, or even species that were known, but very strange, these very large unusual insects. And we had to go very well prepared, because there were snakes that could bite. But the teachers were professionals, of course, so we were doing everything the way it had to be done with college students. It was another experience of becoming part of an actual scientific research.

ROBERTS: Well, that's one of the things I was interested in, is you say from a pretty early age you were setting out on this path; you wanted to be a researcher; you wanted to do this chemistry. But I want to know what you thought that meant since you didn't have a lot of models around you. You had a lot of teachers, but you didn't have a lot of exposure, it seems like, to actual researchers.

MOLINA: That's right. When I was very young, that's why I told you I had this misunderstanding that maybe that's just something to do for fun because it was just too attractive, too nice, to be able to earn a living [from it]. I remember that quite clearly. The examples were from the biographies **<T: 35 min>**. So I read a bit more, and these were scientists, obviously, and some of the biographies were not necessarily science books. They taught about the people, what they did, and their problems—[Louis] Pasteur, Madame [Marie] Curie, and so on. And so that's probably where I got this image when I was younger. So I then realized how chemists might do research, [and probably] that's where it came from. But the actual experience, I only had it in biology.

ROBERTS: Do you remember any specific biographies that really stand out? You mentioned Curie.

MOLINA: Well, I've mentioned three now: van Leeuwenhoek, Madame Curie, Louis Pasteur. Those were the three that I happen to remember. There were probably several others, which don't come to mind at the moment. But, yeah, maybe those are the ones that happened to be at home for some reason.

CARUSO: So you've spoken about some of the limitations of, at least, what your teachers were doing in terms of science [while] growing up. You had an experience abroad, which I think was a bit better in terms of science, math, going on at the same time. Did you ever consider not going to university in Mexico for your undergraduate degree?

MOLINA: I did consider it, but it was not a real option I had. I guess the idea was that it was much easier to do that after college. And, again, it's not something I probably considered very seriously, mostly because it's not an option I thought I really had. So that was probably the reason.

CARUSO: And so going in as an undergraduate, you started off as a chemical engineering major?

MOLINA: That's right. That's right.

CARUSO: Okay. How many students were in that discipline in your entering class, would you estimate?

MOLINA: Well, there were . . . see, the National University is the largest one, so there were . . . overall, twelve to fourteen thousand students at the chemistry college, so larger than MIT [Massachusetts Institute of Technology]. But at the entering class, well, probably a couple of thousand or so.

CARUSO: So there were a lot of students going into that discipline. Can you tell us a little bit about how your training went for your undergraduate degree? I know in the US. a lot of colleges, even if you're in a science or engineering major, you're required to do a lot of humanities work. So I'm curious to know about that.

MOLINA: Right. That's quite different here. Once you choose a career, it's normally quite focused. If it's an engineering career, then you might take physics, some math; and if it's chemistry, then chemistry courses, but no more humanities. So that's something that's more unique in the US, where you have an option to change fields later on. In Mexico, that would have been harder to do. But that's perhaps one reason also that you have many dropouts.

So you start with a large class and then many students just don't make it. But in medicine it's even more [noticeable], the large fraction of dropouts that they have. Anyhow, that, for me, that was not a problem. I remember doing well; I, perhaps, was at the top of my class a number of times. But soon thereafter, [I had] a similar experience to the one I told you in elementary school, because I realized many of the classes were very boring and were not really run by experts. I did have a handful of very good professors; again, one of them was from this same Spanish group, [Francisco Giral], he was a very well-known organic chemist, and so we had some [very good classes], even though [they were] very qualitative. I still had this **<T: 40 min>** physics and mathematics attachment, but that's fine. Chemistry can be different things, and part of it is . . . like, organic chemistry could just be very descriptive, in contrast to what we would now consider physical chemistry. So that's one thing I didn't get in college, which is a focus on issues like quantum chemistry and so on.

But the engineering component turned out to be quite useful because it was very mathematical oriented, and it was geared to a problem-solving attitude, which was quite useful. [. . .] Other than this handful of very good teachers, I lost interest in the normal classes, and did them routinely. But one special thing, perhaps it's of interest: in college here, particularly in the engineering school, in contrast, say, to physics and math, which are quite unusual because there you have students that are already thinking [. . .] of academic work, [. . .] normally students who study engineering or law or whatever, [have] in mind the jobs that they would eventually get. So particularly at that time, and still a bit the case, things are set up so that you can work almost full time while you're a student, first point. Second point, which is also, again, something quite unusual—it's fortunately no longer the case—the professors were full-time engineers in industry, except this handful that I was telling you that were very good. So, they taught really as a hobby. And now, of course, I realize why is it that they were not really professionals: they were not academics doing research, they were just practicing engineers, and in hindsight, some of them didn't quite understand what they were teaching. But, again, because the emphasis on the molecular components was not there, and I can see now some of the actual mistakes that we had to learn.

The point here is that we had a lot of free time, that many students had to earn a living, so they were working [during] that free time, and that's why we had classes very early [in the day] and then relatively late. And so, at the beginning, since I didn't have to work, I was just fairly lazy, just enjoying life with friends. But then afterwards I had an interesting experience, because I did get together with several friends and we said, "Well, instead of getting a job somewhere, let's do something." So we created a business [. . .]. I could tell you briefly what it's about.

I remember just a little bit by chance: some of my friends were working full time [. . .] with plastic foam. And one of the things I realized [is that] you have to use a catalyst to blow polyurethane foam. And Mexico had the peculiarity at that time, which is no longer the case, [due to] trade laws that if you could manufacture something in Mexico, you could, what we called at that time, "close the border": [companies in Mexico] had to buy [the product] from you. And so this is just a connection. Then we realized, "Ah ha." Well, this catalyst, it had to be imported. "Maybe we can make it." It was actually very tricky, but with the experience I had doing so many experiments, I came up with a way to synthesize it, and we closed the border, so ultimately we had all the foam industry depending on these garage experiments that we were doing. But it was great. It was a very good experience: first, with research, because we really had to do something that worked; and second, just seeing how a business runs in Mexico. Now, I remember these students getting very worried, but we were able to show them that it worked and so it was a nice experience overall.

My friends were more focused on the business **<T: 45 min>** part, and so I [focused on] the chemistry aspect. [. . .] One of the raw products was ammonia, which, of course, [has a strong] smell; we had some big tanks because, after all, we had to make it in industrial proportions. Fortunately there were no big environmental laws, so we could have this somewhere in a garage in the city, imagine with these awful smells. But, anyhow, it all worked quite well, eventually. That's an experience I had as a student. In some sense, it was a consequence of my interest in doing experiments, and it was also a reflection of the time we had in college.

[. . .] In connection with this, when I went to get a PhD at [University of California at] Berkeley, instead of having many [subjects], like in [college in] Mexico, where ironically you had to take six or seven subjects simultaneously [at the] PhD level, you [had to] take three or four classes at most, but really [had to] spend a lot of time in them. So I could see just a very large contrast. [In college] I did take a few classes in the physics department, math classes particularly, when I was at the National University. And so that was already like a different culture. But [for] the engineers and lawyers and so on [in Mexico], it was the normal way universities were run, I guess, in Latin America, and probably in many other countries in the developing world.

But, anyhow, that's [. . .] my experience in college. We had to do experiments, but they were more the routine experiments that [you find] in some experimental courses. They were not particularly interesting, [and for me] it was much more interesting to do something research oriented.

CARUSO: So I have, I think, a number of questions based on your descriptions here. Were there any opportunities to do research with faculty beyond just what happened in typical laboratory courses?

MOLINA: No, and that's another [thing] I forgot [to mention]—a slightly unusual thing. You need a thesis [to graduate college in Mexico], and it's not just like in the US for master's or PhD, but to actually get your engineering degree, which is a five-year college, not just four [. . .]. In chemical engineering, say, they would involve more some business-chemistry-type connection and some study or something that could be manufactured. But I did take that opportunity to actually do some additional research, so the one place where research was carried out in chemistry had nothing to do with engineering: that was in the Chemistry Institute [at UNAM] and that was a group of organic chemists. That was probably the best-known [group of] chemists in Mexico, certainly at that time, working with natural products. You know, Mexico was actually quite important. The [birth control] pill and a number of things were actually developed in Mexico with a combination of American and Mexican scientists, but there are some very well-known chemists from that school, which I had little contact with because that's one thing that was separated. In chemical engineering, except the first few years, that was a different career, say, as an organic chemist. And, again, more qualitative: it wouldn't be physical chemistry, or practically no physical chemistry. Anyhow, I did find the professor working at this Institute and was able to do some research, which wasn't physical chemistry research, [it was] working with gas chromatography, but much more combining my engineering background with chemistry research.

[When I was an undergraduate student at UNAM I didn´t do research work until I had to do my thesis. After finishing school I traveled abroad and when I came back to Mexico I gave classes at UNAM and created the Master's in Chemical Engineering for that institution. During my stay there, professors were not full-time teachers like they are now. In my time we gave classes and conducted research individually.]

CARUSO: So part of the reason I asked about that is I'm curious to know more about how you did your research for the business that you developed. I mean . . . .

MOLINA: Oh, I still had my . . . let me see, what did I do? Oh yeah. I'm not sure I still had the bathroom available, but some of that I did in . . . one of these friends with whom we collaborated was connected with some sort of industry, I guess—family or whatever—so we were able to use some of those labs. I did not need a very sophisticated lab, but I did require [using] instruments, and so I was able to borrow much of [them].

CARUSO: Okay, because that's what I was curious about, because, I mean, you can sit there and do the experiments all you want, but to verify products . . . .

MOLINA: That's right. Yes. Yes, so we had . . . fortunately, we did have these industrial connections at that time. It was sort of a . . . we were able to . . . .

ROBERTS: So how were you able to convince a host to let you do some of your own experiments?

MOLINA: Well, it was almost for fun. It was something that did not cause any problem and they thought, "Oh, it's [. . .] an interesting idea. Let's see if you can do something." But we did not take up much space or we didn't need much resources, again, because it was very much the type of experiments I had done before. You could just go and buy chemicals and we could borrow some analytical instruments, in fact, from the college itself, that were used for the routine experiments. So I did not require any unusual facilities, in other words. Of course, the main point there was to, once we had the product and as we realized, "Ah, yeah, this is it," because we were able to then do an actual commercial-scale experiment, [. . .] we knew we could close the border. So we managed to do that. Once it worked, then it was fine.

CARUSO: But scaling up, I mean, just talking to chemical engineers and chemists over the years, you know, scaling up from a bench reaction to an industrial-sized reaction, there can be problems with it. I mean, if it's batch reaction, if it's continuous flow . . .

MOLINA: Right. Well, that's why . . . perhaps I didn't explain. This was a catalyst. So you needed very small amounts, as far as the large industry's concerned, and so again, I did it laboratory-scale first and then it was not too much problem to do it [on a larger scale] . . . what for us was the commercial scale, just with very large vessels. Okay? In fact, that was a big investment. We were able to—I remember, again—borrow money from these same connections we had because they realized this [would work], we were able to make enough to test it, but they were relatively expensive reactors because they were large stainless steel vessels. They were coated with glass because we had to dissolve tin in hydrochloric acid, so there [were] fairly nasty chemicals. So the reactors were expensive, but the rest [of the materials were] not particularly expensive.

So, yeah, there were some difficulties. I hadn't thought about these details for a long time, but yeah, we did manage with our connections, I guess, to do that. But you are right: normal scaling up, if you really do it industrial . . . in some sense, that's what chemical engineers do, so that was closer to our main topic, but for a catalyst, we could **<T: 55 min>** get away with almost laboratory scale in large vessels.

ROBERTS: So it's a group of friends, but you said that all of them are business-minded and you're doing the technical research.

MOLINA: Right.

ROBERTS: So what gives you the confidence to think you can go in and borrow somebody else's lab, do this research, start developing this catalyst, and then scale it up?

MOLINA: Well, because . . .—

ROBERTS: As a college student?

MOLINA: I realized, okay, you have to make a chemical that had been identified, with a certain standard weight; it's an organic chemical and there was, of course, no literature on how to make it, so you had to come up with some ideas and know organic chemistry . . . . So the trial experiments were not expensive, doing it very small scale. So it was, "Okay, well, let's try it. If it works, fine; if not, we don't lose very much," but it was the sort of thing that I thought I can tell if I have the right compound, because it would have these properties and so on. It's an organic salt of a metal. Tin, of all things, which is relatively expensive. So once I was able to make the organic compounds and know that we had metal, then I thought, "Well, this must be it.

Let’s try it.” So, yeah, maybe it was an adventure, but I was lucky. It was a very interesting experience, after all [. . .].

CARUSO: So another question I have is, again, you’re going through a college experience. You took on chemical engineering because you were interested in physical chemistry.

MOLINA: That’s right.

CARUSO: Chemical engineers, at least the ones that I know of, when they go out into industry, they’re more about problem-solving and scaling things up. They don’t necessarily do the basic research to develop new things.

MOLINA: That’s right, yes. But it’s mixed. Okay, so it turns out, just to change that a little bit, that the chemical engineers were sort of proud and they’re, “Now I'm going to MIT,” because MIT’s chemical engineering school is very famous. They did some of the original work with unit operations and whatnot. So a little bit of the culture of chemical engineers is that you have a very well-grounded, basic education and then you can do all sorts of things, like doing research or [something else], if you have the right education. That’s the only real thing I would qualify with what you said. So there are, certainly in academia, many chemical engineers doing research, some of it very scientific oriented, some other more engineering oriented.

CARUSO: Part of the reason I’m asking that question is you did comment on the fact that most of your chemical engineering professors were from industry.

MOLINA: That’s right.

CARUSO: They didn’t necessarily know how to teach.

MOLINA: That’s right.

CARUSO: But there were some academic chemical engineers that you were learning from, but you still wanted this route in research. And I'm curious what you’re thinking about as a student, in terms of, “Where do I actually go to become a research physical chemist?”

MOLINA: Notice a big change. The experiences I was telling you about before, I was in elementary school or just going back as still a very young kid, but in high school, I learned a lot more and certainly by the time I got to college, I was able to read literature and so on. So I was no longer very naive about that, so I knew where and how academic research was carried out and I knew at that time, yes, [Mexico is] a little different, but in the well-known universities in the US and in Europe, professors do research full time.

So I already had the correct picture and, being in Mexico studying chemical engineering, had made up my mind: as soon as I finish, I'll try to get a scholarship. I realized I was no longer going to be funded by my family, but there were not that many students interested in these [. . .] things, and so I knew there were probably scholarships available. So even towards the beginning of college, I already had more or less clear the idea that [I would go study abroad. To answer what you asked before on whether I] considered going to study in another country. Yes, almost from the very beginning, that was **<T: 60 min>** already a goal I had set up.

And furthermore, just to clarify a bit more, realizing that I really wanted to do research in fundamental physics and chemistry, that chemical engineering was a little bit of a detour. That was already clear in my mind from the beginning, and I was, in fact, not far from the truth. I could see that later at MIT, that as a student you have to take fundamental physics and chemistry and even quantum mechanics courses. , and so that helps.

CARUSO: Just a quick follow-up.

MOLINA: Yes?

CARUSO: What were you reading that helped you understand what it was to be a research scientist? Was it still biographies or were you . . . ?

MOLINA: No, no, no. That was only as a child. That was already textbooks and the news and probably some magazines. I don't remember very specifically . . . .

CARUSO: Were there any national journals for . . . ?

MOLINA: No. If at all [they] might have been international. At that time, it was also quite clear that we had to master English, not necessarily speaking it, but reading it because most of the [relevant] science literature was in English. So, there were no national resources [. . .] that were particularly important. [Perhaps in other] very specific fields, [but not in mine]. I did have some connection with very academic professors because of the classes I took in the science department, which were not official. I just went for fun because I realized that's where you could learn, particularly math. I was also interested in math at that time.

And so somehow or other, I began to develop enough connections with the academic world to know how it would work, but I don't remember specifically if there were some reading materials that . . . in fact, many textbooks, I remember—not many but at least some of them—had biographical notes and give you more general overview and so that was quite useful as well, I guess.

ROBERTS: So I was interested in a related question, and I think that was how did you become aware of the fact that the people who were teaching you were not experts?

MOLINA: Okay, yeah.

ROBERTS: You know, how did you experience them and realize there's something they're not telling me.

MOLINA: I realized by that time—to repeat what we were just talking about—I realized that they were not academics, so I might have been a little suspicious. By then I was able to read textbooks, to study on my own, basically. So it was my own stories versus their interpretation, probably with the help of these few really good professionals, one of them, and more in statistics. So there were certainly some exceptions.

I was able even to find the errors [. . .] they were making [sometimes], but I remember at that time, I might have brought it to [the professor] in class, but not very loudly, because the culture was still that the professor was the one talking and the students were not prompted to ask questions—not much interaction and even less to find errors, which is quite different, of course, from academia now, but you could picture that type of story.

You know what comes to mind—this is on the side—but I remember much later reading some of [Richard P.] Feynman's biographies, because he spent some time in Brazil. [. . .] I do remember finding a few places where he criticized Brazil's academic environment just along the same lines. They were sort of routine, and students were not asking or apparently not interested, but that was a cultural thing. So, **<T: 65 min>** he went there anyhow. I thought it might have been a good experience, but I was surprised at what [he did] . . . he learned more. In his books [his life experience] is excellent, but the science was a letdown in some sense. But it just reaffirmed my impression that it's not at all uncommon, and probably even in the US, except, of course, in the very best universities, in routine small colleges also where research or asking or questions also is not [common]. So that was quite clear.

In fact, I was talking about Germany. It's in part true of Germany, as well, which is something they try to change a lot because there you have . . . the image of Herr Professor was a very well-known faculty and no student would dare to interrupt or ask any questions, so that was quite the opposite from . . . I'm [. . .] here talking perhaps of my experience, already, after doing PhDs, because I spent first some time in Germany. In the US, what is excellent—but the Europeans are now changing that way also—is that you work with your mentor, you use first names and so on. In Germany, you would use last names, even for your schoolmates. So that was a different culture. But, I just noticed, because first I told you . . . yeah, in Europe, that's the way academia worked, but the human distance, if you want, is something that was more German-style than U.S.-style in these universities here in Mexico, except in the science department. There the classes were small and so you could approach your professor a lot more.

ROBERTS: Did you have much experience with English at this point?

MOLINA: [. . .] I knew what I called theoretical English, because in Mexico, obviously, we had English courses starting almost in elementary school. But again, looking in hindsight, very poor courses, okay, things we had to memorize . . . enough, eventually, to be able to read, but not to speak, so they were not geared to practical English. You had to memorize grammar rules, things of that sort; not particularly useful. What I remember, because of my interest, I somehow or other was able to read science in English. That was not a problem, but, yes, I did not speak English.

I remember when I, then much later, came to the States, I did have—oh, it didn't take that long—but I did have to learn how to speak fluently and not just how to read. But that's another story. In the US—that was in Berkeley—you had to, at that time for a PhD, you had to take a language exam. And so I remember, you had to translate from German to English and, of course, I had just been in Germany, so for me the problem was to write it in English, not to understand it in German. [laughter]

CARUSO: So I can sort of guess that you decided to do your post-baccalaureate work in Germany because of your familiarity with the language.

MOLINA: That's right. That's right.

CARUSO: But how did you choose?

MOLINA: And because I thought it was academically also something that was close to the top.

CARUSO: Okay. How did you wind up choosing the University of Freiburg and did your interest in polymerization kinetics, did that precede your time at the university or did that develop during the time?

MOLINA: That developed, but it was connected with my interest in physical chemistry and chemical kinetics is—since you study the rates of reactions—is sort of a central part of chemical engineering in terms of the more complicated parts of it, which is to design chemical reactors, okay? So you have to understand very well how chemical reactions proceed and so on. So it was more or less a natural extension of the influence I had from chemical engineering, but this time not with a **<T: 70 min>** goal of making it large scale. It was not the engineering component, but just the more fundamental science how those chemical reactions take place and so chemical kinetics is . . . you have to understand how molecules change, how fast they do it. So in some sense, it was an opportunity that I said, "Ah ha. Well, I can . . ." and plastics, of course, see that was still part of my engineering interest, as I developed . . . and plastics are so important for engineering. But it was a combination. I could do something connected with a very applied field and yet very fundamental.

And I remember . . . well, in Freiburg, there's a very well-known polymer institute [Institute for Macromolecular Chemistry], [Hermann] Staudinger [Nobel Prize in Chemistry, 1963] was . . . of course, I didn't get to know him anymore because I think he had already passed away, but he was a Nobel Prize winner for polymer chemistry. So it was something attractive to me.

CARUSO: So that's why the University of Freiburg?

MOLINA: That's right. That's right.

ROBERTS: Can we step back real quick, and I want to ask about the Chemistry Institute.

MOLINA: Oh yeah, in Mexico.

ROBERTS: When you were still an undergraduate, yes. So you spent some time over there because you had some spare time built into the rest of your course load. And I'm curious if you have any . . . if there are any names of specific people that you recall encountering there or working with?

MOLINA: Yes, I can probably give you the names of . . . it's so many years ago, but if I think a little longer . . . . There were three or four professors that I became particularly close to; I'll give you the names in a moment. Yeah.

There was one [in] physical chemistry—his name I do remember—[. . .] Javier Garfias; [I remember] because there were practically no physical chemists. He was one physical chemist, but I remember [. . .] I could not work with him because he did not have a lab, he did not have equipment. He was thinking maybe it will come here sometime, so he was not practical. [He was] the only physical chemist. Raúl Cetina, was [a] physical chemist also, but very organic chemistry oriented in this Institute.

