CHEMICAL HERITAGE FOUNDATION

ENRIQUE IGLESIA

Transcript of an Interview
Conducted by

Hilary Domush

at

University of California, Berkeley
Berkeley, California

on

27 and 28 January 2014

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Center for Oral History
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Interviewee's Name

21 October 2021
Date

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

ENRIQUE IGLESIA

1954	Born in Havana, Cuba, on 27 August

Education

1977	BS, Princeton University, Chemical Engineering, *summa cum laude*
1979	MS, Stanford University, Chemical Engineering
1982	PhD, Stanford University, Chemical Engineering

Professional Experience

	Exxon Research and Engineering Company
1982-1993	Corporate Research Laboratories Research Associate, Section Head, Catalysis Science
	Stanford University
1988-1993	Consulting Professor of Chemical Engineering
	E.O. Lawrence Berkeley National Laboratory
1993-present	Faculty Senior Scientist, Chemical Sciences Division
	University of California at Berkeley, College of Chemistry
1993-present	Professor of Chemical Engineering
1999-present	Director, Berkeley Catalysis Center
2006-2009	Chancellor Professor of Chemical Engineering
2009-present	Theodore Vermeulen Chair in Chemical Engineering

Honors

1976	Tau Beta Pi, Princeton Chapter President, 1976-77
1977	Silver Medal of the Royal Society of Arts, Princeton University (highest-standing graduating senior in Schools of Engineering and Architecture)
1977	Phi Beta Kappa
1992	Golden Tiger Award, Annual Exxon Award for: “Inspirational Leadership and Outstanding Contributions in Catalytic Science and Technology”
1995-1996	American Institute of Chemical Engineers Award for Chemical Engineering Excellence in Academic Teaching, California Chapter

1997	Paul H. Emmett Award in Fundamental Catalysis; North American Catalysis Society
1998	Award for Excellence in Catalysis and Eminent Visitor Award, Chemical Society of South Africa
1999	Best Teacher Award, Berkeley Chapter, American Institute of Chemical Engineers
2003	Richard H. Wilhelm Award in Chemical Reaction Engineering, American Institute of Chemical Engineers
2004	Award for Excellence in Natural Gas Conversion
2005	George A. Olah Award in Hydrocarbon Chemistry, American Chemical Society
2005	Donald Sterling Noyce Prize for Excellence in Undergraduate Teaching, University of California (highest teaching award in the physical sciences at the University of California at Berkeley)
2006	Robert Burwell Lectureship Award, North American Catalysis Society
2007	Humboldt Senior Scientist Research Award, Alexander von Humboldt Foundation
2007	Doctor Honoris Causa, Universidad Politecnica de Valencia, Chile
2008	National Academy of Engineering
2009	Tanabe Prize in Acid-Base Catalysis
2010	Fellow, American Chemical Society
2010	Best Teacher Award, College of Chemistry, University of California at Berkeley
2011	Francois Gault Lectureship Award, European Federation of Catalysis Societies
2011	Alpha Chi Sigma Institute Award, American Institute of Chemical Engineers
2011	Cross Canada Lecturer, Chemical Institute of Canada
2012	ENI Prize, New Frontiers in Hydrocarbons
2012	Gabor Somorjai Award for Creative Research in Catalysis, American Chemical Society
2013	Fellow, Japan Society for the Promotion of Science
2013	Honorary Fellow, Chinese Chemical Society
2014	Fellow, American Institute of Chemical Engineers

ABSTRACT

Enrique Iglesia was born in Havana, Cuba, one of two children. The family lived in Havana until Enrique was about fourteen years old; he was then approaching military age, at which time he would not be allowed to leave the country, so they moved to Mexico, where they lived for six months, awaiting papers to enter the United States. In Miami, Florida, Enrique's intellectual abilities were recognized, and he was placed in advanced courses in math and science, and he also took college-level math classes at Florida International University. Iglesia entered Princeton University because of his math teacher's recommendation, intending to major in chemical engineering, and he found the education there excellent. John Weikart of Exxon Corporation began to recruit him. Iglesia had summer internships at Exxon and became interested in catalysis. He entered Stanford for a PhD and began research right away in Michel Boudart's group, working on the applicability of model systems to real-world catalysis. Iglesia married at the end of his first year in graduate school and by the end of his PhD, they were expecting their first child, so Iglesia needed to have a job. He accepted Exxon, expecting to return to academia eventually, and they moved to New Jersey. Iglesia advanced to the position of section head, supervising about fifty scientists and support staff, and found the science to be first rate. He also taught a seminar at Stanford during several summers.

Ready to return to academia he accepted University of California, Berkeley's offer and also became a consultant to Catalytic Associates. He and Fabio Ribeiro worked together at the start and his research on membrane thin films continuing a project of Heinz Heinemann, the "father of organized catalysis." BP organized a collaboration of scientists from Caltech and Berkeley, a small group called Methane Conversion Cooperative, that lasted ten years and worked on gas conversion. Iglesia promoted thinking over excessive use of technology; he wanted to see real-world materials under real conditions, not just in models. He has started a new, smaller, group, the X Conversion Cooperative, which has reached its fifth year and continues beyond. His group has been working on Fischer-Tropsch synthesis again, as well as other reactions of C_1 molecules, such as carbonylation and tripane synthesis. In addition, Chevron Corporation has been funding research into zeolites, which the Cooperative has learned to form around a precursor, and van der Waals interactions, and auto manufacturers have supported research into exhaust problems. He has also been co-editor in chief of the *Journal of Catalysis*.

During his interview, Iglesia mentions many other scientists who have been important in his career and describes some of their work. He talks about liking teaching, though he finds that working with the different personalities of students can be challenging. Iglesia says that academia provides freedom to do what interests him. He analogizes a scientist's students and their students to a family "bloodline." Iglesia says that predicting too far in advance can narrow one's vision. Iglesia is proud of his family, of having close friends, and that he is still learning things.

INTERVIEWER

Hilary Domush was a Program Associate in the Center for Oral History at CHF from 2007-2015. Previously, she earned a BS in chemistry from Bates College in Lewiston, Maine in 2003. She then completed an MS in chemistry and an MA in history of science both from the University of Wisconsin-Madison. Her graduate work in the history of science focused on early nineteenth-century chemistry in the city of Edinburgh, while her work in the chemistry was in a total synthesis laboratory. At CHF, she worked on projects such as the Pew Biomedical Scholars, Women in Chemistry, Atmospheric Science, and Catalysis.

TABLE OF CONTENTS

INTERVIEWEE: **Enrique Iglesia**

INTERVIEWER: **Hilary Domush**

LOCATION: **University of California, Berkeley**
Berkeley, California

DATE: **27 January 2014**

DOMUSH: Okay. So today is Monday, July 27. I am Hilary Domush, here at UC Berkeley [University of California, Berkeley] in Gilman Hall with Dr. Enrique Iglesia. Did I say everything correctly?

IGLESIA: Yes. Very good.

DOMUSH: Okay. I always like to make sure. As I mentioned to you, we like to start at the very beginning, and I know that you were born in Cuba, and I was wondering if you could talk a little bit about your childhood there, if there are any particular stories or reminiscences that you want to share from those early days of childhood.

IGLESIA: [Yes]. Well, there's a very early part I guess that I don't remember very well, of course. But also there is a part of my childhood that, of course, I learned from others, who [told] me the stories. [Such early experiences have], I think, colored a lot of who I am and how I am, to point out the very accidental nature of my birth, so to speak. It turns out that all births are accidental births, okay? But in my case, it required that first my grandparents [Encarnacion Gonzalez and Daniel Vecin] leave on the one hand Spain during the First World War, and end up in Cuba, and that's my mother's [Emelina Iglesia] side. I came from there. My mother was born in Cuba.

My father's [Rogelio Iglesia] side was even more accidental in the sense that he left as an adolescent from Spain and went to Cuba during the Spanish Civil War, so they were . . . sort of, the wars in Europe somehow brought the two of them to Cuba. They came from totally different parts of Spain. They would have never met each other, and they met as a result of all the upheavals that stirred the world at that point.

And then they met there [in Cuba], and again, I'm sort of the result of all those accidents, so if any point along the way I begin to take myself a little bit too seriously, I remind myself that the probability of my being here today is 10^{-32} or something like that, and then all of a sudden I'm human again. [laughter]

So I was born in Havana, [Cuba]. I was born in the historic part of Havana, not very far away from the Presidential Palace, in a middle to lower class neighborhood, with the hustle and bustle of a fairly large and vibrant city at the time, but also in a location that exposed me early on in life to a lot of the incidents of the [Cuban] revolution. So I got used, very early in life, to shootings that happened when the guerillas would attack the Presidential Palace, or when the Bay of Pigs invasion came in, and the planes were trying to bomb it. So I was not very far away from sort of discovering my mortality right then and there.

I remember quite clearly being thrown under a bunch of dirty clothes in my father's dry cleaners whenever the shots began to be fired, and my sister and I would stay there, you know, half of the night, so I have to say that it was not what you would call a fairly placid childhood. It was also in the middle of an area in Havana that was a commercial and residential district, where there was a significant amount of pollution. I mean, there were buses running one street over. I had [some] respiratory problems when I was a kid as a result of it. So, I mean, it was not an auspicious start, I think, for the most part.

DOMUSH: [Yes]. May I ask really quickly—your sister, was she older or younger?

IGLESIA: Younger than I am, and not a scientist. So I was the scientist in the family. And again, from what my parents tell me, I was fairly introverted, and to this day I am, okay, but [also] someone who could concentrate on doing something for hours at a time. So I was an early reader. I liked to make up my own games. I devised a way of making a baseball game with dice and I made up all the rules along the way. I would sit there for hours to the point that my parents sometimes worried. This was before Asperger's was properly diagnosed, I guess. [laughter] Otherwise, I would have been put on some sort of medicine to deal with that. But I had the ability to sort of shut down the world, and part of it was [about] surviving a difficult situation. I think a part of it was that I had pretty good powers of concentration.

I did, as I was growing up, very well in school. **<T: 05 min>** By the time that I entered primary school, the revolution had ended. You know, the previous [regime of Fulgencio] Batista was out of power. [Fidel] Castro had already taken over. A lot of the school system was sort of populated by teachers that had been recently trained. So one of the few positive sides of what happened in Cuba was that education became an important objective, but mostly by spreading education throughout the entire country. So, I mean, there was [before] a very large division between the haves and the have-nots in the cities and the countryside in both health and education. And as a result of it, a lot of the teachers that would otherwise have taught in the city schools were now sent out everywhere [else] to be able to train the teachers that then would teach everybody how to read and write.

So I ended up with a lot of young teachers that didn't have very much experience, but the educational system [in terms of content] was extremely good. And, you know, a lot of what we would cover [here in the US] would be covered at a much earlier age there [. . .] to roll the movie forward. There was a level of separation in school between the children of the parents who were part of the government system and were part of the party and all that, and the children of those who were businessmen like my father was, and who ultimately declared their intention to leave the country. So to some extent, I was in the difficult position of being someone who had been identified as somebody who would leave [the country], but yet was clearly talented enough not to be sort of abandoned altogether by the teachers and so on. The [teachers] were very nice to me and they treated me very well.

But what they did do, which sort of marked me for life, I think, and did not help me very much with my social skills, and my wife [Terry] will tell you, is that I skipped two grades along the way, because I was more advanced than [the rest of] my classmates, and I tended to be disruptive by being bored. So I did not do third grade and I did not do fifth grade. And what happened is that made me two years younger than anybody else in class along the way. So the idea of socializing with the opposite sex was not, you know, appropriate at that point, because I was the little wonky kid that was younger than anybody else. So that stunted my growth socially, to some extent. [laughter]

But other than that, the other thing that I would point out is that my parents . . . none of them went past probably junior high, so I was the first one to develop any sort of apparent talent [for intellectual pursuits]. But my father in particular was very, very smart. He just never had the opportunity.

DOMUSH: Right.

IGLESIA: I mean, he lived in a small village in Spain where there was hardly any access to education. He—

DOMUSH: May I ask where in Spain?

IGLESIA: It's in Galicia, the northwest part of Spain in the Province of Lugo, but in a very, very tiny village where the family really had cattle, farmed a little bit, and had a quarry that provided a lot of the stones for the dirt roads that they built along the way.

DOMUSH: Oh, wow.

IGLESIA: And at the age of twelve or thirteen, they would effectively stop going to the place in the village where they were taught, and there is a story about that, but I'll get to it perhaps later. And at the age of twelve or so, they would stop the schooling and they would begin to work in the family farm.

DOMUSH: Okay.

IGLESIA: He would tell me the story about how he left because of the Spanish Civil War. I realized much later that he left because he felt like he was not going to be able to do anything in that village, and apparently, the school there or the school teacher had told him that he had a lot of talent, and [. . .] so they began to sort of put ideas in his head and so on. And he left because he wanted a bigger world, somehow, than what he could get in that village.

I guess there were two people that were very influential for me [in the context of education] as I was growing up [. . .] my aunt and uncle [Maria Dolores Vecin and Fernando Garcia] on my mother's side were among the most educated people in the entire family. My mother, **<T: 10 min>** by the way, was an orphan at age four. She had [five] sisters that my grandmother had to raise including at various times along the way having to spread the children among family and even an orphanage [that accepted boarding students], where my mother spent a few years of her life. That marked her for life. But this aunt in particular had been fortunate to finish high school and was married to an individual whose social skills were not particularly good, but who was very smart, and had a PhD in education, and he was very heavily involved in the government's attempts to educate. They also recognized that I had certain talents, and they always encouraged me. They were the ones that would give me books to read, and tell me that I was going to be famous one day and all that. So, you know, even though throughout my life there wasn't that much in the way of building self-esteem by part of my family—they never had time to do it—these were the two people that sort of patted me on the back and said, you know, "You're going to be great one day."

It's funny, because that marriage broke up, and [. . .] they divorced when I was like eight or nine or so, and to me, that was one of the . . . You know, I understand what happens to kids when their parents divorce, because here was my favorite aunt and uncle, and all of a sudden he ran away, okay, and that was crushing to me. It turns out that there's a happy ending. He came back many years later, twenty years later, and they remarried, but I was gone by then, so I didn't benefit [but I never forgot].

So that was my childhood, for the most part. I was . . .

DOMUSH: Now you mentioned that your parents were perhaps too busy to be encouraging. Were they . . . did they want you to do well in school? Did they talk about, you know, doing well in school? Or as you said, were they just really busy doing work?

IGLESIA: So I think it was more the latter. I mean, both of them worked for a good bit of the time. My father in particular had purchased a dry cleaning business. He had . . . I mean, he was trying to make ends meet. He had to deal with, you know, becoming . . . from being the owner to being the manager to being just the person that did the laundry, to losing the job altogether, and being sent out to the farms, because we were leaving the country. So they didn't pay that much attention to me. I would say that for those people that worry about how much time they spend with their children, they spent quality time with me, and I knew I could count on them if I needed them, but they were never the ones that sort of doted over me and kept patting me on the back or anything like that. They were I think your average low to middle class, you know, trying to make ends meet.

DOMUSH: [Yes].

IGLESIA: I did have an aunt [Amelia Iglesia], who passed away [only] a few weeks ago, the last of that generation, who moved from Spain [to Cuba right] before I was born, specifically to take care of me. So she had a very difficult marriage, and she left that marriage. She was one of the most conservative feminists I have ever met, but she just left her husband and said, "I'm not putting up with this anymore," and she came to Cuba from Spain and was the person that took care of me most of the time. She was sort of my soul mate. You know, we were very similar. She was not the driven, smart type, but she was the one that always [tried to do] the right things and thought very carefully about what she did, and did everything for others. So she was very influential in my life.

So now we are I guess in 1968. I was in tenth grade at the time, so I was thirteen and a half years old and I was in tenth grade. I should have been in eighth grade at the time, right? And when you become fourteen and a half years old, you're not allowed to leave the country anymore.

DOMUSH: Oh.

IGLESIA: At that point, you go into the beginning of the military service.

DOMUSH: Right.

IGLESIA: And even though you don't go in until you're seventeen, you can't leave anymore. So we were at thirteen and a half, and the papers would not come. So we were getting pretty desperate that we were never going to leave. So whenever I feel that things are not going well, then I say, "Well, what would have happened if I had never left?"

DOMUSH: Right.

IGLESIA: And then all of a sudden things begin to look better. So **<T: 15 min>** at that point, my father had been absolutely against leaving and going back to Spain. I thought he had really bad memories of the war. No, he didn't want to come back the defeated child that had left in order to build his fortune and then he had to come back. So he would refuse to leave through Spain, which he could have done, because he was still a Spanish citizen.

DOMUSH: Okay.

IGLESIA: The direct flights to the US were not coming through, and it was [becoming quite unlikely that we would leave by this route], so we had an opportunity to go to Mexico and to leave the country, but without any promise that we would ever be able to enter the United States, so that's what we did. So in 1968, December 1968, we left for Mexico. We had some cousins and an aunt and an uncle there that were able to put us up for the time being, but we didn't have a permit to work, so my parents could not work in Mexico, and there was a quota for Spanish citizens coming from Mexico to the US that needed to . . . our turn needed to come.

DOMUSH: Right. Now is it just your parents and you and your sister at this point?

IGLESIA: [Yes.] My aunt had [stayed] behind, and then she came directly to the US.

DOMUSH: Oh, okay.

IGLESIA: So we were there for six months, you know, taking repeated trips from the middle of nowhere to Mexico City, [Mexico], on a regular basis to fill out paperwork. So that was another marking experience for me. That was a time when I decided that I was going to get a job that didn't require manual labor, okay? [laughter]

And that is because we didn't have a permit to work, so none of us could work, but there was a black market for young kids whose parents had taken them out of school and put them to work in order to help support the family. So these people worked under the radar, and very respectable businesses would hire them in order to load and unload trucks [. . .] and in order to stock the shelves in the supermarket, and all that. So that's what I did for six months. I was the sole source of support at the age of fourteen and a half for my family. And I got to mix with a class of society that, you know, I'd never met before. These were the people that were truly [trying to make] ends meet, the people that [often] had truly dysfunctional families with mothers that were . . . well, and fathers that were alcoholics. I mean, it was really, to listen to their stories, made my life seem so much better than theirs.

But it was an exhausting life. I mean, this would start at 6:00 in the morning, and there were these 50 kilo bags of rice that we need to [unload] . . . okay? And at the end of the day, all I could do was have a meal when I got back and just fall asleep in bed, and wake up . . .

DOMUSH: Do it all again.

IGLESIA: . . . the next morning, do it all again. So from that point on, I said, you know, "I'm not just going to be smart, you know, and do the least I have to do in order to succeed. I'm actually going to make sure that I never have to do this again, right?" [laughter]

So every child should go through that at some point in their lives. So on the one hand, you know, I was sure by then . . . I mean, I was arrogant enough I think at the time to say, "I'm smarter than all of these kids around me are," but I was also insecure enough that I thought one day I may actually end up doing what they do, and I'd better make all decisions the right way from now on, [or at least try].

DOMUSH: Right.

IGLESIA: So I began to plan my life in a way that was probably excessive, okay, but forced by the circumstances.

Okay. So now it's May 1969, and we come to the US. They finally let us leave, and we arrive . . .

DOMUSH: So you finally . . . finally one of those trips to Mexico City, you were able to get the proper paperwork?

IGLESIA: Right. [Yes]. So one of us had to go back and get all the luggage because, you know, we didn't want to go back there.

DOMUSH: Right.

IGLESIA: So all of a sudden . . . but it was the uncertainty of not knowing what was going to happen next. That's something else that marked the rest of my life to some extent, is that, you know, throughout, I had tried to put myself in situations where I can control, not anybody else, but my own surroundings. In other words, I want to know what's going to happen next, okay? And when we come to my Exxon [Corporation] life, that's one of the reasons why I left Exxon for academia, I think, because there, I could not sort of build my electrified fence in some way and say, within this fence, I have some measure of control.

In thinking back, you sort of look at how these experiences have colored the decisions that you have made later on in life, even though at the time it was not apparent to you that that was really what was driving you. **<T: 20 min>** But clearly, the not knowing what's going to happen next, okay, was something that I also never wanted to repeat later on in life. Of course, that's a foolish attempt at controlling what you can't control, but nonetheless, it's something that you try to do subconsciously throughout.

So we get to Miami, [Florida], where all the Cuban immigrants generally end up, with literally the luggage that we had, whatever pesos we had been able to convert into dollars from the money that I had earned throughout, which was not very much, and we needed to figure out where to live. My parents had to get a job. Everything started from scratch all over again.

DOMUSH: [Yes].

IGLESIA: Now—so here's my dad. This is the second time he does this, right? I mean, so he's now become a nomad of sorts, and now he has to start all over again.

Well, I mean, the short story is that we were put up by family for a few weeks. We found an apartment.

DOMUSH: So there was some family in Miami?

IGLESIA: [Yes]. There were [second] cousins [Elena and Ricardo Mendez], actually.

DOMUSH: Okay.

IGLESIA: Nothing any closer than that. They sort of spread us around [with] some of the other family members, and we were able to find an apartment. And then we began school. My dad began to work two jobs, one from six o'clock to three o'clock, and the other from three o'clock to eleven o'clock, okay? Then he would get home, after the two jobs, but I didn't have to work at the beginning. Actually, I could not work, because at the time, the working age was like sixteen, and I wasn't quite sixteen, so I couldn't help. My mom had to work. My aunt had to work, you know, and we lived in this tiny little apartment.

So this now gets a little bit personal, but I didn't have a room. I was the one that slept in the living room, [on] a mattress that they would put on the floor, and that was my life for the four years that we lived in that place, so there was never any privacy. And the only place where I could get privacy was after everybody went to sleep I went to the bathroom and read there, and did my homework there, and all that. But we had a single bathroom, so I couldn't do that if somebody had to go.

DOMUSH: Right.

IGLESIA: But to this day, my wife laughs at me because I still find that a bathroom is the place that you go to sort of escape everything.

DOMUSH: [Yes]. [laughter]

IGLESIA: All right. So here's the fortunate thing that happened, and that is that because I came in and I was fifteen-and-a-half years old, and I was supposed to go into eleventh grade but the US system refused to put me in eleventh grade. They said, "We have to put you back in the grade where you belong."

DOMUSH: Right.

IGLESIA: And the grade is ninth grade, so you're going back to ninth grade. And that was good in the sense that socially, okay, that's where I belonged. I mean, that was my age group. All of a sudden I had people my own age to interact with. And I think that was a bit of a relief. Also, I didn't speak any English at all.

DOMUSH: Okay. I was going to ask about that.

IGLESIA: So, I mean, I could say, "Hello, how are you?" and "John is a boy and Mary is a girl," and that was about the extent of it. [. . .] And the bad thing about the system at the time was that they had this program called English as a Second Language. And that's a program where they would teach many of the subjects, except for English, in Spanish, right? And that to some extent was a bad experiment for me, because I was able to pick up sort of the elements of English. I was not able to speak English very well until the next trauma in my life. But at least I was able to understand it, and I learned it very quickly, but I was still held back by a lot of the sciences and math and everything else were now being taught to me in Spanish rather than English.

DOMUSH: Right.

IGLESIA: So I was not immersed in an environment where I was forced to speak English for everything that I did, okay? We lived in a community that was ninety percent Hispanic.

DOMUSH: Right.

IGLESIA: I went to a school that was ninety-five percent Hispanic.

DOMUSH: Wow.

IGLESIA: Where fifty percent of the kids were still in English as a Second Language. So it was almost like we had never left, insofar as being able to think and talk in a new language. So I—

DOMUSH: And were the majority of the Spanish speakers in this area from Cuba? Was that where—

IGLESIA: At that time, yes.

DOMUSH: —the majority of people were from?

IGLESIA: [Yes]. I mean, at that time, that was almost exclusively so.

DOMUSH: Okay.

IGLESIA: Now it's no longer the case. Now it's no longer ninety percent Hispanic, either. And to me, it was on the one hand very easy to get away with not doing very much work at the time, because I had [already] taken all of the math and the sciences [being taught]. I was already in [tenth] grade by the time I left Cuba, right? So a lot of these things were being repeated to me. They were repeated to me in the **<T: 25 min>** same language in which I had learned it, right? And the only thing that I had to do is learn how to speak English, something that was very difficult to do because of the pace at which most of the other students were being taught. I mean, it was English for two hours a day, but it was English at a pace that, you know, I could have done in six weeks as opposed to a year.

So at that point, I have to say that at that point, I didn't remember that I really wanted to work really hard and never load and unload trucks anymore, and that was probably one of the points in my life which I could have gone to the dark side and ended up in trouble, just like many of the people I used to hang around with at the time. So I was pretty bored. I didn't have [all that] much to do.

So I was . . . the way that the system was set up, junior high was up to ninth grade, and then after that you went to high school. So I go to high school the following year. It is probably the oldest high school in Miami. It is ninety-five percent Hispanic. They don't have English as a Second Language anymore. The classes are now all in English. The subjects are still subjects that I had already [mostly] covered before. So I'm able to sort of do the math and do the sciences, and the only thing that I had trouble with was English, for the most part. But actually, I had trouble with Spanish, too. Because the Spanish was so rudimentary, the way that they taught it, that that was the class where I never even tried. So if you look at my high school transcript, I have As in everything except Spanish. I have Cs for the most part. [laughter] Okay? So if you want to get me to do well, you have to catch my attention.

DOMUSH: [Yes].

IGLESIA: That was the other issue. So the first year was uneventful. And then that summer, the math teacher said, "You should really take Algebra 3," or whatever, "during the summer, because, I mean, you really should move faster along. You should not be taking all this. Take Algebra 3 during the summer." And that was probably the end of my tenth year. That summer was the moment where the school—[or at least this teacher]—realized that I was not [quite at] the same [level] as the other students.

DOMUSH: Right.

IGLESIA: Okay? That the classes that I had taken were the result of my personal situation and not of my talents, okay?

And the teacher that taught Algebra 3 that summer, Mr. Tyler, was the one that discovered me, so to speak, okay, and he said, "You really should be doing something outside of the high school. They have this program at the University of Miami where you can take calculus. You know, you're ready to take calculus right now." [. . .] So I continued to take the levels of math in school, but I went to the university on weekends, on Saturdays, for about four hours, and we had these intensive math classes.

