CHEMICAL HERITAGE FOUNDATION

CARLOS T. MORAES

The Pew Scholars Program in the Biomedical Sciences

Transcript of an Interview
Conducted by

Helene L. Cohen

at

University of Miami
Miami, Florida

on

13, 14, and 15 March 2001

From the Original Collection of the University of California, Los Angeles

Carlos T. Moraes

ACKNOWLEDGEMENT

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REFORMATTING:

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UNIVERSITY OF CALIFORNIA, LOS ANGELES

Oral History Interview Agreement No. _____

This Interview Agreement is made and entered into this 2nd day of April, 2001 by and between THE REGENTS OF THE UNIVERSITY OF CALIFORNIA, a California corporation, on behalf of the Oral History Program at the UCLA campus, hereinafter called "University," and CARLOS T. MORAES, having an address at Department of Neurology, University of Miami, 1501 NW 9th Avenue, Miami, Florida 33136, hereinafter called "Interviewee."

Interviewee agrees to participate in a series of University-conducted tape-recorded interviews, commencing on or about March 13, 2001, and tentatively entitled, "Interview with Carlos T. Moraes". This Agreement relates to any and all materials originating from the interviews, namely the tape recordings of the interviews and a written manuscript prepared from the tapes, hereinafter collectively called "the Work."

In consideration of the mutual covenants, conditions, and terms set forth below, the parties hereto hereby agree as follows:

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If to University: Department of Special Collections
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Attention: Victoria Steele, Ph.D.

If to Interviewee: Carlos T. Moraes
Department of Neurology
University of Miami
1501 NW 9th Avenue
Miami, Florida 33136

University and Interviewee have executed this Agreement on the date first written above.

INTERVIEWEE	THE REGENTS OF THE UNIVERSITY OF CALIFORNIA
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X _____ (Signature)	_____ (Signature)
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Signature: Signed release form is on file at the Science History Institute
Interviewee's Name

____Oct 2, 2021_____
Date

Chemical Heritage Foundation
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CARLOS T. MORAES

1962 Born in São Paulo, Brazil on 17 July

Education

1983 Baccalaureate, Biomedical Sciences, Escola Paulista de Medicina, São Paulo, Brazil
1987 M.Sc., Biochemistry and Molecular Biology, Escola Paulista de Medicina, São Paulo, Brazil
1991 M.A., Genetics and Development, Columbia University
1993 Ph.D., Genetics and Development, Columbia University

Professional Experience

University of Miami School of Medicine
1993-1995 Research Assistant Professor
1995-1997 Assistant Professor
1997-present Associate Professor

Honors

1993 The Samuel W. Rover and Lewis Rover Award for Research in Genetics and Development
1995-1999 Pew Scholar in the Biomedical Sciences

Selected Publications

Dietrich, C.P. et al., 1985. Isolation and characterization of a heparin with high anticoagulant activity from *Anomalocardia brasiliana.* 843:1-7.

Moraes, C.T. et al., 1988. Structural characterization of several galactofuranose-containing, high mannose type oligosaccharides present in glycoproteins of the trypanosomatid *Leptomonas samueli. Biochemistry* 27:1543-49.

Schon, E.A. et al., 1989. A direct repeat is a "hot spot" for mitochondrial DNA deletions in humans. *Science* 244:346-49.

Moraes, C.T. et al., 1993. Atypical clinical presentations associated with the MELAS mutation at position 3243 of human mitochondrial DNA. *Neuromuscular Disorders* 3:43-50.

Manfredi, G. et al., 1996. Identification of a mutation in the mitochondrial tRNA-Cys gene associated with mitochondrial encephalo-myopathy. *Human Mutation* 7:158-63.

Ashok, V., et al., 1996. A MERRF/PEO overlap syndrome associated with the mitochondrial DNA 3243 mutation. *Neurology* 46:1334-36.

Celia, H. and Carlos T. Moraes, 1996. Detection and analysis of mitochondrial DNA deletions by whole genome PCR. *Biochemical and Molecular Medicine* 58: 130-34.

Barrientos, A. et al., 1998. Human xenomitochondrial hybrids. Cellular models of mitochondrial complex I deficiency. *Journal of Biological Chemistry*
273: 14210-17.

Barrientos, A. and C.T. Moraes, 1999. Titrating the effects of mitchondrial complex I impairment in the cell physiology. *Journal of Biological Chemistry*
274: 16188-197.