So, these were the two most closely connected with the physical chemistry field. And Jose Luis—see, the name came back—Jose Luis Mateos . . . he's still alive, by the way. I was very proud because [. . .] in the chemical society here in Mexico they created a prize that used my name, [to recognize] life achievements. [It was an honor for me that José Luis received this prize because] I was a student [when he] was already a well-known researcher, [and years later he received] this prize with my name, so that made me very proud. But he's one example of somebody not in my field directly because he was really an organic chemist, but with whom I did have connections and some advice, working in the lab. [. . .]

ROBERTS: Well, I'm thinking about this time, too, because, in terms of natural product synthesis and organic chemistry, this time in the United States, at least, there's a large shift in the practice of organic chemistry taking place, mostly around the introduction of instrumentation. And you've already mentioned very specifically some work with gas chromatography. Was this experience also changing the work that was happening inside of the Chemistry Institute here?

MOLINA: I think so. Although at that time it was still very organic chemistry oriented, doing synthesis, and it's still a practice, as you know, in organic chemistry: a lot just to know how to make chemicals and to be able to come up with some complex chemical structure **<T: 75 min>** by a combination of synthesis and analysis, which, you're right, has changed a lot. But they had at least the basic machines, possibly NMR [nuclear magnetic resonance] and some others. But it was relatively primitive [. . .] at that time from what I remember in the labs, which is something that changed. I mean, the chemistry school bought the large NMR and things of that sort, and more physical chemistry oriented, but they did have the basic instrumentation.

I happened to remember as we were talking about names, the name of the U.S. scientist that was, I believe, at Stanford [University]. It's Carl Djerassi and he did some of this very important work in this company in Mexico [Laboratories Syntex SA], which is where this . . .

had this very strong connection with this particular institute.[1] So that was just the example. But, again, it was not very sophisticated instrumental analysis, as far as I remember at that time.

CARUSO: So I do want to hear more about your time in Freiburg and what you were working on and your advisor, but I also noticed that during your postgraduate work, I think, you also held an assistant professorship at the university here?

MOLINA: No, that was just before. See, I had sort of two waiting periods. The reason is, it did take some time to get a scholarship, and I don't remember the details, but it's much easier to get the scholarship once you have a degree. So there was a catch. Okay, so I got the degree and then I applied for the scholarship. I was able to—because I was obviously academically oriented also—I was able to work at the National University before I got the scholarship. And I did a trip to Europe lecturing already, [. . .] essentially creating one of the first graduate programs at that time, other than the organic chemistry, one where you could, in principle, get a PhD, but even that was very rare.

But I did create a master's program in chemical engineering, so that's when the experimental labs were received. They had some, because as chemical engineers we had to do experiments, but they were not at all research oriented; they were more teaching oriented. So, I was able to push for this and, of course, times were changing, so that's when at that time, perhaps, it was a time when [the] university began to change to be more academic oriented, to have more full-time professors. But it was just the very beginning of that time. So I was able to contribute to that, but it was just the right time.

CARUSO: I mean, it sounds like there's probably more to it than you just contributing. I mean, it's very nice of you to say it that way but, I mean, establishing a graduate program when you're there waiting for a fellowship seems like a relatively major event. I mean, what was your vision; or if there were others working with you, what was the vision for what a graduate program, a master's program, would be?

MOLINA: Yeah, the vision was a combination of these things we're talking about: making the chemistry school—which had chemical engineering, pharmaceutical chemistry, whatever—making all of it more academic; hiring—of course, at that time also, there was a problem with the salaries, but that all was part of the change—hiring full-time professors and doing research. And so I remember just pushing, "Well, you have to get people involved that are finding out new things here at the university," just like, [. . .] for example: "Look, we do have these organic chemistry centers," which by the way, this is also something fortunate as change, but it was in

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[1] Carl Djerassi, interview by Jeffrey L. Sturchio and Arnold Thackray at Stanford University, Stanford, California, 31 July 1985 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0017).

my mind. That Chemistry Institute with these very well-known chemists in connection, was completely separated from the university. None of them taught any course, practically, or maybe they taught one or two. So it was: what a waste having the potential to do research, but they had to . . . in some sense, I **<T: 80 min>** remember, they wanted to be sort of isolated from . . . .

ROBERTS: So what was its purpose?

MOLINA: To do research.

ROBERTS: Just to do research?

MOLINA: Right, because [there were] fourteen thousand students or so. [Researchers] saw teaching more as a burden, because if you had so many students you had to spend so much time: "We want to spend full time doing research." Because it was so disconnected from teaching, there were practically no students helping them. They were all individually doing their things in spite of the fourteen thousand. So it was a very different image. You see how things . . . so that was also part of my push together . . . "Well, let's see whether we can change all that." And this Institute, by the way, is still there, but it's much more closely connected with the university now. But yeah, those were different times.

But I forget, you were asking me . . . .

ROBERTS: About the graduate degree.

MOLINA: Yes, okay. Yes. No, that is . . . again, this was also the beginning. It was the right timing and I probably helped . . . .

ROBERTS: Well, the beginning, but, I think . . . like, what's your model? You're building a graduate program and you're just finishing your bachelor's degree. Where do you . . . one, what gives you the . . . what makes people in the university administration believe that you can even do any of this since you're . . . I can't imagine if I was graduating from college and pitching a graduate program that they would have listened to me, they would have taken note and put it aside after I left. But you're also . . . I assume you're looking for models of how other graduate programs might work?

MOLINA: Right. I do remember two things, again, [about] people. There was one of these teachers I was telling you, which was . . . in fact, he was not of Spanish origin. He was very closely connected with the Spanish community, Manuel Madrazo was his name. He was very academic oriented and became the chairman of the chemistry school and so he was a lot of help because [. . .] of his academic interests, we shared that vision of how things had to change. So I had some sympathetic ears, if you want, with some professors that fortunately had sufficiently important positions. And, in fact, some others . . . the previous chair was from the Chemistry Institute, a bit of an exception, somebody that decided, "Okay, I've done enough research, so I'll do some . . . ." So he had to spend quite a bit of time as an administrator because just of the sheer number of students and things to do.

So that was one example. A handful of—which was certainly not the majority—but a handful of professors or people working there that recognized the need to change. The other thing I do remember is we were able to invite a few well-known foreign professors. And I might have the times a little bit scrambled . . . I'd have to look it up, but I remember we invited some well-known German professors and I had a great time with them. They were [a little bit] more organic chemistry oriented, so I remember doing some field trips with them and they offered a lot of help. It was a little bit part of the help going to Germany.

But also what was quite important . . . . There must have been some connections so that we did invite—and he came afterwards a couple of times—a chemical engineering professor at Berkeley whose name was [Theodore] Vermuelen, Ted Vermuelen, a very nice person—of course, I was just a student. He was a good professor, and he came to give us some lectures; that was great. But we were able to work sufficiently close with him. I even remember having this time some discussions. He had some ideas, had to do with chemical kinetics, and I said, "Well, but what about this and that," and he got sort of excited. So it was nice to interact with somebody at that level, but this became important because there was this small group of us, several other friends who were . . . actually, **<T: 85 min>** well, just a little bit younger than I was, because the difference is I went to Germany, spent there a couple of years, whereas these other friends were just coming up. We were admitted to Berkeley; that was unheard of: there were no Mexicans. I mean, who would admit a Mexican student coming out of this type of school like I was criticizing? Okay? But this professor, I mean, having interacted with us, suddenly the door's open, and so that made it feasible to be admitted to Berkeley's graduate school, and that's why I chose chemistry. Of course, my friends went to do chemical engineering, several, a couple of them. There were only three or four, but, in fact, one of them that's still [a] very good friend of mine, [Francisco Barnés de Castro], became Rector [and President] of the National University. So these were people that were well known.

CARUSO: So I think now would be a good time to return to Germany.

MOLINA: Okay.

CARUSO: So you were going there to do postgraduate work. Was this some sort of equivalent of a master's program?

MOLINA: It was open. Yes, I had . . . I remember, it's something I didn't have very clear whether the master's was a requirement for a PhD, but it was clearly graduate school. And I do remember it was nice. I was able to do experiments and so on. The one thing that was missing, which I realized from whatever I knew even knowing Ted Vermuelen, is this closer contact with the professors: you had to do things on your own and you didn't have that much access to the professor.

And you could go and sit in some lectures perhaps, but for me, what at that time I had realized already is that what was missing in my undergraduate education was more the physics, the quantum mechanics, the much more molecular perspective, which, by the way, now is a very integral part of chemical engineering, but at that time it wasn't—not even in the U.S. That became, slowly, a very important part of it. For example, for chemical engineers, again, catalysis is terribly important, so you have theoretical chemists working in chemical engineering departments nowadays.

But in any event, the German system was not easy to take advantage of, and by that time I became much more aware and I knew more details about how research was carried out. And so I decided sort of halfway, "I think for this interest I have, I'm going to change and I'm going to go to the States. This time here in Germany is not wasted, but I much prefer this environment in the U.S. for a PhD," where you work very closely with a mentor, and where I would have the opportunity to formally take courses in physics, quantum mechanics, or mathematics, still, that I had missed as a chemical engineer in Mexico.

And that turned out to be correct, in fact. So, fortunately, I did have enough information so that it actually happened that way. In the US. I was able to take these courses, I was able to work a lot more closely with my mentor, George [C.] Pimentel, and I don't regret having been in Germany, but it was a very different experience. In hindsight, it's possible that if you do very well in your undergraduate courses you already know what you want to do, you don't need much advice, you're not changing fields, that you can do a great PhD. But it's a lot harder—or it was a lot harder—under my specific circumstances.

So, again, that was my time in Germany. In fact, it was not even required, doing research. You could just be all day in the lab doing routine things without even taking courses because there was not . . . it's only a formality when you are an undergraduate **<T: 90 min>**. And taking courses, people just go on and listen to a professor. It was not interacting with him. And, again, I became aware in Germany, they were trying to change a lot of things, but that . . . I didn't get that change myself. So that was my experience in Germany. Basically, at some point, it was again waiting to get admitted to Berkeley and to get a scholarship. So I had some time . . .

CARUSO: That's what I was going to ask. So did you stop doing what you were doing in Germany before you had received acceptance to a graduate program or . . . ?

MOLINA: Yes, yes, because I realized I need to now pay full-time attention to getting admitted in Berkeley. So it was just a matter of timing, I remember, because you have to apply almost a year ahead of time. I did something also unusual, like I had done something in Mexico in between, which we talked about, but this time it's like, "Okay, why don't I . . . I want to remain in Europe, but have a different experience," so I went to Paris, [France], because in the Sorbonne [University of Paris-Sorbonne], they [were also doing research on polymer chemistry].

[.. . .] I had decided not to get a PhD in Germany because of these things we talked about. So having decided that, I said, "There's no advantage for me to remain another year in Germany if I am not pursuing the PhD, so why don't I change course and see how things work in Paris?" It turns out from an academic perspective it was not all that different: you have very big classes and you could listen, you could do work in the lab, but it was just very easy to get admitted to the Sorbonne.

So I was admitted to the Sorbonne just as a graduate student [. . .]. Of course, I didn't tell them this—but in my mind it was just a waiting period. And it was a time in my life there where I realized, well, I can do much better studying on my own. So I ended up studying math and so on, [since] I had a lot of time. At a very interesting moment in a student's life, because that was in the 1960s—1968, 1967, with all the student unrest—so I was just able to participate, had a good many friends in Paris. I had sort of an important component in politics, cultural life, and other things, and did not neglect the science. But the science was essentially on my own at that time because there's not that much I can do at the university other than . . . well, the reason is the following. It would be the same reason I decided not to remain another year in Germany—I could have started the research project, but it didn't make any sense. I was already applying to Berkeley. I had good prospects knowing Ted Vermuelen, so it was essentially just saying, "Look, I have this opportunity. I learn about all these other aspects of life as well. ." I remember I realized I had to learn more, essentially, math at that time, also physics as well, which was useful then to get me ready to actually do the PhD in Berkeley. So it was more or less along these lines, so I did not give up—as I'm thinking back of those years—I did not give up my goal of becoming a research scientist. I just did what I think in hindsight was actually the right decision: "I think I can do it much better in the US, given my specific background and the changes I wanted to undergo." So that was it, essentially.

CARUSO: Were the classes conducted in French?

MOLINA: Yes, yes, of course.

CARUSO: And you were . . .

MOLINA: Yeah, [. . .] German was much more difficult, but I had learned . . . one thing I did is take some French courses as well, but that was not particularly **<T: 95 min>** . . . I could understand. A bit more of a challenge was to speak, but there was not very much problem just attending the classes. I don't remember if I started learning French . . . probably when I was in Mexico I might have taken some classes. Yeah, [here] there is a [French school, Alianza Francesa] where I remember just going to take some French lessons [when I was young] just for general culture. It was not very scientific at that time. French was a very important language in Mexico at the beginning of the twentieth century because all the European—

GONZALEZ: We had a president . . . Porfirio Díaz.

MOLINA: Right.

GONZALEZ: A president that was very, very interested in French culture. [David and Jody are] staying at the Hilton in Reforma [Mexico City, Mexico]. You're right in front of a very large white marble building, beautiful. All that type of architecture in Mexico was started at the late [1800s], early [1900s] with that president. He was in power for thirty years and those thirty years we had all types of French influence. He was very Mexican . . . very Mexican, but he was very interested in France, for some reason.

ROBERTS: The US has presidents like that, as well, who love their French architecture, right? The only other piece with the time in Paris at the Sorbonne, you mentioned sort of briefly the student protests. What was your experience, your take on being there at that time?

MOLINA: Well, I do remember being part of it, with Jean-Paul Sartre and all that. And besides reading or learning math that I was telling you, there was all this cultural component—which it was not something like we were talking [about] initially in Mexico, that once you go to college, you don't do that—but it was part of my general cultural interest; which by the way, something much more recent, I've reinforced it a lot more in terms of history, even [in] teaching. , okay? There's [. . .] that cultural component which is very important, which you could easily neglect if you're a single-minded scientific research person that only worries about your own specialty. It's obvious that you need to do things, [. . .] all sorts of interactions with other fields. So in some sense, that was a beginning of that experience because it was also cultural, of course; the cultural life in Paris was active at that time, not just concerts, but the theater and things.

But there was student unrest. You could go to the streets and participate and read about it all and go and see what was happening. [. . .] I did have interactions, also, I don't remember . . . that was in fact quite interesting and important. There were a number of Mexican students in

Paris at that time and, being Mexican, we were friends—we got together. But none of them were scientists; they were all in other fields and several of them became very important afterwards: a foreign relations minister was there at that time.

Because we were young, they were part of that generation. But it was, again, this . . . we call it in Spanish '*vivenciar*:' this living something, experiencing something actively rather than just reading about it. It was discussing politics and discussing these issues with students and so on. And literature . . . I was already interested in Latin American literature, so that was an interesting time, which in hindsight, again, it was not bad because I had to change quite drastically, then going to Berkeley. , because getting a PhD in Berkeley and changing [the focus of my interest] topically, I had to devote full time there to do the studies, except for the little bit in Berkeley was also the time of student unrest. , okay? So I didn't have [a lot of time to spend]. You could just see that. But . . . **<T: 100 min>** I didn't have time to read literature or to go to the theater or to do the sort of things I could enjoy in Paris. So it was fun [in Paris at that time].

ROBERTS: Were you ever tempted while you were spending this time in Europe to rekindle your music career?

MOLINA: Yes. In fact, I did something which might have been . . . I switched to learning guitar because I thought that might be easier. That's probably a mistake; it's not any easier. But I did learn to play classical guitar, so that was . . . so, like Spanish guitars. And again, just for my own [pleasure]. I regret very much that I couldn't keep that up when I was in Berkeley. So I certainly regained my great interest for music, but more for listening.

Maybe I'll take it up again because just doing music . . . to tell you more, the last bit of really enjoyable music I did was still in Switzerland when I was a young student, still taking classes, because I did get to realize how wonderful it is to play. I played well enough—I was very young and so on—to join a student quartet or something, to play with others.

So when you do music and you are part of a group, then it becomes a very unique experience. That's what I remember and I regret, because I have several examples of scientists that, like . . . [Albert] Einstein, of course, is the best known. He was apparently not all that good. But anyhow . . . but he did like to do . . . and, in fact, at MIT I have few friends, professors . . . so those who could keep up their interest to the extent that they play an instrument. It's something I regret not doing [. . .], but, anyhow, that was that part. But I always kept listening to music very actively; I do remember taping all sorts of things in Paris.

CARUSO: I know you mentioned . . . I mean, you had this interaction with the Berkeley professor. Did you consider applying to any other American universities or was it just Berkeley?

MOLINA: No, not really, because Berkeley was very well known and we had this connection, I thought. There were some which were vaguely familiar with some other ways to get a scholarship, where you just got the scholarship and yet you were assigned some college somewhere. I thought, “Well, that doesn’t compete at all with Berkeley.” So once we had the connection, then I became confident that we’re going to be able to do something. And, indeed, so that was not very difficult afterwards.

The only thing I remember in Berkeley . . . again, I must tell you about the misconception I had from Europe, [which is that] I applied as a master’s student because I thought that was a requirement at the time, and then, of course, Berkeley, “No, you get a master’s a little bit as a . . . for PhD students that cannot make it, okay, we’ll give you a master’s.” Okay. But it was very easy for me to change. [. . .] I mean, they asked me, “Do you really want . . . ?” “Of course.” [. . .] It was just a misunderstanding that it was easier to get admitted as a master’s student. But, again, in the academic department in chemistry and so on, there are relatively few master’s students or students that apply that way.

CARUSO: I want to know a lot more about your time at Berkeley, but one question I'm also curious about . . . you spent time living in lots of different countries with different cultures. You mentioned that you had a reading knowledge of English, but you weren’t necessarily conversational in it.

MOLINA: That’s right.

CARUSO: Was it difficult . . . was there a culture shock in coming to the United States or was it just the language barrier for you?

MOLINA: I think it was just the language barrier, and it was not a big deal at all because, again, I did understand. I just remember the fact that I was very fluent in theoretical English. I had no problems reading any science textbooks; and then I realized, “Hey, but it’s different to really communicate.” And it’s sort of funny that, **<T: 105 min>** probably, the English teachers I had in Mexico, none of them spoke English. So that was part of the [problem], they were just teaching. But, no, that was not an issue, essentially, the language barrier is something I probably took relatively little time [to overcome]. And, well, I do remember a few funny things. I joined Pimentel’s group at the same time as another student from . . . I believe it was New Zealand, and we could barely communicate. He had such an accent. So, no, I couldn’t understand all English. It took a while for me. So that tells you about my limitation, but American English was relatively easy.

And, no, one thing to note [regarding culture], I was, again, in an academic environment. And it was interesting because in Berkeley there was People’s Park, lots of hippies, so it was interesting, unusual . . . and not particularly a cultural experience because the hippies were very primitive as one thinks of them and just singing, but there were things going on that I went to a few . . . again, I didn’t have much time, but there were always interesting people that were invited that you could go and see, but I . . . for example, I didn’t have much time to go to concerts or anything. Anyhow, I was listening to music on my own. But, no, I knew more or less what to expect. And, again, in academia, if you look at it in hindsight, it can be quite a different cultural experience than if you go to some small town in the South. That would have been a cultural shock, I would imagine.

CARUSO: So starting your graduate career, I'm assuming that Berkeley had you taking courses to start out with?

MOLINA: That’s right. Yes.

CARUSO: How were those courses different, or were they different, from what you had experienced previously in your other . . . ?

MOLINA: [Yes], they were different, but it’s what I was expecting, in that you really had to put a lot of time, and it was the homework but also the interaction in class. They were graduate courses, all relatively small courses, sorry, relatively small classes with students and it was just a much more active . . . I mean, I was able—remembering back in Mexico, so I tell you—to take it very leisurely because they were not very demanding. You just had to do the routine things. But I knew there, you really had to understand what you were learning, and I was taking demanding courses.

I did take, by the way, some—to finish [this thought]—some sort of upper division or graduate math courses, and there again, that was not easy because you had to participate quite actively in those courses and it was a lot of work, but it was very satisfying. I was worried, “Will I be able to change fields and do this?” Fortunately, I was rewarded, if you want, because in the courses, you also get the feedback. If you just listen to the professor, eventually perhaps you have to do the final exam, well, maybe you get lucky or unlucky, but if you have a much more active participation, then that’s much more rewarding, but it’s a lot more work. So I had only, I guess, maybe three, four courses at the beginning, which was a lot. I, on purpose—and I talked to my professors that it was all right—I was going to take a bit more time at the beginning than a normal graduate student because I did want to take these courses and that worked all right.

CARUSO: Were you doing laboratory work at the same time?

MOLINA: Well, at the laboratory work was when I started doing the research. So I started . . . and again, I was able to postpone it at least one semester or something like that. And then I really became integrated in a research group **<T: 110 min>** with Professor Pimentel and that was essentially full time. Well, I could still take one or two courses, but at that stage, it was full-time research, original research.

CARUSO: Did you get to choose what lab you wanted to go into?

MOLINA: Yes. Yes.

CARUSO: And how did you choose Pimentel's lab?

MOLINA: I chose based on what I knew the different professors were doing research in, which topic, and again, it was something . . . very fundamental physical chemistry connected also with the nature of chemical reactions. I had several options. There were some very good people, but to me it was quite attractive, the type of research that Pimentel's group was carrying out. He was also very well known in education, had some very important textbooks.[2]

So it was an attractive group to join. In some sense, I was a bit surprised, but I had no trouble being admitted to the group because each professor only takes a couple of students each year, but I was lucky or had the choice, but that's also . . . later on, of course, I've been a professor myself, you talk to . . . many students come and see you. You talk to them and some of them choose to work with you and occasionally, the student doesn't get his first choice and then he goes somewhere else. But I was lucky, so I joined the group I did want to do, which was experimental research, but with very fundamental chemistry, having to do with the nature of chemical reactions.