DOMUSH: Oh, wow.

IGLESIA: So one semester I took calculus [I], and then the next one I took the next one. And then I went to Florida International University the following year and took linear algebra, and algebraic topology. I mean, the kinds of things that people don't do in high school.

DOMUSH: No, not in high school.

IGLESIA: Okay? And that was a lot of fun. My English continued to be fairly poor from the speaking side, and I have never, as you can tell, gotten rid of my Spanish accent. I mean, it is impossible to do. But at the beginning, it was truly terrible, because I spoke English with the average street Hispanic-English of Miami, okay? And yet I could write in English very, very well. And the contrast between the two was apparent to my English teacher, [Sheila Fawcett], who also took an interest in me. And she was adamant that I should go into English literature instead of . . .

Okay. There is an aftermath to that story, and I will come in a moment. So my English teacher took an interest because she saw such a [large] difference between what I could write and what I could say, right? And my math teacher thought that I had been sort of hurt by being neglected along the way, when somebody should have been paying attention to me. So at the same time, he also put me in the advanced placement chemistry. I had not taken the regular chemistry.

DOMUSH: Oh, wow.

IGLESIA: Okay, but I had taken chemistry in Cuba, right?

DOMUSH: Okay.

IGLESIA: And the system is so advanced there that I could have finished high school and passed the high school test with what I had been taught off of tenth grade in Cuba. And the same in physics. So all of a sudden, I was taking AP [advanced placement] chemistry, AP physics, I had algebraic topology in math, okay?

DOMUSH: And **<T: 30 min>** understanding the English, understanding the teachers, as far as the English?

IGLESIA: No, I had—

DOMUSH: You were okay with all of that at this point?

IGLESIA: I had no problem understanding. And, I mean, the language of chemistry and mathematics doesn't require that much of a command of the English—of any language—because it is a symbolic language for the most part. So that was very helpful, the fact that you didn't need to know all the nuances of the English language in order to do it. English was a little bit more challenging.

DOMUSH: [Yes].

IGLESIA: But the other two were not. Okay. So now I'm in my senior year, and the question is, so where should I go to school? And everybody around me was either going to drop out of high school or they were going to go to University of Florida or University of Miami. And, of course, my parents fully expected that I would do the same.

My math teacher from that momentous summer had always wanted to go to Princeton [University], but he could never make it. So he began to say that there were a bunch of schools up in the Northeast. He never told me about the weather in the Northeast, by the way. He just said, "The schools are really good up there." He didn't tell me that's because people have to stay indoors because they can't go outside most of the year, as you probably found out this year.

So he began to plant the idea in my mind that this is what I should do, and I began to talk to my parents about it. And at the beginning, I didn't think that I would have a chance, okay? But my parents began to get concerned that I would leave them and move very far away.

And all of a sudden—I mean, now I realize it. I didn't realize it then. My mother had the fear of abandonment because she had been abandoned as a child along the way. My father said to himself, in retrospect, "This is the same thing that I did. I left Spain because I wanted a better life, and I never came back."

DOMUSH: Right.

IGLESIA: "So you, my son, are going to leave, and you are never going to come back." And that's exactly what happened. I mean, I never came back to Miami. But both of them kept insisting that there was nothing wrong with the University of Miami or the University of Florida. Why did I have to go far away in order to do this? And that was the point where my dad bribed me to stay. He was—it was funny. But it backfired on him. So I had—I should have kept it. I had a 1967 [Ford] Mustang. This is the classic Mustang that were actually not very good cars. I mean, they were dangerous, for the most part. I never crashed it, but I [skidded it] around [full circle] a few times in the rain. And the car was beginning to give me trouble, but I said, "Dad, but I may not be here in six months." He says, "No, let's go buy a new car, okay?" So we bought this Dodge Challenger that was what every boy around wanted to have, in order to have the girl of their dreams. I'll get to the girl of my dreams in a moment, okay, because it came before the car.

DOMUSH: Okay.

IGLESIA: And he bought me this car, and this was really a good car, and my social skills went up immediately as a result of what I was driving around. The funny thing is that he ended up having to drive that car for four years afterwards, and it was the funniest thing, because here's a fifty-five-year-old with this teenager car going to work every morning. [laughter] And so anyway, so he paid for his [gamble], but he couldn't bribe me to stay there.

So I failed to tell you about the girl of my dreams along the way, okay?

DOMUSH: [Yes].

IGLESIA: So I met my wife in high school. We have been married for thirty-six years now. And I met her in, of all places, chemistry class, okay? A class that she was not doing particularly well in, and I, you know, being a teacher at heart, and her being [so] good-looking . . . [laughter]

Okay? Sort of drove me to become her tutor, and to help her with the chemistry questions that I was apparently better at than she was, okay? And then we have been together ever since. She will come back in a moment when I get to graduate school, again.

DOMUSH: Okay.

IGLESIA: But I went away, in spite of the fact that I had a Dodge Challenger, a beautiful young lady, and a family that wanted me to stay there, right? So on September 6 of 1973 . . .

DOMUSH: Sorry. Before we get to that, at this point, you know, you've been in Miami for about four years. Was your dad still having to work two jobs? Were things a little bit . . .

IGLESIA: It was pretty much the same way. I mean, not much had changed up to that point, okay? I mean, eventually he ended up buying a dry cleaners of his own, but that probably **<T: 35 min>** came about a year after I left.

DOMUSH: Okay.

IGLESIA: We were fortunate enough to be at a level of income where at that time the Ivy Leagues were providing a significant amount of support, as they do now. So I was actually quite inexpensive. It didn't cost him any more for me to go to Princeton as it would have had to go to the University of Florida, so that worked out quite well.

I have to say—I didn't tell you what my SAT scores were.

DOMUSH: No, you did not.

IGLESIA: Okay. I didn't. So my [math] SAT scores were quite good. They were like 780, 790, out of 800, in math. But the verbal was like 380 or something like that, right? So the counselors in the school, with the exception of Mr. Tyler, said, "There's no way any Ivy League is going to touch you. I mean, here you are, 380. I mean, these guys have 700s, 750." And actually, both Princeton and Yale [University] accepted me. The funny thing is that Yale did not have a chemical engineering department, [but I did not even notice].

So I applied for what I thought was the chemical engineering department, [which] was actually an engineering general department, and I was accepted at those two. I was accepted at Tulane [University]. I have no idea why I applied there, to be honest with you. And then I was accepted at all the local universities there, and so on, but why Princeton accepted me is not clear to this day. This was before the time when you wrote heartfelt statements written by some counselor that taught you how to write it. I mean, you just filled out an application and you told them something about where you were born and all that, and that was it. So somebody looked at that application and said, you know, "There must be a reason for the four-hundred point difference between the verbal and the math here, and this is something that I'm certain will change with time."

DOMUSH: [Yes].

IGLESIA: And they actually gave me a chance, okay? And I struggle today, as I go through applicants here, you know, as to how it is that you can tell which one of those applicants actually has done what he has done in spite of circumstances, and who has done it because everything was served [and given] to them.

DOMUSH: Right.

IGLESIA: [. . .] But somebody took a chance on me at that point, and I guess it worked very, very well. So again, I repeat myself, but it was September 6 or 7 of 1973 [when] I arrived at Princeton Junction, [New Jersey], okay? The temperature is around 42 degrees or so. I'm still wearing my Florida clothes, and I have a thick jacket that my dad had bought for me in the luggage. So we were sitting at the train station opening our luggage, a friend of mine, [Jorge Vizcaino], who also went there, so that we could dress properly in order to walk the mile that we needed to walk to the campus and find our way around.

DOMUSH: So you knew someone who was also going as well?

IGLESIA: Yes. There was somebody else from my high school was also going.

DOMUSH: Okay. That's nice, though, that you're not necessarily alone, going that far away.

IGLESIA: Right. But it was a culture shock. I mean, throughout life, I have had a problem that I often end up places where I don't think I belong. Okay? And I certainly got there, and I was sure that I didn't belong there. The weather was not right. The surroundings looked quite a bit older. I mean, you had all these Gothic buildings around you. Everybody spoke with a different accent for sure.

DOMUSH: [Yes].

IGLESIA: Everybody made fun of mine.

DOMUSH: Oh.

IGLESIA: Okay? I mean, this was not a happy moment. There were moments in the first six weeks or so where I felt like just repacking the bags and going back again. And it was probably just the activation energy of having to pack everything and get in a train and then get into a flight and go back. But it was clear that they had made a mistake. I mean, this was not something that, you know, was going to work for anybody involved.

So some people when they find themselves in that situation, they lose their confidence, right?

DOMUSH: [Yes].

IGLESIA: And some people that have some sort of internal arrogance say, "No, if I work harder than anybody else, I'm sure that I can make it." My English teacher wanted me to be in English literature. So we have to take a literature course during the first year, as part of the humanities. And I took Shakespeare in Old English.

DOMUSH: Sounds hard.

IGLESIA: The professor was a British actor that had become a professor there, that would actually act out *Hamlet* in his most polished British accent, and I couldn't understand anything that he was saying. That was one **<T: 40 min>** of my three B's in Princeton, okay, but I made it through that course.

DOMUSH: B is not so bad for . . .

IGLESIA: [Yes]. I know. But I still have nightmares to this day that I forgot to go to class, and then at the end of the semester, I didn't know the material. So, I mean, these are the kinds of things that you acquire, where I still wake up in the middle of the night, and I'm still going to school, and it looks like Princeton, and I forgot to go to class all semester and now I have to take an exam at the end. That's the insecurity part that goes along with the arrogance, that somehow you're arrogant enough to say I could do it. You're insecure enough that you say, "I'm going to have to work harder than anybody else." Okay?

DOMUSH: [Yes].

IGLESIA: And then at the end, if you balance those two properly, I think you make it, but I was lonely. I mean, I missed my family. I missed my girlfriend. I [even] missed my car. It was cold all the time. And all these people around me were not really all that nice. That was probably the first time when I truly felt that people thought I was alien, alien in the sense of a planetary alien, not just an immigrant at that point. That I didn't belong there.

DOMUSH: What were the other classes you were taking? Because you had said that you applied with the intent of majoring in chemical engineering, so presumably you had to take something along the lines of that.

IGLESIA: [Yes]. So to be honest with you, I don't know why I chose chemical engineering.

DOMUSH: Okay.

IGLESIA: Other than chemistry and math were the things that clearly I could do without the full command of English, I had a head start on everybody, and I was told that people made a lot of money if they became chemical engineers. All these things came together as, well, it looks like I could do it, and if I do it, I don't have to load and unload trucks the rest of my life, and all that. I would not say that there was anything more profound than that. That's just . . . and I haven't tried to psychoanalyze, you know, how it happened. I tell everybody that they shouldn't try to plan their lives, because at the end, you know, you don't end up where you wanted to go, but you may end up in a better place. So I can't take any credit for having done that.

At that time, the first-year engineering students would take the usual physics and chemistry and all that. So you had to take a humanities course, and you had to have three technical courses, chemistry, physics, and math.

DOMUSH: And you had already taken quite a bit of college-level . . .

IGLESIA: Math. [Yes]. So in the math part, I mean, when I took differential equations, I already knew how to do it, so that was the one that I didn't have to work very hard.

DOMUSH: Right.

IGLESIA: But even the other courses, I realized that a lot of the people around me were not as smart as they looked. They were pretty smart. They were smart enough to know they shouldn't work as hard as I did, okay, but if I worked harder than they did, I could actually do better than they would. The only thing that got in the way is that socially I never really connected at Princeton. Part of it is that at the time, [Princeton] was truly a Northeast prep school environment. I mean, this was 1973 to 1977. Women had been accepted for the first time only five, six years earlier, so the [male-to-female] ratio was something like five-to-one, and it was a socially awkward environment for me. I mean, again, I didn't belong. And in a sense, that stunted again my social skills, but it gave me more time to sort of work harder than anybody else, okay, and therefore to rebuild my confidence and to finally convince myself a year later that intellectually, I belonged, but [that] socially, I was never going to belong there.

DOMUSH: [. . .] I was going to ask if perhaps there was any sort of adjustment, because in Miami, you were in such an urban environment, and Princeton is not, but perhaps that's not the right question, because like you said, everything about the environment was so different. Maybe the urban to not urban thing was kind of an afterthought to everything else.

IGLESIA: I think that at some level, that was too far removed from my main issues with the environment there, that had to do with the way that people socialized, and the level to which socializing to some extent . . . So, I mean, in my Hispanic culture, when people get together, they eat and they have a drink, okay? And they eat and they have another drink. But everything is sort of around, you know, you sit around and you talk and all that. There was seemingly an intent **<T: 45 min>** to imbibe as much alcohol as possible and to get happy right before you fell asleep. [laughter]

So that was not the way that we socialized. I mean, generally, there is something that is not contrived about the way we socialize. I found they need to do it on Saturday night, right, and Thursday nights, and they need to do it with a large amount of alcohol or other things that were around at the time, to be somewhat contrived and to this day, if you want to make me uncomfortable, put me in a cocktail party. That's sort of the sign of the introvert somehow, okay, and it doesn't matter what you give me to drink, I'm still going to be uncomfortable.

It didn't help matters that my dorms were at the edge of campus, that at that time they put all the freshmen together at the edge of campus to get them to bond, but then we never mixed with anybody else. We were engineers, and as a result of it, we were automatically [members of] a different class. I mean, I was different in many ways than the others.

But I have to say that it was like working in the salt mines for four years in order to make sure that I could control the rest of my life from that point forward, and that's what kept me going throughout my years there. I don't remember them as not being fun. [. . .] So remember when I was a little kid and I would concentrate for four hours in reading a book? I could do that there, and to some extent it was the equivalent of running the marathon. You know, the endorphins sort of kicked in at the end because you're so exhausted mentally rather than physically.

So I don't remember it being painful. I remember it being awkward socially in that kind of an environment. We had a fairly small chemical engineering class. There were maybe twenty-five students in the class. Princeton is a unique place in that the undergraduates are the ones that rule the world. I mean, the graduate students are there in order to sort of serve the undergraduates, so I got to know my professors quite well. I got them to pat me on the back on a regular basis, and to pay attention to me, and I got a couple of them who decided that, again, I was special, okay? I mean, I hate to brag, but I'm going to brag for a moment.

DOMUSH: This is a good place to do it.

IGLESIA: Okay. But, I mean, I walked in on September 7, 1973, [or so], with the idea that I would never make it there. By the time I got to be a sophomore there, I was sure that I belonged there, and that I could compete with anybody. By the time I was a junior I was sure that I was better than most of the people around me. And by the time I graduated, I had the highest GPA of anybody else in the School of Engineering. Okay?

DOMUSH: Wow.

IGLESIA: So how this happened along the way, I cannot tell you, other than I remember distinctly from year-to-year becoming more confident, more arrogant, and saying not only am I good enough for this place, okay, but maybe this place is not good enough for me. Now I can outgrow this place and I can go elsewhere.

And there were two people in particular that were very influential in sort of building my confidence, not to call it something else, okay, and that was Dick [Richard K.] Toner, who was the old-timer [who no longer] did research, did mostly undergraduate teaching in thermodynamics, but taught the thermodynamics course. And I remember that after one of the exams in thermodynamics he called me over and he said, "You know, it's not the grade that you got on the exam. It's the number of points between you and the next person that I'm puzzled about. So what do you do, so that I can teach everybody else how to do it in the same way?" And I said, "Tell them to go lock themselves in the library like I do, as often as I do, and maybe they'll get it."

And the other one was Leon Lapidus, who was another old-timer, but one of the brightest applied mathematicians in chemical engineering, who thought that most of chemical engineering didn't have enough mathematics, but thought that I knew enough mathematics to talk to him about it, okay, so he took an interest. He happened to pass away two weeks before we graduated, and that was crushing for the class—and for me in particular. But he was the one . . . so we all do a senior thesis, and **<T: 50 min>** he was the one that suggested that I work on the mathematics of transport of hydrogen and deuterium through stainless steel.

Now you may wonder why. At the time, thermonuclear fusion, controlled fusion, was in one of its [many recurring] heydays, and the Forrestal Lab at Princeton actually had research on the Tokamak [fusion reactors], which was one of the ways by which they were trying to do it, and they were worried about tritium diffusing through stainless steel, and creating radioactivity problems. So we couldn't use tritium, so I had to model hydrogen and deuterium, and then extrapolate for kinetic isotope effects. And most of it was done with mathematics, and the experiments never worked, but the mathematics was beautiful. I got to do perturbation theory, you know, in order to solve a problem before we had computers to do it, mathematically, and he was extremely impressed with that.

So he and Dick Toner said, "You have to go to graduate school." So maybe I will roll back the movie a little bit. So at this point, I'm getting to my senior year, and I could go and get a very nice job in industry. And there was a particular individual by the name of Jack [John] Weikart, who was the Exxon recruiter. And this was the time when companies would actually have a recruiter who was a very talented engineer that would spend their time going to the top fifteen, twenty universities, getting to know all the professors, asking them who were their best undergraduates and their best graduate students, and then literally following you through your entire career. So Jack Weikart would come to visit twice a year, and from the time that I was a sophomore, he would take me out to dinner every time, okay? So imagine that now. I mean, they show up in our senior students, they say, "You want a job? Fine. If not, you know, we'll get somebody else," right?

DOMUSH: [Yes]. [Yes]. [laughter]

IGLESIA: So he had followed me for now six dinners, okay, by the time I get to my senior year, and [. . .] he made me an offer to go work for Exxon for some unreasonably high salary that was very difficult to turn down. And I had two faculty members that were saying, "You really—you want to be like us. You want to go to graduate school, and you want to be a professor." And I had to say that here is where the third characteristic I think of success comes in, and that is resisting the impulse to take the most attractive offer at any one point. And I remember a conversation with them about, well, so what's the benefit of going to academia? And they said, "Well, you'll get a slightly larger salary, but you'll never recover all the money that you lost along the way." In other words, if you take the time value of money, right, you're better off just going to industry, but what convinced me was they said, "But if you go into academia, you will have more to say about what you do and how you do it." And all of a sudden, they sort of pressed the control freak button that I have somewhere in me that said, "Ah, if I can have more control over what I do and how I do it, that's probably worth delaying gratification, to some extent." [. . .]

By the way, I was doing all this with my bride-to-be in Miami, who went to school there, and I was in Princeton, and things were working out. We would see each other a few times a year, and that was it.

So there was a faculty member [William B. Russel] who joined Princeton [about the time that] I joined Princeton. He came from Stanford [University]. And he became the faculty—I forget what the name was, but each one of the eating clubs had a faculty advisor, and he became the faculty advisor for an eating club. Princeton had selective eating clubs, and they had two non-selective eating clubs. Of course, people like me ended up in the non-selective eating club. It was actually a kosher place, which had the best food on campus. But again, you know, I was an outsider, obviously, right, as were many of the other ones. But it was right behind the computer center, and that's where I would work [for spending money].

DOMUSH: Okay.

IGLESIA: So I would work part time at the computer center, so it was very convenient. But anyway, Bill Russel, who was this young assistant professor, I got to know him very well. And he had come from Stanford, and he said, "Well, you know, you have to go to Stanford," because at that time I was interested in surface chemistry from the **<T: 55 min>** isotopes of hydrogen going through stainless steel. And I thought that I wanted to do catalysis, because that's what Exxon did, and, of course, Jack Weikart had got me two summer internships at Exxon in the meantime, right?

DOMUSH: Okay.

IGLESIA: So I worked at the Baytown, [Texas] research labs of Exxon the summer after my junior year, and then at the Baton Rouge, [Louisiana] labs of Exxon, again, the following year. So I had actually gotten to do some work in catalysis already, and that's what I wanted to continue to do. So Bill said, "Well, you have to go to Stanford, because that's where the best person in the world in catalysis is. And by the way, I'm going to introduce you to him, because he's coming over to give the Wilhelm Lecture." These are the named lectures in the department. These are the most prestigious lectures, and he was coming to give the lecture, and I got to meet him at the time. Coincidentally, [and somewhat circularly], I will give the Wilhelm Lectures [at Princeton] this year.

DOMUSH: How exciting.

IGLESIA: So it looks like the cycle is now getting complete. So I come back, and, you know, thirty years later I show up to give the lecture that my advisor, the one that would become my advisor was [giving], okay, and how I ended up meeting him.

So now the other interesting thing is that I knew that I did not want to stay in the Northeast. Actually, I knew it so well that the only two schools that I applied to—actually, three schools—were Stanford, Caltech [California Institute of Technology], and Berkeley.

DOMUSH: Okay.

IGLESIA: Okay? And I was sure at this point that I would get into all three, and I did, right?

DOMUSH: [Yes].

IGLESIA: So there was no question of not going there.

DOMUSH: Was there any interest in going back to kind of the Texas, Louisiana area—

IGLESIA: No.

DOMUSH: —where you'd spent some of the summers?

IGLESIA: No. So I could tell you a little bit why, but it's perhaps not pretty. Okay? And that is that, again, I didn't feel like I belonged, either, but for a very different reason. I mean, as opposed to the prejudice based on aristocracy and prep school versus public school versus Hispanics, in Baton Rouge in particular, I found the overt racism and the—I mean, remember, this was 1976 in Louisiana. And even within the working environment, okay, I found that it was clearly not a place that I would ever want to raise children. And when I get to Exxon and I tell you why I left, you will see the same things coming up again, and that is that I wanted to make sure that my family was raised in a place that was accepting of others, and not in a place that had a history and, you know [. . .] a significant component of stratification of society and all that.

DOMUSH: [Yes].

IGLESIA: But no, it never crossed my mind to do that. The job that I would have taken at Exxon would have been in New Jersey, so it was not very far from Princeton. My parents, again, came into the picture, and saying, "Why don't you come back? I mean, "Why don't you go to the University of Florida?" Well, the University of Florida, no offense intended, but it's not at the same level as Stanford, Berkeley, and Caltech.

DOMUSH: Right.

IGLESIA: And again, they expected that I would come back because that is what is expected in my culture, is that you come back home and you live close to your family, and so on.

DOMUSH: [Yes]. And your bride-to-be at that point, she'd been in Florida for school? She'd been living closer to home, closer to family, and—

IGLESIA: [Yes]. So in a moment I'm going to have to extricate her from Florida, okay?

DOMUSH: Okay.

IGLESIA: And be held responsible for stealing a daughter from her parents. But she was a year behind me, so she was going to a Catholic university there, Barry College, and getting a nursing degree. So at the point that I graduated from Princeton, she still had one year to go, so I couldn't quite take her out of there, but a year later, I will, okay?

I don't think anything else happened, except that I got accepted at Stanford, Berkeley, and Caltech, and at the insistence of Bill Russel, you know, Stanford was the only place I would consider. At that point, I chose Stanford. [Many] years later, when I had to make a choice again, I chose Berkeley, but it was at a different point, and a different choice I had in front of me. But Stanford will come back again when I move from Exxon and **<T: 60 min>** I ended up here [in 1977]. But at that point, clearly, that was the right decision to make.

It is funny how one ends up making the decisions that one makes. If I had reversed my training and I had been an undergraduate at Stanford and a graduate student at Princeton, I would not have been nearly as well-trained as I was by being an undergraduate at Princeton and a graduate student at Stanford, because the emphasis of the two places was aligned with my presence there as an undergraduate at Princeton—

DOMUSH: Undergraduate studies.

IGLESIA: —and as a graduate student at Stanford. I mean, it was totally unplanned. No credit taken [or due].

DOMUSH: Right. And some very good advice from people who were—

IGLESIA: That's right. And advice that oftentimes you sort of integrate and it becomes your own decision, but it's colored by many people who you trust, who have given you accumulated advice somehow. So when people ask me, so how do you make decisions, I mean, I think you do it by being thoughtful. You know, I have been known to say that there are no wrong decisions. There are only decisions made wrongly. That you just have to have a process by which you do this. And it is sometimes subconscious, but it is being thoughtful and hearing what others have to say, and checking your gut feeling and see what it feels like. Beyond that, I cannot teach anybody how to make the right decisions.

So I came to Stanford. The only thing interesting about my getting to Stanford was that the famous Dodge Challenger came with me. But it died in Flagstaff, Arizona, where no one at the time seemed to know how to fix a transmission in a Dodge Challenger. [laughter]

DOMUSH: That's unfortunate.

IGLESIA: So that was a bit of an ordeal that was nontechnical in nature, but made me wonder for a moment, you know, why it is that I'm driving that car all the way here. But my dad wanted to get rid of that car so badly, that he said, "You take it, please," [so he could get an age-appropriate car].

DOMUSH: I'm going to have to ask my dad—I'm staying with my parents right now. I'll have to ask my dad when I go home—a similar—a couple of years before you were going cross-country, he made a very similar trip cross-country, and he had his car die somewhere, I think in Arizona, and they couldn't fix his car, either. [laughter]

IGLESIA: Well, they tried to fix it, but I had to sleep in the car one night, because the only guy in Flagstaff that could fix the ball bearings, which is what he told me was wrong with it, needed another day in order to [finish] machining the bearings. And so I slept in the car, right outside. And then the following day he told me it's fixed, okay? So he charged me some outrageous amount of money, and then I went on. Two hundred miles later, it begins to make the same noise again.

DOMUSH: Oh, no.

IGLESIA: It's too far to drive back, and do it. So here I was crossing the Sierra [Nevada mountain range] somewhere, okay, and I just said, "I hope it's downhill from here, because this car is not going to make it." So I made it within twenty miles of Stanford, and then one of my roommates at Princeton was also going to Stanford, so we actually roomed together the first year. And he went and picked me up, and we towed the car, and they fixed the transmission, and that took care of all the savings that I had at that point. Mind you, I had to save for a wedding a year later, okay?

DOMUSH: **[**Yes].

IGLESIA: Now, so the first year at Stanford was mostly uneventful. Except for the fact that as a later good friend told me—Henry Taube, Nobel [Laureate in Chemistry, 1983], and I served together at Catalytica [Associates] as scientific advisors, and I gave him my story about that first year—he said, “You were probably right, but you were speaking out of turn.” And what it meant was that I came in and I knew exactly what I wanted to do. I wanted to do research with Michel Boudart. I didn’t want to take all these courses that you had to take that had nothing to do with my interest. And there was one particular course that I detested, and that was a course in fluid mechanics, because everything was a slender body, and everything had a perturbation solution that needed to be used, and nothing was practical, but it was a rite of passage, and that is that if you wanted to be anointed as a good student, you had to take that course and do well. And I took the course, and everybody failed the course.