Rana, M. et al., 2000. An out of frame cytochrome beta gene deletion from a patient with Parkinsonism is associated with impaired complex III assembly and increase in free radicals production. *Annals of Neurology* 48:774-81.

ABSTRACT

Carlos T. Moraes grew up in São Paulo, Brazil, one of three children. His father was in the military at first, but then became a mechanical engineer and a professor. His mother completed a degree in physical education. He discusses some of his childhood activities, which he says were much like those of American children's, and some of his memories of his private-school education. After assessing the value of his education at a private school he discusses his reasons for attending Escola Paulista de Medicina and describes some of his college experiences.

Moraes then pursued a master's degree; he explored several career options after his internship, including a course at the Instituto de Investigaciones Bioquimicas Fundacion Campomar, where he worked under Armando J. Parodi. He eventually enrolled in a doctoral program at Columbia University, where he worked in the Eric A. Schon lab. Moraes's decision to come to Miami was abetted by his love of windsurfing. He professes no religion, but in his youth was involved in Pró-Vida; he feels that one can define God to be compatible with science.

Moraes continues with his first impressions of the United States; his admiration for Alex Tzagoloff; obtaining dual citizenship; the shortage of American students in American science; and his fundinghistory. He talks about the grant-writing process, explaining why he believes that he writes better than he speaks. Lab management for him includes the difficulties of article writing in a lab with many native languages. Moraes's administrative duties are substantial, but he has few teaching responsibilities. He compares American and Brazilian graduate students in medicine; discusses the ethnic makeup of graduate students at the University of Miami; describes a typical workday; again talks about his love of windsurfing; and gives us his thoughts on the underrepresentation of women on science faculties.

A major reason for Moraes attending Columbia University was his fascination with mitochondrial abnormalities. He accepted a position at University of Miami to study mitochondrial diseases; he also has devised some related projects and possible applications of his DNA mutation studies. He discusses the advantages and disadvantages of being a principal investigator and of competition and collaboration in science. Moraes explains his thoughts about ethical issues in science; his concerns about overpopulation; and his thoughts about the use of animals in scientific research.

Moraes concludes the interview with an assessment of his professional and personal achievement and an intimation of his future plans.

UCLA INTERVIEW HISTORY

INTERVIEWER:

Helene L. Cohen, Interviewer, UCLA Oral History Program. B.S., Nursing, UCLA; P.N.P., University of California, San Diego/UCLA; M.A., Theater, San Diego State University.

TIME AND SETTING OF INTERVIEW:

Place: Moraes's office, University of Miami.

Dates, length of sessions: March 13, 2001 (92 minutes); March 14, 2001 (102); March 15, 2001 (92).

Total number of recorded hours: 4.7

Persons present during interview: Moraes and Cohen.

CONDUCT OF INTERVIEW:

This interview is one in a series with Pew Scholars in the Biomedical Sciences conducted by the UCLA Oral History Program in conjunction with the Pew Charitable Trusts's Pew Scholars in the Biomedical Sciences Oral History and Archives Project. The project has been designed to document the backgrounds, education, and research of biomedical scientists awarded four-year Pew scholarships since 1988.

To provide an overall framework for project interviews, the director of the UCLA Oral History Program and three UCLA faculty project consultants developed a topic outline. In preparing for this interview, Cohen held a telephone preinterview conversation with Moraes to obtain written background information (curriculum vitae, copies of published articles, etc.) and agree on an interviewing schedule. She also reviewed prior Pew scholars' interviews and the documentation in Moraes's file at the Pew Scholars Program office in San Francisco, including his proposal application, letters of recommendation, and reviews by Pew Scholars Program national advisory committee members. For technical background, Cohen consulted J.D. Watson et al., *Molecular Biology of the Gene.* 4th ed. Menlo Park, California: Benjamin/Cummings, 1987; Bruce Alberts et al., *Molecular Biology of the Cell.* 3rd ed. New York: Garland, 1994; Horace F. Judson, *The Eighth Day of Creation.* New York: Simon and Schuster, 1979; and recent issues of *Science* and *Nature.*

The interview is organized chronologically, beginning with Moraes's childhood in São Paulo, Brazil and continuing through his baccalaureate work at Escola Paulista de Medicina, his graduate work at Escola Paulista de Medicina and Columbia University, his postdoc at Columbia, and the establishment of his own lab at University of Miami. Major topics discussed include his professional relationship with Armando J. Parodi, his research on mitochondrial DNA mutation at the University of Miami, and possible applications of his research.