CARUSO: So there were eight or ten graduate students in his lab at any given time?

MOLINA: He had a large group. He was probably more like fifteen or so. No, I take it back. Maybe ten. But he had five postdocs and maybe two visiting professors. He was a very well-known person already at that time.

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[2] Axel R. Olson, Charles W. Koch and George C. Pimentel, *Introductory Quantitative Chemistry* (San Francisco: W. H. Freeman), 1956; George C. Pimentel and Aubrey L. McClellan, *The Hydrogen Bond* (San Francisco: W.H. Freeman), 1960; George C. Pimentel (editor), *Chemistry: An Experimental Science* (San Francisco: W.H. Freeman), 1964.

CARUSO: And did he give you direct attention once you got into the lab? I know some professors, they're busy with their research and they don't necessarily have a ton of time to work with graduate students.

MOLINA: There were two things that happened. One, I did work with a postdoc that was there, closely. In terms of the experimental details, I was able to learn from him, we had very regular group meetings. I mean, the group meetings, it was up for grabs. Everybody could talk and then besides the group meetings, we had something like at least a weekly meeting with just myself and the professor, so it was set up to have very close interaction at that level. And you're right, there are all sorts of different personalities. There are some professors that every day go to the lab and see—but more to push you—see whether you produce something or not. And some others tell you, as I remember afterwards having fun with some of my physics friends, "Okay, here is your . . . "—a little bit more like German—"This is your research project. Come and see me at the end of the year and see what you . . . ." So everything goes, but the normal, the most productive thing, most good professors were that way: you had a fairly important interaction quite often with a group at large.

ROBERTS: How big was that research group?

MOLINA: Well, the group, again, it must have been fifteen, eighteen people or so. And again, some visiting professors, postdocs, and graduate students, but once you're there, there was no difference. We were all the same in some sense, except at the very beginning, we were shy and learning how things worked, but very rapidly, you could get integrated. Yeah.

CARUSO: Were you assigned a project or did you develop a project with Pimentel?

MOLINA: No, it's quite open. I was assigned the project as a result of talking to him, because in some sense, there might have been a few options, but with the idea that the project could evolve and then you could talk about it and change direction a little bit. I didn't have to change much. The project at large was working with chemical lasers, which was something precisely that Pimentel had found with one of . . . a German visiting professor; in fact, they discovered the first chemical laser just shortly before I **<T: 115 min>** arrived, maybe a year or a couple of years or so. [3] And then it became clear that you could learn a lot. It was not applied, the research, in terms of developing lasers based on chemical reactions, but using the laser emission as a tool to learn about the reactions. So that's what I essentially chose and did as a research topic.

[3] Jerome V.V. Kasper and George C. Pimentel. "HCl Chemical Laser." *Physical Review Letters* 14, no. 10 (1965): 352-354.

CARUSO: So molecular dynamics?

MOLINA: That's right. That's right.

ROBERTS: And so using chemical lasers is a relatively new research area at this point.

MOLINA: That's right. It was very new because lasers had been discovered not that long ago, I remember. Yeah. [. . .]

Of course, Charlie [Charles H.] Townes [Nobel Prize in Physics, 1964] was at Berkeley and I remember going to his lab. [. .] I didn't meet him, but was very envious at Berkeley that he had a space assigned because he was a Nobel Prize winner. And it's fun because later we became good friends. He's very approachable, so I got to know him very well, but imagine at that time, I was just arriving.

And this is just in passing, but why not I mention, we were . . . let me spend half a minute, because it's sort of fun. I was just at a meeting in PCAST [President's Council of Advisors on Science and Technology] at the National Academies and we were worried about [United States] Congress . . . . . . some Congressman who should have taken science in elementary school but didn't. They wanted to impose some restrictions on NSF [National Science Foundation], that NSF would have to guarantee that the research was for the benefit of the nation, which is just crazy.[4] I mean, you have the peer review system, but it's very easy to find names of projects that are very weird. Okay, so it's, "How the hell can they tell?" And so we chose Charlie Townes, because he had a very academic, very unusual name for the research he was doing. There was "masers" and whatnot. Just as a classic example of something they wouldn't have the foggiest idea what he was talking about and that's nothing. There's then the laser, okay, which we use every day in so many things. So it's a perfect counterexample of something that you expect to be applied immediately. No, that's not the way research goes. But anyhow, it goes back to Berkeley to those times when the laser was just coming up to speed.

ROBERTS: So this PCAST meeting must have been very recent because it was . . .

MOLINA: Yeah, this one was just last week, sure.

[4] A Discussion Draft of the High Quality Research Act began circulating in April 2013, http://news.sciencemag.org/scienceinsider/HQRA13_001_xml.pdf, (accessed 30 May 2013).

ROBERTS: That Congressional proposal was just made I think a week or two ago.

MOLINA: That's right. We're going to write a letter or something. We're going to complain. [laughter]

ROBERTS: I'm glad that you'll be doing that *en masse*. So was there a particular interest in working in this area because it was so new?

MOLINA: I guess so, because it was new and interesting because it was a new tool that still had to be developed and that could provide you with some insights into chemical reactions that were unique also, which, in fact, was the case because we learned a lot. I mean, it worked in some sense that we were able to do original research. But again, this is going . . . it took a while, of course, but if I start from being interested as a child and then doing things in high school, in college learning about it, but then it was really at Berkeley that I could, for the first time, do something new in research: create something or learn something that had not been learned before anywhere and be able to write some original research. In some sense, that's experience I was looking for since I was a child and it did materialize while doing the PhD. It was quite interesting.

And that's something very nice, if I push it a little further, something that comes out of your interaction with a group, with a professor; but eventually you create that. You find, "Ah ha! Here is something that I found." It was not something I was . . . because sometimes the professor tells you, "Do this and this experiment because we'll get the results and we'll publish them." That's what I meant by **<T: 120 min>** flexibility. You work in a certain area, but if you do things right, you could find your own interpretation, specifically how the laser light comes out and how it interacts with Einstein's equations, and so it was very rewarding in some sense. That keeps you going as a research scientist.

ROBERTS: It sounds like the chemist's version of what you said you experienced being on the field trips with the entomologists.

MOLINA: That's right. Yes. That's right. That's right. This excitement, exactly. Something new there. Yes. Not requiring a lot of math, but observation and knowing, of course. You have to know what you're doing. So yes, you are right.

ROBERTS: You didn't have to fight off any snakes at Berkeley?

MOLINA: That’s right.

ROBERTS: So nobody had to tend to snakes.

CARUSO: Just hippies.

MOLINA: We did have one experience, although not quite with snakes, but since we were working with chemical lasers and there was student unrest. I remember at some point, we were very worried we would have to close the lab because some of the unrest was trying to find things that were going wrong. “Chemical lasers, that’s a weapon! So we’re going to fight against that.” We were not . . . we had nothing to do with weapons, but the Army was already using some chemical lasers there because they were very powerful. So we got worried and we were in the basement, but yes, that was a real threat. Okay, not snakes, but in this case, our own fellow students.

CARUSO: Were you involved in the writing process for publishing articles coming out of the lab?

MOLINA: Yes, yes; particularly doing original things like this, yes, very much so. Of course, with a lot of help from the professor, but yes, in principle, we were writing our own articles and then sometimes it was part of a larger piece of research also, but yes, that’s part of the job, to develop the skills to write scientific articles.

CARUSO: Did you find it difficult . . . I mean, it is a very stylized way of writing. Was it difficult to pick that up?

MOLINA: Yes, it probably did. I don’t remember the details anymore because probably we were so close to, in our case, to our mentor. It's not something I saw as a barrier, but it was just a learning process. I do remember it must have happened quite naturally because once I became a professor afterwards, it was never a big worry. It eventually became a worry with my own students because I had some Russian students that were writing terrible English, okay [laughter]. It’s time consuming. You have to correct, you have to rewrite things. So that’s something just that takes time. But you’re right. It’s a very special language, which is fine for scientific articles, but talking a little bit about the sort of things that we were doing, it’s terrible for communicating to the public, of course.

Yeah, but it’s part of your learning experience to be able to write. Not only that, it’s a similar experience to go to a conference and give talks, so that’s also connected to that. You had to learn how to talk to your fellow scientists.

CARUSO: So I have a few questions. Some of them relate directly to the work that you’re doing, but there’s also an outside question. I know that you get married in 1973, and yet your description so far has been that you had no time for things outside of work.

MOLINA: Right.

CARUSO: So I am curious to know a little bit about how that came about, but I'm also . . . one of the other questions I'm curious about is I know that you continued on to do a postdoc at UC Berkeley, and I'm wondering how it was defined, you know, when your PhD really ended and when your postdoc began, because some of your work is continuing.

MOLINA: Right. It was essentially a decision of my professor, Pimentel, who said, “Hey, you’ve done enough. You already earned a degree, but we have this other thing, so why don’t you stay another year?” And it also gave me time again, as you see, I'm not very good with planning way ahead, but it gave me **<T: 125 min>** time to define the next step, which was going to work with Sherry [F. Sherwood] Rowland. But yes, it was essentially that. He thought, “You have enough for a thesis. Why don’t you write your thesis? Of course, if you want to leave already . . . ”—I have the degree, I could have done it—“but we have these several exciting things that you propose that we could keep doing that.” So that was essentially what happened.

In terms of my [first] marriage, as you know, I have a second marriage now, [. . .] I lived many years with my first wife, but she was a student of George Pimentel as well, so that tells you I didn’t spend too much time looking around. [laughter]

ROBERTS: So were there many female students?

MOLINA: No, there were not very many female students at that time. I'm trying to remember. No, it was mostly male students, there were just a couple of female students. In fact, I don’t remember. I think at some point, she might have been the only female student, but I don’t remember exactly. That’s a possibility. But then later, yeah, there were other . . . of course, there was no, as you know, no actual discrimination. It was just less likely for women to do research at that time, which is something that fortunately changed a lot.

CARUSO: How did you meet Sherwood Rowland. I know it was while you were at Berkeley.

MOLINA: Yes. He was working also in chemical dynamics. So it was very natural as I was telling you, it was part of our experience to go to meetings. And again, George Pimentel was a wonderful mentor in many respects, but this was one of them that he encouraged us to write papers and to go to meetings, usually with him also. So he would bring . . . students were not at large formal meetings, but meetings of . . . in California, for example, I remember this one . . . . I had met Sherry Rowland on several occasions at the meetings of this sort and had similar interests. He just had an entirely different experimental approach to learn about basically similar questions that we were posing about the nature of chemical reactions. And his approach had to do with radioactivity.

So it's what's called "hot atom chemistry." And so you prepare . . . in a nuclear reactor, you can have some atoms at tremendous speeds. Well, eventually they slow down and you could study unique aspects of certain reactions. So to me that was attractive. Hey, this is quite complementary but it's of interest to pursue this quite academic research. I was assuming I would remain in academia if I had a chance, and working in other aspects of physical chemistry, like [many of my colleagues], so this was a natural thing to do and Sherry Rowland was a very pleasant person. In these meetings, we were able to socialize a little bit and so on, so it was natural to talk to him. I remember I did consider several others, but this was one of the most attractive experimental research groups and again sort of unique, interesting, something quite different that became an opportunity.

ROBERTS: So before we move into the postdoc, I had two questions about your experiences as a graduate student. One had to do with whether or not you were responsible or had experiences looking for funding, or was that something that the advisor does and the graduate students don't have to worry about that sort of thing?

MOLINA: Yeah, at that time, that's something I did not have any experience. I later learned, of course. You have to write proposals and it's a little bit like writing papers. Okay, so that experience is important. But no, that part was not something I had to do while a graduate student. **<T: 130 min>** I mean, we were aware of some of the difficulties or the things that . . . and some important science funding came from the Department of Defense at that time. It was for very fundamental physical chemistry which was sort of strange, but it was understandable that these were important developments in science. Some other was connected with NASA [National Aeronautics and Space Administration]. One of the other interests . . . George Pimentel had a couple of interests: chemical lasers was perhaps what he might have been best known for, but the other one was working at very low temperatures, doing spectroscopy at very low temperatures. And so that was more connected with some peculiar properties of matter at these very low temperatures, which is why he became interested also in life on Mars. That's why I will tell you two different—connected through the fundamental chemistry—but two very different fields, chemical lasers and chemistry at low temperatures, with all sorts of important connections from the point of view of the fundamental chemistry, looking at different aspects of that. But spectroscopy was very much at the core of these things. I mean, we became aware of that because life on Mars was one of those things that could get funded or could get a lot of attention, more in the public domain rather than the fundamental chemistry we were carrying out, other than chemical lasers. Yeah, but that's interesting. In hindsight, it's not something we usually have students learn and perhaps it's something we should.

ROBERTS: Was it surprising to you to learn what the variety of sources of funding were for the lab that Pimentel was doing?

MOLINA: Yes, I guess it was surprising, but then I realized what the reasons were. But it was all government funding, essentially. I became aware also [that] more of it was funding from industrial sources, but much closer to applied research or perhaps to organic chemistry or things of that sort. So it wasn't all that important, after all, just different government agencies somehow or other financing fundamental research.

ROBERTS: My only other question was around becoming a part of the larger community of chemists and scientists. You were talking just a few moments ago about how you met Sherry Rowland and the importance of some of the meetings that Pimentel put together, but that these were smaller meetings sort of California based. And I'm thinking about you as earlier in our discussion you said, you know, what U.S. institution, especially Berkeley, would ever have a graduate student come up from Mexico City? I'm thinking about you entering into a larger community, like the American Chemical Society, and how you experienced becoming part of that larger community as a graduate student, if you did it at all?

MOLINA: Yeah, that's quite interesting. I should have clarified. The smaller California meetings, that's where I had a chance to meet Sherry Rowland, but in fact, working with George, we did participate in some of these larger meetings, like American Chemical Society. So it was also an—[coughs] [speaking Spanish] [. . .]

It was an interesting part of that academic experience to participate in these larger meetings. At that time, it was not that common as it became later to have poster sessions because there you could have every student doing something. They were not that common. So it was **<T: 135 min>** possible for us students actually to make some presentations, or at least to listen to others, but I remember doing some presentations myself.

So that was quite interesting, that learning about the larger community. And after all, I had a chemical engineering background, so I knew about the industrial aspects of many of the activities of the American Chemical Society. But yeah, that was something interesting to do to the communication with the society at large.

But one thing that [a little] later [. . .] in my postdoc career [became] a special [type of] world. You communicate with a certain language and you talk to a certain group of people so it's a little bit narrow. If you then address a real life problem, it's something you're not trained to do. You have to be able to communicate with specialists in other fields and to communicate with people in other fields as well, which is not part of the normal training in an academic environment.

But I was, in some sense, forced to do that, again, with my subsequent research, the postdoctoral research, but it was not part of my PhD.

CARUSO: So do you want to take a short break?

MOLINA: If you want; five minutes or so. [. . .]

[END OF AUDIO, FILE 1.1]

CARUSO: All right, so like I just mentioned, I think we're going to talk a bit about your postdoc time at [University of California at] Irvine working with Sherry Rowland. You mentioned that he was focused on hot atom chemistry, chemical properties of atoms with excess translational energy. When you went to Irvine, from what I understand, you received sort of a list of different things you could work on while there and the CFC project was the one that was most intriguing.

MOLINA: Right, and it was quite unusual in that . . . [speaking Spanish] [. . .]

Okay. Yes, what was unusual is that Sherry Rowland had this [notion of integrating different research areas]. Normally it's reasonable for a postdoc, as opposed to a graduate student perhaps, where it's more likely that you assign him to a project or it might be flexible, as a postdoc you sometimes have different options; sometimes you don't. But, Sherry became interested in this issue that he had learned about in one meeting, as you know—I don't know if you have that background, but somewhere I sent to you; anyhow—I guess, the American Meteorological Society or the American Chemical Society, I don't remember which organizations, but they very wisely decided, "Hey, let's have some meetings where we have scientists from these two very different disciplines, see what can come out of that," but very much oriented as meteorologists were at that time just using chemicals as tracers.

So the chemists in some sense were playing a secondary role there. If you can analyze and find something in the atmosphere, then you can help the meteorologists learn about how things moved. I didn't quite know that at that time; I realized that later. But, one of those set of chemicals, it's Jim [James E.] Lovelock had just invented this electron capture gas chromatograph and he was able to measure the CFCs [chlorofluorocarbons] industrial compounds and not of natural origin, although at that time, there was some question whether perhaps small amounts were coming out of volcanoes, a vector, so that was really not the case, or if at all, they were extremely minute amounts. So these were—it's a unique situation—industrial chemicals in the atmosphere, but the CFCs had a lot of affinities, if you want, with the types of molecules that both myself, with George Pimentel, and Sherry Rowland were using, not because they were important by themselves, but because they offered unique opportunities to learn about fundamental chemistry. One of the core atoms is fluorine. Okay, so CFCs . . . and I had studied the chemical lasers produced by some similar molecules that had halogens. Just to remind you, [it just dawned on me] but it's not that far-fetched. Chemical dynamics was just evolving at that time.

So we were interested, and we were not theoreticians ourselves, but I remember even in my exam in Berkeley—and by the way, that was another experience as a PhD student that was quite unique. I'll tell you about that later—but because this came up, understanding chemical reactions from first principles **<T: 5 min>** was just beginning to be possible. Even now, it's still . . . quantum chemistry is complicated, but one of the few reactions that you could more or less do calculations and so on was F plus H2. Because it's very simple and small molecules, so it somehow or other came up during my PhD exam, which in Berkeley has the peculiarity that your mentor is not there, so you have to defend it without help of your mentor.

But I'm just bringing this up because it tells you what the connection between all this is. And we saw, hey, that's interesting. These are the molecules we understand and they are found in the atmosphere. So Sherry proposed—I knew that he was not an atmospheric chemist at all—but he proposed, "Hey, why don't we learn a new field where we can apply this fundamental chemistry that we're interested in," because as opposed, say, to complex biology, this is a place where small molecules are important.

And so this appears to be a very nice excuse. You find the problem, you try to solve it, and you learn the new field. We had—in one of these small California meetings I was telling you about—we had one presentation about atmospheric chemistry, which was way up in the atmosphere. It had to deal with chemistry of ions, which are essentially not connected at all with the sort of things we were interested in, but I guess in the back of our minds was in one of those common conferences that we were, that this was a place where you could apply these things. I realized, oh, they still have so much to learn that it's sort of primitive.

So it certainly attracted my attention. I thought, "This is different from the other areas that he's proposing," [. . .] I could see perhaps more routine research. You could learn about these reactions, but if you're looking for a place where you might have an opportunity to do something new and different, these look very attractive. So that's essentially how we started. Basically, the only thing we knew from this meeting and later was published in a [very important and relevant] paper[5], that these compounds were measurable at very small concentrations, which were reasonable concentrations to expect from the industrial production up to that time of these compounds.

Then learning a little bit more about those with a unique characteristic that these were very stable compounds, so they would probably survive in the atmosphere, but the question was, what happens? It's a very open question. What process would destroy these molecules in the natural environment? Biology, or some chemical reactions yet to be discovered? And so that essentially was the way we started and from the very beginning, being quite different—although, of course, we interacted with all the other students—but this project was different from all the others; and very open. So I guess Sherry did trust me enough to say, "Okay, this is something quite different where we're both beginners," okay? It's because normally would you go and do a postdoc with somebody very well known in his field and you work in his field. So this was different from that perspective. That's how we started.

CARUSO: But it was within a relatively short amount of time that you developed the ozone depletion theory.

MOLINA: Right, because with that question, you might think, "Well, what can destroy these things? And there might be some catalysis in the Sahara Desert, gee whiz, but that's probably . . . if it occurs, it's not very important." I just systematically [thought], then that's something I have to do myself. Of course, [I] could interact with Sherry quite a bit. But in some sense, we were both at the same level analyzing these results. And said, well, no, none of these things **<T: 10 min>** are likely to be important, but what about photolysis which is something [. . .] that has to be way up in the atmosphere. Then we had to learn, okay, how does the atmosphere function, and how long it takes.

Okay, well, you think that's the most likely place where this species . . . after all, oxygen itself is destroyed at similar altitudes, actually, maybe a little bit higher, but because you get short enough radiation from the sun, so it breaks everything. And then say, "Okay, well, we might have a reasonable hypothesis as to where are these molecules destroyed and hence where will they be and hence it might be interesting to meteorologists," and so on. Okay, let's . . . I remember this was, sort of . . . as far as I remember, something in the back of our minds, we didn't talk it very explicitly at the beginning, but we both had it in our minds, well, so what?

Well, you have this species now in the stratosphere. And just as chemists, we knew some basic chemistry [of aerosols]. "Hey, we have chlorine atoms there." Well, the question for completeness in the back of our minds is, let's look at the whole cycle. So these things are produced industrially, so they are destroyed there and then eventually, the atoms combine,

[5] J.E. Lovelock, R.J. Maggs and R.J. Wade. "Halogenated Hydrocarbons in and over the Atlantic." *Nature* 241, (19 January 1973): 194–196.

perhaps in different molecular forms, make it back to the earth's surface. But is there something in between? Yeah, well, they must react with ozone. So we knew that the potential for catalytic reactions . . . what I still remember, this is something [. . .] I repeat, we both knew that it was important to also examine the possibility of some consequences. It did sound a little far-fetched at that time, I remember. Yeah, but these are small amounts of industrial compounds. But then actually doing the calculation work, how much is there and do we know—here again, was learning about how the atmosphere functions—do we know what controls the natural levels of ozone and that's where Paul [J.] Crutzen's work was very important because he's the one—being an engineer, of all things, and not a chemist—but he learned chemistry very well, and he proposed the natural cycle of ozone in the stratosphere. Of course, it was nitrous oxide, but just a very small fraction decomposing to give nitrogen oxide that would then destroy ozone. We were aware from Hal [Harold] Johnston's work without being very familiar with it that there was some concern about SST—supersonic transports—affecting the ozone layer.