DOMUSH: Oh, no.

IGLESIA: Everybody failed the course—and I won’t name names, actually, in this case—everybody failed the course because the instructor, the professor said, “None of you have worked hard enough **<T: 65 min>** here. I won’t give anybody a grade. I’ll be nice to you. Instead of giving you an F, I won’t give you a grade. But you all have to take the second semester, and depending on how you do the second semester, I’ll give you a grade.”

And I said, “I’m not going to take the second semester. There’s no way I will do that.” So I refused to take the second semester. I wanted to start doing research right away. Michel Boudart told me I could start doing research right away. That’s what I did. But this individual never forgave me for that. So at the time of the anointment, okay, I was not anointed at that point.

DOMUSH: How was your initial feeling about Stanford and kind of coming to the Bay Area? You know, you said pretty much right away when you got to Princeton you knew that the school was good, but the environment and everything—

IGLESIA: It wasn’t me.

DOMUSH: —wasn’t for you.

IGLESIA: [Yes]. So part of it is relative, right? I mean, so I came from Princeton to Stanford. The place was a lot looser than Princeton and not nearly as uptight. The weather was a lot nicer. I already knew some people. My roommate was the same one I had at Princeton. I had a car, okay? You could drive places, right?

DOMUSH: [Yes].

IGLESIA: So to me, it was not nearly as much of a shock. If anything, it was a relief now to be able to leave that little cold prison in Princeton, and now have some open space, and that was the time when I took up running, and I would go running in the hills. In Princeton, you have to run on pavement, and it was frozen most of the time, so I took up bike riding. I began to run. I got in really good shape. You know, I was the thinnest I was ever in my life, probably. So, I mean, other than that particular incident with fluid mechanics, everything was great. I began to do work with Michel Boudart. Michel was an aristocrat. I mean, he was an individual from Belgium that was related to the royal family. He was the ultimate in aristocracy.

DOMUSH: [Yes].

IGLESIA: But one only noticed in the way that he conducted himself, not in the way that he treated you, in contrast with the prep school kids in Princeton, which actually not only did they act differently, but they treated you like you were different. So I never really had any difficulty. It was one of those things where, I think, he also had a way of training graduate students that said, "If you're good and if you do your part, I'm going to spend time with you. If you don't do it, I'm going to abandon you, okay, so my time is very important to me. I am going to pay attention to those of you who are paying attention to me." And as a result of it, you know, he would abandon some of the students, okay?

DOMUSH: [Yes].

IGLESIA: And he would actually take an interest in some of the others. And I was one of the ones that for I hope a good reason, you know, he paid attention to. And over the years, I think he began to realize that I was very serious, I was very intense, and I was pretty smart. And I would listen to what he had to teach along the way. And towards the end, I think it was to the point where he perhaps, he felt that I actually taught him something at the end, which is I think what every good graduate student does at the end of their careers, they teach their advisor something that they would not have otherwise learned.

DOMUSH: How big was his group?

IGLESIA: It was probably around eight or nine graduate students, a visitor once in a while. He would also bring people on sabbatical, for example, and we would get to interact with them. One of the people—one of the persons who undoubtedly is in the Chemical Heritage Foundation files is [Professor] Paul [H.] Emmett, a classmate of Linus Pauling in Oregon, and probably the father of modern heterogeneous catalysis. And at that time, he was in his eighties, and he would come in, and he would sit with each one of us. And, you know, he was outwardly at least one of the most modest people that I have ever met, and he would sit with every one of the students. And we didn't realize what a giant he was at the time.

And actually, you know, it's the kind of thing that happens because of where you are, and not because of any planning. But we were exposed to—I mean, so Henry Taube was next door in chemistry. Jim [James P.] Collman, I mean, all these people that we would see occasionally, who would come to seminars, that would ask questions. We would see them in action. It's not how good Michel was at teaching us, because the interactions were limited. It's how he surrounded himself with very good people at Stanford, but also very good **<T: 70 min>** graduate students in his group. And it's one of the things, Hilary, by the way, that I learned later on in life. And when we get to my life at Berkeley, I will tell you a little bit about my transition from someone who was the single point of contact for everything that happened in my group to someone that decided that the group was good enough that they should rely [more] on each other. And Michel's way of running his group was that you guys are very good. The ones that are not good, I've already let go, so you don't have to worry about it. And you're much more likely to find the answer to your question by looking around you than by coming into my office and asking me the question.

And that's a transition that took me from somewhat of a micromanager at the beginning, where I wasn't trying to micromanage the group, I was trying to do science myself because I enjoyed doing it, to the point that I realized that I was getting in the way of the group bonding together. And when I stepped away, the group sort of bonded together. But that's a bit of a tangent to which we will come later.

DOMUSH: Did the Boudart group—sometimes you hear stories about groups that play bridge or play poker together, or watch baseball, or do kind of these things outside of the lab together. Did you guys do anything like that, or have time for anything like that, or was the bonding more about just being in the lab type of bonding?

IGLESIA: I think the bonding took place in the lab. It probably spilled out to some things that we did together, that were mostly the kinds of things that one does in the kind of climate that this area brings about. So we used to go hiking and backpacking together, for example. We would take drives out and do day hikes together, a subset of the group. But it wasn't anything very organized, and it wasn't something that—I mean, we would not get together in anybody's houses on a regular basis to do anything sort of passive. It was really in the lab that the bonding took place.

It also to some extent did not help that there was a detachment between the advisor and the group. There was a social detachment. It was clear that the advisor was at a different level socially and intellectually, and all that that sort of kept the social activities from including the advisor at any point along the way. It's something that I think actually to some extent is positive, okay? I wonder sometimes whether that gap sometimes gets so small that you can't tell who's who in the relationship, and who is the mentor, and who is not. But in his case, it was quite extreme. So, I mean, there was no—he was not in any way the single point of contact for any social interaction.

DOMUSH: Right.

IGLESIA: He was aloof socially. He was in his own circles. We were in our own circles, for the most part. The bonding was technical, it was scientific, and it was the time, the sweat, and toil of the lab work that actually made it happen, especially because some of us would work in the evenings.

DOMUSH: Okay.

IGLESIA: Even though it was probably not the safe thing to do. It was still 1978, 1979. Some people worked in the evenings because their instruments require perfect quiet and no vibrations or anything like that. Some of us, me in particular, worked in the evenings because by the end of my first year, I got married. My wife was a new nursing graduate. The only shift that she could work was the night shift, right? So she would work at Stanford Hospital, and I would work in the lab from 11:00 at night to 7:00 in the morning, and then we would spend some time together. So that was also bonding [in the lab], because we were all sitting there at the wee hours of the morning doing experiments, and we needed to make sure that we were there for each other, because there were usually [only] two or three of us. If anything happened, we knew we would have to deal with that.

DOMUSH: How did your research go? Some people that I speak with, their graduate research they say went kind of smooth as can be. Some people hit a lot of kind of speed bumps along the way, with instruments not working or reactions not working, or things like that. How did yours go?

IGLESIA: I would describe it as boringly predictable, for the most part.

DOMUSH: Okay.

IGLESIA: I have to say, it wasn't such an exciting project. So, I mean, I tried to keep the excitement in my life at some reasonable level, right? And I have to say that my PhD dissertation was solid, it was well-done, it was meaningful, but it was not exciting in the way of being unpredictable **<T: 75 min>** and sort of pushing the state of the art to the point that I was going to do things that nobody had done before, go to where no man had gone before. The only thing exciting about my PhD dissertation is that unbeknownst to me, the project was destined to show that somebody else's work was not relevant. Okay? So there were a lot of people at the time that were working on model systems, and Michel Boudart thought that those model systems were not appropriate descriptions of what happened on a real catalytic surface at relevant conditions.

And the project that he gave me was to look at a real catalytic surface under steady-state, relevant conditions, and to show that the kinds of very interesting behaviors that other people were seeing on model systems under transient conditions were unique to the model systems and to the transient nature of the conditions. In other words, it's not that they were wrong. It is that they had no applicability for the systems that were relevant [and which] we cared about.

DOMUSH: Right. Not as good a model as they thought they were.

IGLESIA: Right. [Yes]. So there were beautiful sort of explosions on the surface that were actually due to the transient nature of the experiment, and of the fact that you had a microscopically flat surface where you could form these islands and so on. Well, needless to say, the people whose work I was challenging did not take it too kindly.

DOMUSH: May I ask whose work you were challenging?

IGLESIA: So it was the work of another faculty member, [Robert J. Madix], in the chemical engineering department who happened to be a member of my PhD dissertation. So that was the only excitement when it came to the oral defense, is that, you know, this was memorable. I think that was a time when Michel Boudart described me as a "classical Cuban street fighter," whatever that meant. [laughter] To him, it meant that I had been able to keep the barbarians at the door somehow. But I did put a hole through the [projection] screen by pointing, and so we had one of those extendable pointers. And somebody had taken the little protective cap at the end and not told me about it. And I kept pointing to one place to make a point to this individual, and I put a hole through the screen, and ten years later that hole was still there. [laughter]

But that's about the only exciting thing that happened. That, and the fact that the period of my life from 1978 to 1981, was probably the most affluent period of my life, until recently, because we had a stipend. My wife was working full time. We had no children. We had a subsidized apartment, one car, and two bicycles. And we had more expendable income than we had up until about three or four years ago, when the last one of our children went to college and left college, and then all of a sudden we acquired [it back] . . . . But I remember that we used to go out on weekends and stay up in Mendocino, and, you know Motel 6 was our level at that point, but it was still quite affluent by the standards of the graduate students. So I remember it fondly, and I remember it as a time where socially I grew quite a bit. Part of it was the environment at Stanford. Part of is that my wife is an extrovert, and, you know, it's very difficult not to grow extroverted when she's around, right? So all these things I think, you know, made me a very good scientist, but also a more rounded person than I had been, after a childhood and adolescence, and then my years at Princeton left me a little bit malnourished when it comes to the social skills department. Oh, my wife may tell you I still need some more nourishment in that department. [laughter]

DOMUSH: How did publishing go? You mentioned that even when you were in high school, when you were—perhaps struggling isn't the right word, but—but having difficulties with some things related to English, that writing was going very well. Your English literature teacher said that this is something you should pursue. And of course, writing, you know, science papers, writing chemistry, chemical engineering journal articles, is a completely different style of writing. But how did that go, when you started to do that?

IGLESIA: So I think that . . . what my English teacher I think noticed in high school was not that the language was particularly florid or that I had a large vocabulary. I actually had a fairly small vocabulary, **<T: 80 min>** but I was very logical in the way that I wrote things. So I would use short sentences and short words, but I would order them in the right way, and I would naturally stop the paragraph at the right point, and I would magically have topic sentences that nobody had taught me, but I knew should be there. So there was something about the structure of the writing that was what impressed her, and not the sort of content in the sense of being particularly poetic or anything like that.

DOMUSH: But still, that's a lot more than can be said for a lot of native English speakers and writers.

IGLESIA: It is. But I think what is missing is not the vocabulary. What is missing is the logic of how do you structure it. To some extent, it helps that English is a second language in that sense, because I had to study the rules, but I also had recognized that logic transcends the actual language, so when people come in and tell me, "Well, English is not my native language," I say, "Well, write it in your native language, and make sure it makes sense, and then we'll translate it together," because in ninety percent of the cases, it's a question of logic. It's not a question of vocabulary or grammar or anything like that.

It also probably is an indication of how I write and how I think, at least in English, that as I was growing up, my favorite English author was Hemingway, because it is the one that had the short sentences and the short words and a fairly limited vocabulary, but my favorite author in Spanish is Jorge Luis Borges, who is one of the most complicated writers [. . .]. But that's my native language. I can handle the complexity, right?

DOMUSH: [Yes].

IGLESIA: And then I was never able to get through James Joyce. I mean, I still have not read any of his books, because I can't go through those sentences that never end. I tried to teach myself a little bit of German, and I could never, because it takes too long to figure out what the sentence is supposed to tell you, because the verb is [often] at the very end. So whenever I'm limited by the vocabulary, I always go back to write it short, and make sure that it is logical, and that I think is what made it easy for me to technically publish, because the part that is common was there and the vocabulary may not have been there, but it was not necessary, and in most cases, is very local to the particular area. So I would have to say that because I analyzed the language in order to try to fit during my Princeton years, I ended up being more careful about how I write English than most of the native speakers do.

They do it almost subconsciously, where I do it with a certain level of mechanical sort of rigor that says this is what a paragraph should look like, this is where sentences should end, and this is where a period goes. It also helps that that English teacher was a bit of a freak when it comes to run-ons and fragments, so right now I can pick up a missing period or a comma. [. . .] Train your children to pick up where the commas and the periods go, and then after that I think it becomes really mechanical.

DOMUSH: It's very interesting. Whenever I interview people where English is not their first language, this is a very common theme about some of these grammar issues. And my husband, English is not his first language, and he is a huge stickler for where commas go and where periods go.

IGLESIA: Right.

DOMUSH: And all these things that to me, you know, it's a little bit more fluid. There's a little bit more leeway. [laughter]

IGLESIA: That's because you grew up in the permissive society here that tells the children that they can make up the rules as they go along. [laughter] So whenever the students come into the group, we have our manuscript guidelines, which are pretty pedantic, occasionally funny, but I also make them read Strunk and White.[1] Now Strunk and White is the most pedantic thing that you could ever read, but it's right ninety percent, ninety-five percent of the time, and it is thin.

And actually, once you read the absurdity of some of the things that people would write if they don't follow it, you begin to believe that there is room for being pedantic to some extent in order to avoid foolishness along the way. But no, it also helps that the Spanish language is more structured than the English language. It is also to some extent more orderly. You know, so things mean what they mean. It is a phonetic language. It is a language where the only difficult thing is memorizing conjugations, but the rest of it is rules rather than exceptions. So you get used to doing things in a more mechanical way.

DOMUSH: Right.

IGLESIA: There is less room for improvising, **<T: 85 min>** probably—although people try—than in English. But when it comes to technical writing, I don't want creative writing. I want writing that will never confuse anybody, and putting a comma in the wrong place or having a sentence that goes longer than two verbs gets to be quite disruptive and sometimes can give the wrong message, so being precise, I think, is important, even if it's pedantic to be precise. Okay. Do you need a break?

DOMUSH: I think it'd be a great idea to take a break.

[END OF AUDIO, FILE 1.1]

DOMUSH: Okay. Back on after a short break. [. . .] We'd been talking about publishing and writing at Stanford, and I think we were pretty much kind of closing up your time at Stanford.

IGLESIA: Right.

DOMUSH: So . . .

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[1] William Strunk, *The Elements of Style* (New York: Penguin, 2007).

IGLESIA: [Yes]. So now my graduate years were happy years. They were fun years. They were affluent years, as I pointed out. They were also much too short for what they should have been, okay? I mean, I finished in a little bit less than four years, mostly because I needed to finish in less than four years. So my wife was very pregnant at the time, and she very much wanted to build a nest before she gave birth. So she said, "We need to be somewhere. Wherever it is that we're going, we need to be there to deliver this baby, because there's got to be a nest that we have," so I sort of rushed through the end, and I probably left six months earlier than I should have left. When my students tell me, "How long is it going to take me to get a PhD?" I say, "You know, if you're having fun, try to make it as long as possible, because life gets a little bit more stressful afterwards, and nobody will ever pay you again to learn as much as you're learning right now." I had family reasons of my own doing, but we had to finish and we did.

At that time, there were two choices, two career choices for me, one of which was to go into academia, and that's what my advisor said I should do. And the other one was to—I forgot to tell you. Jack Weikart had taken me every year in graduate school out to dinner—

DOMUSH: Oh, he continued in graduate school?

IGLESIA: —twice a year, okay, and would take my wife for the last six of those, because she was already there. And Jack Weikart, by the way, is still alive. He's ninety-four years old. He lives in Delaware in a little cottage, and about a year ago, he wrote me a card because I got an award from the AIChE [American Institute of Chemical Engineers], and he said, "Congratulations. I hope you remember me." I said, "How can I not remember you? I had more dinners with you than I did with my wife." And he says, "And I hope you will allow me to take some credit for your success." [laughter]

Okay. So this is another one of those persons who, again, took an interest in me from the very beginning, when nobody should have taken an interest, and sort of to this day still keeps track of me. So anyway, I could have gone to Exxon. I could have gone to Chevron [Corporation]. I got an offer from Chevron here. Or I could have interviewed in academia, which was the path that my advisor sort of set out for me.

DOMUSH: Now did the majority of his graduate students go into academia? Was that kind of the norm for his group?

IGLESIA: He had a history for having built sort of an academic pyramid because he has been the most influential of all [academic "dynasties"] in the United States in the area of heterogeneous catalysis, to the [extent] that sometimes the group is affectionately or not so affectionately called the Boudart Mafia or the Boudart Clan, but there have probably throughout the years been about ten of us that have sort of continued to reproduce. And by the time we put history together, it was clear that there were a lot of very fertile people that had reproduced a lot, so there was a history that his best students went to academia, and they did quite well.

I have to say that I was terrified to go to academia. I mean, I had just finished working for four years on a fairly narrow problem. I had no idea what I would do if I had to start raising my own [academic] children at this point, and I don't know how anybody can get a PhD and then a year later begin to train their own PhDs.

So, I mean, I think that there is a problem about children having children there that concerned me a great deal, not openly, and not that I could articulate, but it was clearly something that I didn't think that I was ready to do. So I had the conversation with him, and I said, "I'm not sure that I'm an academic at heart to begin with, but I certainly am sure that I wouldn't know what to do if I went to academia now." And he said, "Well, fine. So why don't you go to Exxon? They have a corporate research lab that they are trying to expand." And several of his students had actually gone there [. . .] and then gone to academia afterwards. So he said, "Well, go there, okay, **<T: 05 min>** and then in five years I'll come and rescue you." So I went there. I mean, I went back to New Jersey. Now the weather was not a great surprise to me, but it was for my wife.

DOMUSH: [. . .] And this is up in Annandale, [New Jersey]?

IGLESIA: [Yes], this is in Annandale, except Annandale was not there at the time, so Annandale was in the planning stages. They had bought the land. The building had not been completed. And they had an awful place in Linden, New Jersey, across from the Bayway Refinery, that literally dated back to the early 1900s, but I was promised that this was only temporary. So we lived [near] where the new building was going to go, and then I would commute all the way to Linden for two years, until we finally moved there in 1985.

The job at Chevron was interesting. It was also a very good offer, but they didn't have anybody with the charm and persistence of Jack Weikart. And also, it paid less. And you think that California is expensive now, California was more expensive than anywhere else [still] then, so I didn't know [how] I was going to raise a child, keep a wife, and be able to buy a house if we stayed here. And that's where we decided that we were going to battle the weather. But we said, "One day when we can afford it, we will come back." Now it turns out that we came back and we [still] couldn't afford it, but that's beside the point. We did come back, so at least half of it was right. [laughter] But we were poor for a while, and we came back.

So I went to Exxon in late 1981. We got there at the end of, I guess, October of 1981, and my wife delivered the baby on December 31 of 1981.

DOMUSH: So you got there just in time.

IGLESIA: Just in time. We were all pushing very hard. [laughter] And [. . .] Daniel was a terrible child, okay? He was very nice later on in life, but when he was a baby, he was the absolute most colicky baby anybody has ever known. And my wife and I would take turns trying to calm him down and all that. But then the good thing about the commute . . . it was a one-hour commute all the way to Linden in a van pool. I would just sleep there and sleep on the way back, and then I was fresh when I got there, to be able to relieve my wife, but it was a very stressful year at that point. We [said that] we would not have any other children. Well, that didn't work too long. Amnesia, I guess, kicked in.

The corporate research labs of Exxon were at a very interesting time, and it was a time when—a couple of years earlier, the company had decided that we were going to run out of oil - they do that on a regular basis, you know - and that therefore they were going to have to get into things that were not oil, and they wanted to get into electronics. They bought an electric motor company. They wanted to get into photovoltaics. They wanted to do computers and photocopiers. I mean, this was Exxon, you know, sort of giving up their roots because they were going to run out of oil. And in order to do that, they needed to create a laboratory that would parallel the Bell Laboratories.

So they took a former manager from whatever AT&T was called at the time—I guess it was called AT&T—who had been Nixon's science advisor. This is someone by the name of Ed David [Edward E. David Jr.], and they said, "We want to build the Bell Laboratories of the oil industry." And I guess I was part of the staff that was hired in order to make that the Bell Laboratories of the oil industry, except that I was one of the few who had a chemical engineering degree, because they began to hire physicists and chemists and mathematicians and all that, in order to be able to create, you know, the center of excellence that out of its sheer neural connections was going to make the next transistor happen, right? And then Exxon will be able to live again after oil had been depleted.

Okay. So that obviously didn't come to pass, right? Four years later, they decided that we weren't going to run out of oil, that they didn't have enough money in order to maintain the staff that they had put together at that lab, and overnight, in 1986, four years after I got there, the labs were cut in half, so we moved [into the new facilities in 1984] or **<T: 10 min>** . By 1986, the lab was half [the size] of what it used to be, and I guess I survived that, mostly by being young and invisible, okay, but it was a very traumatic moment for me, to watch the human impact of a series of misguided decisions, where the decision makers were never affected, but the people who followed the decisions were actually disrupted.

DOMUSH: Right.

IGLESIA: And there were a lot of people around me at that early and impressionable age that had a very difficult life as a result of what others had decided. The people that were paid the large amounts of money to plan for the future made a miscalculation, and all of a sudden, humans along the way got hurt.

In a sense, in a perverse sort of way, I think it was also a career changer for me, because they had not hired very many people after I was hired. Then the laboratory was decimated in 1986, and all of a sudden I was unique in a different way: [. . .] I was one of the very few that actually had the chemical engineering background that they needed now that they were getting back into the oil business again. And on top of that, Exxon had a way of psychoanalyzing you to try to figure out what is it [that drives you]. I mean, they didn't do it any malicious way. They did it in order to find out what you would be best at doing, right? And I gather from the results of those experiments, okay, that they had decided that actually I was someone who should be in management rather than science.

To be honest with you, I don't know why, other than I have gotten as a function of age very good at compensating for my shortcomings, so I can look like an extrovert in the right environment. I can look like I really care about your problems, okay? [laughter]

But there is a word for that, but my vocabulary isn't . . . ["empathetic" yes], I guess, is a way to put it. So I'm not hiding my feelings. I'm trying to make you feel better, right?

DOMUSH: Right.

IGLESIA: But it's something that I do to compensate for my natural tendency that if you walk into my office with a problem, I say, "Go work on your problem. Don't bother me by talking about it." But somehow, somewhere along the way, I was identified as one of those people that would be given more responsibility. Honestly, I don't know—nobody asked me along the way whether that's what I wanted to do, but it is very [flattering]—it sort of puts you to sleep a little bit when people give you more responsibility, they give you more money, and they said, you know, "We're doing this because we think technically you're very good." So you begin to move through the organization. You have more responsibility. You don't realize for the moment that the higher up in the organization, the less your technical skills are probably essential. And the more those skills that I was compensating for, because I didn't have, become more essential. So I became a group leader. Then I became a section head. And—

DOMUSH: And all the while, you're doing less and less [. . .] science.

IGLESIA: Right. In a way, I mean, I looked back at it and I said, "You know, I was pretty clever," okay, because every time that they gave me the responsibility, I sort of made the pact with the devil, so to speak, and that is that, [yes], I will take the responsibility, but you have to give me resources to continue to do some science. So when I became the group leader, there was an additional technician that came into our group, and then I was able—and he was very, very good. I mean, Joe [Joseph E.] Baumgartner didn't even have a bachelor's degree, but he could get a PhD anywhere, okay? And he was one of the people, along with my children, that convinced me that I could actually teach people how to do things, okay, because he was so bright and he learned so quickly.

So the pact with the devil was I would do some administration, but then I would get resources so I could keep my science going. **<T: 15 min>** Then when I became a section head, and now I had fifty [scientists and support staff] in my group, okay, I said, "I need a postdoc [postdoctoral fellow] in order to be able to do that." So at every point along the way, I was agreeing to do something that seemed like they were rewarding me by giving me the responsibility, but still sort of keeping the science going, okay.

But what happened as a function of time was that the administrative component became more and more onerous, not in the number of people, but in the amount of administration. To put this in context, *Exxon Valdez* happened along the way when I was acquiring that responsibility.[2] The company became much more structured in the way that they did things to make sure that all procedures were documented, and that the company went by the book and everything. And that generated a lot of administrative overhead that fell on me to do, and even though I had a laboratory and I had people working in there, and they were productive, they were providing the few publications that bought me the exit visa to come here afterwards, I was not getting directly involved in what they were doing. They were doing very well because they were very good, but I wasn't doing the science myself, which explains at least in part why I came here, where I began to somehow micromanage the group a little bit, because I never wanted to be detached from what was going on in the lab. Otherwise, the group would grow to a size that I couldn't manage anymore, and I would feel that I was no longer indispensable.

DOMUSH: Right.

IGLESIA: In other words, that anybody could do it if I'm not there doing it with them. It was a time where life was moving so quickly. I was getting so little sleep because we were having three children at the time and everything was the equivalent of the fog of war, I think, at that point. My career was moving very smoothly. Everybody thought that this was a success story. Everybody seemed perfectly happy with my ability to do more and more, "responsible things". But I was less and less comfortable with how I was using my time, and I think what happened is what people dismissively call a midlife crisis. Okay?

I happen to have a very scientific rationale for what people call a midlife crisis. I came to the age of thirty-six or thirty-seven and I said, you know, "What do I want to accomplish for the rest of my life?" And if it was becoming the vice president of corporate research [at Exxon], it was probably attainable, but it would require certain sacrifices. I would have to give [up] my

[2] On 24 March 1989, the oil tanker *Exxon Valdez* collided with a reef in Prince William Sound, Alaska, causing the tanker to spill eleven million gallons of crude oil into the sound over the course of the next several days.

scientific output, I would have to move and get [different] experiences with the company, and so on. And then I would look at a vice president position and say, "Is this really what I want to do for the rest of my life?" And the answer was clearly no.