ORIGINAL EDITING:

Deborah Kolosova, editorial assistant, edited the interview. She checked the verbatim transcript of the interview against the original tape recordings, edited for punctuation, paragraphing, and spelling, and verified proper names. Words and phrases inserted by the editor have been bracketed.

Moraes reviewed the transcript. He verified proper names and made minor corrections and additions.

William Van Benschoten, senior, writer, prepared the table of contents. Deborah Kolosova assembled the biographical summary and interview history. Romi Keerbs compiled the index.

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INTERVIEWEE: **Carlos T. Moraes**

INTERVIEWER: **Helene. L. Cohen**

LOCATION: **University of Miami**
Miami, Florida

DATE: **13 March 2001**

COHEN: So let's start with something easy, like when and where were you born?

MORAES: I was born in São Paulo, Brazil in 1962. I don't remember much about it, but I think it went okay.

COHEN: It went okay. Good. Your mother probably remembers it.

MORAES: I have an older brother [Sergio T. Moraes] and a younger sister [Silvia Regina T. Moraes].

COHEN: Okay. Why don't you tell me a little bit about your family history, your parents, maybe your grandparents?

MORAES: Let's see how far I can remember. My family is about 75 percent descendant of Italians, so my father's mother [Maria S. Moraes] is Italian, and my father's father [Custodio Rodrigues de Moraes] is actually a mixture of some Brazilian Indian, Portuguese, and God knows what else. Now, my mother's side is all Italian, but my mother [Dirce T. Moraes] was already second or third generation Brazilian.

COHEN: So what took them to Brazil from Italy?

MORAES: Good question. There were thousands of people that went. I'm not even sure. I think that was before the Second World War, so I'm really not sure. That's a good question. I should ask.

COHEN: Well, that's one of the side benefits of doing these things; it makes you ask questions.

So let's start with your grandparents, then. What did they do?

MORAES: My mother's side was relatively poor. My grandfather [Guerino Torres] was painting walls, houses, and things like that. My grandmother [Isabel Torres] would just be a housewife, and she would make clothing and stuff at home to sell. But they were quite remarkable, at least my grandmother, because she had to work a lot, but she was very adamant about getting my mother into school, [which], I guess, early on my grandfather thought was not such a great idea. They were not living in a big city, they were in a small rural town, but she went on to high school and then to the university in a big city, that's São Paulo. So she actually had to leave her home, and my grandmother was very supportive of her and that was quite unusual, I think. So she finished her degree in physical education, and she became a teacher, a phys[ical] ed[ucation] teacher. Now, my father's side—

COHEN: So she was pretty athletic, it sounds like, which was also not so usual then for women to do.

MORAES: Yes.

COHEN: Or at least it wasn't in this country, maybe.

MORAES: Yeah. They would also do a lot of events, like dances, or she'd organize big parades and things like that. This was all part of phys ed at the secondary school where she taught.

COHEN: Okay.

MORAES: Yeah, but the unusual thing is that she had to leave my grandparents, and she went to live with some relatives, I guess, a cousin in São Paulo. That's where she met my father [Waldyr Rodrigues de Moraes] later on. So my father's side's a little different. They were a little bit more educated. My grandfather was a colonel in the army; not the national army, but it was like the state army. I don't know what the comparable position—

COHEN: Oh, like the National Guard here?

MORAES: Kind of. Actually, it was more like a military police. But in those times there were quite a few revolutions where the state would go against the union, and my grandfather participated in several of those and got shot and all that. He was very active. My grandmother,

the mother of my father, was a housewife and she raised two kids, my father and his brother [Wilson Rodrigues de Moraes]. They had some money, and I guess they always got a good education. They were living in a rural area, but not a very small city to start with, but eventually they moved to São Paulo, which is a big city. My father went on to the university, and he finished a degree in engineering. He became a mechanical engineer, and he worked for the government making dams and things like that. He also became a professor at the university in São Paulo.

COHEN: So did he go on for a Ph.D. somewhere?

MORAES: No, he never did.

COHEN: So you don't need to do that there for the university.

MORAES: Now you do. When he did he didn't have to, but he had problems later on. So when he was about fifty or sixty he had to go and get a master's, at least, in education, actually. He had to have some kind of degree. Yeah, so my father was very much into education all his life.