So in the back of my mind, I said, "Well, it doesn't . . . you don't need huge amounts." But then you do the calculation and then repeating that, "Hey, I can't believe it, but it is something that might matter." Of course, then discussing it with Sherry and he immediately realizing, "Sure, let's look at this and we'll perhaps even talk to who we want to talk to . . . ." Hal Johnston.

And shortly thereafter, he had a sabbatical in Austria, in Vienna. So, much of what happened afterwards while we were writing the paper was by correspondence. But we did reach that level of awareness, which again, it all happened very fast. I do remember saying, "Well, to do this, I have to do some experiments." There were various experiments just to do: to take some ultraviolet spectra, even that was not well known. Ironically . . . okay, this is later. At that time, we were not quite aware, but industry did fund some research on these compounds, but it was in the back of their minds. "Well, let's worry about it. Does it matter?"

And they funded a couple of research groups and one of them was a spectroscopist, I don't even remember his name, which was interested in vacuum UV spectroscopy. He was looking completely at the wrong range that far in the atmosphere, but that was all that was known about these molecules. The atmosphere, of course, it was not extremely short wavelengths, which was the specialty of this person, but what we learned later on is that **<T: 15 min>** industry was more or less reassured that these compounds are so stable and even if it wasn't clear what they would destroy, but there's absolutely nothing to worry about.

And so that's what led me just . . . I'll do a few experiments. So I took some spectra. Again, a very easy experiment, but we had to get the samples, just to be able to predict more accurately or more carefully how long it would take and at what wavelengths would they be destroyed and where do you find those wavelengths. And in those months, I also had to learn about atmospheric models and do some calculations then [with diffusion coefficients] and so on, to eventually put more numbers, to have a more quantitative hypothesis. But the basic hypothesis, as I was telling you, just became clear just by looking at these numbers and comparing them to the natural processes in the atmosphere. So that's why it all happened relatively fast.

CARUSO: And so this came out in *Nature* in 1974, the results?[6]

MOLINA: Yes.

CARUSO: Did you have any difficulty when submitting the article? Did you get criticism from *Nature*'s reviewers?

MOLINA: No. Here is what happened. Two things which we learned afterwards . . . it was taking a long time for the review. Well, we thought, what's happening? We get something . . . no, it's because it was a very new field and they didn't know who the hell to send it to. So there's Michael [L.] Klein, turns out to be one of them, was a chemist we knew very well because he was doing experiments and knew a lot about chlorine chemistry, but he was not an atmospheric scientist at all.

And they might have sent it to . . . that, I don't even know anymore who were the other reviewers, but they were not many. And at the same time, we had checked with a key atmospheric scientist in the field, the first one being Hal Johnston because I had just come out of Berkeley, so it was easy. We went to see him [. . .] it was a very nice meeting, and we offered him, "Hey, why don't you join us and we publicize this together?" He said, "No, that's your finding. You do it. I'll support you or whatever."

So he didn't want to be part of it, not because he had any doubts about it, but he thought that was our stuff. He was in the middle of the stratospheric ozone worries from the SSTs. We also talked to Paul Crutzen, so the experts in our community all immediately bought the idea [. . .]. So two things happened. First one, just for fun, Paul presented these results in what he thought was a closed meeting in the Swedish Academy of Sciences. So it turns out there was a reporter there and so in the *Svenska Dagbladet* it made the news and we were very worried because *Nature* has this condition that you cannot have a press release [before the publication]. Fortunately, it didn't make enough news [. . .] because it was taking many months [to publish in *Nature*]. But that was our scare story.

And the other thing that happened in terms of criticism, was not from the experts. [. . .] We had to be very quiet about this because of *Nature*'s conditions, et cetera, and our friends and so on, but once it got published, then we talked more about it and eventually at an American Chemical Society meeting . . . but before that, we did get feedback from other colleagues and chemists, not directly to us, but we heard the rumors that we were just seeking [to make news] .

[6] M.J. Molina and F.S. Rowland, "Stratospheric Sink for Chlorofluoromethanes-Chlorine Atom Catalyzed Destruction of Ozone," *Nature*, 249, 810, 1974.

. . the criticism at that time was if you are in academics, you don’t publish in the newspapers. But we didn’t. We had a few colleagues that actually did like to publish first in the newspapers, and <**T: 20 min**> they were not very highly regarded as scientists, although in society they were better known. But that was [another] criticism, that it [seemed] like we were just trying to make noise. But fortunately, we didn’t take that too seriously. We thought, well if we’re putting this as a hypothesis and we’re checking that the science is right, and so that’s when slowly we decided, hey, this looks serious enough. We have to make an effort to learn to communicate with the media. And so not only not to pay attention to this criticism, but to do it the other way around. If this is real, we should do something about it. Including what did work is to push the academic community connected to this to check it and to begin doing experiments, okay, to check the hypothesis.

So that actually worked, but to continue with this, I do remember we decided to take advantage of—talking about the ACS [American Chemical Society]—a national ACS meeting, because we knew there [would be] press releases. It was a place where you could communicate to, at least, other chemists. So we sort of naively organized a press conference. And we thought, okay, let’s make a list. We’ll invite first somebody that will tell about the measurements that are measured in the atmosphere, repeating, summarizing Lovelock’s results, and then for Crutzen how the atmosphere works, and then we’ll tell our story. Why was it very naive? Because that’s not the way press conferences work with the media. You have to come up with a punch line at the very beginning.

So practically all the reporters left after the very first [statements]. So nobody was there when we talked about our findings. Later, of course, we learned and then we did begin to make news, but slowly because it was not something common. It was an unusual thing to talk about: invisible gases, invisible rays, anyhow, invisible atmosphere, but eventually the media sort of picked it up slowly, but we had to make an effort.

And simultaneously we thought, well, let’s begin to talk to some politicians in California, so just to see how that would work because spray cans were part of the issue, and so on, and so essentially, that’s how we started. And I certainly continued to do research. It was no longer experimental research in the lab, but more modeling and working with the rest of the community and at that time measurements were carried out with balloons, so working closely with people in the rest of the community just to promote measurements: are these balloons really getting there and so forth. So it . . . that’s how it took off.

ROBERTS: Yeah, back on the technical stuff. I think I was struck by this is a new area. There isn’t a whole lot of established literature about atmospheric chemistry writ large, especially at this altitude. You’re using new equipment. You’re talking about Lovelock's electron capture detector is just sort of being used and experimented with in recent years. So you know, on the one side, how do you even go about modeling and doing your own experimental work to try to simulate what’s happening up here and was that a concern that you and Rowland had about presenting those results into the larger community, that this was . . . you know, not only did you discover something that could potentially have dramatic effect, there wasn't really an established foundation of literature for people to sort of judge you on your accuracy.

MOLINA: We thought, and that was indeed a worry. We became sufficiently confident in the science [behind our findings], and enough was known about the atmosphere; as well as with Paul Crutzen's type science, there were measurements. Balloons were already sent. Much of the measurements, much of what was **<T: 25 min>** known was really dominated by physics of meteorology was dynamics as they called.

Of course, we chemists, when we took dynamics, it's chemical dynamics, but in that community, that's atmospheric dynamics and there was already a well-developed interest in learning about atmospheric movements in the stratosphere. So much of what was known about stratospheric ozone was just with the objective of seeing how—the stratosphere is quite different from the troposphere—how do things move in the stratosphere. So it was a new sense that ozone was chemically active and not just a good tracer.

So, we knew about the movements and we knew about, at least in relatively simple terms, how do species get to the stratosphere and what are the main species that are there. So it was enough known. And again, carrying out the simple calculations that this [gave us sufficient data] . . . of course, we were not sure, we knew this was a hypothesis, but it was sound [based on reliable information]. And that's when we decide, "Okay, it's not something just entirely far-fetched. Maybe this will be," but no, we were able to document this with the science because it was not terribly complicated, you know.

We realized that additional experiments had to be carried out, measuring the CFCs that we were practically sure if these compounds are stable, even if some of [those get] destroyed at lower altitudes, they certainly have to get there. One of the questions was, is there a process that will destroy them in competition through destruction in the stratosphere, which means that only a smaller fraction will get there, but the numbers appear to indicate that it was . . . because the numbers of what was in the atmosphere were comparable to what had been industrially produced.

So we knew there was no fast process. So there were a few key ideas like that, and then we realized where we needed to carry out research as a community: [Were] chlorine compounds actually measurable there? [. . .] We knew it would take some time, at least a decade or so, before anything could be measured because of the large natural variability. But what you mentioned was a concern, and we were sufficiently convinced that we thought this is worth pushing without such additions. So we always expressed [that] experiments needed to be carried out. This was a hypothesis, but it was something . . . our push was, let's do the research. Let's not just ignore it. It's real . . . with the science that we have and with the support of the experts in the community, that's what reassured us, if you want, that this was not crazy. This made a lot of sense. Let's invest. And we were not talking about huge resources. Let's use some of the balloons that are already being used to learn about movements. Let's do some chemistry with those.

And so that's how things got moved. Then later, that perhaps is more for tomorrow if you want, but some of the important subsequent steps very much connected with this question, though, was, okay, let's see how can we reassure the scientific community? Well, the National Academy [of Sciences] study, so that's how they got started.[7] This was sufficiently sound, so the National Academy, we have many friends there, of course, who . . . we were reasonably well respected in the academic environment in spite of this criticism. There were always people criticizing.

So that study by the National Academy did make sense. And in those years, furthermore, [. . .] we had to do more science. The chemistry's not quite as simple as we initially postulated. There might be other complications. So I continued. One peculiar thing, but that's . . . I was a postdoc for a relatively short term because once we published this, then Sherry very, very nicely said, "Well, let's keep doing this, but you will do it **<T: 30 min>**, you should be a faculty already." And the one complication is when you're a faculty in the U.S. in particular, the first years are hard to get tenure. You have to show you can do research independently, so, in some sense, I had to stop collaborating with Sherry, but I mean, [by then] we had already a background established. So the experimental work that we did was then independent for these reasons. So I kept doing laboratory work, but it was already much more defined, connected with what we thought might be the chemistry in the stratosphere.

ROBERTS: So you said that the two of you set out on this project to combine at the same time what you liked about this project versus some of the other things that Sherry was working on when you came to postdoc. This was something that there was a real question to be explored and resolved and there was some fundamental chemistry to be learned about the physical chemistry and the kinetics behind how some of these things were happening. When . . . so you've published the paper in *Nature*. Did you feel like you were still maintaining that balance or did you feel like you might be getting pulled in one direction over the other at this point?

MOLINA: No, we thought we had made the right choice and in some sense that was indeed correct, because it turns out that the stratosphere is chemically relatively simple and most of the important chemistry occurs with relatively small species. So we were not that far. That's why I brought this F plus H2. Okay, these were reactions that you could attempt to understand at a very fundamental level and again, I'm jumping way ahead, but yes, in fact, much was learned in chemical kinetics, which is what I told you I was interested even when I went to Germany, but this was now with not all that different, although we were talking about big molecules before. Not all that different, but much was learned about the fundamental science, doing research that was at the same time quite practical. Here we have something that is happening in real life and

[7] Committee on the Impacts of Stratospheric Change, Panel on Atmospheric Chemistry, Assembly of Mathematical and Physical Sciences, National Research Council, *Halocarbons: Environmental Effects of Chlorofluoromethane Release* (Washington, DC: National Academy of Sciences), 1976.

we have a chemical reactor, which is the planet. We can, of course, try to select some of that but we can go and check it.

And just to give you one example, [. . .] we know chemical kinetics, with chemical reactions you have an activation energy and normally reactions go faster as you increase the temperature, that's why you can cook. That's because everything breaks down. Hey, there were some experimental results we had, and this is a very good friend, [A.R.] Ravishankara there were some reactions working the other way around. So the first reaction of the community was oh, these are experimental errors. There are complicated things to study. No, and then it was realized, this is just one example. No, there are complex reactions where you have an intermediate and you have channels and some of them we now understand very well why they go faster at lower temperatures. That was strictly a result of stratospheric chemistry.

So it's one example of something along the lines we were asking. Yes, fundamental research moved on. The other very important component, jumping ahead, is chemistry of free radicals. In the laboratory it was just beginning to be possible to study reactions with free radicals directly. Before, it was all done in [inaudible] vessels indirectly, but at that time, we and others developed means of measuring directly the free radical and then you can do much more reliable chemistry, not just inferring, "Ah, the free radicals were there and they probably did this." There was important historical examples of reaction mechanisms that were indeed inferred that way, but this was another very interesting opportunity to push the science and yes, it was very exciting to combine fundamental lab science with something that's happening that you could actually check. So that did work.

ROBERTS: Well, that seems like a good stopping place [. . .] until tomorrow, if that's all right for you.

MOLINA: Sure, by all means, yes.

CARUSO: Thank you.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

INTERVIEWEE: **Mario J. Molina**

ALSO PRESENT: **Lorena González**

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

LOCATION: **The Mario Molina Center**
Mexico City, Mexico

DATE: **7 May 2013**

CARUSO: Today is the seventh of May, 2013. This is the second interview session with Dr. Mario Molina here at the Mario Molina Center in Mexico City. I'm David Caruso. Here with me is Jody Roberts and also joining us is Lorena Gonzalez. So, I think, Jody, you wanted to pick up with some questions about where we left off yesterday?

ROBERTS: Sure, thank you. You did a really lovely and succinct closing yesterday around the work that you did with Sherry Rowland, but we didn't poke at it too much, and I obviously wanted to spend some time today fleshing that out more and getting your thoughts on some of the things that happened. I wanted to talk about the reaction more and dive more into the reaction. You made a few comments yesterday about, you know, you don't go to graduate school and learn how to do public communication, and you suddenly found yourself in need of public communication, but I don't want to constrain how we think about this. I wanted to just maybe get your thoughts first on how you experienced, how you reacted to the reception to the work that you and Professor Rowland had done.

MOLINA: Yes. Perhaps repeating a little bit of what I said yesterday, but expanding it also. We had, as expected, a mixed reaction first within the scientific community. [. . .] Our colleagues or the other scientists that we felt were familiar with the field, they immediately supported us and so that was reassuring. But again, with the rest of the scientific community, some [of them] thought maybe we were exaggerating or just trying to "make noise" in some way or other.

And a similar thing happened with the media. There were a few reporters and then eventually a couple of people that were even writing books—that came a little later—so we had some more thorough long interviews with a few of them, and with reporters in general. We just had to summarize things. But, as expected, there were a number of criticisms in the press, that this was sort of far-fetched, and so on. But others immediately received it as something potentially quite important. We were stressing this at the very beginning; this was just a hypothesis and it had to be tested.

But it didn't take long to learn that we had to communicate with the media in a different language and trying to set up the background information or to put things in context. And so overall, I remember just making an effort; we had to spend some time. But, we felt that it was overall reasonably well accepted, considering that there was relatively little precedence for this. Of course, at that time, the environmental movement was taking off, but most of it, if not all of it, was talking about local pollution, water or air or whatever, so what was new with this problem that we were dealing with was that it was truly a global issue and so there was really no precedent for that. It didn't matter where these things were emitted.

ROBERTS: So before you published the paper, did you anticipate what the response might be **<T: 05 min>**?

MOLINA: Well, yes, but with a lot of uncertainty. We thought, well, since this is an industrial compound and it's used in . . . I remember [. . .] we were told some statistics later: Thirty to forty, maybe, but at least twenty to thirty spray cans on the average in each American household. At that time, everything was spray, so everybody had some contact with it. Of course, refrigerators, so we thought, yes, well, this must make some noise. And in some sense, we were sort of disappointed. Nothing happened at the beginning because it was just too strange. And then just little by little, it became better known. And also, as we anticipated, there were objections, but not in general. Not insulting in any way, but the industry people were saying, "Well, this is . . . "—not necessarily putting it down as something ridiculous—but saying, "This is not proven at all, so there's no need to stop this because this is just a hypothesis. It would not be correct right now to do something about it."

But I do remember a few strange instances. There is a magazine which is called—I don't know if it's the same—*Aerosol Age*, or something, but it was devoted to spray cans, and of course, now many years afterwards, we get along extremely well with them, they replaced things, so I even recently I just point it out because they invite me to some of their functions, so that's fine.

But early on, there was somebody's [George Diamond, president of Diamond Aerosol] article claiming that this was . . . what was it? This was some sort of conspiracy that had connections with . . . what was it? With the Soviet Union, something very strange. Yes, as you know, this is clearly something that we wanted to attack American culture.[8] And it made some news, so it was sort of funny because it didn't have any more consequences. But as always, there are people that really get annoyed at these things and they think it's—all this environmental issues—it's just people that want to . . . they're probably communist and there's a political agenda and so on.

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[8] For the details of George Diamond's Soviet disinformation theory, see Michael L. San Giovanni, "Keeping a High Profile," *Aerosol Age* vol. 22, no. 9 (1977) 34-36.

ROBERTS: Was there ever any concern about that because you were not an American?

MOLINA: No, not that I . . . I never became aware of anything like that, no. We were scientists, and I imagine in the scientific community it's common enough to have all sorts of strange names. At least, it never came up to my attention.

Well, talking about names, because something funny that happened a little later, we began to talk to people in Congress first in California, but then we traveled a little bit and so on. But the reason, when you were saying about names, there was a bill introduced . . . I believe it was in Congress, Tom [Thomas D.] DeLay, who is still around, so he was . . . but the two Congressmen that introduced it were Delay and [John T.] Doolittle. So, that's why it stuck in my mind that they introduced a bill that nothing had to be done for all these years.[9]

CARUSO: It's interesting, I was about to ask you how it came about that you started to testify in these national and state legislatures. Was that something that you were asked to do? Is that something that you took up on your own?

MOLINA: It's something, I believe, resulted from the news media. I mean, we had to respond **<T: 10 min>** in other words, but I don't remember either Sherry or Paul taking the initiative to call. We were not connected at all with politicians on either a state level . . . so it's something that just happened and a few politicians were sort of sympathetic to the environmental movement or had some reasons to worry about spray cans. And, again, we realized we had to put things in context. We didn't do that very explicitly, but it was clear that we could have easily exaggerated or made ourselves part of just the environment, in general.

So we really wanted to stick to the science, of course, realizing that certain policy has to be made, but we only wanted to state what the risks were or what happened, and not be perceived as advocates for a particular environmental cause. But nevertheless, it's something that Sherry and I did, but Sherry did very explicitly that became quoted quite often, but much later that, "If not us, who? And if not now, when?" Because that's the other thing that happened at the beginning, I remember. There was . . . in the context of this environmental movement, there were already a number of environmental organizations, not as many as there are now, but they were all so far removed from these issues that we did not have support or communicate with any of these existing organizations at the beginning.

[9] The Congressmen introduced two separate pieces of legislation: H.R. 475, "To repeal provisions of the Clean Air Act dealing with stratospheric ozone," introduced by Representative DeLay, January 11, 1995; and .H.R. 2367, "A Bill to Amend the Clean Air Act to further protect and enhance the public interest by ensuring an orderly transition from chlorofluorocarbons (CFCs) and halons to substitute compounds," introduced by Representative Doolittle, September 20, 1995. Neither bill made it out of committee.

Then later on, after probably some of the National Academy reports and so on, then yes, then several organizations approached us and then we sort of worked together more closely, but it was something that was not there at the beginning. So we did not have . . . I mean, that would have been another option to just let somebody else do the politics, if you want, or advertising in some sense, so it all happened.

ROBERTS: Did you have any support from the department or the university or just other colleagues in learning how to navigate the Congressional or public face of the science?

MOLINA: No. That was interesting. That's what I remember. That's why we had this experience I told you about in the American Chemical Society. So what we learned, we learned soon. But no, we had no . . . I mean, maybe in some conversations somewhere, but what I do remember is that in our community, which was not the environmental community, this was academia, so this was quite unusual. There were no similar issues. Perhaps the closest thing was the . . . but we did not immediately learn enough from that, but the closest thing was the supersonic transport issue and the way that worked, Hal Johnston and so on, they were also well-respected scientists who were not going up and advocating things. But there were mostly scientific meetings, which were some of them a little bit more open, like, to people in the medical community, but I don't remember learning anything other than the science itself from colleagues.

CARUSO: How was it . . . I know in 1975, you became a professor at Irvine. Right, so you're transitioning from this postdoc. How was it balancing these outside activities with establishing yourself as a professor in the US?

MOLINA: It was, I remember, a little bit of a risk because Sherry was already established. He, of course, complained later—I didn't see that **<T: 15 min>** personally—he complained later that for I don't know how many years, he did not get any more applications from postdocs, for example. So he was clearly noticing that in his position because he was the chairman of the . . . he sort of established the chemistry department there, he did notice this contrast that I was telling you about, that our close colleagues very much in agreement with us, but the community in general looking at this with a lot of reservations.

I had no problems. I got graduate students, which I wasn't expecting postdocs anyhow, and fortunately, although it's a time where you have to work very hard to get your tenure and so on, but in general, did not have much of a conflict in terms of time because what took most time, some of the interviews for the several books that were written, but we thought that was very worthwhile so that was okay.