I remember distinctly at the time that that I—okay, so what precipitated this? What precipitated it was the fact that the next job that I would have would be a job outside of New Jersey, would be outside of corporate research, and it would be in either Texas or Louisiana. So now I go back to the comments that I made earlier about my own willingness to go [only] to certain parts of the country where I thought my children would grow up with an acceptance of others that were different than they were. And clearly, that was the path that I would have had to follow. So even if I wanted to ultimately end up in that kind of a job, it would have required that I do something that I thought was not for the family the right thing to do.

DOMUSH: Right.

IGLESIA: I have to say that the family, the children in particular, didn't think that moving to California was such a good idea, but then they changed their minds after a while. [laughter]

[Our younger daughters, Christina and Elizabeth, in particular, were heartbroken, at four and seven years old, to leave their friends, but they were real troupers, and all is well that ends well.]

But at that point, I was going through [all these scenarios] on the little time that I had to think, because my life was moving so quickly, I had to begin to think about all these things. Plus I think the realization that there was a window beyond which if I had stayed [at Exxon] and I had detached myself from the science, there was no way that I was ever going to go to academia. [And also] that it would be very difficult at Exxon at the time for me to go back and become a scientist again, and that I had gone to Exxon in order to finish my education, right?

So that perhaps I should now consider academia, and listen to my advisor all along, with now a much **<T: 20 min>** more polished knowledge of what life was all about and what research was all about and what areas were important, and so on. And I have to say that the education that I got at those labs [of Exxon], that, you know, from the comments that I have made perhaps looks negative rather than positive, [but it] was always positive. Everybody was doing what they thought was the right thing for me. They just forgot to ask me what I wanted to do, but that's because they were so excited about what they were doing for me. I got to work with some of the best people that anybody has ever worked—I mean, that lab has a history, just like Michel Boudart's group, of being the place that populated academic positions, because the people there were at the top of their field. They were doing first-rate science, and it was very fundamental in nature.

But also, the people for the most part, in spite of the fact that the company is a very assertive, arrogant company, the people there were anything but that. I mean, there were people like the [elders, the] Gary [B.] McVickers of the world, the John [H.] Sinfelts of the world. I mean, these were people that were well-known for doing first-rate research, and yet I could walk into their offices any time, go to the whiteboard, okay, and ask them to help me with a problem. I also have to say that this was my disappointment in coming to academia, is that professors are much more of a compartment than scientists in industry are, and that it is very difficult for me to do what I used to do with John Sinfelt or Gary McVicker or [with some of the younger Turks, like] Stu [Stuart L.] Soled [and Sebastian C. Reyes], than it is for me to go and walk into somebody else's office here [at Berkeley and have] the same kind of technical exchange.

I mean, I was involved in projects that were—I worked on the AGC-21 gas conversion project, which was going into large-scale pilot plant. I did both the fundamentals and the engineering part of it. I got to learn about bubbles going through liquids, which had nothing to do with catalysis or what I do, and I got to work with some of the smartest people that I have ever worked with, [a group that includes many of my current Berkeley colleagues].

And I did that while getting a very good salary, a salary that was even higher as a result of the management responsibility, which is what made it so painful [financially] to come to Berkeley afterwards. [laughter]

[. . .] So Michel Boudart was a consultant at Exxon, but I was very concerned about talking to him about my midlife crisis during the consulting [session], because I thought this was Exxon paying for his time, right?

DOMUSH: Right.

IGLESIA: I had been a consulting professor at Stanford for three years, and that meant that I came every summer for a few weeks, okay, and I taught a short course [or gave a seminar], I worked with Michel's group. I wanted to do it. So Michel told me that I should do this in order to get sort of—that was the pressure valve, the pressure relief valve from my life, because he was noticing that I was a little bit stressed over there. I don't think he had any indications or any feelings that I would actually use this as a test of whether I would like to [move to academia], but that's exactly what I was doing. I was looking at whether I could sit for four weeks and watch academics and graduate students, and whether I would be able to sort of survive in that kind of environment. But I never had the conversation with him [during those times that] I would come out to Stanford.

I actually had it during one of his visits, one evening when I said, "We're going out to dinner, but I'm paying for dinner tonight." [laughter] And he said, "No, no. I'll pay for dinner." I said, "No, no, no. I'm paying for dinner tonight." So we went out to dinner, and I said, "You told me that you would come [and] rescue me in five years, and you forgot about me." So here's my Catholic guilt, [the giving part]. [laughter] And he says something like, "I thought you were too happy to move you anywhere. You know, everybody that looked at it said, 'He forgot about science. He's going to become a manager, and, you know, we'll never have him back in academia.'" And I said, "Well, that's just, you know . . . I do the job that they ask me to do, and I do it well enough that they want to give me the next job. But if I went on strike right now and I said, 'I don't want to do this anymore,' okay, I'm not sure what would happen, so I'm not going to do that, but I'm ready to consider other options."

And he said, "Other industrial options or academic options?" I said, "No, if I want to work in industry, I cannot **<T: 25 min>** think of a better place than Exxon in order to do it. I'll take my chances. I'll go back into the laboratory. It will take me five years before they accept me, because now I have gone to the dark side, and now I'm coming back to the bench again, but that's okay. I'll take my time, and I'll prove myself to be pure again, and get away with it." [laughter] "So it will be an academic position." And so he went and said, "Well, we'll—I'll make some phone calls, you know, see what happens." And within two weeks, three weeks, I had three phone calls. Within six weeks, I had three interviews, one semi-official. And within probably two and a half months I had offers from the three places.

DOMUSH: Wow.

IGLESIA: And this was too fast for me. I wasn't ready—

DOMUSH: [Yes], I was going to say, that's moving really fast.

IGLESIA: [Yes]. So now I don't have time to prepare the family. I can't begin to act out so that they notice I'm unhappy, right, you know? [laughter] I can't do this drama routine that would convince them to do this, right? So this is the point where they really thought—my wife thought, "He's having a breakdown now. It's time to intervene somehow."

There were some difficult choices to make. I mean, one of them was that one of the offers was from [the University of] Delaware, [which] was probably the number four or five chemical engineering department [at the time], and it was a much better offer than the other two. It was in a more conservative East Coast environment; my wife had adjusted to the climate, and actually liked that kind of environment. And she clearly remembered California as the wild place where we spent our childless years, and she wasn't sure that she wanted to raise children in such a wild place as the one that we lived in. And the people at Delaware were so charming that it was clear that I was going to have to work very hard in order to do anything else. Berkeley made the second-best offer, and included a significant amount of charm in the equation. And the charm came because at the time the chair of the department, Morton [M.] Denn, was a very charming individual, but also because he enlisted the help of several of the former members of the department, the emeritus professors, that had some of the old world charm that he didn't have.

So I came in, and they treated me not particularly well, but then when I came with my wife, then they brought out all the charming people in order to charm her. So I remember a famous dinner with Charles Tobias, who was one of the fathers of this department, and I didn't know what the seating arrangement was, but they insisted that my wife sit next to him. And, you know, he was, I think, instrumental in sort of making her forget that this was not the place where she wanted to raise children. And then we took [off for] three days. It was one of the first times that we left the children with their grandparents. And I rented a convertible, and we went up north on the Golden Gate Bridge with the top down, okay, and [. . .] by the end of those three days, we were not making any calculations. We said it's either Stanford or Berkeley. It's not Delaware anymore. So we crossed that threshold.

Stanford actually made—they either did not make an offer or they made the worst offer. I cannot remember exactly what they did, because I don't think I ever got a piece of paper. But clearly, they were very concerned about whether I could teach or not. And they were very concerned about tenure, and here I was at thirty seven. I had been out for ten years. I had twenty-five publications or so, and I wasn't about to move a family with three children and then have to go through a tenure process in order to do that. And also, by the time all was said and done, I think that we would have been able to get over that hurdle, but there was clearly the expectation that I would replace Michel Boudart, because he was retiring at the time.

DOMUSH: Okay.

IGLESIA: And that was a bit much for me, because I didn't want the responsibility of having to live up to the expectation that I would become one of the giants in the field and take his place.

DOMUSH: Right. Shoes like that are hard to fill, to step right into that.

IGLESIA: They are. [. . .]

DOMUSH: It's almost impossible to live up to whatever the expectation is then.

IGLESIA: Right. And again, I didn't want that as a responsibility or as an objective. My objective was to do science and to see how good **<T: 30 min>** I could be at doing science on my own, you know, as an [academic] businessman. Part of the—so I'll give you two pieces of my background that somehow later on became clear to me. One of them is that I'm the first college graduate from my family, but I'm also the first one that is not a businessman. So there is something in the genes or in my history that says that I wanted this control that I wanted over my surroundings perhaps could be best achieved by being in business by myself, and really, academics are a lot more like sole-owner proprietors, than they are anything else, right?

The other one—and this is something that I did not know until 2007—is that it turns out that my great-grandfather, [Jose], was the teacher in the village. So in 2007, I go back, and for the first time I return to the village where my father was born. I had never met my family there, but I have aunts, [uncles, and cousins whom I had never met], and they took me to the church [where my father and aunt were baptized]. They took me to the old school where my father had gone. They found people there, "See, I remember your dad. He was really smart." And then they took me to the cemetery where my ancestors [are buried], and on top of [a grouping of tombs], it says, "Casa do Maeso," the house of the teacher [in the Galician dialect]. Everybody in my family had been buried in [the pantheon that] is known as the "House of the Teacher," because my great-grandfather was actually the teacher [of the village]. And there was no school, so every day they brought them to his house, he would teach everybody, all the grades, you know.

DOMUSH: Wow.

IGLESIA: [. . .] So there's some sort of teaching in my genetic code somehow. They would select the smartest, and so, I mean, this was the time when teachers were highly recognized. They were the smartest ones in the community; they were the ones that effectively became the "House of the Teacher," okay? And I had no idea of this. I [gave] a speech afterwards, because I [received] a *doctor honoris causa* from [the Universidad Politecnica de] Valencia, and I told the story about how unbeknownst to me, actually, there was some genetic pool that should [and would] take me to academia, because I came from the house of the teacher, and [would] also make me a businessman, because everybody else in my family had never done science. They had always done business.

It was difficult to decide between Berkeley and Delaware, because the offer was so much better, the housing was so much cheaper [at Delaware]. My family I think would have been probably less disrupted by the move. The environment looked the same [as in New Jersey]. The houses were East Coast type. And there was open space, and all that. And one of the reasons I didn't go—well, there were two reasons. First of all, I thought that Berkeley was a little bit like me, a little bit different than any other place around. Okay?

DOMUSH: Okay.

IGLESIA: And that this place really was more of a cross-section of society than either Stanford or Delaware were. Stanford to some extent is a bit too orderly for my taste. This place, [Berkeley], has the randomness of [the real world]—and also the reputation for a level of political openness that is no longer here, by the way, [even if] the reputation [remains]. And I wanted to be in a place that was accepting of others. This was the antithesis of moving to the South and being treated the way that I [felt] at Princeton or the way that I was treated in Baton

Rouge or Texas. And that I wanted to be in a place like that, and I wanted to sort of contribute to the heterogeneity [and the non-conformity] of the place.

And the other one is that Delaware had some of the brightest people in chemical engineering at the time, but they were all my age. So we were all within six to eight months of each other. And I said, "If I walk in there, we're all going to be fighting for attention at the same time." It's like having quintuplets, okay? We all want to be fed at the same time, and I think it's going to be difficult to sort of keep this department together. And sure enough, I mean, most of those people either moved into administration or left and all that because they couldn't contain that much talent with the same needs at the same time [in the same place].

DOMUSH: Interesting.

IGLESIA: And that was actually more thoughtful than I intended to be, but it was clearly something that concerned me a great deal.

[. . .] And I think it's probably okay that I spent a little bit of time, because it may be better to leave the Berkeley experience for tomorrow so it's self-contained. **<T: 35 min>** It is more recent. But there's some things about the transition that I think were probably marking, or at least [possibly] interesting to hear.

So here's someone who is very concerned about controlling his surroundings. That's me. The immigrant never wants to be in a position to have to go back again. You know, to have the infrastructure crumble one more time and be left with two pieces of luggage and have to start all over again. And therefore, it is not in the nature of the immigrant to be impulsive in the way that they give up a perfectly reasonable position for one that may be potentially better. [But I did].

DOMUSH: Right.

IGLESIA: But not necessarily in a material way, in a practical way. A few things happened, okay? So one of them was I was sitting there and saying, "I'm doing this, but this really financially does not make any sense. This could very well be a midlife crisis and nothing else, right? I mean, should I put my family through all this if this is just a momentary thing and a very bad financial decision?"

At that time, there was an inquiry that was made by another company that effectively said, "We have this position of Director of Research. You don't have to move very far." It was on the East Coast. It was working for somebody who I knew fairly well. It was a significant increase in salary, okay, but it did not have science at the bench. It was clearly at that point a high-level responsibility.

This was at the time when that movie that created so many ripples—it was called *The Last Temptation of Christ*—came out. So Michel [Boudart] described this as the last temptation of Saint Enrique, okay, [laughter] because it was almost—I mean, the movie was coming out at that time. He said, "You know, you're being tempted one last time to make sure that this is what you really want to do." Right? So I visited the company. They made the offer.

DOMUSH: May I ask what the company was?

IGLESIA: I'd rather not [say]—it was a major chemical company—only because it was kept pretty quiet at the time.

DOMUSH: Oh, that's fine.

IGLESIA: But it was remarkably effective at making me decide to go to academia, because I said, "If this is the step up from Exxon, okay, this takes me farther away from science." [. . .] Certainly there were financial upsides, but if I wanted financial upsides, I can go back to the lab at Exxon and I can still get a reasonable salary. The house is paid for, okay, and I don't need to disrupt the family by doing that. So that was the moment, I think, when after that last temptation, it crystallized in my mind the fact that this was more than a midlife crisis. This was a moment in which you introspectively look at what you want to accomplish for the rest of your life, and you have a unique opportunity that may not materialize again, and you take a chance, a chance that is much more difficult for someone who is insecure about their ability to preserve their comfortable position. [I guess you would and should then call it a midlife crisis].

And this led to [both] a conversation with my father, but also to a realization as to why it is that my father worked so hard when he didn't need to work so hard anymore. And it is that he was insecure, that he wanted to make sure that he would never fall back again. He was twice an immigrant.

DOMUSH: Right.

IGLESIA: Under unusual circumstances. He wanted to make sure that there was enough money that nobody would ever have to worry about it. And I think because I only had to do it once and not twice, I was able to overcome the similar feeling that I had, that I should not give this up and be sort of poor in order to get this whim that I had of going and being an academic.

And I had a conversation with my dad—I remember that he was not very happy about it. He said, "How can you leave a perfectly good job with a high salary at one of the most successful"—I don't think he called it the most respected, because it was after *Exxon Valdez*—

"the most successful corporations in the world. They have great plans for you. You have a family. You have three kids, okay, and you're going to move them to California? And of all places in the world, to Berkeley, the place where communism started?" [laughter]

You have to realize, he was a [conservative relic of the Spanish Civil War]—he was on the [Francisco] Franco side of the civil war. "Of all the places in the world, you have to go to the place **<T: 40 min>** where communism started," okay, and that's when I decided I was coming to Berkeley, because there is this rebel that says that if my dad says it's not okay, I'm going to do it anyway.

But it was financially very difficult at the beginning. I mean, the salary was twenty-five percent lower. The house was twice—actually two and a half times the one that we sold. We had to buy a house in either a reasonable school system or put our children in private school. And we did have a bit of a war chest, but not enough to really get us over the first two years.

DOMUSH: Right.

IGLESIA: I mean, two years later I think we would have had to do something fairly dramatic unless something happened. And what did happen was that, again, Michel Boudart came to the rescue [once] again. He had [founded] a company called Catalytica that was thriving at the time, and I began by consulting with them. Then I became a scientific advisor. Then I became a senior scientific advisor. And, you know, that's the place where I got to meet the likes of Henry Taube, Éric [J.] Derouane, Jim Collman, from Stanford sort of at the same level. For the first time, I was sitting at the table with a Nobel [Laureate] and discussing where the electrons would go in this particular [chemistry]—and I was getting income that meant that there were no longer any negative numbers in my balance sheet. We were still not saving money, but at least were not bleeding money along the way.

DOMUSH: Right.

IGLESIA: I have to say that [. . .] I tell my children right now that most of us will die with more money than we need, but we don't have the money when we need it the most, okay? Which is at a point in our lives where perhaps you are right now, where you're raising a family, and trying to balance two jobs, and all that. And then later on you realize that if you could only move some of the money now to that point before—so I'm a firm believer of getting in debt now, [early on in life] okay? [laughter]

Because I did get in debt to some extent, and everything worked out all right. But the last thing that I would say is that I had an interesting discussion [a few months before the move]. So at the time, [our] two daughters were a little bit too young to have a serious discussion, but [our son Daniel] was about eleven at the time, and so I sat down with him at the dinner table when everybody else had left or was throwing a tantrum or something, and I said, "You know, this may not make any sense to you right now, and actually to some of the people that look at this decision, it may not make any sense to them either, but in order for everybody to be happy, I need to be happy, and I'm not happy doing what I'm doing right now."

DOMUSH: Right.

IGLESIA: "And, you know, we're going to be poor for a while. That means that we may have to reuse our clothes more often. We'll have enough to eat. Don't worry about it, okay? We'll manage, but, you know, we're not going to be able to take family trips to Disney World on a regular basis. It's going to be tight." And he sat there, and either he didn't realize the magnitude of the difference, or he actually was that thoughtful at the time, but he said, "No, it doesn't matter, Dad. I think you should do whatever you think is the right thing to do, and money is not that important." Okay? An eleven-year-old that says that money is not that important. [I was convinced that he had learned it in a song from the Beatles.]

This is why—he was a terrible [baby] until the age of one, and then after that he was actually quite reasonable. And, you know, the touching thing about this conversation is that it repeated itself many years later, but the roles were reversed. So when he was at the end of his PhD or so, he was trying to decide whether he [would go into] the corporate world, and this was a time when a lot of the apps for iPhones and all that, and, you know, Apple was hiring people with degrees in music and computer science, and he was trying to make a decision as to whether to join the corporate world or hang out and wait it out for [one of those] academic positions. So we had a conversation at the dinner table, and so we talked about his options and all that. And I said, "You know, but Dan, if you wait for an academic position, you're going to struggle **<T: 45 min>** for a while, until it comes up. You're not going to have very much disposable income and all that." And he said, "Well, you know, money is not that important."

So I guess he was doing the same thing that I did, okay? Sort of giving up something that was a certainty and financial stability for something that was ill-defined and certainly less [profitable]—because he thought at the time that was the right thing to do.

It turns out that he has not [or at least has not yet] been able to secure that academic position, which is why he's coming and finally joining the corporate world, but he has learned an awful lot, both about human beings and about his area [of expertise], by doing as many things as he has done. He has worked for a small company. He has done contract consulting work. He has an artistic side that he has been able to develop in New York, and now with software here, because it's not New York. So actually, he's a very different person, and a much more unique individual as a result of it, and he did it at a significant sacrifice in income, in order to be able to do that. So who knows? Perhaps he will leave Google one day and go into academia. But to me, when that conversation was like ten, fifteen years later, I was the one that was telling him he should worry about money, right?

DOMUSH: [Yes].

IGLESIA: And he's telling me that money doesn't matter. At the beginning, I was trying to convince him that money was not—so watch out what you tell your children, because they will repeat it to you in your old age, okay? [laughter] In some way.

But, I mean, that was a teary-eyed moment when we sat there, and [. . .] we had this conversation. I said, "I remember this," but he was only about yea big at the time. Now he's got a beard, and so I think we raised all three of them to worry about what matters, and I think even though it was very disruptive socially for all of them, I think they recognized that one makes sacrifices, you know, in order to accomplish something that one feels is important, okay? [And the two young ladies struggled with the move and with the disruption, but they are also well on their way to careers that will serve them well and will serve others also well].

DOMUSH: [Yes].

IGLESIA: And that may not always be the right economic decision, but you have to do what you think will actually yield the returns [in other ways]. I think as a result of my coming here, I have been able to balance my life [somewhat] better between family and work. In spite of the crazy life of the academic, it is a controlled crazy life. It is not a "tomorrow you have to go here because somebody woke up in the wrong mood and you have to go and have a meeting with somebody." Here, the craziness is long-term and planned, okay? It is sometimes sort of monotonous in nature, but I like a certain level of reproducibility in my daily life. And the life as a manager at Exxon I think would have been a lot less accommodating of the balance between family and work than this has been. [Here, you bring the craziness upon yourself and there is a feeling of control in doing that.]

DOMUSH: Interesting. Well, I think that that's probably a really great place to stop. And aside from it being the right time for us to stop. And tomorrow, we'll be able to start up with how the research and how everything kind of has played out in your time here at Berkeley, and talk a little bit more about some of the science and all of that tomorrow. [. . .]

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

INTERVIEWEE: **Enrique Iglesia**

INTERVIEWER: **Hilary Domush**

LOCATION: **University of California, Berkeley**
Berkeley, California

DATE: **28 January 2014**

DOMUSH: Okay. So today is part two of the oral history interview with Dr. Iglesia, and today is Tuesday, January 28. Again, I am Hilary Domush. And yesterday, we finished up by spending a lot of time talking about why you decided to make the transition into academia, how you chose Berkeley, and some of the difficulties associated with the transition from a kind of personal standpoint. And I was really curious to hear about the transition from a research standpoint. We didn't really get to talk much about the work that you did at Exxon, and I'm not sure how much you can talk about it, but I'm very curious what you could kind of take with you, if anything, from Exxon, to bring to start your new research program here at Berkeley.

IGLESIA: [Yes]. I mean, interestingly, those are two things that I pointed out to myself at the end, and that was that I did not really do justice to eleven years of very sound and fundamental research at Exxon, even though most of it is publishable and not confidential in any way, and that I was going to spend a whole two hours today talking about the last twenty years. And it seems unfair to the people with whom I worked and who educated me the rest of the way, the people that actually made it possible for me not to feel any longer sort of incapable, or ill-equipped, to train other graduate students, as I did at the end of my PhD. [There were some special people at Exxon in those days, who were my teachers and became my friends. Stu, Stuart L., Soled was a bit of my alter ego and taught me about making catalysts, about intuition, and about midlife crises. The late Sebastian Reyes rekindled my interest in the mathematics of chemical engineering and helped me to put to equations the chemistry and engineering of very complex processes. Along with Michel Boudart, they were the most influential teachers in my early scientific life.]

So I think it would be fair before I make the technical transition and I tell you what the challenges [in the transition] were and how some of the challenges were imagined, and some of the ones that I didn't imagine became real, and how foolish one is at any point in one's life [and how unpredictable life tends to be. [. . .] When one jumps, the landing isn't what you expect it to be, for better or worse. Sometimes it's better than you expected and sometimes not. But I spent eleven years . . . and throughout the eleven years at Exxon, I think I was able to balance my life between, again, what I call my pact with the devil, and the things that I did in order to protect a catalysis group that had been badly hurt during the cutbacks, what I did with my own science, and what I did to try to teach some people in return from what others [in turn] taught me. [But it was not who I was and it took its toll.]

To put it in perspective, in 1982 or so, when I joined Exxon, it was a time when there were some very exciting things going on in the oil industry. One of them [was that] we were going to run out of oil. As I already told you, that never came to pass, okay? The other one was that one of the ways that we were going to replace oil was by using natural gas. And at that time, geopolitically, the natural gas was much better situated than oil, because it was spread broadly throughout the world, but it was mostly in fairly remote areas, so it needed to be liquefied. And you can liquefy natural gas by either using temperature or pressure, or you can liquefy it by doing chemistry. In other words, [by] converting it into molecules that naturally have a boiling point that will allow them to be liquids at room temperature.

So that was the heyday of gas conversion, gas conversion being the process by which you turn methane into synthesis gas, and then do what is called Fischer-Tropsch synthesis in order to build high molecular weight hydrocarbons. That was where I started my career at Exxon, and in that context, I have to mention the person that became my unofficial mentor at the time was Ross [Rostam J.] Madon, who had been a previous student of Michel Boudart, and as I told you, we tended to hang around together and protect each other and all that. And he was one—and still is—one of the world's experts in Fischer-Tropsch.

And there, I began by looking very carefully at a system that actually could have been made better if we had only understood how it worked. I was actually given the luxury of studying the mechanism of how the reaction takes place, to recognize how some of the products would come back and continue to grow, to make higher molecular weight hydrocarbons, and also how things like chemistry and physics came together in coupling reaction with transport and so on. And therein, from my work with him, and my work with Sebastian [C.] Reyes later on, came what today is the accepted model for describing the selectivity of Fischer-Tropsch synthesis reactions in terms of this diffusion-enhanced and diffusion-inhibited monomer formation and olefin readsorption.

The work in that **<T: 05 min>** area was bookended by the two people that sort of brought me up. At one end there was the issue of how do you describe complex systems that needed hydrodynamics and chemistry, and that was the expertise of Sebastian Reyes, who actually interestingly came in as a postdoc of a senior statesman, [Lee "Skip" Scriven], who [sort of] abandoned him, and then he became my surrogate postdoc, even though we were only maybe three [or four] years apart in age. Then he became a permanent employee. We continued to work together, and he's probably solely responsible for the part of what I do that has to do with chemical engineering modeling with diffusion-reaction problems, with how do you describe and measure the transport properties of porous structures.

So that was the bookend from the chemical reaction engineering side. And then I had another bookend in the Fischer-Tropsch area from the sort of solid-state chemistry material synthesis side, and that was Stu Soled, who was the one that taught me first that I wasn't entitled to understand everything, okay? That I could actually make useful things before I understood them altogether. And we kept getting into arguments as to whether it was worth discovering things if you had not predicted them ahead of time [based on understanding]. I mean, so this got into fairly philosophical discussions as to whether what actually drove me to do things was figuring out how things work and then making it better, or whether I would settle for making them better and then understanding them later on, okay? And we certainly took different views of the subject. Where I always derived my pleasure from having discovered something only because of what I understood, rather than understanding it after a great thing actually happened. But in that sense, we were alter egos, and we came from totally different backgrounds, and it made for, I think, very interesting . . . and I think he has become quite the kineticist, and I have become a reasonable material synthesis person, as a result of that. [laughter]

So we passed on not the worst of our traits, but the best of our traits, to each other. And then there was another person that was quite influential, and I mentioned him, [Joe Baumgartner], yesterday already, and he was a technician who was assigned to me at a time when I accepted management responsibility in exchange for some help in the laboratory. And he and a visiting professor by the name of Geoff [Geoffrey L.] Price, now at the University of Tulsa, sort of got me along the ways of how do you use isotopically labeled molecules in order to probe mechanisms, something that had been known early in the history of catalysis. Paul Emmett was one of the pioneers. And then it was abandoned because everybody fell in love with electron spectroscopies, and the fancy techniques, as opposed to the ones that really require a lot of work in order to understand them.