COHEN: So he was one of two children, and how many in your mother's family?

MORAES: My mother had only one brother [Dorides Antonio Torres], and he didn't get much in terms of education, even though later on my father tried to motivate him and push him, but he never finished a degree or anything like that. So he was always selling things here, selling things there, and he's still doing that.

COHEN: These were relatively small families for the time, weren't they?

MORAES: Yeah, but my mother's family was relatively small for her family, because she had, I think, fifteen cousins or even more. My mother's mother had, I think, twenty brothers and sisters, so it was a huge family.

COHEN: Wow. That's a whole clan.

MORAES: Yeah, it still is. They still meet and have huge parties. But my father's side was small.

COHEN: Well, now, what about your siblings? What are they doing?

MORAES: My brother, who is two and half years older, started the university for an engineering degree, but after one or two years he didn't like it; it was too technical for him. He dropped out and went for architecture; that probably was more on his lines. So he finished that, and then he went to Italy and spent, I think, five years doing a master's in architectural restoration. Eventually, he went back to Brazil and tried a few things. Now he teaches in different universities—or maybe they're not full universities, a couple aren't—and also some schools. I don't know. Like colleges, I guess.

COHEN: Like art schools?

MORAES: No, they're colleges, but they're not full-fledged universities.

COHEN: Okay.

MORAES: But they have degrees in architecture or arts or something like that. My sister is one and a half years younger than me, and she went to the university for an arts visual communications degree, and she finished that. Then we had a bit of a traumatic experience where we went to the beach—myself, she, and her boyfriend—and they went into the water, and to make a long story short, the boyfriend drowned. So it was terrible for her, so she went to Italy, because my brother was there, and she stayed there awhile and started getting jobs and now she's still there after twelve or fifteen years. So she stayed there, she got married to an Italian guy [Enzo Sallustro], they have a girl [Alice Sallustro] [who] is ten now, and she works editing movies.

COHEN: Oh, okay. All right. So you guys are spread all over the place.

MORAES: All over. My brother went back to Brazil, thank God. So we feel better that at least someone is there with my parents.

COHEN: Yeah. Well, you said that education was important to your parents. Did you have any sense when you were growing up what, if any, expectations they had for you kids?

MORAES: Not in the sense that I felt any pressure to be something or to do something. I guess when I was a little older I felt an expectation of us getting a higher degree or something like that, but I was really older when I felt that. So I think they did a good job in providing us with lots of stimuli and things like that, but of course, like anywhere, in Brazil there is an underlying pressure that you should become either a physician or a lawyer or something like that. But they were never very strong on that, and they always respected us in our choices. I can't say they were happy when my brother dropped engineering school to do architecture, [and] they were not thrilled when my sister chose to do visual communications, but they accepted it.

COHEN: Okay. So what was life like growing up as a little kid in São Paulo?

MORAES: It is remarkably similar to American cities, because São Paulo is relatively rich for Brazil. Let's see, what would be the differences? We had a lot of freedom, I guess. At that time maybe São Paulo was not as dangerous as it is now, so we really would run around and would go to school— I wouldn't say very early, but at a certain age that maybe parents here would not let kids go alone to school. [There was] lots of public transportation that we would take, and the bottom line is that that freedom allowed us to explore a lot. Very early on we had friends here, and groups, and we would do things that I don't think were things that parents would like, but we had that space and freedom. Maybe it was a little dangerous, but it provided us with a lot of experience, I think. I think also for South America, men have much more freedom than women when they are young. So my parents were much more relaxed with myself and my brother than they were with my sister, even though eventually she broke out. The fact that I had an older brother also helped, maybe.

So there was a lot of freedom for exploration in general, outside the home, and when I was about maybe fifteen and my brother was a little older, we started traveling together. We started to surf, so on the weekends, vacations, and holidays we would always travel by ourselves or with friends, and that was great in terms of experiences, also, away from home. But overall, I think it's quite similar to an American city. Family life is similar. My parents, when we were there, wanted us to be at dinner at the right time. They were quite strict with that. Another difference is that in Brazil it is much easier to have maids, so since I was born I always had someone else besides my mother taking care of me.

COHEN: Like a nanny?

MORAES: Yeah, and some of them stay for quite a long time, so they become almost like a second mother to you. So that's a bit of a difference, I think. Here that is less common; there it is very common.