And so there were three or more books that appeared on the overall issue, interviewing Sherry and myself and sometimes other scientists, as well. But yeah, no there was no . . . I had no problem myself that I perceived in terms of the rest of the community, perhaps because I was mostly interacting, again, with colleagues I already knew or the colleagues that were involved in the field. We certainly had the support, but just informally, there was no formal support for it. At Irvine, of course, the chemistry department, they knew Sherry and knew myself, so we had no problems with our close colleagues, okay; this was more perhaps the community at large.

CARUSO: Thinking about the tenure process, I think you might have mentioned something yesterday about the concerns with staying at the same university where you were doing your postdoc, making sure that your science is distinct from the person that you were previously working with. How did you and Dr. Rowland separate that out or was it not really an issue?

MOLINA: It was not an issue because we did not have to separate anything in connection with the communication aspects to the public or to the media or to other colleagues. What we really had to separate was just purely the scientific work. I had to write papers where I was the main author with my students and Sherry continued his research writing his own papers. We had a few joint papers and so on, but I knew they were not going to count for the tenure process because of the question of . . . these things were a little exaggerated, okay, because afterwards, me myself, a professor in analyzing tenure cases, you can sometimes tell, even though there are close collaborations, you can still make a judgment of whether somebody was just following somebody else or his contribution. But anyhow, we took it very seriously and there was no problem because I was using different experimental techniques.

So even if we at some point were looking at similar systems, it was with a very different laboratory approach, so that turned out not to be a problem. In fact, at some stage, I remember one important issue that came later is the realization that reactions on particles were going to be very important in the . . . that was some years later in the polar stratosphere and we both came up with a . . . we knew that was the case, but we had very different experimental approaches to the same question. So in summary, it was really no conflict in terms of separating what was scientific research from our joint role there as spokesmen for this issue.

And I do remember things, but sort of in some sense simplified, when **<T: 20 min>** the community took it seriously enough, as I mentioned to you yesterday, that the National Academy started to do reports and, of course, that was then a clear signal that this was an issue recognized by the community as something worthwhile to further analyze. And we thought that's terribly important because this is one way to communicate this issue to the public in which it will be clear that it is not advocacy, that it's not because we came up with this idea and hence, we're blindly just pushing it. This happened all . . . I don't remember. I should . . . I probably should have looked at the dates and so on, but in any event, early on, if not the first maybe the second report; rather early, along these lines that I was telling you about earlier, was doing my own research, I pursued the chemistry further and finding out what are the likely chemical reactions that would take place.

So essentially this is something I did independently of Sherry; of course, we were working closely anyhow, but that was more part of my line of research, to identify some chemical species that were new, and so in terms of one very important one came later, that's chlorine peroxide but that was to explain the stratospheric ozone hole. That came much later, but very early on, I realized that there was one species which is called chlorine nitrate. I thought, "This might be quite important because it's relatively unusual in that it doesn't absorb light very strongly, hence it might build up somewhat in the stratosphere." But it was a very weird species. I realized that at least the chemical compound existed in contrast to the other one.

The chlorine peroxide, that had not been characterized because it's not stable at room temperature, so it was a new technique, and I was able to show, yeah, this molecule not only exists, but will be there in significant amounts. But this other one, yeah, it had already been characterized, but in a very obscure paper in the German literature by some German professor and nobody had paid any attention.[10] But I found it and then began to do experiments, and then, of course, the National Academy report was along and first I talked to Sherry, I said, "We have to let them know because this is something that we need to find out more about this species to be able to better quantify how much ozone would be depleted," because it has a complex role, it might slow it down.

And it was interesting because it caused a big uproar because some of the models at that time were still relatively simple and the simple models were just putting this species in say, "Wow, there's no more ozone depletion or very little," so this sort of stopped that second National Academy report of the . . . I don't remember which one was it.[11] It delayed things and then later it became clear that it was just . . . yes, it had to be considered. It might delay things a little bit, but the ozone depletion was still important.

So it's just one example, but ironically, years later—this was much later when the Antarctic ozone appears—that turned out to be a crucial species to explain polar ozone depletion. So you see historically how that came about. But this is an illustration of this; the different lines of research, and I was able to—with my students, of course—to make the species, to synthesize this in the laboratory, take spectra and so on and so forth. It's a very unstable species which surfaces, decomposes. But anyhow, those are the techniques I was specializing on to work with rather unstable species that you can't handle, just a conventional, just [inaudible] vessels to look at them.

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[10] M. Schmeisser and K. Braendle. "Halogennitrate und ihre Reaktionen. Zur Chemie der positiven Oxydationsstufen der Halogene." *Angewandte Chemie* 73, no. 11 (1961): 388-393.
[11] Committee on the Impacts of Stratospheric Change, Assembly of Mathematical and Physical Sciences, Committee on Alternatives for the Reduction of Chlorofluorocarbon Emissions, Commission on Sociotechnical Systems, *Protection against Depletion of Stratospheric Ozone by Chlorofluorocarbons* (Washington, DC: National Academy of Sciences), 1979.

ROBERTS: So is this one of the reason that the National Academy decided to actually do two reports? So I was going back and checking some of our notes last night, they decide to do a report earlier that basically confirms that, you know, **<T: 25 min>** what you and Sherry observed is observable and sound, but they postponed doing a report that really started to talk about the larger chemistry and the implications of that chemistry.

MOLINA: I think that was it, yes, yes. But eventually, eventually they did a report and so it just . . . essentially, if I remember correctly, the bottom line was maybe ozone will not be depleted as fast as the simpler models would tell us, but it's still a big issue and still we need to do experiments in the atmosphere and so on. So that was the consequence of this making the chemistry more complete.

ROBERTS: You mentioned that Rowland had some difficulty attracting some postdocs for a short time. Did you have any difficulty attracting students to your new lab?

MOLINA: No, because I was . . . in fact, I did work with . . . I had a few postdocs and maybe I was lucky also, but with graduate students, that was less of a problem because the graduate students are admitted and then they choose to work with somebody that is there. So I did not have much of a problem. And then at some point—that was later—I decided to concentrate more on research, less on teaching and that's when I moved to the [California Institute of Technology] Jet Propulsion Labs.

It was something sort of temporary, but there, of course, you have lab work and the other thing that was . . . again, I have to look at the timing, but part of the reason is that as a professor in a university, you hardly have time to do research in the laboratory yourself, whereas at the Jet Propulsion Labs, many of the groups, it's just two or three people, and the main researcher is the one that is doing the experiments. But it's mixed. I had several people working with me, but I was doing experiments myself, as well, which was unusual. And then, of course, eventually said, no, I do have a larger group and I like teaching, so I went to MIT, but that was later, but still long before the Nobel Prize.

ROBERTS: Sure. Well, before we get to the JPL, I'm struck by the emphasis you've been putting on trying to dissociate the focus you wanted to maintain on the technical work. There was still a lot of work to be done and you've been talking about some of the work that you continued to do to clarify those original experiments and the original observations and trying to separate that from you said being a more public persona.

But I'm thinking back to the . . . again, going back and looking at some of the materials last night, the letter in *Science* in the year after your publication in *Nature* where you and Sherry are responding to an advertisement that [E.I.] DuPont [de Nemours and Company] has put into *Science* and a number of other magazines, and I'm wondering about the experience of trying to respond in a technical manner to an advertisement inside the pages of *Science* and the response by DuPont on a technical way to your letter in response to an advertisement in response to . . . because their response seems to be jumbling the world in a way that you're trying to keep more neatly cut.

MOLINA: Yes. Okay. Let me explain part of that. I had forgotten about that letter, but I need to go back and repeat because it was fun. Here is what was peculiar. And contrasting this, this is yet another issue because, as you know, more recently I've done a lot more with climate change, so I can see the differences. But one thing that was different then, that's why it brought the climate change issue, is that DuPont—that was the largest chemical company—they had a tradition of doing scientific research. They were funding scientific research, much of it with polymers, of course, that was applied, but they were very proud of expanding that to the point that they have people publishing in the scientific literature. So it was not all applied, just for the company. So we were dealing with a company that was proud of their acceptance of science as an important issue, and so on. So in some sense, that's why this sort of initial friction **<T: 30 min>** was unique because if it was just like this other magazine just attacking us, we didn't pay that much attention, but with DuPont, we felt that we had to be more responsive along the science itself. We were not necessarily pushing, that's clearly, right, I think Sherry also . . . we were not pushing for political responses.

We were, at that stage, just trying to make the science clear, recognizing that, of course, experiments still had to be done, but our statements were that, look, it's sound, certain things need to be sorted out by experiments, but it's certainly a serious issue. We think this is potentially very serious, so we feel a responsibility to communicate this to society and so on. So I don't remember the details but it was . . . we were not really, how shall I put it, at least personally on any bad terms with DuPont, we were just fighting on the scientific level. But it was then much clearer afterwards . . . I don't remember the dates very well, the way industry responded through the CMA, the Chemical Manufacturer's Association, DuPont playing a very important . . . they decided to fund scientific research. So they put money into funding research on the chemistry of the stratosphere because inside DuPont, they did not have experts in this field.

They eventually hired, much later, I don't remember when, Mack McFarland, which was a very well-respected researcher in our community, one whose expertise was measuring nitrogen oxides with balloons, and so on. So, with other words, DuPont did have, not too long after our initial announcement, their own experts, scientists in this field that were working for DuPont, but not doing experiments. They were, however, funding our colleagues. They could write proposals, and so on. And so they had meetings, at least one annual meeting or sometimes several. And that's one thing that Sherry and I were . . . was a difference. Sherry never went to those meetings. He was sort of on a personal level more in trouble, if you want, with this community, whereas I was sort of accepted. I went to all these meetings. We did not have any problems. I just remember over the years that, gee whiz, they really have bad luck because all the research that they were funding was supporting our ideas.

But here is why I recognized DuPont. Of course, afterwards, we became very good friends. DuPont in those years made a statement: "Should the science really prove to be correct, we will stop making these compounds." Okay. So what it took for them to recognize the science was really much later, was the Antarctic ozone hole where the sign was strikingly clear, and they did stop. Okay. And Mack McFarland was very instrumental in that. So with other words, I never had any personal conflict with them and it was clear that they needed something. They were not about to stop producing chemicals just in case. They really wanted to make sure.

By the way, there was a misunderstanding in general in the community that DuPont stopped because they had developed replacements that would be very profitable. It turns out that that was not the case, not literally at least. There were only five large industries, four or five or six, you know, Monsanto, some of the large chemical industries, and they did early on in their labs, the research they did was not the stratospheric chemistry, but are there other compounds that they could use.

So they did start to do some research on replacements. And then when things slowed down, chlorine nitrate and this sort of thing, they stopped that and they very much regretted that afterwards. They started that again, but no, they were earning money with the CFCs and so they had to make a judgment **<T: 35 min>**. I knew this more or less and because I trust Mack McFarland and I don't remember the name of DuPont's chairman at that time. Eventually we became good friends. I've forgotten . . . I'm bad for names . . . because later I became friends of the more recent chairmen of DuPont, but I forget there was a key person at that time. Anyhow, I remember getting the inside stories: "Look, we at DuPont have to make a decision, eventually. First, we have made this commitment. Second, public relations is important for a company of our size, so even if we were going to lose money because we had to stop this production, we thought that was still the right thing to do from a purely economics perspective because they would lose trust in the community."

But then they did eventually develop replacements, and all of them did. So it turns out that it was not a big issue in terms of a big economic loss, but—I'm making some remarks that happened much later—but I remember then DuPont became very proud and we were, of course, proud as well, that it became an environmental company, not just because they stopped making CFCs, but because they had a campaign with the staff and with their workers and they are all very proud of being an environmental company.

So we sort of turned that around because initially they had no . . . they were making Teflon and so on, but most of the relations between a large chemical company and society were a bit more friction in terms of what was the perception of chemistry is bad for the environment in general. So that was one company that early on wanted to turn that, sort of—how do you call it—it's a misconception that all chemistry is bad for you and so we have to stop chemicals in general. But anyhow, this is a description of the relation with industry and with the news media; eventually, I guess, what happened is that people that kept approaching us probably were self-selecting; the ones that believed that this was something to worry about. Whereas critics, in general—well, there were a number of exceptions, of course—but in general, did not approach us. They simply brought their opinions and so on. But occasionally I remember having a few interviews which appeared friendly, and just answered in what appeared to be the science and then the article was completely negative or was not based on the science itself. It's just, that's the sort of thing one expects from all different opinions in the media, and that happens.

ROBERTS: What was driving the search for alternatives at DuPont at that point?

MOLINA: [. . .] That's why it started early on. It was already the realization that, "Hey, we might have to stop." And not necessarily accepting the science where it was at, but just looking at what society might do. Okay, this is a risk and it's associated with public health, so we'd better put some money in finding something else, just in case. And, of course, as the science became stronger, and obviously after . . . when they decided to support the science, it became very obvious. Then it was really just to maintain. I mean, they had big business and all the connections with industry and they knew a lot about it, and so they realized what would it take to make replacements. So, I presume that those were the reasons. But initially, I would imagine, it's just that they realized that this was something serious enough so that they better do something, just in case.

ROBERTS: So one other question about the 1970s, that larger context. So you are going to Congressional hearings. You and Sherry are both testifying both at state level and at national level, the 1970s **<T: 40 min>** are the big . . . at least taken to be the cauldron of the creation of environmental regulations, the environmental state inside of the United States, but you also mentioned that the environmental movement really didn't know what to do with this. It was new but it was already pretty solidified by the time you're coming out with some of this research. And this isn't a locally produced phenomenon. This is a dispersed phenomenon. This is a global phenomenon.

So I'm curious where you fit or how you were received, in a little bit more detail, both by the regulatory community and government. So you're having these Congressional hearings, but I'm not sure if the people you're testifying before have an idea of where they're going to wedge this into the new matrix of environmental regulations that are out there, and how the environmental community is or is not responding. You've addressed the latter a little bit.

MOLINA: Yes. I remember more or less what people in government . . . this Congressman or so, think about regulations. They thought probably the spray cans was the weak point. Spray cans had also a bit of a bad name, but not for clear reasons. It was just because it was maybe considered to be a waste or because we were using chemicals. Of course, we were in no way connected with that approach, but it was clear that that was perhaps the easier target at the beginning. And in fact, that's what happened. In the environmental movement, spray cans got a bad name at that time.

I remember one symptom of this is in TV, there are these . . . *All in the Family* was very famous, and the young guy that appears there made some statement on it about not using spray cans, so that was a big thing because that's . . . because that's, of course, millions of people were looking at that, so that was sort of publicity. But see, it was just spray cans in general are bad for you

Ironically, there are still some people who still think that spray cans are bad for the environment even though, of course, they don't use CFCs anymore, but it's a little bit that misconception. Okay, so that was the target and, in fact, the use of CFCs in spray cans was forbidden in the US and in Canada. It was the first thing that happened, widely separated from the later Montreal Protocol and the environmental agreements. Okay, so yeah, so that's perhaps some of the environmental organizations then took that line as well. I remember I was a little uncomfortable, saying, sure, it's important for spray cans to switch propellants, but we have nothing against the use of spray cans in general. And in fact, I remember even talking to somebody, for example, shaving cream does not use CFCs, they use nitrous oxide, so we were . . . at times, like, "No, no, this spray can is no problem," and so on. But that was a dilemma, sort of.

ROBERTS: That . . . Dave and I were talking about that on the drive over here this morning, the difficulty of conflating the specific for the general, and so people's focus on the word 'aerosol' when aerosol is a large category and very specific aerosols might be problematic, but, you know, growing up and thinking about aerosol cans and they're explosive and they cause damage to the environment, and not an understanding of the differences of where . . . you know, again, the conflating of specifics and generals.

MOLINA: That's right. Sure. And that typically is one of the disadvantages of some environmental organizations, that they a little bit blindly push some issues which are not very well supported by science. And that still happens. And, of course, we know in recent times, sometimes that actually backfires, when people become more and more informed, these sort of things don't help.

ROBERTS: Can you give an example of something like that?

MOLINA: Well, this is more with . . . **<T: 45 min>** much more recently with climate change, just not using anything but biofuels, but they could be economically just not practical yet at all, but I mean, they advocate a certain set of actions that they are just not realistic. There's no hope that society will do that and there are more realistic, much better ways. There are reasonable ways to consider economic aspects and everything else, okay, but the one organization that is sort of a stereotype of this is Greenpeace now because they often exaggerate. But there are others. But it's a little bit along those lines.

And the position we had at that time was—still with climate change and so on—is that there are very rational ways in which society can respond to these things that are well justified, but not everything is well justified. And I was talking about biofuels. Biofuels can be very badly misused, as well, if you do away with tropical forests just to make biofuels, that's a disaster. That's just one example, but at that time, the spray can issue was the clear one. Look, there are spray cans that are all right or if you replace, if you use hydrocarbons or whatever, then it's all right. Then there might be other issues, but they are not . . . it has nothing to do with the global issue.

ROBERTS: So there was a lot of momentum building up here at the end of the 1970s. You've got two National Academy reports. You're revising a lot of your own research. You've been going off on your own trend. And there seems to be some recognition, at least in some of the reflective pieces that you wrote in the 1990s, about the great halt that happens because of the change in political climate. And I'm wondering if you could talk a little bit about the change from the 1970s to the 1980s.

MOLINA: Here is what I remember. Those were a little bit frustrating times because we had, initially—as we were talking about—at the very beginning, the issue was not recognized, so strange, but then it was essentially accepted by society. With industry we had these relations that I wouldn't even call them fights, but the science was still moving along, and it was a little bit more complicated than we anticipated, I think, at the beginning. So we realized, well, it's not likely that you will notice a very striking effect in the next few years because that's what the model tells us and there are these complications, so this might take a decade or two. So things slow down and so we're a little bit frustrated, but decided, well, we just have to keep doing research. It began to . . . well, not began, it did not receive as much public attention as it did at the beginning. And so that's the way things were moving, until the Antarctic ozone hole.

Just about the same time also there were indications that ozone depletion at mid latitudes and so was also measurable. But at the very beginning, it was clear that, yeah, we were not expecting ozone depletion to be measurable because of the large fluctuation. So that was, if you want, uncomfortable times in terms of knowing that not much was going to happen until we could prove to society that something really worrisome was actually happening. The threat of something worrisome happening in the future was not enough to move the international community, beyond the U.S. and Canada having banned the CFCs in spray cans and so on. And so that's why things changed quite dramatically with the Antarctic ozone hole.

So I can tell you about that if you want.

ROBERTS: Sure. Sure.

MOLINA: That happened already . . . so that was in the early 1980s and it was quite interesting because we learned from Joe [Joseph C.] Farman who was the head of the British Antarctic survey **<T: 50 min>**, because he approached us. He was one of those . . . you know, there are all sorts of types of scientist who like to do all sorts of strange measurements, and you need a very dedicated scientist to spend the night in Antarctica, because it's many months, many months.

But they were measuring stratospheric ozone levels, still with the motivation—like in Switzerland, of course, I remember also they had some long chain of measurements from the Alps in Switzerland—all trying to learn how ozone moves in the stratosphere as we talked about yesterday briefly, just to learn about how the atmosphere of our planet functions, the chemistry not being terribly important. Anyhow, so that was essentially the motivation of these ground measurements where you can measure ozone from the ground because it's the only species absorbing ultraviolet light. So you just measure how much ultraviolet light gets to the ground and you get a measurement how much ozone there is. So, it was interesting to see . . . it was interesting to measure over the poles because enough was understood that ozone is made in the tropics, but then it's at higher altitudes and it [moves to] lower altitudes and so [it ends up] piling up over the poles. Okay, so it gives you a lot of information of the movements which are different in the stratosphere. And so Joe Farman [and his team] of other colleagues and so on, eventually they found—they didn't believe it—that there was practically . . . very little or practically no ozone, but they were able to . . . they didn't approach us immediately. Took them several years because at the beginning, nothing was normal and then it began to disappear. And so they approached, "Hey, could this be connected to the CFCs?" And, in fact, they wrote a paper announcing their findings, suggesting—to their credit, because it was not suggesting—that it could be the CFCs, when in fact, the rest of the community of the . . . - those were not atmospheric chemists, but were mostly the people studying the atmosphere or the stratosphere and so on.[12] They all thought, "This must be natural phenomena. How can this possibly have something to do with CFCs? Because it is spectacular. Something very big is happening."

And then one question was, "Well, we have been measuring ozone for years with satellites. So are these guys wrong?" So they went back. The results were there, but the satellites had been programmed to ignore the data. So it was very striking, but immediately became a big issue. Of course, that satellite also corroborates what the hell is going on. So, then it really took just a few years then. But the beginning, a very skeptical community and they say, "Well, if it's chlorine, we should be able to measure it." So that's when the ground expedition went with Susan Solomon measuring things from the ground and all they say, "Wow, it appears that it's quite possible and even likely that it is, in fact, chlorine from the CFCs."

And then a few years later, with Jim [James G.] Anderson and the ER-2, which is the cousin of the U2, the plane that had been developed to spy over Russia, and you remember Gary [Powers] was shot down and it was a big scandal, of course. So fortunately those planes with

[12] J.C. Farman, B.G. Gardiner and J.D. Shanklin, "Large Losses of Total Ozone in Antarctica Reveal Seasonal ClOx/NOx interaction," *Nature* 315 (1985): 6016.

little modifications could be used to—it was a very daring thing—but could be used to fly over Antarctica and at high altitudes. But it was very daring because there was no place to . . . any emergency and the pilot was gone. And at low temperatures the fuel begins to get more and more viscous. But fortunately, they said, 'Well, let's plan. Let's do it." And these crucial experiments were done. And the first . . .