And it was the experimental talents of Joe Baumgartner, the previous experience that Geoff [Geoffrey L.] Price had had in isotopic type experiments [from his PhD with Joe W. Hightower at Rice University], that drove my group at the time, and then my group here today, into an area that had been largely abandoned by the discipline. That was quite a niche for us.

Another person that taught me a lot at Exxon, and then marked the rest of my career in the area of spectroscopy, was George [D.] Meitzner. He was another one of the Boudart clan. He was a bit younger, but almost my age. [. . .] He became an expert in X-ray absorption spectroscopy. This was the time when Exxon was pioneering some of these techniques by building beamlines at the [national] synchrotrons in order to do those kinds of experiments, and building *in situ* cells in order to be able to look at the chemistry as it actually happened.

And that's where my interest began at that point in using spectroscopy, but only by building reactors that actually doubled up as a spectroscopic cell. And that's where, you know, later work here in infrared and UV-visible and [Raman spectroscopies built on to] that realization at that point, that moment of clarity. What we said, a lot of what we see when we look at things before and after have nothing to do with what is there, but also, a lot of what you see even while things are going on may actually be just [spectator species] watching the chemistry taking place, rather than being the intermediates [involved]. That also was the point when I began to get interested in doing transient experiments, doing spectroscopic measurements, in order to make sure that when I introduce a transient, that both the spectral features and the output of the catalytic reaction had similar time constants.

So, I mean, these are the things that you learn in childhood that then sort of stay with you the rest of the time. And all these things happened in an **<T: 10 min>** environment where the stress level was high enough that you didn't realize what you were learning [or even that you were learning], okay, until much later. And you didn't realize also how good the work was until much later, when you had time to sit back and actually look at it, or whenever you finally published it and other people told you how good it [really] was. Really, in that environment, everybody was encouraging everybody, learning from each other, producing things that were practical, and as I always said, if you do relevant things and you do them fundamentally, you actually have the intellectual debris, which is the knowledge that you leave behind, even when the [specific] project doesn't [lead to usable technology].

So we worked in the area of Fischer-Tropsch. We worked in the area of zeolite-type chemistries. This was also the time when oxygenates were introduced into gasoline, when sulfur had to be taken out of gasoline to very low levels. We needed ultra clean fuels. This was the time when there were the beginnings of concerns about energy inefficiencies, and actually, there were people at the time—in spite of denials later on that there were any concerns about greenhouse gas emissions—in fact, that far back, there was a realization that this was one day going to be a problem, and that even if it was not, there was an economic incentive of actually creating processes that were both cleaner and energy efficient, even in the absence of legislation [that forced you] to do so.

[. . .] And there was a part of what I did there that had nothing to do with my training, and it had to do with effectively blowing bubbles through liquids and looking at how mixing took place, because that happened to be the kind of contraption that was going to be used in order to carry out the Fischer-Tropsch synthesis. So I learned by working with Erich Herbolzheimer there about high-speed videography. How do you analyze pictures in order to be able to tell where the bubbles are going? How do you measure transient type behavior that told you how large the bubbles were? These were things that I think they probably did not do anything for me intellectually in my own work, except to convince me that I could learn how to do anything. Okay?

DOMUSH: Right. So you could still learn new things.

IGLESIA: Right. So to me, there was something that sort of clicked at the time that said that, you know, I think I still can learn quite a bit more, but I'm not sure that I'm going to do that by becoming a manager at Exxon, or even by staying at Exxon. And that because I was able to learn new things, I convinced myself that I could teach all of the subjects that I had taken fifteen years earlier, and so on, and that actually what I had forgotten, and I remembered by watching bubbles go up, actually were things that you could teach. They were teachable, and I could probably teach them.

DOMUSH: Now out of curiosity, how big were these bubble columns? I've never really seen one. I've kind of heard of them, but I don't really know what to picture in my head.

IGLESIA: So, I mean, the commercial units would be things that would be ten to twenty meters in diameter. But in order to be able to get representative data [in the laboratory], you had to build something [smaller but] where the walls were not affecting everything. So I could hug half of one, but not a whole one, okay? If that gives you an idea.

DOMUSH: Okay. [laughter]

IGLESIA: And—not that I did that frequently, but— [laughter]

DOMUSH: But sometimes.

IGLESIA: But sometimes. So all these things were sort of working in a way that, you know, it was clear that fate was going to take me in a certain direction. And at the end, fate was crystallizing the midlife crisis that somehow said, "Now is the time to do it." You know, it was colored by a little bit of arrogance but, again, you need that little arrogance in order to take the step that says, "I think I can do this, and I think that I have a lot to learn."

And one of the things that I had done through my career here [at Berkeley] is continue to do things that are conceptually new and that require that I learn something new, whether it be a new spectroscopy, a new kind of material, the ability to do molecular simulations. I mean, these are things that I continued to talk myself into saying, "You're not too old to learn new things." And to be honest with you, without being negative about my surroundings, that's something that is not all that common in academia. It is much easier to dig very deep holes and sort of surround yourself by the glory of a narrow community that already knows who you are than it is to write things and do things that you know will not be highly cited because you're not a card-carrying member of that particular community.

We got into membrane thin films here, and we learned how to make them and all that. We have never been recognized for making the contributions, **<T: 15 min>** because we never came in and stayed long enough in order to be able to receive, I think, the benefits of our presence. But nonetheless, what we learned was enough of a reward, okay, that the rest of it was not really necessary. [And these films, ten years after we made them, still stand as the state of the art in hydrogen transport membranes.]

And finally, as I mentioned already, one of the things that I did when I was at Exxon was that I was a consulting professor at Stanford for about four weeks a year. And I got, as I told you yesterday, a picture of what it would be like to be in a group. It wasn't a very large group, but it also allowed me to supervise, although supervision is probably too much of a one way relationship to explain what we did. But there was a student in Michel Boudart's group at that point that was getting towards the end of his PhD career. Michel was a very well-recognized and a very busy individual who never was really hands-on with the students, but in this particular case, they had a beautiful piece of work that was [unlikely] to be written [as it could be] unless somebody stepped in.

And towards the end of Fabio Ribeiro's dissertation, I got involved with him. We wrote together all of his work, and Fabio is a professor today at Purdue [University].[3] But he calls himself my first graduate student. So it's nice to have people that actually like to give me credit for something that I was not officially in charge of.

DOMUSH: Now were you also teaching a class during those four weeks, or were you just kind of in the lab, and—

IGLESIA: I was giving some seminars and then hanging around and talking to the students one-on-one for the most part, but there was nothing very structured that happened during the summer. So when I finally came here—so now we're going to move on.

DOMUSH: Sure.

IGLESIA: I think I have now done well by describing the [people who were very] important to me [personally and intellectually]. This gets a lot more difficult to do now, because there are so many more. I mean, one of the things that happens here is that every good graduate student becomes one of your teachers by the end, right?

DOMUSH: Right.

IGLESIA: And there have been many, many of them that have gone through. So their names will come up only when there is a particular point in which we changed something or we did something or something like that, but there are many more than the ones that I will mention.

DOMUSH: Well, and like I said before, don't fret if you can't remember someone's name, or you feel later like you forgot. We can always add them in later.

IGLESIA: Right. I remember their names I may just not remember them at the right time. And I do have a list here to sort of remind me, to some extent. So here were my fears as I came here

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[3] Enrique Iglesia, Fabio H. Ribeiro, Michel Boudart, and Joseph E. Baumgartner, "Synthesis, Characterization, and Catalytic Properties of Clean and Oxygen-Modified Tungsten Carbides," *Catalysis Today* 15, no. 2 (1992): 307-337.

to Berkeley. My fear was that teaching was going to be a chore, that it was going to be time-consuming, and I was not going to be particularly good at it. That I was going to have to work really hard and write these twenty-five-page proposals in order to get small amounts of money that then would buy me all kinds of emotional commitments to do this and that, okay? And I did some of that, but fortunately not [for] very long.

I did not think that managing a group was going to be very hard, because I had managed much larger groups of much more difficult people. Many of them, actually most of them, were older than I was and well set in their ways. So I never thought that the component of management was going to be a challenge. And I really did think that I was leaving behind what I had imagined or what I had interpreted [to be] a bureaucratic organization for something simpler. So that was the almost opposite to what I found when I got here. [laughter]

So first of all, teaching was never a chore, and I actually either was already pretty good at it or became pretty good at it as a result of the environment. I continue to call teaching the one [part of my life] that averages the ups and downs of research. In other words, research goes like up and down, up and down, right, and some days we're in the doldrums, and some days we are sort of ecstatic about what we have discovered. Teaching is something that if you prepare for it will always go well. So you have control over how you feel at the end of the lecture for the most part, if you have a limited amount of talent [but] also spend a lot of time preparing.

And a colleague of mine, whose name—whose face I do not—I don't know who told me this, but there is apparently a quote from Joel [H.] Hildebrand about the secret of good teaching, and he said that—that lectures [must] be spontaneous, but **<T: 20 min>** spontaneity [must] be carefully rehearsed.

And I think that summarizes to a great extent what you do, is that . . . nobody notices how hard you prepared for the lecture from the way that you deliver it, because it does appear as if these are things that you know by heart, that are easy [for you] to understand, and that therefore the audience should be able to grasp [them]. So there's a level of confidence that comes from being prepared to show how simple this is. And I've used that as my guiding rule for teaching. And again, I think I have some of the things to show for it hanging around [this office, the ornaments of war in the classroom in the shape of diplomas] . . . . So that was not a problem.

Writing long proposals and acquiring all kinds of emotional commitments was perhaps a difficulty at the beginning, when I had to apply for an NSF [National Science Foundation] grant and a DOE [United States Department of Energy] grant and so on. But as I will tell you as I go through how various entities came into our research and what happened as a result of it, it actually became easier to do as a combination of my establishing myself, but also having a bit of an aura of credibility that came from having come from the real world. I didn't realize that I could actually—my [exit] passport from Exxon was actually good in companies that were Exxon's competitors, but in fact it was.

DOMUSH: [Yes].

IGLESIA: So a lot of the [funding] came later, and it comes still today, for very fundamental studies that are actually supported by industry rather than [federal] funding agencies. Nonetheless, I think that there is some credit that needs to be given to the ones that got me started, and I will try to do that.

So I came in in 1993, and believe it or not my lab, [that] was to do all kinds of almost dangerous experiments, was in the second floor of Gilman Hall, which doesn't look very different than my office [today]. There was no hood. There was no ventilation other than a window that you could open up and down. And there was a bunch of equipment left to me with his best intentions by Gene [Eugene E.] Petersen, who was a faculty member here. This was his office. He left me his office. He was retiring at the time. And then he left me a lab with all his old equipment there.

I came from a state-of-the-art laboratory at Exxon, which was, among other things, air conditioned and heated. [It] actually had hoods everywhere, had detectors everywhere, and everything went through a safety review. And we now had three graduate students. They came to be known as the three B's: Joe [Joseph A.] Biscardi, Rick [Richard W.] Borry, and Dave [David G.] Barton. Now imagine. Out of an entering class I get three students whose last names start with a B. I think they were designated to come into my group somehow.

But anyway, these guys were actually excited that they had to tear everything down and build it from scratch. These were the ones that took the piping out the door, out the window, and figure out how far it had to go from the window, so that the CO would not back into the room again. And then we would put a CO monitor in there in order to make sure that if it came back, we could actually get out. This was 1993. This was not the time of the caves, okay, but that's effectively what my first shock getting here was.

So this was not something that I predicted. I also did not predict that the bureaucracy of academia could actually overwhelm that at Exxon by the mere fact that it didn't accomplish [much with the same amount of aggravation]. It was not that it was more bureaucratic. It's that what came out at the back end was so much smaller than anything else, that the amount of [recycles] that you had to go through, and it has only gotten [much] worse. So that continues to be my main disappointment, and something that I spend time today trying to get undone most of the time. My mental picture is they throw boulders in my path to make sure that I move slower.

And then there was the issue of how difficult it is to manage a small group of younger people. And how much of what I do has to do with the psychological aspects of how one mentors, rather than the technical aspects [of mentoring]. At Exxon, I could not change anybody into being someone else in the way that they looked at things. They were set in their ways, and it was a matter of for the most part sort of minimizing the impact of their [least productive] traits, so to speak. Here, you actually had a chance to actually get people headed in the right direction, and that created a problem for me. The first one is that as I already told you, I am not very good at talking through problems. And my first inclination is, “Always go work out your problems and then **<T: 25 min>** come back and we’ll talk science”, right?

And the other one is that I felt that I should not really play “master of the universe” and try to move people too much in what I thought was the right image of someone, and that continues to be a challenge for me, especially when people come up to me and say, “You know, he does exactly the same things that you did when you were his age,” right? And my children do the same thing, and I have equal worries about that. I’m not in the business of creating clones. I’m in the business of getting them to be who they are in a productive sort of way. So that summarizes what I predicted incorrectly, which was [pretty much] everything. [laughter]

And the fact that everything turned out pretty well in spite of all of the wrong predictions, which means that I came here for the wrong reasons and it was the right place to be. Now I could turn that around and say that maybe there are people and characteristics of people that will [make them] do well wherever they go, because they will actually adapt and benefit from the positive, and not dwell on the negative. And I like to think that I tried to do that to the extent possible.

DOMUSH: Now can I ask before we move on, especially kind of early on, when you were just starting teaching, what were the courses that you were teaching?

IGLESIA: So traditionally, I taught at the beginning the undergraduate chemical reaction engineering course. So we teach one course a semester here. I have taught mostly graduate and undergraduate core courses, very few electives along the way. And I would teach the graduate version of that [reaction engineering] course. [. . .] And then at about the same time I would teach an elective course in heterogeneous catalysis. And then later on, I began to teach the most theatrical of all chemical engineering courses, which is the introduction to chemical engineering, the mass and energy balances, the one where you really have to be spontaneous in order to keep them awake, and where the content is trivial, but the ability to keep sophomores [awake] at 8:30 in the morning or so and get them to do mass and energy balances is a bit of a challenge, okay? And yet that’s probably the course that is most challenging in the theatrics, but is the one that I probably enjoy the most. There is something about the introvert that doesn’t mind being the center of attention, as long as you [can] force the audience to pay attention. That’s my conclusion, and that is that I don’t want to have to justify myself in a cocktail party and get people interested in me. But in a classroom, a large classroom with a lot of students, they’re forced to look at me, to listen to me [and if I speak loud enough and sound interestingly spontaneous, most of them will to stay awake, at least the eyes will remain open].

DOMUSH: Right.

IGLESIA: And that's the best place for an introvert. You don't have to work the room. The room is set up for you to be able to do that. So those are large classes where you really have to change the loudness of your voice and your pitch to make sure that you're sufficiently screeching to keep them awake, and they can't sleep through your frequency fluctuations, and all that.

DOMUSH: Did you have any adjustment period to figure out how to give exams and things like that? I've talked with a number of people who were very excellent students as they went through undergraduate and graduate school, and they said when they first started writing their own exams to give to students, they wrote exams like the ones they thought they had taken, and everyone failed them, because they realized that their students were not uniformly like they had been.

IGLESIA: [Yes]. So you asked if there was a period of adjustment. [Yes]. I mean, it's still going on right now. I mean, I have not quite adjusted quite yet. I am sure that the exams that I gave at the beginning were the same as the one I [remember once taking], okay? There's no question [actually] about memories, because I actually pulled them out from [folders from] my Princeton days, so obviously something had changed, and at the beginning I took it quite personally. It was my inability to teach, obviously, and then as a function of time, I realized that I was suffering a lot more than they were from their poor performance in the exams, and that there was no significant benefit of sort of convincing them that they should do better, because it was only reflecting on my own personal self-esteem, [on] the fact that they could not do what I thought that I had taught them to do. So I'm still adjusting. And my exams are known as being difficult but fair, okay?

I **<T: 30 min>** always tell the students that it is better to take a difficult exam because a dumb mistake will not take you to the bottom of the curve, whereas if I give you something and everybody gets between ninety and one hundred, and you're the poor soul that makes a dumb mistake, then you're going to end up with an F in the course. So there is a benefit of having something that has a breadth. And you cannot have a breadth [in the distribution if your mean is] pegged at one hundred, right? So—but anyway, no—it was classic. But it was not my bad memory. It was my bad teaching, obviously, that was having that effect.

But that has been, for the most part, fun. I mean, there have been difficult times. There have been difficult times usually because of the impact that performance in class has on a student. There have been students who have really had personal problems and so on that have been heartbreaking to watch and all that. And I sort of feel sometimes responsible for the kind of stress that we place on them in order to demonstrate proficiency.

But other than that, I mean, I think the number of students who have flourished and those who have fallen apart, that the ratio is still one hundred to one, so I think it's something that is par for the course. It's something that comes along with the good times. But those are the difficult moments that, every couple of years we have something like that, remind me that there is a human side to what we do that is more than just molecules and equations. Although I have been known to say that one of the reasons that I left Exxon and I came here, under false pretense nonetheless, was because I find the molecules and equations to be difficult to understand, but logical and not malicious, whereas I find humans difficult to understand, seldom logical, and occasionally malicious, right? And I'm willing to work hard to understand something that is logical, but I cannot stand a lack of logic, because [logic is] that what makes us do what we do [and be the humans that we are].

Okay. Any other thing that comes to mind before I follow my quasi-script here, somehow?

So when I came here, there was a particular individual who was very kind to me, besides Gene Petersen, who gave me the office and the lab, and then became sort of the surrogate grandfather for my children, because they had this beautiful house up in the hills of Lafayette, [California], and they had a place where the kids could sleep on the floor, and my kids would go up there, and then Kathryn Petersen would take them out walking in the woods and all that. And the grandparents were not here, so they became the surrogate grandparents.

And the other one was another grandfather figure of sorts, [who] was really an old-timer by the name of Heinz Heinemann. So Heinz was one of the fathers of organized catalysis. He was at Mobil. He then retired after many years. He was a refugee from World War II and escaped Germany just in time. And when I got here, he had mounted a bit of an effort up at LBL [Lawrence Berkeley National Laboratory] funded by DOE. He was the one that gave me the membrane project that he had been working for so many years, along with equipment that was not particularly useful. But he was also the one that said [sort of figuratively], "My son, you take it from here, because I have done my part," at a time in his life when his wife was very ill, and he had dedicated himself [wholeheartedly] to taking care of her in her last years, and could not really pay [close] attention to what was going on.

And then afterwards, when his wife passed away and he had a little bit more time and he came back, he became a regular participant in our group meetings. He would come to visit, and so on, but now as the distant grandfather rather than the supervisor of the work. So he, I think, was a very nice example, both of a human being, and of somebody who gave what he had for the sake of having it survive at a moment when he could not feed it anymore, so to speak. And that was the beginning of a DOE project that continued for several years after that, which effectively took us on the one side into this project in which we were trying to convert methane to other products.[4] This is a process that is thermodynamically limited. And then we were trying to remove the hydrogen **<T: 35 min>** through a membrane in order to drive the equilibrium of the reaction. All this was at 700 degrees [Celsius] with selectivities for hydrogen [permeation] that required that we had solid state [dense] membranes. And this is an area that we continued to work over many years, and then we sort of abandoned along the way, because we didn't know what to do, and we couldn't keep a critical mass in the area of membranes. And this is a project

[4] See: Zheng Liu, Lin Li, and Enrique Iglesia, "Catalytic Pyrolysis of Methane on Mo/H-ZSM5 with Continuous Hydrogen Removal by Permeation through Dense Oxide Films," *Catalysis Letters* 82, no. 3-4 (2002): 175-180, and references therein.

that was revived about two years ago as part of a demonstration project with ARPA-E [Advanced Research Projects Agency-Energy] and this [small] company Ceramatec [Inc.].

So again, it began with Heinz Heinemann. It then went into rather labyrinthic areas, where we learned how to do X, Y, and Z, and it didn't really solve the problem. And then as I think, I hope—most good work does—there is a time and a place when it finally becomes applicable. And it seldom becomes applicable when you want it to become applicable. I sometimes, Hilary, have a mental picture of how we discover things, and that is that scientists are working in the salt mines, increasing the amount of knowledge a little bit at a time, and the interest in the particular findings are going like this [draws an oscillatory curve in space], and then you intersect the interest once in a while, and there is a particular time when the amount of knowledge and the interest are actually sufficient in order to make it happen, and then we call that a breakthrough, [with the benefit of self-serving hindsight].

But in fact, it was sort of the shoveling of salt in the salt mines as we went along coincided with the rather unpredictable fluctuation in the interest of mankind on a particular solution. It is also in my view why it seems foolish to prescribe solutions, or when they need to come into effect, or to work with milestones that do not measure the amount of knowledge [gained]. In other words, all that we can really measure is the slope of the incremental improvement in knowledge that we know is happening. We cannot control when it's going to intersect reality in any [meaningful] way. [So much for what passes as strategic planning.]

To me, I mean, just like the boulders in my path by the bureaucrats, this is the mental picture that I have. And somebody gave me a coffee mug at Exxon once. It said, "Milestones are for wimps." [laughter] I didn't publicly display it on my desk all the time because it would have made me sort of *persona non grata* in the eyes of some of the people that were religious about [such] things. But I think I only accept milestones that measure how much more I know than the day before. [Those milestones do matter.]

DOMUSH: Right.

IGLESIA: And whether I did the right experiment in order to move that in the right direction, but not a [level of catalyst] performance at [any] point in time, it's entirely artificial, and it is not very motivating I think for the people that are actually shoveling salt in the salt mines to do this.

So that's the history of the Heinemann membrane methane project. As a result of that project, I guess it was impressive enough on the part of a program manager at DOE that a couple of years later we were able to get together with Air Products [and Chemicals, Inc.] at a point when people were trying to make higher alcohols from methane by going through syngas-type chemistry, and I will tell you some of the things that came out of that. And there was an NSF project at the very beginning, an NSF project that was supported by a fairly difficult individual who was in charge of NSF. But I still think he was one of the fairest individuals around, and that was Farley Fisher, who was in charge of the [Division of Chemical, Bioengineering, Environmental, and Transport Systems]. And he didn't know anything about me, and I wasn't a very well-known entity at the time. And I wrote probably my only fifteen-page proposal I have ever written, and it was reviewed relatively well, and then he funded that for a period of three years, and then another three years.

At the end of that, I never even applied for NSF money anymore, in part because I didn't need it, but in part of it because funding agencies became driven by initiatives. They became driven by [a mode in which] every three years you have to propose something new. And that was the antithesis of my slow, incremental improvement that hits the fluctuating interest as a function of time, and I was not willing to reinvent what I considered to be important problems every three years in order to do that. And I'm also, as you'll find out from the way that we do research and how we do it, I am unable to work in very large teams. I joke that it is because if you have more than three people in the room, I'll pick a fight with one of them, okay?

DOMUSH: [laughter]

IGLESIA: But that's sort of the simplified version of this. But the other **<T: 40 min>** one is that I have a firm belief that new and creative things come out from, in many cases, introversion, from sitting in a room and literally getting confused [all] by yourself in a way that is not embarrassing in front of anybody. And going back and forth, showing yourself to be wrong, [alone] or with a small group of people, but not by brainstorming, not by getting into large initiatives, and certainly not by getting twenty people who speak a different language, a different technical language, to try to solve problems at the boundaries [among] disciplines.

The way that I solve problems at the boundaries of disciplines is I make one-to-one lifelong relationships with people at that boundary. We teach [each other] about the common language, and then I begin to invade theirs, to some extent, by learning more and more about what they do, [and they start to invade mine]. So that's something that has made me a bit of an odd man out in many of the large initiatives. I'm not a member of any EFRC, these Energy Frontiers Research Centers of DOE, or any of the NSF centers. I mean, I like small groups. I like intimate relationships with a few people. Perhaps that is the classic introvert. And that is how you get lifelong relationships with a few people rather than a large number of Facebook friends.

DOMUSH: Is that something that you try and teach to your graduate students, your group, that that's a perhaps more useful way to do science? Or do you think that it's something that you graduate students kind of pick up on through osmosis kind of that that's the way your group is run, that that's the way your research and your style of research is successful, and then they can choose as they go on to emulate that model or to not emulate that?

IGLESIA: [Yes]. I mean, that's a very good question. I have to say that there is my hope and then there is the reality, and I don't know where to draw the boundary between the two. I think that they watch how we work, and then I hope they decide by themselves whether that it something that is consistent with their psychological makeup, and I think for introverts, it will be easier to do that. For extroverts, it may not be.

I think that—again, it is the example that they watch, rather than what I tell them is the right thing to do, although I do tend to make fun sometimes of—we have these short talks at the group meeting, where we take a paper from the literature, and occasionally comes this paper with fourteen authors, for example, and it's wrong, okay, and I will make some comment that suggests that I will take wrong from two or three, but when there are fourteen, there's got to be at least one of them that picks up what's wrong with the paper, right? So they know that I don't believe that you pass things on from somebody who makes a material to someone that looks at what it is, and then somebody that figures out what will work. They know that we try to teach them how to get through the entire cycle: how do you make materials, how do you figure out what you make, how do you figure out how they work, how do you figure out why they work the way they do? And that even though we may interact with other people at the boundaries, that they [each] still have a [very personal] responsibility for moving along that cycle and learning how to interact with people with different backgrounds.

I mean, we don't have any projects in which we work with any more than one particular individual [outside our group]. And I think it shows at the end, again, you'll find very few papers that have more than three or four authors from [our] group. That's perhaps not a badge of honor of itself, but it does show that we actually tend to interact deeply and individually. [. . .] I also dislike intense complexity in interactions, whether it be human or technical, only because I'm not very good at managing the complexity in that sense. I mean, I like simple things. I like simple solutions. I like simple ways of interacting that somehow expose everybody to the best technical knowledge in the area.

And I'm also—maybe to the chagrin of some of my colleagues here—I don't view geographical distance as an impediment to an interaction. I view chemical incompatibility [of the personal type] as a barrier to interaction. In other words, I want to find the right person, wherever they are, and that right person has to be someone who I admire technically, but whose personal traits of honesty and intensity and **<T: 45 min>** all that are also commensurate with the way that we try to do conduct ourselves.

DOMUSH: How do you find some of these lifelong collaborators and people that you're going to kind of work with intensely? You know, are they people that you have met through conferences? People that you know from your time at Exxon? How do some of those relationships start?

IGLESIA: [Yes]. I mean, I think some of them—[each one] of them has started in a different way, but usually, it's easy to figure out why that relationship happened retrospectively. [. . .]

Also, it doesn't include many interactions that started but never went anywhere because there was something along the way that said, "This is not the right interaction." [. . .] It's not quite a random walk, but you are doing a [sort of] random walk, and whenever you find a point of attraction, then you spend a little bit of time there, and then if that ends up being a real attraction point, okay, then you begin to build some relationships at the level of a project, and then it becomes a relationship—[for life].

DOMUSH: Right.

IGLESIA: I've been fortunate enough to have many of the kind that lasted for a while, but some of them that have been sort of lifelong. Some from Exxon, some from after I left Exxon, and so on, but I don't think that we consciously go out [and seek such relationships]. We usually have a problem that we don't know how to solve. One of the things that was very useful to me when we get there [in our discussions] is that at a very young age, I became very knowledgeable about the community by becoming the editor-in-chief of *Journal of Catalysis*. I might as well get that one out of the way, since I may forget later on.

DOMUSH: Sure.

IGLESIA: In 1997, at I guess the age of forty-two—out of the blue, and without ever being as much as an editorial [board] member of the *Journal of Catalysis*—I was asked to become the editor-in-chief, along with Roel Prins, who was already an editor-in-chief. There were two of us. I have no idea, except I suspect that they needed somebody that rejected all their papers, because I was known for writing very detailed reviews for the papers that I got [to review], and for being probably unduly negative—[or at least cranky enough]—about most of them. [laughter]

So be careful of what you do, because it may come back to haunt you. In this case, it came back to haunt me in a fairly unexpected way, okay? And being the editor-in-chief of *Journal of Catalysis* is almost like an [optical fiber] that [allows you to peek] into the community, and you get to look into all the nooks and crannies, and you get to see people in action behind the scenes. You find out who holds a grudge against whom, who is fair and who's not fair, who is honest and who is not so honest, and you get to see the best and the worst of human nature, okay, from a fairly confidential standpoint, but in a way that you learn the community better than you could have ever learned it from the outside. Thoughts and feelings that became my parting words as I moved on in 2010 are in, "A Farewell (of Sorts)".[5]

There is a big price to pay for doing that. One of them is that it is a lot of work if you do it seriously, that it is a lot of work if you read every paper that you end up rejecting and

[5] Enrique. Iglesia, "A Farewell (of Sorts)," *Journal of Catalysis* 269 (2010): 254.

accepting, as I did. And it is also not something that people do early enough in life to be able to benefit from the knowledge that you gain [for many years afterwards]. But also when you do it at an early age, you retire from that job with the need for friends and [with] a large number of enemies. [Yes]? [laughter]

DOMUSH: [Yes].

IGLESIA: So if you do it at the end of your career, then it makes no difference at the end, because you don't need friends or enemies at that point. But to do it at forty-two and then to have the nerve to retire at fifty-five, as I did, leaving behind ten, fifteen years of exposure at that point was probably a bit suicidal, okay? But nonetheless, it was the right thing to do. I wasn't going to hang around forever.

But that is the point where I learned who were the people who were solid, because of the papers they wrote, and even more telling, from the reviews that they wrote on other people's work. So I don't know the very young people that are coming in because I haven't done that for three years. But in fact, at that point, I had a very clear picture of the community. I have to say that towards the end I had a somewhat cynical picture of the community also, but that came from the accumulated burden **<T: 50 min>** of every one of the diatribes and arguments and fights and ethical breaches that I had to handle, and so on.

But that's one of the places that I went to try to find out who the right people were. Also, for better or worse, I have never been attracted to individuals in politically important positions. I have treated talent much more highly than reputation, and as a result of it, as I selected my friends and colleagues along the way, there is a level of fairness in the system that said that because they were good, they became politically well-situated later on as a result of their merits. But we became friends before they were there, and they became my friends before I was there. So there was never anything [except] what my wife [has called] the "mutual admiration society". It is a group of us that supports each other both in the work that we do and in the way we highlight the achievements of the others. We do so in a very reciprocal manner because we really think that we're good, and not because of anything that we [do or could] do for each other. That to me is [important]. . . . Some people begrudge the fact that there is this mutual admiration society, but it certainly excludes many politically well-situated people, because we do not respect what they do [or how they do it].

DOMUSH: Right.

IGLESIA: Anyway, that's a bit of a detour, but that takes care of all the marks [and scars] that I carry from those years as editor-in-chief of the journal. I will never edit a journal again. [laughter] I was asked to edit the fiftieth anniversary [issue of the *Journal of Catalysis*], as a guest editor, and that was definitely my swan song. I will never do this again.

So we're now at Berkeley, 1993. We have some funding to do research from NSF and DOE. We're working in the area of methane activation and driving the thermodynamics of that chemistry [to its useful limits]. The NSF project actually started us in the area of oxide nanostructures, and how the size of a domain of a metal oxide changes its catalytic properties. That required that we learn for the first time how to do UV-visible spectroscopy, because we needed to know about the electronic structure of these domains. These domains were too small to be seen by microscopy, and as a result of it, we had no idea [about] what size they were. And these domains actually had electronic properties that changed with size in the same way as quantum dots [in electronic materials] change with size, but in a way where the larger you were, the smaller the HOMO-LUMO [highest occupied molecular orbital, lowest unoccupied molecular orbital] gap was and the easier it was to reduce [these oxides]. Therefore, since the oxidation chemistry is a redox cycle and it is limited by the reduction part of the cycle, the smaller the HOMO-LUMO gap, the lower the energy to which you could put the new electron as part of the redox cycle and the faster the chemistry went on.[6]

And out of that came a series of quantitative correlations that began with acid chemistry, then they moved to [the extension of these concepts to] oxidative dehydrogenation, [including] work that we did [later] with Alex [Alexis T.] Bell, my colleague here, and which actually allowed us to develop a spectroscopic technique that for the first time was able to count the number of sites that were actually undergoing redox chemistry.[7] Nobody had ever been able to measure the number of sites in an oxide because there were many [possible structures] and not all of them were working [catalytically]. But this was, actually . . . so here's my Exxon training on *in situ* spectroscopy, the recognition that spectators were not reactive intermediates and the need to do a transient experiment to be able to [learn this]. This was also the place where for the first time, we began to . . . . At Exxon, we used to unravel mechanisms to the extent that it was needed in order to solve a problem. So this was also the first time when I got to unravel the mechanism beyond the need to solve any problem—in other words, just because we wanted to know everything, [we wanted to know how nature worked and why].

And in heterogeneous catalysis, there is seldom either the tenacity or the tools in order to be able to do that, and for this oxidative dehydrogenation chemistry, we actually took it all the way to [the fact that] oxygen activation doesn't matter, but we still wanted to measure the rate constant of oxygen activation and figure out how it [happens]. To this day, we're still trying to figure out why it works that way, okay, but at least we figured how it worked. And it was only through the advent of theory, of density functional theory [specifically], much later on, that we [have] begun to be able to answer the question as to why it works that way or why the oxygen chooses to be activated in that manner.

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[6] David G. Barton, Max Shtein, Ryan D. Wilson, Stuart L. Soled, and Enrique Iglesia, "Structure and Electronic Properties of Solid Acids Based on Tungsten Oxide Nanostructures," *The Journal of Physical Chemistry B* 103, no. 4 (1999): 630-640.

[7] Morris D. Argyle, Kaidong Chen, Carlo Resini, Catherine Krebs, Alexis T. Bell, and Enrique Iglesia, "Extent of Reduction of Vanadium Oxides during Catalytic Oxidation of Alkanes Measured by in-situ UV-visible Spectroscopy," *The Journal of Physical Chemistry B* 108, no. 7 (2004): 2345-2353.

But here were the coattails of the **<T: 55 min>** Exxon *in situ* spectroscopy, the interesting [isotopically-marked] molecules, and a kind of [catalytic] system that lent itself to that kind of analysis. [. . .] I think we're academics, and the angels on the head of a pin sort of come together in some happy marriage that says that some things you want to know just because they're beautiful, and some things you want to know because they are practical, and one never knows . . . . What you don't think is practical today will become practical, so you might as well archive that knowledge. So the next time the cycle comes around, you know a little bit more about things that you thought were useless. For the first six years that I was here, I didn't have any industrial funding, and there's probably a story about that at the end. Part of the story is that this was a period of time when companies were downsizing their laboratories, and they began to look at academia as a way of subcontracting what they should be doing.

To me, that was philosophically wrong because academia was turning into development-type laboratories. The students were being used as technicians in order to do work that really did not have much of a fundamental content, [work that did not teach]. And the questions that were being asked by industry, by the many people that knocked on my door at that point, came always with a statement that said, "We have a problem." And in fact, they couldn't even articulate the problem in anything that went beyond the phenomenology of the problem. The catalyst deactivates, or the selectivity is too long. Once you got below the surface and you began to ask, well, do you know anything about how it may be happening, the answer was either a hallucination of some sort or it was a [more] honest, "[We] don't know".

[. . .] I would show them out the door in a very nice way by saying, "I'm sorry but we are academics, and we're so inefficient at doing these things that we will never be able to solve your problem in time for you to benefit from the solution to the problem," and that pretty much made them go away. It left us without any industrial funding, but at least I was not throwing them out of my office with anything but an honest assessment of the fact that this is not what we do [or could do]. This is not what we can help you with. You need to go back and you need to pose it in terms of a set of fundamental questions that [we] can help you with at that point. You have to do your homework.

Many people did not come back, but some people began to come back, and I think they took my honesty for what it was, that was, an attempt to not take their money under false pretense and then not to deliver what it is that they needed [or thought they needed].

The first company that came back after being sort of shoved aside a couple of times, I think, was BP [British Petroleum Company, subsequently BP Amoco, PLC and this was probably 1998 or 1999. Those are the six years in which I was at the whim of my startup funds, NSF, and DOE. And now BP came back with a proposal for a major program. This major program was called the Methane Conversion Cooperative. It was an attempt at putting together several faculty members from Berkeley and Caltech. The number [of faculty members involved] was small. The number was four or five so it's actually manageable. I managed to stay friends with most of them, I think. But that program lasted for about ten years.

And it was not a small project. I mean, these were fairly large amounts of money, of over one million dollars for each one of the two [institutions], of which a significant part of the Berkeley component came to [our group], and this was the umbrella under which we did a large amount of work in methane conversion under various rubrics. So it began as methane conversion. It became sort of C_1 chemistry, which in our context it means any molecule that doesn't have a carbon-carbon bond. This is the place where Junmei Wei did all of her work on steam [methane] reforming and showed that you could very simply explain everything that people were confused about by a simple mechanism, that if you worried about thermodynamics and transport, the rest of it was pretty easy chemistry, but if you didn't peel the layers of thermodynamics and transport, it became very complicated, because people forgot that those were there, and attempted to describe everything in terms of [chemistry and] kinetics.[8]

This was also the time when **<T: 60 min>** we showed that . . . . So one of the things that companies have been grateful to me about is when I tell them, "That cannot be done." So you would imagine that a company comes in and says, "I want you to make gold out of lead," okay, and that they would be very unhappy if I told them that you couldn't make gold out of lead, right? But in fact, actually, companies would like to know the things that they should not be trying desperately to continue to hope for with some religious belief, when in fact it cannot be done. [They know, if you are rigorous and convincing, that others will not be able to do it and they can sleep better at night.]

There is a particular kind of chemistry in syngas [conversion] that says that we can do something called catalytic partial oxidation. That means that you can convince a methane molecule to convert directly to hydrogen and CO in the presence of oxygen without forming CO_2 and water. And the fact is that you cannot do that, because the kinetics of oxidizing hydrogen and CO are orders of magnitude faster, but you can always go all the way to CO_2 and water and then come back and react with the methane and give you something that stoichiometrically looks like partial oxidation. But the exothermic reaction was here and the endothermic reaction was here, and you haven't coupled [them at the molecular scale], and therefore, you have not achieved the [intended] benefit of catalytic partial oxidation.

We demonstrated that by a combination of theory and experiment . . . and I think that bought us the credibility that they would look at us and say, "You know, these people can tell us when it can't be done. I mean, they're not going to continue to sort of string us along to do something that does not make any sense to do."

DOMUSH: Right. [. . .] Who were some of the other professors, the other faculty members that were receiving some of those BP funds for the Methane Conversion Cooperative?

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[8] Junmei Wei and Enrique Iglesia, "Mechanism and Site Requirements for Activation and Chemical Conversion of Methane on Supported Pt Clusters and Turnover Rate Comparisons among Noble Metals," *The Journal of Physical Chemistry B* 108, no. 13 (2004): 4094-4103.

IGLESIA: So for the first cycle, it was John [E.] Bercaw and Jay [A.] Labinger at Caltech. It was Alex Bell here, and for a time Arup Chakraborty, who then left for MIT. And there was somebody else at Caltech, Jonas [C.] Peters, I think, at the beginning. But these were the early days. The person that was most instrumental in putting this together was at the very beginning an individual by the name of Graham Butler, who had this idea that BP was going to reconstruct their research infrastructure, not just by subcontracting the university, but by using the interactions in order to justify the rebuilding of the infrastructure, and another individual by the name of Theo [H.] Fleisch, who ended up managing the project for the first ten years, who was a champion of methane conversion from the Amoco days before the merger with BP, and who then became a champion for the use of dimethyl ether as a C_1 molecule [. . .] as both a replacement for diesel fuel, but also as a way of producing energy in remote locations, as a way of making chemicals going through dimethyl ether, and so on.

That was also the project that led us to work in an area that has ended up being, I think, one of the most unexpected findings that we had throughout our career here. But actually, I take pride in it, not because it was unexpected. By the time we got it to work, it was already expected, so it was okay. But the fact is that for many, many years, people had tried to use CO as a nucleophile on an acid site [to form carbon-carbon bonds], okay? So we know that CO is a poor nucleophile, but we know that we can do carbonylation on organometallic complexes and all that. You can do it by sort of activating the methanol with iodide. I mean, it's very messy [but profitable] commercial chemistry that is used today for making acetic acid and methyl acetate out of methanol and CO.

But the dream had been, because it had been shown that with very, very strong acids, you could actually carbonylate methanol on an acid, that we were trying to do it on acids, and everybody said, "Well, it should be doable. It should be doable." And it doesn't work. And there were [tens] of papers on it. And we sat around, and we actually understood why it didn't work. It didn't work because if you put methanol in the presence of an acid catalyst at the temperature that normally solid acids work, the first thing that it does is it dehydrates. It forms dimethyl ether and water. The water then interacts with the proton and effectively solvates it, and if you solvate a proton, you get a hydronium ion. If you get a hydronium ion, it's not a very strong acid.

So what people did at that time was they increased the temperature in order to make that poor acid work better and better, okay? And then what happened was not only did they finish dehydrating the methanol and made more **<T: 65 min>** water, but they began to make hydrocarbons and more water, and it was a mess. The catalyst deactivated in a manner of hours. They made hydrocarbons. They didn't make methyl acetate or acetic acid.

Well, so if water is the problem, what do you do? Don't start with. Start with dimethyl ether, right? But if you start with dimethyl ether, you don't make acetic acid. You make methyl acetate. Well, so what, okay? [laughter] All right? So it turns out methyl acetate is better than acetic acid because you [can make other things from it and] can always hydrolyze it to acetic acid very selectively. You can do this [carbonylation] now at less than 200 degrees [Celsius] on a zeolite. You can convince CO to be a nucleophile, but you can only do it if you give it an enzyme-like pocket where the transition state for the CO addition, which is really a nucleophilic attack, it's a [backside attack], actually is comfortable. So that solved the problem, and some people may actually do something with it—[the same people that said we had to make it work with methanol]. I'll leave it at that. But first of all, we solved the problem by learning why it didn't work [with a certain approach in the hands of others].

DOMUSH: Right.

IGLESIA: And that fit my idea that I need to understand things before I discover them. But actually, it took us a long period of time, into now looking at how unique confinement actually causes some reactions that take place. If you take a zeolite that has large channels, nothing happens. If you put pockets that are almost enzyme-like on the side, you can actually get the right transition state to fit there, and all of a sudden, things that began as a very practical way of carbonylating methanol to methyl acetate actually became a how do you separate the effects of confinement from the effects of acid strength in zeolites?

So there was a whole avenue of research that now left that MC^2 [Methane Conversion Cooperative] program and actually became part of the program that I will talk about in a moment with Chevron, that was based actually on how do you exploit zeolites in order to do certain types of chemistries?

These are the unexpected debris that I like to think we're very good at catching, and that is that we solve one problem, and what we learn all of a sudden opens up a whole new way of looking at things that then we can follow up.

DOMUSH: Right.

IGLESIA: And because we never get attached to any one chemistry, because again, we have this strategy that says that you can understand everything by making materials that have well-designed or well-defined structures by characterizing that structure at the level of atomic connectivity, by looking at the mechanisms and what steps actually limit the rate and limit the selectivity, and by then doing the right analysis—whether it be at the chemical reaction engineering or the molecular simulation—because that method to our madness does not change from problem to problem.

We're not restricted to work on any one molecule, as long as the molecule is small enough that we can use the typical analytical techniques. And if you walk into my laboratory, you will get bored after a short period of time, because what you will see is that things repeat themselves. So there are a couple of spectrometers. They have the same cell inside, because they're supposed to do the same thing. Then there are a couple of other spectrometers. There is a bunch of reactor units that are supposed to be able to work with any molecule at any one time.

There are analytical devices that are set up as gas chromatography that can do any of the separations, right? And therefore, it's almost like you can flip things around and do almost anything with the same structure, and you can teach the students to teach each other how to use, you know, certain pieces of equipment. And you can buy spare parts that are actually useful for more than one [thing].

So this is one of those things that I learned at Exxon, and that is that you set up a structure in a way that it functions almost irrespective of the people that are using the structure. Now that's an exaggeration, but that is the way that you run armies, right? You don't want people to be very creative. You want to give them the right armament and then you have to roughly tell them shoot that way and then they do it. That's not quite a proper analogy, but it was pretty close to the way that a well-run corporation should work. It is not by any means the way that a creative research group works, and clearly, what we do is significantly influenced by the talents of the people that are sitting around the equipment, but it doesn't hurt to give them what they need in order to shoot in the right direction and with the right armament. And that's what we tried to do in the group, and why we can take the debris that comes out of one project and very quickly incorporate it. [. . .]

DOMUSH: Now when you mentioned some of that instrumentation a minute ago I talk with some people **<T: 70 min>** where it's like every couple of years, they're working with a brand new type of instrument or a new generation of whatever type of instrument that they commonly use. Are you using pretty much the same types of instrumentation that you were say five years ago or ten years ago, or has maybe the types of instrumentation increased? Have you added new instruments into your lab to give you some new analytical results?

IGLESIA: So I'll try to be thoughtful in answering the question, okay?

DOMUSH: Sure.

IGLESIA: So first of all, we're tool users, not tool makers. Okay? Secondly, we think that there are too many problems whose solutions can be attained using the tools that already exist. Now I'm going to take an extreme position in a moment, and then I'll tell you how there are exceptions to every rule. But I think if I were to look out there at the advances in instrumentation, the advances in computation, the main challenge that we often have is to tie a solution that already exists to a problem that is not clearly connected intellectually to that solution. In other words, that the niche for us is in using our brains rather than our equipment in order to connect the two, and that by taking equipment that is not ancient, but it is only incrementally improved in the last five years, and by continuing to acquire the incremental improvements in some reasonable way, but most importantly, by doing the experimentation under the right conditions, by not sacrificing the reliability of the data, by looking at real catalysts rather than model catalysts, by looking at relevant conditions, and by building cells that can actually do transients at the right conditions, you can actually solve the problem without having to create a glamorous new tool in order to be able to do it.

I have to say, however, that the glamor associated with a new tool is hard to resist sometimes, right? And then you have many, many tools looking for the right problem out there, but until they do . . . you [cannot] get a lot of mileage out of the latest way of looking at the latest thing.

Call me old-fashioned, but when I got into the isotopic studies of catalysis, nobody was doing it because they had been blinded by the new spectroscopies that were being applied under irrelevant conditions on model materials and so on, and yet the isotopic studies were not glamorous, and yet they could actually tell you what was going on. I mean, if you shine a photon on a surface, okay, everything that is on the surface will tell you that it interacted with that photon, right, without telling you anything about reactivity. When you put an isotope there and it appears in your product, you know that that isotope actually went to the active site, reacted, and what it's telling you may be difficult to interpret, but it's generally involved in that question that you tried to answer, right?

So it's easy to look at everything that you can see. It's difficult to actually see among those things that which is really relevant, especially if you don't look at realistic materials under realistic conditions. So you're not going to find too many lasers in my lab. There are some. The infrareds have a laser. You're going to find us going to the Synchotron at Stanford instead of going to the APS [Advanced Photon Source] in Argonne [National Laboratory], which are the state of the art, because a lot of what one can learn from monochromatic high intensity x-rays doesn't require that you have a super intense beam. It requires that you have access to the beamline and that you can actually sit there and do the right experiment. And that's I think where we wedged ourselves, okay, and which may take away some of the visibility from what we do, but this is how we have solved problem after problem after problem—sort of quietly, or at least more quietly than most other people [solve fewer smaller problems], and by thinking very hard and doing a few experiments.

I mean, probably the most touching compliment that I have received when I give a talk out there is not how great the work is, but how hard we [as a group] think. And this is not a comment that has come from a single point. It is that we spend most of the time doing the thinking, and a little bit of time doing the right experiment in order to answer the question. And I tell my students at the very beginning, I say, "You know, humans would rather do than think. **<T: 75 min>** Okay? Given our own choices we will not sit and try to solve a problem by thinking really hard about it. We will think about it lightly, because it's painful to do otherwise, and then we will turn a few knobs and pretend that we're actually doing the right experiment. And you should not do the experiment until you have gone through the pain of actually thinking." [A recent study appears to confirm our reluctance to be alone with our thoughts.][9]

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[9] Timothy D.Wilson, David A. Reinhard, Erin C. Westgate, Daniel T. Gilbert, Nicole Ellerbeck, Cheryl Hahn, Casey L. Brown, and Adi Shaked, "Just Think: The Challenges of the Disengaged Mind." *Science* 345, no. 6192 (2014): 75-77.

And then the same thing happens at the back end. You know, yesterday, we talked about how to write manuscripts. So most of the really hard part about writing manuscripts is the thinking part, the planning part. Most people just sit there and write stream of consciousness, whatever comes to mind instead of sitting around and doing the thinking [ahead], because writing freelance, creative writing, as we talked about yesterday, is actually more fun than sitting there and trying to figure out what it is that you want to say and what is the best way to say it.

So these are some of the philosophical comments along the way that I have been able to distill in some way from the way that I like to do things. And I hope that it doesn't come across as just because I like it, it's the right thing to do. I like it because I [feel that] it's the right thing to do. I'm not interested in doing things that are not productive, okay, and I take a great deal of pride and pleasure at the end from being efficient in the way that we do things. It's actually less expensive to think than to do, too, so I have found that those times when we've had the least [funding] to do research, which [even then has not been] a very small amount, but less than usual, we have actually done some of our most creative work, and that's because we haven't had [the extra funds] to do worthless experiments for the sake of doing experiments.

DOMUSH: Right.

IGLESIA: There used to be a saying at Exxon that you should never build a pilot plant unless you really need one, because once you build it, you have to feed the beast. You have to give it things to do, and in most cases, you really don't have time to sit back and say, "This is work I don't want to do. Let's shut down the pilot plant and let's not do any experiments [for now]." And I think we treat some of our laboratory equipment the same way. It's there. If we're not using it, it's wasted money. If we're doing mindless experiments, it's not wasted money. That's I think not a very good way to . . . . Okay.

DOMUSH: I think before I took us off on a different course, you were saying something about one of the other places you got industrial funding was from Chevron.

IGLESIA: Right. [Yes]. So I was going to get that in a moment, okay?

DOMUSH: Oh, sure.

IGLESIA: Because I wanted to talk a little bit about the coattails of the MC^2 program. Again, ten years. At the end of the ten years, then we had to reinvent the program a little bit, remarkably similar to the previous one, but . . . . So MC^2 was Methane Conversion Cooperative. So I gave it the name XC^2, where X means anything, okay? [laughter]

So this was quite broad at this point. At this point, the program became smaller. There were fewer people involved. That meant that we got more money, because the [total funding] was the same, and there were fewer people [with whom] to share it. And there, I think we began to look at things that were no longer C_1 chemistry. We had already shown that we could understand the syngas conversion and [production]. We got back into Fischer-Tropsch at the time with much less practical and more mechanistic-type insights. We learned how to do C_1 chemistry with methanol and practically, what we were able to do was we were able to make a molecule that has a very interesting backbone. It's called triptane. It's a high octane component of gasoline. And everybody got really excited about the fact that you could do this with the zeolites, and we got really excited, because in fact the only reason that we made that molecule was not because we were [particularly] clever at designing a catalyst to do it. It's because acid chemistry rules forced a C_1 to actually make that molecule, and all we had done was we had shut down all the side reactions that would otherwise not get you there. So this became a telescope [and a microscope] into acid chemistry on a solid that then made it worthwhile for us to try to understand now experimentally and theoretically exactly what carbenium ion chemistry looked like—[the intellectual debris].

Now the expectations on the part of everybody were, "Look, you've made triptane with high selectivity and yields." My job was to tone down the expectations, and say, "We're never going to be able to do this in practice. Look, I mean, you only have twenty-five percent yields. You're wasting a lot of carbon. Okay? What we really have is we have a window of opportunity in looking at what makes acid **<T: 80 min>** chemistry do what it does." So watch."