ROBERTS: [. . .] I just love those things.

MOLINA: Yeah, because we were, of course, fascinated at expecting this . . . they were flying from southern Chile, from Punta Arenas, taking off all the way to Antarctica, thinking, "Will we hear anything else from the pilots?" But they were able to return. The first two flights, the instruments didn't work **<T: 55 min>**, but then the results came in. The smoking gun. So those were very clear, very, very clear results measuring chlorine and chlorine oxide with instruments that were similar to ones we had used in the laboratory, but that was Jim Anderson's specialty, and measuring ozone.

And so what was very revealing is, once you reach the ozone hole, practically no ozone and a lot of chlorine. But ozone was sort of going up and down and chlorine up and down just like a mirror image. The perfect anti-correlation. So how can this possibly be by chance? So I remember Jim Anderson sending those probably to Sherry and to me and a very nicely framed picture with these curves, okay, before and after, so that's the smoking gun. And so that's the sort of thing . . . I mean, of course, many more experiments were done and other species were measured as well and we have more to learn about really how the chemistry worked, but that's the sort of thing that then changed industry's perspective.

But we still had to do some work. The chemistry was not clear, so that's when we did some additional laboratory work with this other species to explain, no, this is very unique chemistry that speeds up at very low temperatures. Other species are involved, like this chlorine peroxide and so on. So that was complemented by this laboratory work.

ROBERTS: So how difficult was it to maintain support and funding for this work during that first half of the 1980s?

MOLINA: Fortunately, once the Antarctic hole [was found and revealed there was continuous] funding for the balloons by NASA. [. . .] It was initially like any fundamental research activity, just let's learn more about the atmosphere, and with no practical connection. Fortunately, that's basic science and so it . . . and it was not terribly expensive.

But people had to send these helium balloons and there was a specialty and eventually they were sending some from Antarctica as well, but that was not the main part. It was realizing, there we really had to send airplanes. But when the Antarctic ozone hole appeared, and because we already had a whole decade, if not more, of knowing that the CFCs were, in fact, there, they were decomposing, so there was . . . it was not very expensive research. There was no . . . I think, no issue. NASA essentially had the funding to do this.

ROBERTS: I was going to say it sounds like very expensive research to me. If you're flying a U2 over Antarctica, I bet that's not cheap.

MOLINA: Yeah, that's right.

ROBERTS: But you were taking advantage of resources and monitoring or observatory work that was already taking place.

MOLINA: That's right. That's right. If you consider other NASA projects, going to the moon or sending telescopes, this was not expensive, okay, in that context. And they had the excuse, hey, this is something that we need to find out because it could be very worrisome for society. So at that time, I remember NASA had no particular funding problems, so that was not an issue. And the planes were there. It's not that they had to construct them, but it was obvious they still had to spend a fair amount of money compared to just doing laboratory research.

ROBERTS: And just again, I'm thinking of the [Ronald W.] Reagan context and thinking about the scrutiny that that executive administration is bringing to federal funding more generally, and trying to figure out if it was accidental or strategic on your part that you were doing a lot of this work inside of one of the few agencies that was not under quite the same amount of scrutiny that others . . . I mean, if this were work being done by . . .

CARUSO: Until 1986 with [Space Shuttle] *Challenger*.

ROBERTS: Right.

CARUSO: I assume things changed.

ROBERTS: Things changed right after the tragedy with the *Challenger*.

MOLINA: Right. It's probably not something we did very consciously, but the community, the community was very much aware of that. It was reasonable for NASA to fund it, but with

Reagan, I remember one of his close friends was **<T: 60 min>** [Robert H.] Abplanalp. I mean, very wealthy guy, made a lot of money because he invented the spray can valve. So, he was in a bind. Eventually, Reagan came around and signed the Montreal Protocol on that with Margaret Thatcher and so on, but that all happened afterwards, okay, but it was a fight initially. You are right.

So that was quite interesting how things changed and, in fact, at that time, there were Republicans—I mean, Reagan, and [others]—in power, in the presidency, and they were the environmentalists at the time, okay. So things did change quite a bit. The Clean Air Act was also showing up at that time.[13] But yes, there were these general problems that you mentioned, but no, NASA, fortunately, NASA continued with funding laboratory work. That was not a major part of the research for a while.

CARUSO: So a lot of this work is going on also while you're at Caltech [California Institute of Technology] and you'd mentioned, I think, that part of your reason for that move was you wanted to do a bit more research than you were able to do at Irvine. I'm also curious . . . I mean, you were in the Jet Propulsion Laboratory, so I'm guessing things are a little different from where you were at Irvine in terms of the colleagues that you have around you. You had a supportive community at Irvine. Did you have the same sort of support from your colleagues while you were at JPL?

MOLINA: Yes, because in fact, JPL—just, again, being NASA-funded and so on—was doing a whole variety of things. Of course, the main activity—that's what JPL does, is known for—is the unmanned vehicles that they send to explore the solar system. So that . . . when something happened that was very, very big, but they funded a number of other research activities. One was connected with the earth's atmosphere, so we were a group of maybe five or six atmospheric chemists doing laboratory research and trying to better understand the atmosphere. And at that time, yeah, they said, okay, it's reasonable to fund and to do stratospheric research.

So that was the main focus of this group, in contrast to what was going on at several other universities, which was more air pollution research. That's another branch of atmospheric sciences. But yes, this group of . . . I remember, well, all friends of mine and very well known, we were all doing work in the laboratory to understand chlorine chemistry, perhaps nitrogen oxide chemistry as well.

ROBERTS: So was this something you sought out or were you recruited to come up to the JPL?

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[13] The Clean Air Act, *U.S. Code* 42 §§ 7401-7671.

MOLINA: No, well, since the community is small, when I expressed my willingness to move away from Irvine University, then this came about. I don't remember the details, but since I knew the people, it was relatively easy to do.

ROBERTS: Were there any of your colleagues or any former students or anybody like that that you took with you or was this sort of a solo transition?

MOLINA: No, well, my ex-wife was working with me, [. . .] but no, when she went there, she actually worked with somebody else at JPL, so she did not join my group, although later on at MIT, we still did some work. At MIT, she was, in fact, part of my group, but eventually essentially just managing things, but she still did experimental work in the early years. But aside . . . I was mixed up now because at JPL, I was working with a couple of postdocs. There, you don't have graduate students, obviously, but these, essentially people or students that function like postdocs. So they already have a PhD, and they just spend what is understood to be a temporary position.

ROBERTS: I was curious just a little bit since you brought it up about the overlap with your first wife, because she seems **<T: 65 min>** to be a coauthor on a lot of your work. Is that an area that she was working in when you guys were both at Berkeley or did her career research tack towards yours and . . .

MOLINA: No, she tacked towards mine afterwards because, if you remember, I mentioned yesterday that our mentor, George Pimentel, was very well known, but he had two big research areas where he was. One was a chemical laser one and the other one was the low-temperature spectroscopy. And so Luisa, my ex-wife, was actually in the other field. But then when she . . . essentially perhaps starting at JPL, but then at MIT, she just . . . it was all still very much laboratory type work with vacuum lines and well, so it was not a big jump in any way to . . .

ROBERTS: It would actually seem like her skill set in low-temperature spectroscopy perhaps even came in handy as you were transitioning towards different modes of monitoring some of the chemical species.

MOLINA: That's right, yeah. That's right and we were . . . that's one thing that was also . . . functioned well at JPL; since we were few and working together, we could learn from each other and borrow equipment and so on and so forth. So that worked reasonably well there.

There was also—it's just a side issue—but what was centered at JPL starting at that time was a major effort for the group of experts in this case—not experts in general on the stratosphere, but on stratospheric chemistry—to evaluate the results of the community. In this case, just to very pure chemistry, what does this group recommend should be the rate constant and what should be the reactions that matter. So we spent quite a bit of time. We got to study and look at the publications and look at all the graphs and eventually we had to recommend, okay, if you're . . . or the models, because the models were not laboratory chemists. Okay, so they used our outputs to feed the models.

ROBERTS: So how sensitive were you about that issue around the models because that seemed to be at the heart of some of the disagreement and controversy after the initial publications in the 1970s. So the difference between presenting results based on mathematical models and the presenting results based on experimentally verified laboratory results.

MOLINA: Right. There was really never a real conflict because we worked closely with the modelers. [. . .] Even at JPL, there was a group of maybe two modelers or so. The good ones were also chemists and they understood very well our recommendations. And in parallel, we understood the nature of the models. Okay, so it's just that we were familiar with the limitations of the models, as well. But there was no real how should I say, discrepancy, or like two groups that were . . . we were working together. It's just that we could only do relatively simple models ourselves. I did some modeling early on when I was at Irvine, just to show that this thing would make sense, but then the models, the complicated models were . . . of course, took a lot of computer time and they were initially relatively simple, one-dimensional. They got much more complicated and two-dimensional and then eventually the three-dimension. They just required a lot of computer time and space and so on. And so that . . . at that time, it was really a specialty. You couldn't do that sort of on the side. And so we were able to work with the modelers and find out what . . . the discrepancy . . .

Okay, I remember now, just to make this more clear. I think this . . . I was forgetting there were three groups. The modelers, we, the laboratory scientists, but then the measurement people. Okay, so essentially three different specialties, but with very close interactions and the . . . while recognizing ones in each of the three specialties were sufficiently familiar with the other two so that we really had no **<T: 70 min>** conflict of sorts. In some sense, the models were also one way to raise questions. Of course, in the lab, if we came up with a new species, then, obviously, that had to be incorporated into the models and measured also. Then the measurement people had to try to do that, but measurements and laboratory work was reasonably closely related. We were using very similar chemical technologies, one except in a balloon, they have to work without anybody touching anything. But so that's why there was really a specialty. But resonance fluorescence or spectroscopy or whatever, so those were issues that we were all familiar with.

Yeah, so that's the way I would describe it. [. . .] The time I described earlier when the models came up with the second National Academy report, it . . . we had realized, of course—that was when I came over and put in the numbers, that this could slow down the chemistry, because here we have what we call a reservoir species. It's a species that is holding some of the chlorine atoms in a form that is not destroying ozone. And so if that species were to be extremely stable, then very little would happen to ozone because the chlorine will always be tied up, but, of course, it was not perfectly stable and later they decompose very fast on particles, so that's why an Antarctic ozone hole . . . this species was so important. But in the normal atmosphere, it does decompose by photolysis, only relatively slowly.

[. . .] I was able to do relatively simple chemical modeling. What was more complicated is when you put it in the models, in particular one-dimensional, I was still able to do that, but then to see how far these things move and so on, but the fast chemistry you could do just simple chemical modeling and get an idea of what was happening. So we realized very early that yeah, this could slow down the chemistry but the models actually . . . we were surprised that when you put it in the models, where they were done because they had limitations. I think they were still one-dimensional, the result was exaggerated. And that's something we didn't anticipate. And then later it was corrected.

ROBERTS: So I'd like to talk a little bit more, if it's okay, about the measurement group or at least the change in instrumentation, your experience of it or your familiarity with it, because this is just barely more than a decade after Lovelock has made these initial measurements over the Atlantic which is just a little more than a decade after he develops his electron capture detector. So the way in which you're doing your work is being modified pretty rapidly by the changing instrumental community for this, or is it? Are you changing how you're doing your measurements and what you're able to measure and concentrations you're able to measure?

MOLINA: Yes, that was part of the creativity of the community there, and that goes back to what I was suggesting yesterday of a very striking interaction between fundamental basic science and in this case something applied because this was a motivation to further develop these techniques, otherwise it was a laboratory activity, but not receiving much emphasis. In fact, just to stress that what was receiving more emphasis were the molecular beam experiments because that was more attractive or more fundamental, but not very practical for atmospheric research because the experiments are under very high vacuum and just with very simple species so we have to do more sophisticated stuff. But that was very much pushed by the stratospheric things.

But just to tell you a little bit about this, what Lovelock had done is analysis of very stable species, but in very small amounts. And so that was important, and similar techniques could be used to just bring air samples from the stratosphere to the lab and see how much . . . what was the concentration of CFCs and some of the other **<T: 75 min>** stable species. You can measure methane, nitrous oxide, and so on, but the bulk of the research in chemical kinetics, if you want, or atmospheric chemistry was for the less stable species.

So those require these different techniques, either spectroscopic techniques, like resonance fluorescence, where you can measure very, very tiny amounts of some very reactive species, particularly, say, chlorine atoms because they absorb very strongly at one particular wavelength, and that's what Jim Anderson used to measure the chlorine over Antarctica. And so, but that had been developed as a laboratory tool. And so that was one approach with flash photolysis.

So you create these things and you follow them on a microsecond time scale; and the other approach was with flow tubes, where instead of looking at things in a very fast time, you flow things and you still have a very short time of reaction, but you didn't have to analyze at high speed because you were creating a system where the time was just a flow time, but at any particular point in the tube, you could spend minutes if you wanted to analyze what was happening there and with a movable injector, you could vary the reaction time.

So I started actually with one . . . with the fields with flash photolysis. In good measure, because my chemical laser work had been done with flash photolysis, but then it turns out that the more powerful technique for laboratory studies was flow tube and initially they only work at very low pressure, so one of the things we were able to do in our lab is to expand the range of the operation of those to much higher pressures.

But these had relatively little if anything to do with Jim Lovelock's approach because we were using either optical spectroscopy or mass spectroscopy to do the analysis, [where] you still require high sensitivity, but in a laboratory setup as opposed to analyzing a complex mixture of chemicals, which might have many other chemicals, but you were able to selectively measure the CFCs.

So much of the laboratory work was done along these lines and these techniques were developed essentially by the community. Some of us pushed it further or even came up with a brand new idea, let's do it this or that other way, and some labs specialized in any one or other technique, but eventually we interacted with each other, and so we were able to slowly use better and better approaches. And then we had to be able to measure things at very low temperatures.

Then eventually what became a big challenge and we also were able to develop instrumentation to do what we call heterogeneous reaction, reactions no longer in the gas phase, either with small particles or we could deposit things on walls and then measure the actual chemical rate of gas phase species, with species that are liquid or solid. So that was the essence of the push in laboratory chemistry very closely coupled to field measurements because they were very similar techniques in the balloons, airplanes or in the lab.

ROBERTS: Did you ever reach out directly to any of the instrument makers to ask them to make adjustments that would help you with the sort of questions you were looking to answer?

MOLINA: Not really because these were mostly homemade. Well, the sort of things we could buy, initially oscilloscopes or photon counters, but no, but these were all essentially all homemade. Or a mass spectrometer, okay, we could buy the end of it, but all the front, it was adapted to a flow tube in the laboratory. So no, we had interactions **<T: 80 min>**. Later, there were very good friends of Chuck Kolb who's . . . they had companies developing field instruments to measure particles, but using essentially similar techniques to the ones we used.

So they were part of the community. But no, we never had to reach out other than to the people making more the fundamental electronics or perhaps, for example, Fourier transform spectrometers, okay, so that was one thing we could eventually buy, of course, that we would not build that, but we had to adapt it and couple it to our lab experiments. So yes, but it's not something we requested. Once it became available, then we used it, just like lasers. There were a few companies developing lasers, and so when we were able to buy a few lasers just to be able to analyze optically certain types of species. Just like general lab research.

CARUSO: When developing these technologies for your . . . these instruments for your research, I mean, when I think of, you know, building something, I'm thinking of you need certain tolerances for what you're constructing, things have to be built to a specific size. Were you doing these builds yourself? Did you have someone in your lab that specialized in these construction projects?

MOLINA: Okay, here is how I remember how we developed in . . . I'm just trying to remember where . . . yeah, but it even started in Berkeley as a graduate student. If you're an experimentalist, then you learn, for example, we have two separate activities. That way they were not really formally part of your graduate studies in terms of a requirement for your degree, but they were very much part of your research. And one was to take a machine . . . I even forget the words now. We had to learn to build instruments. So we did machine work with the . . . I forget all the names of these . . .

CARUSO: The lathes and . . .

MOLINA: Exactly. Exactly. And so our task was to build certain pieces of instrumentation as part of the course. But we personally sort of built that. That was quite important and useful because later on, of course, we wouldn't do that personally and our students would do only a little bit, but we had professionals, machine . . .

CARUSO: Machinists?

MOLINA: Machiners and also you designed your instrument and you could bring it to them and they would build whatever you designed. That's even more of a task for field measurements because in that case, they have to be miniaturized and the electronics has to be more sophisticated. But in the same way that we learned to machine tools and so on, we had to learn some electronics. Early on, we have to get into the boards and fix the boards. Later on, it was just a matter of replacing boards, so things change a lot, but we had to learn all sorts of things from how computers work and so on.

And then the other specialty was glassblowing also. So we had to learn to [blow glass] because things broke in the lab. You have to do it, but in general, you had to know enough so that you could go to the glassblower and have him build your special instruments. Okay, so again, that's where field measurements and laboratory measurements had a lot in common, but it's not something you would order from a company. You would get from a company things that would be advertised, like, in *Physics Today* you see today mostly the electronics or sometimes they have . . . or lasers or low temperature devices, coolers or things of that sort. So you use what is available commercially.

You design your instrument and it's very useful to have worked yourself with machining or with glassblowing because then you can better design things that make sense. So that was very much **<T: 85 min>** part of the research.

CARUSO: I think part of my questioning comes from the fact that when thinking about technology today, there's a certain faith that technologies will work in a certain way over time and no matter where you go, that same technology will replicate data the same way. But when you're building your own instruments and gathering data that way, it seems like there could be the potential for scientists to say, "Well, I don't believe your data because I've never used your device because you built your own device." Does that ever . . .

MOLINA: Not really . . . well, it's interesting. It's an interesting perspective. I hadn't thought about this. Not really, because the idea— when your experiment is important enough—is the results can be reproduced. And so all these measurements, when they really matter . . . okay, for example, you come up with a rate constant and maybe it was a very difficult one to measure and then there's just one additional measurement, particularly if it's important, but more often than not, there are five or six measurements and people might be using different techniques, so you don't necessarily have to depend on or to trust somebody's specific technique because he reports it. Okay, he's like, "We used resonance fluorescence," and so you trust that he was able to build his laboratory machine to work, but what gives strength to the results is that it was reproduced with different techniques in different labs. But again, emphasizing that in a few cases, it's difficult, so there are just a few measurements carried out, but almost never would I remember a case where if something really important was only done once in one lab, you wouldn't trust that very much for those reasons. Let me give you one little story here, but it's very much to the point.

You know, this species that I mentioned, chlorine peroxide that we sort of made in the lab, we realized that it was a key species to explain the Antarctic ozone hole. So we had to measure how fast does it decompose with photolysis, at what wavelengths and so on. We did that early on, and there were a couple of other measurements. Fine. Which were in the right range and so these groups where we recommend results with some uncertainty, we recommended some results, that was fine.

It turns out that years later, it was not that long ago maybe, after the Antarctic ozone hole and everything was already very well accepted, of all places, at the Jet Propulsion Labs, one of my colleagues redid one of those measurements because it was important and again, tried to identify the chemical properties, photolysis rates, of this same species, chlorine peroxide. And they did what they thought was a very careful experiment and came up with unexpected results. Namely that we are just finding that this species is just much more stable than we had anticipated, so that's a big problem because how the hell do we explain the Antarctic ozone hole then? That means there's some missing chemistry, what we thought was very well-established science, there's a problem. It made a lot of news, particularly with the skeptics. And it took a couple of years but then several labs did very sophisticated measurements showing they have done something wrong. So I didn't do that anymore, but with Yuan [T.] Lee for example, a colleague, he's a Nobel Prize winner [Chemistry, 1986], he was not working with these sort of things, but he built some of the finest molecular beam instruments, so he did one of the measurements and Jim Anderson did another set, so several well-known people took this as a challenge. They say, "That's strange." And so they showed that . . . who knows what went wrong with that JPL experiment, but it made news because it was a well-established group. One measurement showing that three or four were wrong. Okay, but it's possible because the three or four maybe they were not careful enough because they had not subtracted this or that. Yeah, but then it got fixed. Then three or four key measurements, very clear, knock you guys . . . but this shows **<T: 90 min>** that what's interesting about this is how delicate the balance is. Okay, you do your research, you put everything together and then you can explain what happened, but based on certain measurements in the laboratory where you reproduce what's happening in the atmosphere based on measurements of what's there in the atmosphere and then you put it all together and it makes sense. So that's how science moves. But it's very much related to your question about instrumentation or trusting people or whatever.

CARUSO: One other small question I had, I sort of get a sense for what the answer to this is going to be, is some of the scientists that I've spoken with, they care more about the data being generated in their lab. That's what they find interesting, and are not necessarily . . . they are sometimes glad to be away from the bench. It sounds like you like being at the bench, and I'm curious why you like doing . . .

MOLINA: Well, what I like, when I moved to JPL, because it goes back to my childhood, was doing experiments and finding something new and doing the measurements. It's sort of exciting. And it's something in which you have to be very patient and you have to design your experiments correctly, but then it's very satisfying when you get results. So it's one activity. Another activity, some other people prefer to do that is just computer models. Okay, they don't touch the lab. But the main activity is just to put it all together and see that it makes sense.

So then what was even more rewarding, at MIT I had a big group then, so even though I was not personally doing experiments, I was working with my students and making sure the instruments were right and then interacting with them. And so it's this joint creativity, if you want, and sometimes the students have their own ideas and we tested them and so that's how research is done. Eventually it's a group activity.

But yeah, it's a matter of taste whether you like to do it yourself. But eventually, you have to . . . the important activities, you have to rationalize, to put it in the context of known theory, to write the papers and interact with your colleagues if you have to, so that's why sometimes—and it happens more and more often nowadays—you write joint articles even with groups that you don't know, that you've never met that are in Europe or something you write a paper with all of these authors because you are focusing on different aspects of a result that you are putting together and it's just different ways to corroborate the science itself.