And to their credit, they allowed us to sort of wander into la-la-land, I guess is not a bad way to put it. And now this was actually our entry into theoretical calculations, and how to do isotopic experiments to find out exactly where in the backbone you put a C_1. I mean, in my wildest dreams, I would have never thought that a company would actually give me money to do that. I think I would have had a hard time going to DOE and NSF [for funding]. They would have wanted me to do something more applied [and relevant than that and to do it with ten other groups]. But I think that by now, they knew that once in a while that [we] actually did the right thing and something unexpected happened.

In the DME [dimethyl ether] carbonylation, we kept striking out with methanol. They kept saying, "You have to do it with methanol. You have to do it with methanol." When we put the DME chemistry in front of them and we showed them how they could turn this into something that made sense from the practical standpoint, [how to integrate the whole process, and] they were so grateful that I didn't do what they told me to do. And actually, it is true, from the very beginning [our BP colleagues] said, "If we knew what we should do, we would do it ourselves, because we don't know what we should do. You should not listen to us all the time. You should listen to us some of the time, to make sure that we have some delusions of influence in what you do. But at the end of it, do some things that we tell you not to do, and good things may happen." [Not verbatim, but a good paraphrase.]

So in the XC^2 program—this is the second part of the BP program [. . .] we took some of the intellectual debris from the early work that we had done on oxygenates, when we were making hydrogen into higher alcohols, and we realized that we had learned a lot about aldol condensation chemistry, and that we didn't have any idea what to do with it, because it worked so poorly under CO hydrogenation conditions that there was no way that we were every going to make it do anything useful.

All of a sudden, you roll the movie forward, and here we are in 2008, and everybody's going [nutty] about biofuels, and they want to convert oxygenates, and they are reinventing aldol condensation chemistry, and we had done all the work on the aldol condensation chemistry. And we went back and looked at materials that actually were unexpected catalysts for alcohol condensation and esterification. And all of a sudden, again, this picture of [mine that] you keep learning a little bit each year, and then occasionally it intersects reality [came clear once again].

DOMUSH: Right.

IGLESIA: Actually, we were there when reality actually struck. And they were very—I guess they don't get [very] happy—but they were [very] interested in the fact that here out of nowhere came chemistry that we already knew about many years ago in the context of something not very practical, and all of a sudden reality and knowledge crossed again, okay, and that I think became one of the high points of the project.

The project [is coming] to the end of the five years. By all indications, it will be renewed for another five years, [but who knows]. There will be different people involved, but [I am sure that] we will still be involved in the project. And I have been asked whether I can come up with another interesting name that is not XC^2. So I'll see what I can do to be able to do that.

Now somewhere along the way I had sold [several] of my graduate students and postdocs to Chevron for many years.[10] [laughter] So half in jest. I mean, the reason is that they're twenty minutes from here, and as you well know, having been around here for a while, people like to live around here. So whenever it is that . . . . Remarkably, the ones from the Midwest want to go back to the Midwest. Don't ask me why. Humans are not logical. But those want to go back. But everybody else seems to want to stay here. And there are not too many places [to practice catalysis] around here. And some of the early students in the group, some of the postdocs early on, ended up going there, and apparently creating a very good impression, that they became sort of a vacuum pump, and every time that we had somebody that was interested in industry and didn't want to go back to the Midwest they would just take them. So there was a point in time when there were eight of our students there, postdocs and graduate students.

[10] Many of Dr. Iglesia's graduate students have also gone on to positions in academia.

DOMUSH: Wow.

IGLESIA: And I panicked at that point, because I said, “You guys will cut back one of these days, I’m going to be out there selling postdocs and graduate students for a very low price.” I mean, this is not my idea of diversification, okay? And I said, “Besides that, you need to contribute something.” At that point, they had not funded any of my research. So I **<T: 85 min>** said [jokingly without seeming so], “You have to give me at least fifty thousand dollars for each one of them, on a yearly basis for a few years now, in order to pay back your debts.” So I was joking about that, but at that point, we were already talking about zeolite chemistry and what we could do [together].

So I guess it’s now eight years ago, so I guess that would be 2005 or so. This was not too far removed from some of the work that we had done with BP in the area of carbonylation, you know, what we were learning about the zeolites, and we became interested again in how confinement is able to influence the direction of reactions. I mean, this is something that throughout time we had qualitatively understood as shape selectivity. Some reactants get in, some products get out, the other ones don’t, but no one had really been able to articulate in an experimental or theoretical sense the concept of transition-state selectivity that is so ubiquitous in the case of enzymes, where it’s also hand waving to some extent, right? And part of the reason is that it’s very difficult to distinguish [transition-state selectivity] from reactant and product selectivity, and part of it is that theory, which is the ultimate arbiter of what it is, how things or why things happen, was never able to capture [accurately] the van der Waals interactions that actually stabilized those transition states.

So that all of a sudden became a challenge, in that we needed theory to be able to do that, and we were not toolmakers, so we had to wait for other people to do that. The part of our group that has to do with molecular simulations, [started through] by interactions with Matt [Matthew] Neurock at the University of Virginia [now at University of Minnesota]. He’s one of the members of the mutual admiration [society]. He is also the one who has taught our group how to do molecular simulations, and at least in part [we work with him] because he shares some of our ways of looking at problems, and that is that he’s also not a toolmaker. He’s a [very good] tool user. And he’s someone who knows how to bring a tool that with some tweaking or some approximations is able to solve a real problem without sacrificing reality for the sake of pretending to answer a question.

So whether it be small clusters of metals and oxides, and high coverages, which, again, theory was not doing until [we started] to do it, or whether it be beginning to bring in van der Waals interactions in order to analyze this, these things came together. So we’re looking for things as they develop, okay, and we’re bringing them in order to solve a problem that somehow at that point in time has intersected some interest in our part. In this case, it was zeolites, because you had the prototypical containers that have molecular dimensions. Their [behaviors are driven] by van der Waals interactions. And we knew from the carbonylation chemistry how different a different pocket could be.

Now Chevron is a company who has probably pioneered—along with Mobil [Oil Company] and Union Carbide [Corporation], when [the latter] existed—the development of new microporous materials. They are a company who, as most of us today, has more materials than they know what to do with. Now this is a creative tension between the people that make the materials and the people that use the materials. [. . .] I always say, "There are too many materials out there. I just don't know what to do with them, what to use them for." And they said, "Well, that's because we haven't made the right material yet. When we do, you will be able to know how to use it." [The latter is the voice of one of our valued collaborators Stacey I. Zones at Chevron.]

DOMUSH: Right.

IGLESIA: So part of this creative tension to some extent was what catalyzed the project with Chevron. Again, it began as a five-year program. It has been extended by another three years. It is quite substantial. It allows us to fund [about] four graduate students in my group to have critical mass in certain areas. And there are three areas that we agree were the right thing to do. One of them was this idea of confinement and acid strength, and how it changes the rate and selectivity of catalytic reactions, transition-state selectivity in a modern sense, but now with the support of experiments and theory, and also, most importantly, the support of a large diversity of structures that Chevron has and are able to provide. So the synergy here is that they have the structures that would allow us to confirm or falsify our hypothesis. We have the experimental tools and the characterization tools to find out where the sites are in the structures, and how they work mechanistically. And we have the interactions with others that allow us to learn how to do simulations at the state-of-the-art in order to be able to decipher what is going on.

DOMUSH: Now how does something work, when you have funding **<T: 90 min>** from industry, from BP or from Chevron, and in the case of Chevron, you just mentioned Chevron is giving you guys the materials to work with.

IGLESIA: Or teaching us how to make it.

DOMUSH: Okay, or teaching you how to make them.

IGLESIA: Right.

DOMUSH: How does that work in terms of you publishing that material in a journal or bringing it to a conference? It is material that ultimately you need to publish, your students need to publish? Yet I would imagine or I would guess that some of that is proprietary information from Chevron's point of view.

IGLESIA: Right. [Yes]. I mean, it's a very good point, and it is at least in part some of the things that led the early contacts from companies perhaps to go away. Chevron will not [go away], nor BP or ExxonMobil [ExxonMobil Corporation]—we've had a lot of support from ExxonMobil that I haven't talked about yet— [but they also do not] give us confidential information.

DOMUSH: Okay.

IGLESIA: Okay? They will not give us confidential materials. They will give us materials that we could have made from the patent literature, for example, in some way. So the exchange is at a level where we're not contaminated by things that then we are not able [later] to talk about. We will not take any materials whose provenance and recipes we cannot report, because [the readers of our articles must be able to] reproduce our experiments. We will not ever keep anything from the public at large. In other words, by the rules of the State of California and of the land [on which] we sit here, okay, any public infrastructure cannot be used to support any research that will not ultimately see the light of day.

The other thing that helps is the fact that we [have enticed] the companies that we interact with into posing the questions fundamentally. It is a hypothesis [that we seek to test]. The answer as to whether that is a correct or an incorrect hypothesis may have some practical applications. It may be some practical applications that the company will be able to interpret even outside of the [funded] program. In other words, the knowledge can be used within the company in order to make their own discoveries. And because our main interest . . . and again, because I have a firm belief that universities have a clear mission to develop knowledge and not technology, that also puts me at the outskirts of civilization at this point in time. I think that we answer fundamental questions that ultimately will lead to technology outside of the university environment by those who can invest the large amounts of money required to [make this happen]. Again, we don't make widgets. We develop large-scale technologies that others can use.

The contracts generally involve a cooling off period of about ninety days—that's by law—within which the company decides whether they would like for us to patent the invention. The inventions can be patented jointly, if they have been [typically] joint in inventorship. And because we work so closely with those companies, more than half of all of our patents generally are joint patents with the company. The lawyers then decide how they're going to split everything. At that point, I will pay large amounts of money not to have to deal with the lawyers, okay, and to stay away from it, and to go back into my lab, and continue to mix chemicals together, to make the next discovery.

But what I would say is that never in my career here has there ever been any conflict that has been associated with any sort of confidential breach or any irregularities [of any type]. I mean, people sometimes can't believe that, but it's all about building relationships, and not [about just] getting contracts with a company. It is about working with only a few companies whose honesty and talent you respect, and do that with a sufficient critical mass that you can grab a large part of [our] brains, and they can grab a large part of our [collective] brains.

DOMUSH: Right.

IGLESIA: So when we're talking about things of order half a million dollars a year for many years this is all about relationships and [about] how knowledge is used in certain contexts, but was originated elsewhere. It is not about who makes the most money at the end, in my view. At least, that's not what drives the interactions that we have. And I also have to say that there's no incident in my career where a company [with which we interact] has tried to take advantage of a university or vice versa, [at least in my presence].

So I don't know if we're fortunate, or whether people reciprocate the way that we treat them, but this has been [the result] in my view of that waiting for the right interactions to appear, rather than taking the first money that came [our] way from industry; this was critical in both doing the right thing, creating a reputation for being honest in what we can and we cannot deliver, and then it acted as a significant attractor for other companies. I mean, we have been funded **<T: 95 min>** by GM [General Motors] and Ford [Motor Company] in the area of automotive exhaust. ExxonMobil, for which I am already a known entity, has supported our research at a smaller level, but throughout many, many years in different types of projects.

So the Department of Energy, I don't want to leave them out of it, okay?

DOMUSH: Right.

IGLESIA: I mean, they have been a significant supporter, both through the [Lawrence Berkeley] Labs here, through the Pacific Northwest [National Laboratory], with which we work, and most recently, through this ARPA-E project for the demonstration of the membrane technology. But all of them are substantial, okay? None of them have very onerous administration of any sort. They don't involve meetings that take place every few weeks. I mean, they avoid some of the complexities of twenty people working together in an EFRC or something like that. And I think it allows people to concentrate on what matters, which to me is a few close relationships with certain funding agencies and companies, a few close and intimate relationships with a few people with whom we interact, and a group that feels like [a group and like a family] . . . . The other thing that some people find remarkable is [that] we get money from BP, we get money from Chevron, we get money from ExxonMobil. Toyota has now a project with us here. Everybody in our groups works together. They talk to each other about what they do. They go to group meetings, and there are no secrets on the part of anybody [within our group]. There are certain rules, and that is that you cannot use a material that Chevron gave us for a BP project, [for example].

But beyond that, what makes us more efficient than anybody else is the fact that we don't have firewalls within the group. And if you're being paid by BP, you're still entitled to teach anybody who's working for Chevron everything that you know about zeolites, or about this [or that type of] chemistry. That's your job as a citizen of the group, right?

And yet there has never been any question along the way when we file patents on anything as to who created the knowledge and where did it come from. I mean, the fundamentals are entirely miscible within the group. The actual application is something that sort of all of a sudden comes out in a particular [area of knowledge], okay? But imagine trying to do that by taking people from BP, Chevron, and all putting them in one room and saying, "Come on, teach each other what . . ." they get paranoid at that point that somebody may actually discover something. [Never mind that no one will if they do not teach each other.]

I have to say that the times when the beginnings of friction appear in these kinds of interactions is when one side, usually the industrial side, imagines that there may be something big [and profitable] in what we do, so one of my jobs in life is to make sure that I lower expectations all the time until there's something that is reasonably big, and then we can all salivate together. But the idea of permanent salivation because of something that may be really important has gotten in the way in the past of—momentarily, at least, in some of these interactions.

And actually, as I look at it today, there are a few companies out there that I think would benefit from interacting with us. And in a couple of those cases where I think it would be a natural to do [so, if only] we had the time to do it, it is the paranoia that we may actually discover something that they will not discover on their own because they don't have the knowledge and the expertise to do it, but yet they're afraid that we would discover it, and even though they would get to hear it first, that one day we will publish it. So imagine the absurdity of this situation, okay? You will not support someone to discover something that you would benefit from because of the fear that if you do discover it and you benefit from it, others may actually get the trickle-down benefits of what happened. And they would rather not do it than to let a little bit of leakage—it's like you don't [ever] want to win the lottery because you will have to pay taxes. Right? I mean, I'll take my share, and I'll let the rest of it trickle down. To me, that is—I don't know. It doesn't make sense. But anyway, that gets in the way sometimes of such things.

DOMUSH: You mentioned funding also from some of the car companies, GM and Ford, to work on things related to automotive exhaust. I was wondering if you could talk a little bit about that.

IGLESIA: [Yes]. So I'm trying to think of how it all started. It probably started with the work of Björn Modén, who was, **<T: 100 min>** interestingly, an undergraduate from the KTH [Royal Institute of Technology] in Sweden that came to spend a year doing his master's thesis, and then rather than going back to Sweden, he actually came back as a PhD student, and there's an interesting story on that one, too. We have a [history] in our group for not going out and socializing enough, and then marrying each other in the group, okay? So he's the one that set the trend, because he ended up marrying Patty [Patricia] Cheung, who is one [of the inventors in] the DME carbonylation [process]. And to me, they will always be the DME carbonylation and the NO decomposition [folks], but to them, they are now husband and wife. And they have two of what we call the true-blood LSAC [Laboratory for the Science and Applications of Catalysis] babies. Those are the ones where both parents were actually raised in the group. And we now have three and a half of those [marriages] over the years. [laughter]

But anyway, Björn started because we were interested through a short interaction that I had with this company, Catalytica, that I mentioned earlier, in the area of NO decomposition, and we had learned how to make zeolites with exchanged copper cations. We were learning . . . . Out of the methane project came a whole set of techniques for how you put high-valent cations in zeolites, and what reactions they were good for. And copper is one of those things that is uniquely able to do NO decomposition. No reduction required, the oxygen comes in as O_2. And he discovered a very interesting mechanism by which NO_2 is used as a shuttle, forms a nitrate, and then decomposes, because, I mean, the fundamental question was you have two [NO molecules] fall apart. Each one of them has an oxygen atom that needs to get out as O_2. How do you get that oxygen over there [when the two Cu sites are far apart]? It's not going to leave by itself [as O-atoms].

So the only way to do it is the NO picks up one of them, gives you NO_2. You're decomposing NO, but yet you're oxidizing one NO and reducing the other one. The NO_2 goes to the other oxygen and forms a nitrate, and the nitrate decomposes. So this was the beginning of a significant amount of work that we have done on how do you shuttle two things from two functions by using a [molecule as the] shuttling agent? And in this case, nature had already chosen the reactant as your shuttling agent. So NO decomposition is not NO decomposition. It's actually NO reduction by NO, okay? So you actually reduce an NO, form an NO_2 and then that decomposes to actually give you the O_2 molecule.

But we did [all] that without any support from the automotive companies. And then there was some interest on the part of Ford on NO oxidation. So we knew quite a bit now about oxygen shuttling and making NO_2. It turns out that [scientists at] Toyota had developed a method for removing NO . . . . The way that you remove NO from automotive exhaust is you use a reductant, usually because in a gasoline engine you have a rich cycle that [brings some hydrocarbons into the combustion effluent]. In a diesel engine, you don't ever do that. You always run lean, and as a result of it, there's no reductant. How do you remove the NO? You can't do reduction, because there's no reductant. So they actually said, well, if nature gave you no reductant, you'd better use your oxidants. And the way that they would do it is they would oxidize the NO to NO_2 and then the NO_2 would be trapped, and these are called lean DeNOx traps. They have now been [mostly] replaced by the urea method for reduction, but they were actually commercially [used] for a while.

And so beginning with the work of Brian [M.] Weiss, we began to look at the mechanism by which NO is oxidized by noble metals, and then we began to couple it with the trap, the barium oxide trap, and what we learned is that you could make much better catalysts by putting the trap there, because the trap took away the NO_2, and the NO_2 would otherwise inhibit the chemistry. And we also found that the trap worked a lot better because the metal was there, because you actually needed to have an [oxidized] version of the NO in order to be able to do the adsorption. So, I mean, the beauty of things is that neither [function] would work by itself, but both of them helped each other to work, and we didn't discover that they worked. Toyota was using it already.

DOMUSH: Right.

IGLESIA: But we discovered sort of the hidden message as to why it is that it would [and does] work. And actually, that began a whole area of cascade type reactions, and bifunctionality, and how far the sites need to be away from each other, because the intermediates need to move, [often via diffusion]. I mean, it was to explain one thing, okay, and then [this] started effectively a whole area of research to try to figure out where else you find [similar phenomena]. And once you know what you're looking for, it's amazing how many different places it shows up. So one mystery resolved actually is many mysteries resolved, if you just put it in terms of fundamentals that are recyclable **<T: 105 min>** to other problems.

And this reminds me of a quote from my advisor, Michel Boudart, [who] said that a good bit of the research that is done today is what he called "applied research, non-applicable". And what he meant by that was that [the work] meant to solve a particular problem, and in order to pretend that it was solving that particular problem, it was done in a way that it neither solved the problem, nor it left behind any recyclable knowledge. So he was a [preacher about] recyclable knowledge. What doesn't work for this project, if you did the project right, will be useful in another project. That translated into a bit of a gospel that I [also] preach whenever I go to industry, and that is that fundamental, basic research is a lot cheaper than applied research, because if you do fundamental research, you may solve problems that you don't even know are problems today. Right?

I [have] actually used an analogy that I think has been recorded in history in some of my [preachings] . . . and that is that the objective of an industrial research lab is to hunt for food, okay? In this case, technology, if you're actually doing research. So if your job is to hunt for food and you're not very picky about what particular food you want, then the way that you hunt is by going [out] and shooting indiscriminately, and something falls out of the sky all the time, right? I mean, you can eat, right?

So to me, if you're going hunting for ducks, for example, and you don't care which duck you hit, you use a shotgun, right? And that is basic research. It's effective [because] there is a large number of ducks up there. That's the knowledge to be harvested. And if all you want is one of them to fall out, you just take a shotgun and [aim in the general direction of the ducks].

If you want the third duck from the left, and that's the only one that you, [being a picky eater], will eat, you get a high-power rifle, right? And that is applied research. But if you don't hit that one, then you don't eat. And to me, applied research is so targeted in many cases, so driven by [the fact that] we have to solve the problem and we have to solve it by this date, that you forget for a moment that there are many other things that you could actually hunt for that would have become apparent to you if you only widened the scope a little bit. [That there are tasty ducks that are not always third from the left]. So this narrow scope is one duck at a time, okay? And if you just make it a little less accurate by broadening somehow, you may actually come up with a better duck that you didn't know was sitting right next to [that third duck].

And childish as it may seem, that is the argument that I make for why it's so inefficient to do very targeted research, and why political entities, funding agencies, and so on should stop trying to prescribe solutions that are so narrow in scope, that force people to actually do things that are not recyclable, that don't leave any knowledge by the wayside. [They distort the marketplace of ideas and they misuse our trust in how they invest our taxes on behalf of our future.]

And as I look at everything that I have done throughout my [career], it's always been the intellectual debris that has germinated into other things that were unexpected, and it was seldom the one problem that we were trying to solve. In some cases, it was convincing ourselves that that problem [at hand] could not be solved that finally takes you to do something with the knowledge that you have accumulated along the way.

DOMUSH: Now this idea of fundamental, basic research, but with some goals in mind—diffuse goals, not super-targeted and applied goals, not the one duck . . . . My background is in chemistry, and in doing these interviews, I talk with a lot of chemists, and I'm starting to talk with more chemical engineers. But I've noticed over the years that the engineers tend to be more applied in their thinking, less on the fundamental end of research. So does this put you, again, in kind of an outsider position in the world of chemical engineering? Or is [it] just the people that I've spoken with tend to be a little bit more applied, and that's not necessarily representative of chemical engineering as a field?

IGLESIA: Yes. I mean, it probably puts me outside of the mean, or certainly at the edges of the Gaussian distribution to some extent. I mean, part of it is that I already got my shot at being practical for eleven years at Exxon. I know what works and what does not work. I already had my successes. I **<T: 110 min>** saw my ideas in practice. I have sort of gotten it out of my system, and I've also recognized the narrowness of many of those solutions, and how I could have done better by letting the debris fall [on more] fertile grounds somewhere. I mean, I'm the only one here in this department who has any industrial experience [. . .], so by definition, that has to give me a different place [and a different view].

I also think that I'm not in it [just] for the beauty itself. In other words, there is a kind of fundamental question that one asks because it is a beautiful question, it is a deep question, and it gets to the meaning of life and chemistry and so on. I'm much more of, as Michel Boudart would put it, the street fighter, okay? The one that is gathering the things that fall out and nobody knows what to do with.

I do think that everything that we do has some—at the end of the tunnel, there could be something practical. The point is that the tunnel is rather convoluted in nature, right? And every time that we get to a curve we have two branches to take and we try to continue to take the one that takes us to the end of the tunnel, or what we think is the end of the tunnel, but sometimes there's no end of the tunnel. It is just a mirror at the end of the tunnel, right? And sometimes the ones that we left by the wayside actually go to another tunnel somewhere, and I think that we should not continue to put blinders on and move forward. That doesn't make me a purist. That doesn't make me a pure chemist by any stretch of the imagination. That does put me at the outskirts of chemical engineering, but it also puts me a lot closer to my upbringing. Michel Boudart was a chemist by [graduate] training. [. . .] Chemical engineering is a bit of a mutt, I guess. It's a mixture of the physical chemists that never fitted in, with the mechanical engineers that became too chemically oriented, and to some extent, there is room in this big tent for everybody. [laughter]

It is also a discipline that shares some of the same measures of success. I don't know if you read the article in *The New York Times* this past Sunday about what makes certain groups successful.[11]

DOMUSH: I've not.

IGLESIA: Okay. It's a very good article, and it actually mentions my own Cuban community as one of the examples of success among Hispanics. And they say that there are [sort of] three requirements for success. One of them is a level of arrogance that says that we're better than you are. Then there's a level of insecurity that says, well, maybe we're not, and we're going to have to work harder than anybody else. And then finally, there is the resistance to impulse, on going in one direction because it's the only solution to the problem, and the balancing of [instant] gratification for the sake of some long-term benefit, right?

And I mention it because I think it does describe a bit of how I got here, but also because chemical engineering as a discipline is the same way. We think we're better than anybody else.

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[11] Amy Chua and Jed Rubenfeld, "What Drives Success?" *New York Times*, 25 January 2014. http://www nytimes .com/2014/01/26/opinion/Sunday/what-drives-success.html?_r=0

We certainly think that we're better than the chemists, because we make big things made out of [. . .] steel and we get higher salaries than you do, right? Okay?

On the other hand, throughout the life of chemical engineering, we have always had an inferiority complex, because the chemists are the science. The physicists are a science. The mathematicians have a discipline. The chemical engineers are the ones that, you know, bring everything together to solve problems somehow, and that makes us not aristocrats in the area of science, so we have to work harder than anybody else at sort of finding the little spaces that you leave behind for us in order to do that.

And finally, because we have an inferiority complex, every time that a new area seems more glamorous than we are, we tend to go in that direction, but we always catch ourselves before we become [fully] absorbed by that direction. You know, there was a time when chemical engineering moved into the environmental area, okay, but then they came back. There was a time when they moved in the materials area, and then they came back. The biotech area, they moved there, and you're beginning to see sort of the coming back. [And now energy is the new "plastics".]

So there is the initial impulse to go after something that looks better than we are, but then there is something that [snaps] along the way, that says, don't go too far. So that's sort of the tent of chemical engineering and that it is [. . .] the scientific equivalent of an immigrant, right? Because they came in later than everybody else into a field that was populated by the chemists, the physicists, and the mathematicians, and had to sort of make space, by a group of people that had an identity problem, as I have had throughout my life. I mean, everywhere I go, whether it's Princeton, **<T: 115 min>** Exxon, Berkeley, I always go in through the door and say, "They made a mistake. I don't really belong here." Right? You know, that is the identity problem that one has, whenever one has to come in as an outsider whether it be from a different country or from a different discipline. [To some extent that is also chemical engineering.] So did that answer your question?

DOMUSH: It did.

IGLESIA: Okay. I think that there is a group of chemical engineers that are . . . the fringe is not the right way to put it. They are sort of at the vanguard of where chemistry and chemical engineering come together in a way that retains the practical aspects, but also brings the fundamentals. If I go to Europe today, nobody would recognize me as a chemical engineer, because [many of] the chemical engineers in Europe still build pressure vessels, okay?

DOMUSH: Right.