CARUSO: One thing I'd like to maybe hear a little bit more about, the Montreal Protocol. I mean, in some ways, I don't want to say that's the culmination of your scientific work, but it can be thought of as the culmination of the public aspect of things that . . . thirteen years earlier. So if you could talk a little bit about it.

MOLINA: Sure, sure. What happened is that it became clear once we had the science moving and so on that it was important to have an international agreement. Okay, so it was a precedent for what later with climate change became better known, but the idea was—the components, if you want—was to create a group of experts that would meet maybe once a year or once every couple of years to write a report—a scientific report—and that's what preceded the IPCC [Intergovernmental Panel on Climate Change]. Okay, so . . . and actually still goes on. I haven't participated recently, but we had a tradition of meeting in Switzerland [in Les Diablerets], okay, so we put all the scientific papers, all the work and you had all the experts together and you summarize things in a report which is like an IPCC report, okay, and that's input that you then bring to the community of negotiators.

Of course you had to set that up, and it was set up as a joint effort of **<T: 95 min>** the World Meteorological Organization and United Nations, and so on. But there were a few key people there, because negotiators, each country has to send their own so eventually it becomes a big part. But, like, Mostafa Tolba was an ambassador from Egypt to there, he was very instrumental getting everything to work. And just like the IPCC has a chair who now is [Rajendra K.] Pachauri, a very good friend of mine, the chair before him, of IPCC, was [Sir Robert T.] Bob Watson, who was chair before that of the stratospheric ozone chemistry groups, [which preceded the IPCC reports and specialized on ozone panels].

Bob Watson is a very good friend. He was even at Berkeley at the same time. But he was a researcher at the Jet Propulsion Labs doing experiments in there, but then very early on, he decided he wanted to move to the policy aspects. Eventually he was the chief scientist at the World Bank. But he was sort of representing the scientific community, of course; that's why we worked very closely with him; working with people like Mostafa Tolba and a few others—I have all those names—who were the ones that pushed for an international agreement that eventually materialized in Montreal. They met several times. I went to a few of these meetings. I was an active participant of the scientific reports, but I was invited almost as a courtesy because I was not a professional negotiator. And making statements about the science or whatever, so it was very interesting to observe this.

There were usually reporters there as well, and so it was an outreach effort. But that's how the Montreal Protocol came about, and the first version still relatively weak, just calling for reducing emissions one half, and then when the Antarctic ozone hole and everything became clear, it said, "Let's just ban it completely." And there are lots of interesting thing happening. Initially, the European representatives were actually industry employees which were very much against any regulation. But then Europe changed and Margaret Thatcher, who was [. . .] a chemist, actually, so they took this as something of an interesting issue where they could sort of make a point.

And so it became a competition later on, so that was nice, because the US had dominated things before and the Europeans say, "Okay, we're on board." And so there it was remarkable that there was not much to fight against. The big difference with climate change being that there was this small number of chemical industries, du Pont leading as we were talking about, they had accepted the science so there was no major objection to this. So that's how it all worked. Very well, with, again with different groups, but we were collaborating quite closely.

ROBERTS: What role did you play in setting up the mechanics of how this would function?

MOLINA: Well, I was part of the initial discussions of this group that would judge the science. However, what I remember distinctly, or why I was sort of invited and participated . . . just like with the National Academy reports—being a member of the National Academy, I've been in a number of groups writing reports—but very specifically on the ones related to this problem, I was not in the group on purpose because we were the ones proposing this idea, so we were the ones being judged in some sense, and a little bit the same thing happened with the chemical reports. I was able to participate in other things, but not—on purpose and Sherry did very much a similar thing, helping and participating—but not in a dominant role because the idea was for that not to be perceived or not in practice being something that we were controlling, but we were just contributing to the science.

ROBERTS: And so am **<T: 100 min>** I correct in saying that this is mostly being driven by the U.N. [United Nations]?

MOLINA: Yes. The U.N. and government. Right, right.

ROBERTS: As an organizational sort of . . .

MOLINA: Right. Right, because they are the ones that set up the rules, asking each country to name representatives that would formally be accepted by the United Nations and then be part of the negotiations. Probably as with many of these negotiations, the main components were done in the hallways or before, ahead of time and then you had the formalities with everybody approving it and so on, but it's a very cumbersome way to actually work out details of things.

ROBERTS: So I don't know to what extent you can answer some of this because you were trying to play a slightly more distant role, but I have a couple questions about the actual mechanics of it because it does set up a model for how the international community can come together to talk about a global environmental issue that has a diverse set of scientists contributing to it. And then it's going to do these consensus statements and it's going to format their recommendations in a very specific way, and this gets replicated not just with climate change, but other groups tried to replicate this same model, whether it's around endocrine disruption or other emerging issues, but where did that idea come from, that this was a way in which a vast amount of scientific evidence could be condensed, turned into very specific bulleted recommendations sort of knowing their audience, that policymakers aren't going to read a very technical report.

MOLINA: Yeah, as far as I can tell, we did not have a precedent here. There might have been [but I´m not sure]—there were much more local precedents. If you had a spill in the Rhine [River] or some local problem in the US, then you also had perhaps scientific groups, but it was not the global United Nations effort.

So as far as I can tell, this was the first global effort. It was sort of obvious, so in some sense, it was a very reasonable thing to do. [. . .] I remember the heads of UNEP [United Nations Environmental Programme] and the WMO [World Meteorological Organization], the people that we were in close contact with and [who I was] very good friends with, we sort of designed these things. So it became a logical thing to do. We're not going to have the scientists negotiate. This has to be something done at the United Nations level, so we needed to summarize the consensus. So that's probably how this evolved, but there are not that many really global issues that have occurred [like] this, but again, perhaps the precedent might be environmental problems where several countries were involved, but not the entire planet.

ROBERTS: So the Montreal Protocol gets . . . the story of it now is usually told as the exemplar of how the international community can come together in relatively quick timeframe and act in a very decisive way to actually affect a problem. So I have two questions. One is, did it feel like it was fast for you?

MOLINA: Well, not really, and here is why. Remember we're talking about before there was this whole decade . . . well, the process started before the Antarctic ozone hole came about. So it was moving relatively slowly at the beginning. With the Antarctic ozone hole, then it became faster, but it took a decade. I don't know, so it was not a very fast process from some perspectives. It was very fast according to some other perspective and say, "Wow, you reached an international agreement and it didn't take that long." So it's a matter of perception.

But to me, it was very slow at the beginning, because we were meeting, but it wasn't clear, and that's when, again, I was telling you, many of the representatives were still heavily influenced by industry and said, "Okay, we'll meet, let's discuss that, but this might be very costly and we might lose a lot of jobs." Those were the objections, which all fortunately turned out not to be the case. We were already working with people trying to analyze the problem at large with industry, no, this is something society can deal with and we're not going to stop using **<T: 105 min>** spray cans, let alone refrigerators and so on, of course. But there were some difficulties I remember very important precedents. The precedent of the developed nations coming up with resources. They created the Multilateral Fund.

The US was reluctant for a while. They said, "Wow, this is a precedent. If we accept this, that means that we are going to have to be responsible in part. Also in economic terms for other, possibly other issues," but I remember this was discussed and eventually okay, we'll do it. And that turned out to be very important to get the developing nations on board and to help. In that case it was very clear. Spray cans were not used in many of the developing countries at all, so they had not put anything in the atmosphere, and yet they had to agree not to buy or use these chemicals, which you could consider that, wow, this means giving up a potentially high standard of living under certain conditions. Fortunately, that's why the solution was very important. You don't have to lose your high-quality standard of living, just do it correctly. It might be a little bit more expensive, but nowhere as expensive as if you don't worry about this. So, those were the important precedents. And it's still working. The Multilateral Fund is still functioning, and they developed a highly sophisticated way of checking everything. It was not just a matter [of committing, but there was follow-up and checking] whether there were emissions, whether [they were] illegal, the imports, all sorts of things. So that system [was] a whole community working there, which I don't work closely with at all anymore. I sort of [distanced myself], but I was invited, obviously, to the twentieth anniversary recently. I gave a speech, and it was all very nice [. . .]. The issues now are more to what extent should climate change also be incorporated into the Montreal Protocol. But essentially, all the other issues have been more or less resolved.

GONZALEZ: Sorry, just to interrupt you really quickly. I think an important fact is that Mexico was the first country to ratify the document and the first country to give a calendar for reducing emissions [of these gases]. So, I think . . . I mean, [you have to play it up a bit more].

MOLINA: That's right. Yeah, I forgot about that [laughter]. Yeah, that's right. Mexico was very happy. Sure, because I was, of course, close to the government at that time already. Yeah. But the US was very positive at that time also.

ROBERTS: So my only other question on that is what made that possible?

MOLINA: I think the science was very sound. And it became clear that replacements could be designed, and some of them were already there, ranging from similar chemicals, but that would affect the ozone less or not at all, to completely different chemicals, like hydrocarbons instead of CFCs. So solutions were realistic, at hand in some way, not terribly costly, and [based on] very clear science also. This is a risk; obviously there are always some uncertainties, but who has a clear argument that we shouldn't do this? Nobody. [There] might have been always these crazy opinions in some newspapers, but that we still see with climate change, of course, with a complete lack of understanding of what was happening, just attributing everything to government's trying to pursue control over people's lives. The typical things started earlier, but that was relatively minor. It didn't have any major impact. Another important component was that the world leaders decided to work together. They didn't have to fight each other. They were more rather competing to see who would do more. And so if you have approval all the way to the top **<T: 110 min>**, and approval from industries, I think that made it quite practical, and what happened with climate change, perhaps, what could have been a big barrier is developing countries say, "Hey, no, we want to . . . we're not sure. We want to be able to use these compounds because they give us a high quality of life. But that, fortunately, did not happen because, again, because alternatives were offered and so financial help as well. Lots of these things have . . . as you know, I'm very involved with climate change issues now, so that's . . . a lot of these things are quite interesting as . . . to contrast with what's happening now.

CARUSO: So I think this might be a good point to transition to your time at MIT.

MOLINA: Okay, okay.

CARUSO: And the first question I have is what precipitated that move?

MOLINA: Well, I decided—after I had done a few years of experiments in the lab myself—I decided that the academic life was something I could enjoy. I had learned some of that, of course, when I was at Irvine, and just to have a somewhat greater impact and hence more satisfaction in terms of my contribution to the field.

So it was no longer just my personal contribution doing experiments in the lab, but getting more involved also with some of the larger issues, both scientifically and perhaps even connected, beginning to be more connected with policy issues, although at that time, I was still . . . that was not that clear. But I thought I could have at MIT a much larger group and have more scientific publications, simply be more productive. And I would just give up—I thought that it's fine now—just doing the experiments with my own hands as we were talking about before. Although, if I wanted, I could have . . . I mean, I still did some of that with my students, but clearly if you have fifteen students, the idea is they do the bulk of the experiments, but you meet with them and you discuss them. And even the teaching itself, I realized, okay, it's something I actually enjoyed. It's a challenge, and we sort of improvised. We are not trained to teach, but it's a very good way to learn in spite of the students. So then at MIT with a smaller group, it was much more interactive, and I could already visualize that. So it was just a very attractive thing to do then to move up in the academic environment, if you want, just to have more of an impact in the atmospheric . . . or rather not just atmospheric, in the environmental chemistry type fields.

ROBERTS: So knowing that you were going to perhaps have access to a larger work force and some more funding, what were the big questions you wanted to be able to turn your attention towards?

MOLINA: Well, [. . .] this was just about the time that the Antarctic ozone hole and all that was being settled, so there were still important chemical questions that remained to be answered and more detailed reactions on surfaces, so I started to do a lot more of that type of work. And I remember also at that time already beginning to worry about the much more complicated chemistry lower down, like in Mexico City.

So just expanding with the sophisticated techniques we had learned to use with a much simpler stratosphere, and see how much more could we learn about more complicated chemistry lower down, which is also an environmental problem and requires a lot of science. So it's more dealing with more complex systems. That would be one way to look at it. Then interact with other professors. And I had a joint appointment in the chemistry lab but in the Earth, Atmospheric and Planetary Sciences where **<T: 115 min>** I would also work more closely with the modelers. So it was more of an expansion of vision, if you want.

CARUSO: So I think it might actually . . . I don't know if this sounds like a good idea. I do want to ask about you receiving the Nobel.

MOLINA: Okay.

CARUSO: In some ways, I think of that as, you know, I don't want to say the end, but you know, if you're moving into the lower atmosphere for your research, maybe finishing off with what would happen in the upper atmosphere and this recognition of your work. Maybe talk a little bit about that and then get into more detail about, you know, the joint projects between MIT and the Mexico City government and maybe go in that order, if that makes sense.

MOLINA: Sure. Okay. Now I have to review the timing a little bit. I'll do it afterwards and so if there's something to clarify, I'll let you know. But as I remember, the first years at MIT were still very much an extension of the more focused laboratory work I had done with a very small group at JPL, but still working on the more complicated problems, still in the stratosphere or even in the upper troposphere, getting with slightly more complicated chemical systems and more complicated instrumentation, as well. But it was not . . . well, the Nobel Prize came in between, I think, but the change—I'll come back to that in a moment—but the change in really saying, "Okay, let's worry about pollution in Mexico City." That was a very explicit effort that came out of discussions at MIT with colleagues in other disciplines because at that time, I was already doing what I was telling you about in terms of interactions more with the policy people, with experts in negotiation in the department of urban studies, and things of that sort, because MIT has a program where they give PhDs or master's in these disciplines, where you have a scientific background, but you work more on the practical societal problem.

But we got together, being familiar with that part of the problem, and said, "How can we do more about this to better train students; namely find a problem in which it's essential for the students, even though they have to keep their own discipline and be experts without losing any depth in knowledge in their own discipline, but be exposed to all the aspects required to solve a societal problem, which means they would have to learn a little bit about policy issues, about economic issues. In environmental chemistry, for example, they would have to interact also with epidemiologists or the medical community because that's the impacts that they would have, and with energy sources and so on.

So you have to have a picture of how the whole thing functions. So we decided, let's pick a problem, and being from Mexico, and air pollution in Mexico City being a big problem, said, "Hey, it's not a bad idea. Let's focus on air pollution in Mexico City because it's complex problem, requires lots of things." And I had access, because although it was certainly not working in Mexico except marginally at that time, I had good friends I was in touch with, government people, and it became feasible because there was an organization, Comisión Ambiental Metropolitana, and it was some sort of joint venture which had funding, and that was important, dealing with air pollution in Mexico City, but not **<T: 120 min>** knowing very well . . . or not having, perhaps, the right science component.

Anyhow, all these things got together and that's how I switched really, in terms of my own work in the laboratory, more focused on these sort of things. But for the students, it worked very well because my own students, of course, they still had to do their own laboratory work and learn chemistry or whatever they had to do without losing any depth there, and the same thing with the urban studies people or with the . . . MIT does not have a medical school, so we collaborated with Harvard [University]'s public health school, so we have very close relationships develop.

And there were students that were—in some sense this program I was telling you about was an overview of that—so they were involved with the policy aspects of these issues, and so we organized it so that the students had to do a joint model. They had to work a model involving atmospheric chemistry, physics, both health impact and some economic costs, and so on. And the fact that they had to come up with some results, not just talk to each other, but some results functioned extremely well for the students to collaborate with each other. And so in some sense, that's how my interest changed and then after that, I decided, well, I'm doing so much in Mexico, I might as well come to Mexico and create a center here. But that happened much later.

Coming back to the Nobel Prize—that was just a few years after I got to MIT so I was still doing this basic research moving more to heterogeneous chemistry— [. . .] I remember being quite surprised because some people had mentioned I was on some list, but I thought it was not a very serious contest. But yeah, so I got a call from Sweden and so fortunately I was . . . I remember that call because Henning Rodhe is a good friend of mine. He's also an atmospheric scientist, and I knew him, but he happened to be a member of the Royal Academy in Sweden, so when I first got the call from the president of the Society, I thought that was [a joke] . . . but Henning Rodhe came on the call afterwards and said, "Yeah, this is Sweden, this is all right." So anyhow, that's how it happened and it did . . . what that did to me is first, of course, being very happy and very honored, but then slowly realizing, look, not all the Nobel Prize winners . . . this has the other side of the coin. It's a little bit, like, a responsibility. Okay, how can you use your visibility, your convening power and do more things than just your own personal laboratory work? And so that's how the Mexico City experience was reinforced, with the Nobel Prize. Because I thought, well, perhaps that's why I could also have a closer interaction with the groups here in Mexico and get access to the funding that they managed to get from a small sur-price on the gasoline, just one or two cents or whatever they were able to . . . that was a lot of money. Not meant just to do research, but for public transport projects or whatever, so that was enough resources for us to get this project started.

CARUSO: And you also used some of the Nobel award, the money, to establish a scholarship program?

MOLINA: That's right. That's right. That was not directly connected with this work, but that was just some part left at MIT, still there, and some part here with the Mexican government. Fortunately, both cases with the idea that, I mean, after all, personal money, it's not a lot, but we were able to use it to get additional resources, so we got here some resource from the government and MIT from other donors, so essentially, these were scholarship programs that are still there. They are still working.

CARUSO: It's also during the time that you're at MIT in this period generally that you also become a **<T: 125 min>** U.S. President's Committee, a member of the Committee of Advisors on Science and Technology.

MOLINA: Right, that came a little later, sure. That was with [William J.] Bill Clinton.

CARUSO: Right, in 1994, right, so you were with Clinton till the end, then there's a gap with [George W.] Bush and then you're back on with [Barack H.] Obama.

MOLINA: Right.

CARUSO: And also, during this period of time, a little bit after the Nobel, you became a member of the board of directors of the Union of Concerned Scientists.

MOLINA: That's right. Yeah.

CARUSO: Is this all sort of your general movement towards the public policy?

MOLINA: Yes, in some sense. [. . .] I've got a good friend, another Nobel Prize winner [Henry W. Kendall, Physics, 1990], who created the Union of Concerned Scientists, who tragically died in a diving accident much later. But he was, again just like myself, trying to do things for society, for the environment, so it was very logical to accept this invitation and be part of that. And then, of course, later I became a member of a few other boards and so on, but in general, in the same direction.

But just to tell you a little bit more about this PCAST with Clinton, the first four years Clinton was not particularly interested in science because he delegated it all to [Albert A.] Al Gore [Jr.]. Al Gore was the environmentalist. And I had worked with him before, because he was a senator for [Tennessee] so we had met on several occasions. But fortunately, the second term, they really were [interested]. It's not enough for the vice president of the US to [want to act], you really need the president to do it [effectively]. Fortunately, Bill Clinton himself became very much an environmentalist, maybe a little bit too late. He couldn't get too much done anymore but he went to our meetings, so that was very positive and quite interesting (besides Al Gore, of course). And yes, then with President Bush, of course, were not there and then again, with President Obama.

ROBERTS: So you weren't there because you . . .

MOLINA: Well, two things happened. I was not invited, but I didn't make any move to try to get invited. What happened is that group persisted, but it was very heavily an industrial group, and these groups only work if the president himself has some affinity for the group or for science advice, and it turned out he didn't have much . . . I mean it was there, but it didn't do . . . President Bush was not well known either for his interest for science or for environmental issues.

CARUSO: But it's also during his presidency that Gore comes out with *An Inconvenient Truth*.[14]

MOLINA: That's right. That's . . . of course, if you remember, Gore lost the presidency . . . lost, and so that was quite heavy for him, I mean, quite difficult thing to live through, but then eventually, he [focused on other areas]. I continued interacting with him, but not nearly as closely as we did when he was vice president.

ROBERTS: So what was the atmosphere like, when you came back, rejoined PCAST in 2009?

MOLINA: Well, for some reason, President Obama is quite interested and pays a lot of attention to science, and so from the beginning, we were able to meet with him and have quite a bit of interaction. But, as expected, these were politically very difficult times and so his political advisors, of course, had the last word in terms of what to do about . . . climate change was the big issue from my perspective, but other issues we were able to . . . are still working very hard on STEM [science, technology, engineering and mathematics] education and restoring innovation through the development of scientific research, science also in the US enterprise system. And the health . . . of course, the health issue, as you know, was in some sense . . . some critics say, "Well, Obama could only do one thing at the time because that's the nature of the power in presidency," and so he chose to do health and he could have chosen perhaps on the environment, but with a Republican Congress, that's extreme opinions, it would have been extremely hard.

But, the point is, the first four years were still a lot of work, very productive, but not in climate change. And so now we just got started and it's much more of an opportunity because there's no more political pressure, but the Republican Congress is still there, so **<T: 130 min>** we can do quite a bit short of an international agreement, which is what's really needed. And focusing more and more . . . of course, the science we do here in Mexico and the Mexican government is, again, communication with the public, but with politicians as well, including the hope of communicating with some. Not the extreme [politicians of the party], but, of course, we get along very well with former Republicans, but with current Republicans, there are a few that we might be able to get close to. But it's a whole specialty. In spite of my experience with

[14] Al Gore, *An Inconvenient Truth*, directed by Davis Guggenheim, Lawrence Bender Productions (distributed by Paramount Classics), released May 24, 2006.

stratospheric ozone, it was extremely helpful. The difference now is the realization, this is really a profession. This is something that to learn from others that do these professions, so you have to be even more flexible on how to communicate to the public. So those are the more recent activities related to PCAST . . . well no, I should separate this. With PCAST, we do whatever is reasonable, whatever we can do, what is strictly outside politics. But this communication with the public is something to do, just like I did early on, my activities with stratospheric ozone.