IGLESIA: And they [often] don't know very much about molecules, for the most part, because Europe has applied chemistry and technical chemistry, which [are, as disciplines], closer to chemical engineering than anything that you will find in chemical engineering there. And I think that is an accident of history. And actually, it's an accident of history that came about because of physical chemistry in the US, and how it was absorbed into chemical engineering, that it has a lot more molecular component than it does anywhere else. So—but you still have many chemical engineers who don't have the chemical part of engineering and make do without the level of chemistry that I think is easily attainable, but not always incorporated into what they do.

Okay. I mean, I want to be comprehensive, even though it's not chronological anymore. So the projects that we have had with ExxonMobil, for example, they began with a project that was literally a question as to whether they should spend any time in trying to make this work. So one of the most illustrious members of our community at the time had reported that you could selectively oxidize alkanes, paraffins, at the end of the molecule. This is the Markovnikov, anti-Markovnikov [issue of long discourse]. This is something that had been a no-no. I mean, you're not going to do any autooxidation chemistry and still put the oxygen selectively at the end of the molecule. On the other hand, it would be a dream to take hexane and [selectively] make 1-hexanol, [or hexanoic acid], out of it.

And there was this report in the literature that other people had trouble reproducing, that if you did this within a "romantically" confined structure where only the end of the molecule could get to the active site, that you could actually put the oxygen there. And of course, as soon as we saw that, we said, "But auto-oxidation doesn't work that way. Auto-oxidation works by forming hydroperoxide and then doing a chain cycle, okay, outside of that "romantically" confined place where you actually put the oxygen to make a hydroperoxide." So this would not work.

But this was one of the [oracles] in the field who had reported this.[12]And Exxon was interested in somebody showing that it could not work, so that they would stop [worrying or] [. . .] spending their own money in trying to make it work. And I was called in order to break a fight effectively between a set of people that said we should stop hallucinating and stop doing this, and "No, no, I mean, this may be right, and if it's right, what if it's right, okay, then we could make a lot of money."

And the only way to settle that argument is to bring in an arbiter that says, "Let's look at the mechanism by—how this happens, okay, and let's see whether it could happen," right? And for two years, we actually unraveled the details of rather messy chemistry, because these were homogeneous reactions in the liquid phase. There was initiation on the catalyst that required that we do some of the more complex kinetic and isotopic experiments that we have ever done. And at the end, we actually showed how well you could do this. You could actually do it better than the linear free energy relations would have predicted. Actually, confinement could be made to

[12] Bi-Zeng Zhan, Björn Modén, Jihad Dakka, José G. Santiesteban, and Enrique Iglesia. "Catalytic Oxidation of n-hexane on Mn-exchanged Zeolites: Turnover Rates, Regioselectivity, and Spatial Constraints," *Journal of Catalysis* 245, no. 2 (2007): 316-325

work, slightly, by generating preferentially the terminal hydroperoxy radicals and then getting at least one of the products of the chain cycle to be terminally [oxidized].

We also showed how this had been missed in the **<T: 120 min>** literature—why it was wrong. So, I mean, not only did we show it could not be done the way that they claimed to have [done it], but we showed how the analytical method that they had used actually fooled them into [the wishful] thinking that they only had hexanoic acid, because, of course, if you overoxidize terminally, you make hexanoic acid, and that's what they were detecting. But if you secondarily oxidize the non-terminal alcohol, you make two acids, and those acids were too small for them to see. They missed them altogether. But those were the internally oxidized products that actually cleaved internally in order to make the smaller acid. So there is an example where we learned an awful lot. The student that did this work— Björn Modén was also involved in that—and Bi-Zeng Zhan, actually understood for the first time how heterogeneous catalysts do autoxidation type reactions, and what can and cannot be done.[13]

So now what was the intellectual debris from that particular project? [. . .] In doing that, we were trying to encapsulate metals inside zeolites in order to see whether if we put them there we could actually begin to do some chemistry on the metals and not on those [other] sites that we're intrinsically present in these aluminophosphates. So this was the beginning of a [journey]. How do you put a metal [cluster] inside a zeolite whenever the precursors to that metal are too large to be able to make it through the channel in the zeolite? In other words, most [would try to] put precursors by taking a zeolite that has a certain aperture, and then putting them in solution, and then having the [precursors diffuse in] and exchange. But if the opening is too small, as in these materials, and the precursors are too large, you can never get in.

And you cannot get in because you form oligomers in solution. You form a double layer around [these oligomers] and you cannot carry all your [luggage] with you on the way in. And that's when we began two approaches to do this. One of them is bring them from the gas phase and not from the liquid phase. In other words, use precursors that you can sublime as [gaseous species]. So if you want to put a vanadium species on an exchange site, if you put them in solution, they will never get into a medium pore [zeolite]. If you put vanadium oxychloride and you sublime it, you actually go in as monomers without a double layer, [without] any waters of hydration, and then it will react very selectively [with the protons within the zeolite]. This is what people would call chemical vapor deposition, or atomic layer deposition. But it had never really been used, at least on purpose [in the case of exchanged zeolites].

There [have been] many accidental catalysts that had been made. When people mix two things together and they said, "We exchanged it," in fact, they had not exchanged [anything]. They had not gotten [these species] inside the zeolite. But during the treatment, they made a volatile species, and then it exchanged, and the catalyst worked, right? So there were a bunch of accidental catalysts that we could now make on purpose, [by design]. So that's one way that you can bring the high valence cations into a zeolite after you have made [the zeolite].

[13] Björn Modén, Bi-Zeng Zhan, Jihad Dakka, José G. Santiesteban, and Enrique Iglesia, "Kinetics and Mechanism of Cyclohexane Oxidation on MnAPO-5 Catalysts," *Journal of Catalysis* 239, no. 2 (2006): 390-401.

But the other way to do it is to actually make the zeolite around the precursor. In other words, convince the zeolite to sort of crystallize around a precursor, and then whenever it is that you're finished crystallizing the zeolite, just turn the precursor into the metal. And there was a line of work—Howard [S.] Lacheen was the one that developed most of the techniques for the sublimation.[14] There was a line of work that began with very esoteric preparation by Minkee Choi, a postdoc in the group, using mercaptosilanes.[15] I mean, this was sort of crazy stuff.

And, I mean, the funny thing about [all this] is that we thought we had to be really clever at doing this, so we put in ligands [to protect the precursors]. One end of the ligand was supposed to attach to the metal cation. That was the sulfur end. The other one had the siloxane, and it was supposed to build the zeolite around it, okay? And it worked. It worked beautifully [and elegantly]. But it turns out that you didn't have to do that. You could actually get ammonia ligands, as long as the [resulting complexes] were cationic in nature, so we actually developed a whole range of techniques—Sarika Goel is still working on them—that effectively taught us how to encapsulate almost any metal on any zeolite that we could not bring from the outside, by effectively putting them there and convincing the zeolite to form around it somehow.[16]

And again, if you had asked me at some point in my life, what am I going to be doing in four years, I hope the answer is I don't know, and it has truly been retrospectively correct, okay? And that is that I hope that four years from now I'm doing something that is, again, the intellectual debris of something that I end up learning between now and then.

And you asked me a little while ago, I believe, as to how the students took my introversion when it comes to interactions with others, and I didn't give you the complete answer. I think part of the answer is that the graduate students are very acutely aware of who we are [as a group, as a scientific family]. **<T: 125 min>** Psychologically, they have a much better composite picture of who we are, because they look at our group when they first come here, and they see a certain psychological profile. Okay? The psychological profile tends to be at the introverted end, in my group, and I think that's part of the [fabric of the group]. We tend to attract each other because we tend to exclude those that happen to be different than we are.

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[14] Howard S. Lacheen and Enrique Iglesia, "Structure of Zirconium-Exchanged h-zsm5 Prepared by Vapor Exchange of ZrCl4," *Chemistry of Materials* 19, no. 7 (2007): 1877-1882; Howard S. Lacheen, Paul J. Cordeiro, and Enrique Iglesia. "Structure and Catalytic Function of Re-Oxo Species Grafted onto H-MFI Zeolite by Sublimation of Re2O7," *Journal of the American Chemical Society* 128, no. 47 (2006): 15082-15083.

[15] Minkee Choi, Zhijie Wu, and Enrique Iglesia, "Mercaptosilane-assisted Synthesis of Metal Clusters within Zeolites and Catalytic Consequences of Encapsulation," *Journal of the American Chemical Society* 132, no. 26 (2010): 9129-9137.

[16] Sarika Goel, Zhijie Wu, Stacey I. Zones, and Enrique Iglesia, "Synthesis and Catalytic Properties of Metal Clusters Encapsulated within Small-Pore (SOD, GIS, ANA) Zeolites," *Journal of the American Chemical Society* 134, no. 42 (2012): 17688-17695; Zhijie Wu, Sarika Goel, Minkee Choi, and Enrique Iglesia, "Hydrothermal Synthesis of LTA-Encapsulated Metal Clusters and Consequences for Catalyst Stability, Reactivity, and Selectivity," *Journal of Catalysis* 311 (2014): 458-468.

It was also to my comfort, they're on the left end of the political spectrum, for the most part. We have the occasional Republican [who has felt] ostracized at some point [in time, but accepted the rest of the time], okay? [laughter]

And so I think also when it comes to—there are some people that need to know what's going to happen next. So some of the students come in and say, "I want to know what my PhD dissertation is going to look like," okay, and I always tell them, "I hope we don't know what your PhD dissertation is going to look like. I hope that somehow we find so many more exciting things along the way that it will look totally different [than anything we can imagine now]." So what we do is we head in [some sort of] general direction, right, [aim the shotgun], because it seems, again, like there's a light at the end of the tunnel [and a larger number of ducks still above us]. And then as long as you have the machinery, in order to be able to solve problems by being somewhat self-contained, appropriately connected to people that complement what you do, you can actually move in directions rather rapidly to follow the more exciting results.

And with very few exceptions, I have not found that any of the entities that fund our work have ever resisted our excitement about moving in a different direction, because they understand quite well that we get excited about things that we think are going to work and are important, and not just about the angels [that are dancing] on the head of the pin, for the most part.

DOMUSH: Right.

IGLESIA: So in this particular area where we're working today, I couldn't have predicted four years ago, okay? I can give you a pretty good idea of what we're going to be doing for the next year, [six months]. I hope that I'm not able to predict any farther along. To some extent, that is contradictory to my desire to control, but on the other hand, at a deeper level, it's not. My ability to control is [actually] my ability to decide somehow what is the right thing to do at any moment in time. It's not wanting to know where I'm going to be. It's knowing that when I get to a crossroad, I'm going to have the freedom to choose the way. And that's part of what I consider one of the blessings of my academic career, in that I work on things that I like to work on. I work with people [with whom] I like to work. I have expressed in public that my ultimate test of academic freedom is never to have dinner with anybody I don't like. Okay? And [. . .] that I already lost a good bit of that academic freedom in my days at Exxon, because I had a lot of dinners with people I did not like, and at this point I'm into protecting my time, you know, enjoying the people that I enjoy working with, and not worrying too much about—unfortunately for others, sometimes—about the feelings of others and how they may take the way that I feel about them. But that's part of the selfishness, I think, of old age.

There are other things that we have done along the way almost on a whim. So for example, one day I—after looking at oxidation chemistry for a very long time and getting bored with the fact that I already knew that you could not do any better than this, right? And then moving to more complex oxidation chemistry and drawing the next limit, then I decided that oxidation chemistry was similar to other chemistry that people did not call oxidation. And that is that if you look at how we remove sulfur from organosulfur compounds, we take hydrogen and then we bring a sulfur compound, and we get the hydrogen to react with the sulfur. We would call that a hydrogenation reaction, [but it is the oxidation of H_2 by an organosulfur compound].

But if you actually look at the mechanism by which it happens, you could actually call it the oxidation of hydrogen by sulfur, because you're making H_2S, instead of water that you do in an oxidation reaction. And if you look at the mechanism, it's actually a redox cycle. So the material is a sulfide instead of an oxide. The sulfide has a vacancy. The sulfur comes down as a molecule on the vacancy. The hydrogen removes the sulfur and regenerates the vacancy. So in fact, everything that we have learned about the sulfides could now be used in order to understand chemistry that was, by the name that we gave, not an oxidation reaction.

DOMUSH: Right.

IGLESIA: And actually, sure enough, **<T: 130 min>** when we began to look at the effects of size of the sulfides, we began to find that it was the same rules that we had developed for the oxides.[17] It was a connection that was unexpected. It was one of those moments when you get goose bumps and say, "I think this is actually related to this, and people have not recognized the connection." It is not always a breakthrough in performance. Often, it's the unexpected link that somehow it's what it ought to be—I mean, we're feeling our ways in the dark, right? And all of a sudden, we find something that we recognize because we saw it elsewhere, but it was only because we understood the chemistry [so well] at some fundamental level.

On a somewhat peripheral point that perhaps I should have made earlier, but I didn't, the ability to show that something will not work, to me, is as much of a driver for what we do as the making something work better, [when it can work better].

And actually, that's probably something that goes back to my days at Exxon. I mean, again, what you learn in kindergarten is very difficult to unlearn later on. And to me, my Exxon days were the kindergarten [days] of my scientific career. So during my years there, I was often accused of not being a visionary. And that was because I would insist that every new idea had to be put through the screen of reason and logic and if it violated any of those screens, then it was not a good idea. And that was a day when brainstorming was the thing to do, is you should accept everybody's ideas, even if they're bad, even if they pollute the environment, and they keep you from finding the right answer. That was the dogma, the religion at the time. And that is that we should be accepting of other people's ideas [because the gurus said so].

And I was never very accepting of any of anybody's ideas that did not pass the screen of the first law of thermodynamics, the second law of thermodynamics, linear free energy

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[17] Howard S. Lacheen and Enrique Iglesia, "Structure of Zirconium-Exchanged h-zsm5 Prepared by Vapor Exchange of ZrCl4." *Chemistry of Materials* 19, no. 7 (2007): 1877-1882.

relations, common sense, et cetera, et cetera. And they got [coarser, the screens], as I moved down, but at least I had a method by which I would take my own ideas and find out whether they made any sense or not.

So I would go around puncturing balloons. I mean, that was the mental picture that many people had. And therefore, I was not a visionary. I was not able to imagine the world as it should be rather than the way the world was going to be. And I was not able to think outside of the box. And that became a matter of exchange with some of the high-level managers, including the vice president at the time, and that was, where was the box, and what was my job as a scientist. And my conviction was that my job as a scientist was to figure out where the box was. And to never to go outside of the box, because my science says that the box is here [and for a reason], and as long as I used my good judgment and the laws of thermodynamics in order to draw the box, there were bad things that would happen outside the box. In other words, you would waste the money. [Think within the right box, became the mantra that replaced the catchwords of the time.]

DOMUSH: Right.

IGLESIA: And that in the absence of a box, then you could not really tell what was a good idea or a bad idea. You [tended to] judge the idea by how crazy it was, rather than how good it was. That was also the famous statement that I made that somebody else quoted to me several times, and that is when I told one of these people that the difference between a vision and a hallucination was a matter of religion. And that is that [. . .] you can't tell what is a hallucination and what is a vision [. . .] unless you have something that defines the edge between the two, and the edge between the two is exactly where [the edge of] the box is.

Okay. We have fifteen minutes. I want to make sure that—I haven't mentioned everybody, but I've mentioned enough of the people involved in certain places along the way. There have been—there's a particular individual here at Berkeley who is a younger faculty member than I am. He arrived after I was here. His name is Alex [Alexander] Katz. He and I have worked for a certain period of time co-supervising graduate students, and he's an expert at making materials that have combinations of organic and inorganic structures. **<T: 135 min>** [. . .] Particularly memorable [are] the years that we spent co-mentoring a very bright graduate student, [Justin M. Notestein] who's now a professor at Northwestern and who actually taught me, along with Alex Katz, a lot about how do you make materials that because of their uniform structure are easier to understand.

So one of the things that that began for us is a willingness to spend a lot more time and money making materials that perhaps were not going to be made that way practically, but where, by making it that way, they became more transparent to observation, because they were more uniform, and they had single sites that repeated themselves, and so on. And even though that interaction has not continued in a very active way [to this day], although we still interrupt each other with our ideas on a regular basis, it in fact—I think it was a moment for us where we came to realize that we needed the inorganic/organic chemistry, in spite of its complexity, because it allows us to make materials that provide a much better window of opportunity for actually observing the mechanism by which they actually work.

In thinking about that interaction and in thinking a little bit about how my group has evolved with time, at about that time is when a good bit of the group began to go to academia as opposed to industry. So at the very beginning, I think there was a picture of my group as being, "Here's an industrial person that is training people, but probably we have to wait to see what sort of job he does."

DOMUSH: Right.

IGLESIA: And then there were a couple of cases in which some of the members of the group began to go to academia, and began to be very successful. And then I think what began at that point is the reproductive pattern that happens where your students begin to be appreciated as academic individuals. They begin to reproduce, and you begin to have grandchildren. I have to say that as most things in life, I'm a bit of a late bloomer, okay? [laughter]

I mean, when people look at the first ten years of my career, I was rather invisible, so really, I started in my [very] late thirties. Then many of the group members did not reproduce. So, you know, I'm going to have fairly young grandchildren in my old age as a result of that.

But there's something about watching your academic genetic code go out there that is embarrassingly egotistical, I guess would be a good way to put it. It almost reminds you of our evolutionary traits, that our [evolutionary task] was to pass on our genes, and that everything we did . . . . And I think in academia, there is some of that, too. There is something about, you know, this idea of legacy, this idea that a midlife crisis is nothing more than a reminder that you will be here only for a period of time, and that if you're not a religious person, that the way that you will be remembered is by the people whose life you have touched, and the people who have touched other lives as a result of your touching theirs, and so on.

And just as in the case of the biological end of the family, which is closer [on paper] to the evolutionary survival mode, I think it is the same in the case of science and academia. And I have to go back and look at the pyramids of scientists that came from one place, okay, whether it was Michel Boudart, where you actually look at who Michel Boudart worked for. He worked for Hugh [S.] Taylor. Hugh Taylor was the one that trained Bob [Robert L.] Burwell [Jr.] at Northwestern, who created [another]school of catalysis, John Turkevich at Princeton, Kenzi Tamaru in Japan, Charles Kemball in the UK [United Kingdom]. So here is a pyramid all the way to Hugh Taylor. Hugh Taylor actually worked with [Max Ernst August] Bodenstein and [Svante August] Arrhenius, okay? So if you look at that family tree, not to be an aristocrat now, but you have to believe that there's some coalescing around a certain way of doing things and a certain way of passing genes, okay? [A bloodline of sorts.]

DOMUSH: Right.

IGLESIA: That is more in the way you act rather than anything else, but which actually is quite powerful, and can give you the impression that you're important along the way, because of these sort of pyramids that are built after the fact. So that's probably a self-serving comment at the end, okay, and something that will make me less of a Cuban street fighter and more of a scientific aristocrat. But, I mean, there was something behind my midlife crisis that said, "How can I be remembered longer, and how can I be able to change [and touch] more people along the way?" And certainly **<T: 140 min>** academia is a much better place to do it than industry. Certainly Berkeley is a great place to do it, because we get some of the best graduate students and postdocs of anybody out there, but I have to say that twenty years ago I had no idea it was going to turn out this way.

And it's probably the same as you don't want to know when you're going to die. You don't want to know what that far into the future is going to look like. It's better to live day by day with a level of insecurity about what's going to happen next that forces you to work a little bit harder, or learn a little bit [faster], and try to be prepared for the unpredictable, whether it be good news [or] bad news, in a way that if you knew what going to happen, you probably would not do it.

I didn't actually take you to today, in the sense of what we're doing right now. Part of that is my feeling that I don't know how important what we're doing today is. But I have a pretty good idea of what we did five years ago and what is important and what is not. [And that some of what we are doing now will be important, but just not sure what.]

DOMUSH: Right. Because from five years ago, you can already see some of the intellectual debris, how it's panned out.

IGLESIA: [Yes]. So right now, I'm very excited about certain things that are happening today, but I think it will be a rather narrow excitement unless I cannot get excited in a couple of years about the things that fell out of it that were totally unexpected. So thirty years later, we'll really be able to tell. Somebody else is going to be able to tell what actually made sense and what was important, rather than what appears in the journal cover, such and such.

DOMUSH: Right. Well, we'll have to send someone from the Chemical Heritage Foundation to come back here in thirty years and—

IGLESIA: And look at the debris. Right. [laughter]

DOMUSH: Exactly. I do have one last question. We've talked obviously about a lot of things over the last two days, but I'm curious, what is that you're most proud of in your career? You've talked a lot about, you know, some of the ways you do science. You've talked about watching your students grow just now. You talked about the transition you made from industry into academia. But I'm curious what it is that you're most proud of, if you were to look back, or if there is something that you can say that you can single out.

IGLESIA: I don't know. I mean, in the context of the evolution concept, I would say that I'm very proud of still being married to my first wife. That's an uncommon occurrence around here. [laughter]

The fact that she and I had the good sense to choose each other by somewhat random methods that happened to work. That we have three wonderful children, who in spite of the fact that they never agree with me, as they get to my age, they begin to act like me and behave like me, and that whether they have succeeded or whether they feel that they have to succeed in the definition of success by which I have succeeded [and they have] the fact is that they have succeeded by my definition of success, and that is that they're good, honest people who are very talented and use their talents for the good of others. So I think I'm very proud of the family and of the fact that I have been able to [help with] that with as little time as I spent with them, given how much [time I have] spent doing other things.

I don't think that scientifically there is any one thing that I have done. And I tend not to choose among my children, whether it be academic or biological, or attribute my pride in them to any one accomplishment or anything like that, so it's difficult for me. I think that I'm very proud of the fact that our group sort of extends beyond the time that they have lived here [at Berkeley]. In other words, that they have become friends and they have become husband and wife, in some cases, but that the network continues beyond where they [first met], that this is a place where they bonded together. This is a place where perhaps through the fact that I [tend to] abandon them more than I used to at the very beginning, they have learned how to rely on each other, and that later on, they will be a family just like the Boudart clan is a family, and we still keep track of each other, and we still admire those among us who deserve our admiration, okay, and chastise those who do not [behave as they were taught] in some nice way. **<T: 145 min>**

I guess I'm also proud of the fact that just like marriages last thirty-five years and so on, that I have a group of colleagues and friends that actually are friends for life. I mean, these are people with whom we vacation together. [. . .] Our families know each other quite well, even though one is in Spain, one is in Germany, the other one is in Virginia, okay? That these are not marriages of convenience because we happen to work in a [scientific] area, that we actually like each other, and we like our company enough to actually spend time, [a good bit of] it outside of science, although when we vacation together, our respective spouses have to bring us back to [the matter of the vacation from the matters of science] on a regular basis. [laughter]

There was one time on the coast of Valencia where Avelino Corma and I went out and kept swimming out farther and farther, talking about science. And when we realized it, we were like a mile from shore now. [laughter] [We had to try to] figure out how we were going to get back. And so this is the absent-minded scientist getting lost in the middle of the ocean. But, I mean, to me, those things are a sign that I have done more than just contribute to science, that there's actually something there that transcends the science.

I'm also very proud—and again, I used to tell my children, you should not be proud, because pride is one of the deadly sins, and then they explained to me that there is a difference between pride and being proud that sort of escaped me early on—but I'm also proud of the fact that I am continuing to learn every day something new. And I don't know how physiologically I'm going to be able to keep this up. They tell me that at some point I'm going to have to forget more things than I learn, and at some point I'm going to forget everything. And to me, it's both a scary thing to think about, especially because I have seen giants in our community who have not been recognizable at the end, because of the damages of Alzheimer's. [. . .] And that has been difficult to watch. I mean, it did happen to my advisor, Michel Boudart. It has happened to many other [talented people in our discipline].

And sometimes it has happened before they have had the good sense to step away from the limelight, okay? So part of the mutual admiration club has a pact, and that is that we will [remove one another from the stage] at the right time—figuratively, not really—and make sure that we do not embarrass ourselves at the point when it's better to, as Reagan said—what is it—"walk into the twilight of my life," or whatever he said.[18]

So on the one hand, I think that I can still force myself to think really hard and learn new things, and the students help in that regard. On the other one, I accept the fact that this may not be there forever, which on the one hand makes me scared, and then the other one makes me impatient, in other words, that my time is very important because I don't know how long I will be lucid enough to be able to learn each day, okay, and be able to teach each day, and so on. So these are the things that precipitate the midlife crises, and say, "Now it's no longer thirty years [left]. Now it may be ten." Okay? The question is, what is the best way to continue to leave [behind] whatever I can [that is useful] in this next period of ten years.

And to do it lucidly, and to do it with dignity [and to do it with class and thoughtfulness], I think is not a bad way to put it. In other words, there is a time at which all of us should stop scratching our way to the top and should begin [thinking] about, you know, when [and how] you become a statesman. Well, hopefully before you cannot [any longer] do anything else, but at least when you cannot do anything else well. I think it would be a good time to now become the one that breaks the fights, the one that helps the community to filter some of the knowledge that usually comes in.

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[18] "I now begin the journey that will lead me into the sunset of my life." Ronald Reagan, Open Letter to the American People, 5 November 1994. http://www.pbs.org/wgbh/americanexperience/features/primary-resources/reagan-alzheimers/, accessed 10 November 2014.

How does one know when it’s the right time to do that? Well, if you’re not lucid anymore, it’s very difficult to tell, and if you can’t tell or your friends cannot tell you when that is the time, then I can explain a lot of the behaviors that I wish were different. Maybe not **<T: 150 min>** a happy way to end the conversation, but nonetheless—

DOMUSH: No, I don’t think it is too happy, but it does make sense.

IGLESIA: The endings are seldom happy unless you look beyond the ending into what it is that happened before that time.

DOMUSH: Well, I think we’ve had ample opportunity over the last two days to look at all of the exciting things that you’ve been working on and continue to be working on, and like I said, in five years or thirty years, we’ll have to send someone back to interview you again and see where all the intellectual debris fell out.

IGLESIA: Bring a translator. I may not remember any of the English language. I may be speaking Spanish again by that time. [laughter]

DOMUSH: Well, can’t help you there. I only have this one language, so—well, thank you very much for your time.

IGLESIA: All right. [. . .]

[END OF AUDIO, FILE 2.1]

[END OF INTERVIEW]

INDEX