ROBERTS: So I want to come back to the Nobel. I didn't want to stop the threads that we were moving forward. Would you describe the award as a surprise, or as a pivotal change in your career or you know, in your short description when we first asked, I mean, you're very nonchalant about getting a call from Sweden and being told that you're getting a Nobel Prize. Maybe that's . . . I mean, maybe I believe that that's the way you really are, but I'm wondering if there was any sense of closure or if this felt, like, you know, the . . .

MOLINA: Well, not closure. It's not as if I finally . . . "I'm finally rewarded for what I deserved." No, that just didn't cross my mind because I was already very satisfied, rewarded because the Montreal Protocol had been achieved. So I thought, okay, we were very lucky we found this problem and then we were able to actually get something done by society at large. So that part was done. And so the Nobel Prize is not really on politics, but just on science. So it was obviously very rewarding, but not something I was expecting in any way. Furthermore, part of the surprise was because there were practically no precedents for a Nobel Prize in environmental issues. They're all very fundamental scientific work, or this was coupled, this had some of that, but after all, it was an environmental problem, so that's very rare. There have been some, I think, very early on, connected with the atmosphere, but not with the environment. Okay, so it was not sort of unexpected, not to expect the Nobel Prize for the sort of work that we did. There might be other prizes. Of course, now there are more and more where they focus more on the environmental components. So, that's why it was a surprise, but then I realized, wow, this does change your life in some way because of the attention that you receive and I was already moving to having a broader perspective on the issues, so this just helped and made an even stronger point and then as I—over the next few years, knowing other Nobel Prize winners—I realized, they really change people. Of course, many of them received the prize many years after they had done their research, but many change. They devoted themselves to education or to something or other. There are relatively few that go back and do the same thing they were doing for which they received the prize. So that was normal.

But again, it's more like an opportunity or even a responsibility, in some sense. That's how it worked. It's still very rewarding, but I decided, wow, I could easily waste this opportunity if I don't do anything, okay, just sit back and **<T: 135 min>** retire. Fortunately, I now have a number of students that I was able to work with, particularly in my MIT years, that have done very well as professors in the academic community, so in some sense, I feel, okay, I did create my own school and they are doing the sort of research I could be doing. So it doesn't make sense to be even competing with them to do the same type of research.

Let me tell you one of the scientific things that was also very rewarding, in this dealing with Mexico's air quality, it was again very rewarding because we were able to impact decision-makers in government and set up regulations and air quality was pretty bad and so it actually improved quite a bit, not just because of what we did, because it was a logical thing for society to do, but not all major cities are doing that, okay, so it took some effort, but we were able to organize a number of smaller experiments, but two major sort of field experiments. Even if we did not have many resources, but because we had access to the system, we could invite some of the major research groups in these fields to bring their own instruments and so to do a one or two-month very intense field study of a very polluted city because that was an opportunity to learn a lot about how this complicated chemistry works, which would be harder to do in a clean city, It was easier here with big signals.

And so those two field studies which involved millions of dollars—but again, not ours, but just research money was out there—where it's just at least symbolically also very rewarding, something where we were able to push the science. Very applied science, science of complex systems, in this case, but that risk, hopefully has been very useful also to understand pollution and see the chemistry down here is much more complicated. We have bigger molecules, so you have to deal with it not at the very fundamental level, but some of it is more an engineering or a complex system approach, but there's a lot to learn, what really matters, what should you emphasize.

Ironically, some of the very recent developments in climate change are to make a much closer tie with air quality issues because [. . .] black carbon and methane and some of the compounds that matter for air quality also matter for climate change. And so the sort of science that we were able to push, for example, is very helpful for these sort of things as well. But I forget what was the question.

ROBERTS: No, you were fine. It was just around the prize as a pivotal movement.

MOLINA: Oh okay. Yeah, these are all things that were enabled, if you want, by the prize in some sense.

I could mention . . . my problem, if at all, is being interested in too many things because to solve a problem like air pollution or climate change, there are many disciplines, so you have to learn to summarize important aspects of many of these issues but also be able to communicate with people in these different disciplines. So that's a challenge, but it's very worthwhile. Economics, for example, is very important. That's why here in this Center, we're pushing that very hard. But I'm also quite interested in education, for example. So that's just a side issue.

But I was a professor so many years and I tried to help and push that to some extent and advising a group here, but it's connected more to the STEM education efforts in PCAST because there are new ways, new pedagogical systems that are much more efficient, starting with elementary school and then all the way to college, the online stuff, not because it's online, but because you get the students to get very much more involved in what they are doing by being active.

And I mention this as an example **<T: 140 min>** because one of my Nobel Prize colleagues, a friend of mine, Carl [E.] Wieman [Physics, 2001], who got his prize as a physicist working with single atom physics, he switched fields. So his field is education. So his expertise is in these new systems at college level. Here in Mexico [City] we have a big program, but at the elementary school level with the same ideas. Why do I mention it? It's just an example of something I could spend my full time on. There are just so many things to do, so we do this. Part of our activities here have to do something with education. But the challenge is to focus on something you feel you can have an impact on and do the best you can.

And here again, that's where the Nobel Prize is something to take advantage of, in that you have more convening power. Okay, people perhaps listen more to you, but you have to be very careful. There's the other side of the coin is the expectation that you become an expert on everything in general and no, we are experts on just a few things and perhaps we have a broad perspective on certain issues, but only if we put enough work in those issues to deserve that appreciation. But it's not automatic, by any means. But anyhow, the Nobel Prize is, indeed, a big event.

ROBERTS: So, I'm curious, you learned a lot of what you've done through experience, and I'm wondering what you tried to train into your students when you were at MIT.

MOLINA: Okay, on the one hand, of course, the research itself is like the experience I had when I was at Berkeley's fundamental, just to give them the room to be creative and to understand things at the deeper and deeper level whenever they can and to contribute to the advancement of science in very, very general terms.

So that's why it's rewarding when you see that they respond and they learn and you are forming people that can contribute a lot. But then there is this other aspect, as I was telling you, to work with the other disciplines, and so we realized it's important to, if you can, to train students to appreciate—not just to appreciate—but to be able to communicate with experts in other disciplines because the normal tendency we have is to teach a very narrow language so that your students and you yourself, you communicate with experts in your own field but you'll develop a language so nobody else understands you.

Why not learn and have a better perspective of societal problems, a better perspective of what it takes to have an impact in real life. Realizing some students are going to just remain in academia and do their own things, but many others perhaps will branch out and maybe will go to even government positions and so on and so forth. So I think this is perhaps is somewhat connected with this education component I was telling you about in that it's quite feasible to take advantage of the scientific culture we have in the international scientific community, which is not just to be a good scientist, but you have also some values, okay. The values are, for example, honesty, and it's because if you cheat, you're going to be caught, most likely, [. . .] but you do it not just out of worry that you're going to be caught. You develop a culture, which is what you would like to communicate to your students. You have to work in a certain way that it's honest. You also have to worry about, if you have choices, something that will benefit other people as well and so these are the sort of things you can communicate quite well in a university. But if you don't worry about them, they might happen **<T: 145 min>** or might not happen, it's just by chance.

CARUSO: I have questions about training. Maybe this is a poor characterization of it, but in some ways what I'm hearing is that graduate level students in science need to be trained not just as scientists, but as scientific citizens. But there's also, I think, something in there, and I don't know if you spoke about it specifically, but you know, in thinking about what you said yesterday about your teachers in school, there's also this apparent need to have those teaching science also be scientists and how . . . I mean, do you have any thoughts about how that should be addressed more broadly? I mean, you said you had it as a criticism of the educational system in Mexico. I've heard that as a criticism of the educational system in the US, as well. You have non-scientists teaching science. So how do you get past that?

MOLINA: Okay, it's not an easy question, but let me give you some ideas. First, [. . .] going to the beginning of the question, I would expand it. Ideally you do that not only with graduate students. At MIT, we had an important program called URO—Undergraduate Research Opportunities. So you get upper division students, you try to integrate them into a research group already, precisely so that you can communicate with them these sort of things we're talking about, so it's not just graduate students. It's so important that we think it should be part of undergraduate training, as well, to the extent that you can, because then they see how teamwork functions and how creativity works and so on. And not just listening or memorizing things.

In terms of experts, what is clear is that's to be expected at the university level. There you want people ideally who are doing research, who are creative and who are real experts in their own field and not just beginners, if you want, what might have happened to me when I was in college. But in high school and at undergraduate, you cannot expect everybody to be a scientist, but at least to be well trained in science. And we, of course, we see that in Mexico, but also in the US, there's the job as a teacher, in some countries, perhaps Finland and a few exceptional countries, they are so highly regarded that they are at the top of society, but not in the US and Mexico.

And so you end up having, in elementary school, teachers teach everything including science when they themselves did not have anything like a satisfactory science instruction. So they are bound to not teach well. So you need two things. You need the pedagogical tool and so on, but you also need to understand what you are teaching. And the challenge here, it's one of the things one learns, is it's a challenge to simplify things. You have to understand things very well to be able to express them in simple terms, and that goes all the way to elementary school. So ideally, you have well-motivated teachers.

These problems I was telling you about that we are involved in, they get a lot of help. It was developed initially by National Academies, but a very important component is to prepare the teachers themselves. Okay, so the teachers learn a lot and so that's important for obvious reasons, that you cannot judge or teach something you don't really understand, okay, but that's very common, particularly in the sciences or even . . . I remember the history teachers I had, well, they might have been very good in terms of memorizing names and so on, but what is very interesting is what's a historical perspective and sociological and that's not all that common or trivial. That's why I hated history in high school. It was just memorizing names and dates and the same thing happens with chemists.

That's the symptom of that is that chemistry's awful for most students. It's just something, **<T: 150 min>** wow, chemistry. I dislike that so much. I didn't understand it. Or even math. Okay, you could make . . . even math could be quite attractive if you teach it in an interesting way. But yeah, that's the challenge for teaching, but it goes all the way to college.

ROBERTS: So I want to make sure we spend—I know we don't have a lot of time left—but I want to make sure we spend some time talking about the post-MIT years and the co-founding of both the Center that we're in here in Mexico City and your move to San Diego [California] and thinking about those, what you wanted to accomplish moving here and just in case I don't know what role they'll play, but I think one of the pieces that I've been hearing for the last two days that I at least want to put out there and maybe they'll work into some of what is happening here, are these larger conceptual changes that are happening.

I mean, you've been very focused on sort of very specific problems that you've been addressing and very specific technical work that you've been doing, but there was a lot of . . . there was a lot of knowledge shift, cognitive shift, you know sort of larger epistemic shifts going on that you were a part of, that, you know, thinking about the earth as a system. That changed a lot of how, you know . . . the space in which you could do your work and the work that you were doing was changing that.

The idea that the atmosphere is an open laboratory and that it's not just putting species of chemicals out there, but that the species that are out there are themselves having reactions that are out of our control. So the idea of secondary organic aerosols and thinking about the things that are out there that we hadn't thought about before. So there's a lot of other pieces at play and I don't know . . . it seems like when you came here, you know, your time at MIT and then your transition here to the Mario Molina Center and the position at [University of California at] San Diego gave you a chance to start thinking about some of the larger, more complex systems. Anyhow, that's a long preface.

MOLINA: Yeah. Let me give you a few general ideas. What I wanted to do is to have time to open the Center, but to remain in the U.S. as well because of being in PCAST and so on and remaining part of academia there. So it had to be a compromise, and it was difficult to do that at MIT because MIT doesn't like to have part-time people, if you want. But they didn't want to let me go, and I have lots of very close connections with them still, but not being a part-time professor. I had that opportunity in the University of California, which I was at the beginning.

So they were willing to say, "Okay, it's all right. We know you're not going to be here, a full-time professor doing research. It's okay. You can spend time in Mexico, but hopefully you can contribute here, as well." And fortunately, I think that has worked because what I tend to do in San Diego is a more scientific component. So we do have an important effort there, which [.. . .] first I brought my MIT students that had not finished their PhDs. They did their experimental research still while I was at San Diego, so that's why I still had a lab there. But I decided to close it and not to take new students there, but just to collaborate with other groups. But there is a very important research effort looking, of all things, at atmospheric particles, but in the lower atmosphere. Okay, that's one of the main uncertainties still in climate change science.

So there I collaborate with several faculty, and we're trying to get now a large NSF-funded center to do that. And so the science is very exciting and keeps moving, but I'm no longer with my own group doing that. I advise students and collaborate. So it's a challenge, but as far as I can do it, I'll try to remain active in a scientific field. But there [are] these educational issues, but they are not necessarily an important part of my activities in San Diego. They are more a part of the other things I do. Here in Mexico, the specific activities of this Center, if you want, is mostly in connection—to begin with—with the Mexican government, but we're beginning to have interactions and impacts with the rest of Latin America, as well, for logical reasons.

But in between those, there are the larger **<T: 155 min>** complex system issues, climate change and how do you get things to happen. Specifically, in this Center, the goal was to see, can we impact the way society functions? For example, if we want to have an impact on the air quality issue, well, it's not going to be enough to write papers. It's a very challenging objective, but we will only consider we succeeded if the air quality actually improves. And maybe it improves on its own, but, I mean, it certainly will improve because of many activities, but we can tell where we helped.

And because air quality is a bigger issue, but we were able to interact very closely with people here in government itself, and have changed the regulations and things, that renew the fleet, emphasize public transport, all sorts of things. So what we're doing more and more now, perhaps connected also with climate change, not just with air quality or the combination, is working with the Mexican government here but with that overarching goal, very ambitious, if you want, that if we succeed, things will change. We don't want to just write reports because we could do that at a university or something. That's a big challenge. That's where you need this broader perspective because it's not enough to be a good scientist there. You need to understand how [society functions]. What are the political pressures? If there is corruption somewhere, how can we get around it? [. . .] If you want to fix something that is not working, you really have to understand how it is working now, what are the difficulties, and you have to be very creative. Can I work with people elsewhere to change the system?

So a general rule that we end up using is—well, it's something that goes back to MIT with these negotiation courses that came up from urban studies and also from Harvard—is you get the stakeholders at the same table. So this just means from the beginning, if we want to solve a problem we get people from government, the key people to work with us, from the private sector, necessary from society and we don't write a report and tell them, "This is what you have to do." We organize activities in such a way that this comes out of the group.

But nevertheless, all these challenges I was telling you about are there. So this is a very different thing than fundamental science because it involves understanding how does society function, but not in any theoretical way that you could be doing at the department of sociology in some university. It's that the very practical way . . . how does society function? And so we've learned enough so we have succeeded in doing certain things and we're trying to use that, as some challenges are very big. We have to choose things that are feasible and we're still working on many others that fortunately we do have close connections with government and also with the private sector.

We were talking before about extreme environmental organizations. Okay, this is something that wouldn't work there. If you come out as an advocate pushing the environment at all costs, then you're not going to be able to work with the private sector. You have to talk to them. You have to convince them that it's not just good for society, but if you do it in a creative way, it's good for their success, as well; for the country—climate change issues and so on—it's good for the economy. So you cannot count on just values of people being very good citizens. No, you have to be very practical and think, "Okay, this is the way society will improve."

So that's a big challenge to understand all these issues and to be able to communicate them. One example I'll give you, **<T: 160 min>** which it's one project that we have more or less succeeded. It's housing. Why housing? Well, for environmental issues, climate change, you could name it green housing, much more efficient housing than you could otherwise if you don't worry about it. Okay, but that's not enough. So it turns out, we got together with a private business, the private sector, several large companies whose business is to build houses. And they were making all sorts of mistakes because their business was to buy land wherever was cheapest and then with government money to build the houses and then they got the money.

And it turns out, many of them are abandoned because they are already far or not agreeable, so things are very wrong. Okay, what do we do? Well, let's sit together. We get the industry people. But we get the government people because there are government institutions that do that. We provide the environmental expertise, because there is all sorts of literature, how can you build much better insulated houses. And in the end, they are cheaper because you save energy. So, we have a system where we work not just the environmental issues, but the economic and the social issues.

So these people are now building housing developments which are much more attractive. First of all, they are in places where it makes sense, but then they build schools, they build . . . so the social component is crucial. Okay, how do they succeed? And then the government has to provide incentives. Okay, and regulations, so otherwise, you have the free rider. Someone is going to take advantage of that. And this is working very well. So what we did here is provide the measure. We quantified that and so we . . . it's, like, even a grade book. The point is not to grade how well they are doing now, but where should they move to. So we measure the social component, the economic component, the environmental, but with many different measures and so it gives them a very clear guide how to move. But it's just one example. They are happy, big government is happy. We are improving things. And so it can be done.

The transportation sector is even a big challenge because not many people . . . everybody wants to have cars, so you have more and more cars and things get worse and worse, so you have to put barriers to the use of cars and that's very difficult because you have to convince society that that's not good for them. But in New York, okay, you don't have everybody driving their own car to work. Okay, so there are good examples [. . .] but you need very good public transportation.

So, you have to be clear and say, how the hell do I manage to change the course that things are moving onto, which is every day more congestion, spend hours getting to work, more pollution. Well, yes, but we have to . . . this enormous barrier of convincing people not to use their cars. So anyhow, these are just examples of the enormous challenges that require you to have just a much broader perspective of the problems. But you can't forget that there's a very strong technical component, the best practices. Okay, how can you build better houses? Well, there are these advanced technologies. You can . . . and so on, and they have to be cheap and with cars and public transportation, you have to be aware of what's the state of the art. So the other challenge is to make sure that we don't use obsolete technologies. We take advantage of things that are working elsewhere. So there's a very important technological, and, if you want, scientific component here as well.

Okay, so you have to put it all together. So that's the challenge here in the Center and I was talking also about more general climate change. That's a political issue that sometimes we have to begin to work with communication experts. One simple idea here: it's very clear in the scientific community that there is practically . . . it's not unanimous, but almost, 97 percent of experts in climate change agree, hey, climate is certainly changing and it's most likely, not absolutely certain, but it's most likely something that humans are causing. There are a few scientists **<T: 165 min>** that deny that, but it's actually a lot less than one percent.

And we know very well why they are saying that and the science behind that, so it's nothing . . . it's not that we totally ignore them, but they are libertarians or they pick certain . . . we know science is not perfect. You can always find problems, but they don't affect the overall picture anyhow. That's very clear. But if you . . . surveys show that, and of certainly the politicians almost everybody thinks, "Oh, some scientists tell us it's this way, but there are many others. So science is not settled, so we shouldn't do anything about it." No, that's completely wrong. We need to make . . . but if we just write an op-ed or a scientific paper, nothing happens. You have to do something very well-focused if you want to change public opinion on this specific aspect. Extreme events or . . . anyhow, that's a whole . . . but that's another important component of my interest, which is not specifically part of either the San Diego or the activities here at the Center, but it's part of the overall effort, okay, because we certainly want . . . Mexico is one of the leaders in the climate change, in the political arena in the developing world, because fortunately President [Felipe de Jesús] Calderón [Hinojosa] was very interested. I was able to work with him quite closely. And so it's something . . . there's a climate change law here in Mexico, but you have to be realistic, of course, not to do something that backfires with the economy.

But what I'm saying is we have some very specific goals here and projects, energy, housing, transportation, and so on, and then there are these broader political issues that are less tractable, but nevertheless crucial to solve the overall problem eventually. So you see how you have to look at the big picture. That's another way to look at it.

ROBERTS: Well, but it sounds like it's more than just looking at the big picture. It sounds like your challenge and what you're trying to do with these multiple roles is you have to keep an eye on the big picture, but you have to look for the small, focused project that you can do without having to tackle the entire big picture.

MOLINA: Right.

ROBERTS: And I mean, I don't know if you want to share any thoughts on the difference between that experience and the Montreal Protocol. I mean, I think, you know, most folks who say, you know, the Montreal Protocol was this great example of everybody coming together, but there seemed like there was this direct route into how to fix things and it's not quite the same way with climate change. But it is an aggregate of a lot of other potentially solvable issues.

MOLINA: Right. You described it just right. We were lucky in some sense, with the Montreal Protocol, that it could be a much more focused issue. We got a lot of help from the community and so obviously the science turned out to be crucial, so that worked. But if you look, coming around something completely different and much more local, air pollution in a city like this, well, there it's not just a science of air pollution. There, the challenge is which with the Montreal Protocol, it was setting the United Nations emphasis, which we did with not ourselves, but it was an effort of the community.

But here, we would start . . . it's not that we did everything ourselves, but we would start talking to the key people, getting the right funding, giving key ideas and then making sure they get incorporated into the local regulations. So in some sense, this sort of thing is more challenging, although it's smaller, and also because we were set up to do the Montreal Protocol thing, but it was nevertheless an important experience to see what it takes to do these sort of things and to see, well, there are economic barriers and things we have to be . . . you have to do things in such a way that society will accept them. And in the case of air pollution, yeah, it's just like with climate change. When the city was very polluted, then the society itself gave a clear message to the government. You have to fix this. But once it's not very polluted, it's no longer on the table. Then it's harder. **<T: 170 min>**

GONZALEZ: I'm sorry to interrupt, but I think you just ended perfectly. We have about five minutes, five, ten minutes and I don't know if you want to take pictures or . . .

ROBERTS: No, I think we'll just wrap up. I could talk to you for hours more, so thank you very much for the time you've shared with us the last two days.

MOLINA: If something else comes to mind, we can certainly do something over the phone or look at different things.

ROBERTS: Perhaps we could even get you on a train up to Philadelphia the next time you're at a PCAST meeting.

MOLINA: Okay, sure.

[END OF AUDIO, FILE 2.1]

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