COHEN: So what kinds of things did you— I know when you got older you were surfing and

that sort of thing, but when you were a little kid, like elementary school age, what kinds of things did you play?

MORAES: Well, I have to remember. Which age is that, elementary school?

COHEN: Oh, five to twelve, about.

MORAES: Five to twelve. Lots of social interactions. We used to live in a condo, so there was a little space beside the condo where people could get together and play soccer—

COHEN: Kind of like a playground?

MORAES: Right. There was also a little soccer field, and there was a place where cars would park [which] we'd use to play with skateboards and things like that. All kinds of kids' play. Everything that you do here. We played hide-and-seek and all that. The social thing was very important, and there were big groups there. When we had vacations— At that age, I think my parents would go with us to these camps. Not really camps, because they were there also; I don't know what would be the name here. But kind of hotels that are all-inclusive, and they were actually part of where my father worked.

COHEN: Oh, okay.

MORAES: I don't know exactly what you'd call it.

COHEN: I'm not sure that we have a—

MORAES: Like a vacation colony. Something like that.

COHEN: Yeah.

MORAES: So those were great social interactions, because there were lots of kids there, and you felt really bad if you didn't belong to a group, and you felt great if you finally interact with certain people. Then you start to meet girls, and those settings are very romantic for a twelve-year-old. I mean, everything is very exciting. Again, I think it was similar to what an American

kid would do at that age.

COHEN: Well, what kinds of things were you— I mean, you know, life is very different if you grow up in a condo in a big city than if you grow up on a farm somewhere.

MORAES: Right.

COHEN: So the type of play that kids do— Some kids are interested in sports, some are collecting bugs, some are building things. What kinds of things did you like?

MORAES: All of the above.

COHEN: All of the above.

MORAES: Yeah. We did a lot of sports, a lot of collections— I don't think I was really targeting something or concentrated in one thing. I think I was jumping all over the place and trying as many things as would be presented to me. We did a lot of collecting cards, I guess, that they sell here. You'd buy them at the newsstand, you'd get three of those, and you'd put [them] together in an album, and if you fill a page you get a prize.

COHEN: Now it's Pokémon cards.

MORAES: Right. That kind of stuff. Lots of games with balls. I can't remember one thing that I was focusing on. I think it was really the whole gamisch.

COHEN: You were a Renaissance boy, right? You liked everything

MORAES: Maybe.

COHEN: Okay. Well, let's talk about school a little bit, because your experiences were probably a little different from the scholars who grew up in the United States or some of the other countries. What are your first—? First of all, when do you start school in Brazil? At what age?

MORAES: I think it's around five, also. I was trying to remember that, and I think it's around five.

COHEN: Okay. What do you remember about—? What are your earliest memories?

MORAES: My earliest memories are, I think, from '69, '70. I was probably, like, seven or eight years old. [There] was this school that, I guess, elementary school you'd call here. I just remember the social interactions and, you know, playing in the swimming pool. Actually, it was a whole-day school, so we had to get food there, and I was a very bad eater, and I remember people trying to force food on me. I would be devastated. But mostly I remember the social interactions. You know, some weird characters that would start to make an impact for— Like one scene here and one scene there just flashes in my mind.

COHEN: For example?

MORAES: I don't know. Like in the swimming pool, there was maybe a professor or teacher that swam underwater, like, I don't know how [much] time in the water.

COHEN: Oh my.

MORAES: I was extremely impressed.

COHEN: Yeah.

MORAES: Then there were kids that would be playing naked in the— What do you call them? The bathroom where you dress up for the swimming pool.

COHEN: Oh, yeah. The dressing room. The changing room.

MORAES: You know, things that I was not exposed to before. Things that I can remember still.

COHEN: They stick out.

MORAES: It was, like, something new.

COHEN: Sure. Did you like school?

MORAES: Yeah. I think I always did.

COHEN: What was it that you liked about it?

MORAES: I think my interests for academic stuff actually started, really, at home and not so much in school. But my father always bought for us these magazines that— Not really magazines; they were like journals that you collect and then you make a whole volume, either with animals or some life science. Something like that. I'm not sure exactly why he was doing that, because he's more of an engineer kind of person, but I guess I showed interest in animals and things like that, and he just kept feeding me with those things in addition to regular Mickey Mouse and whatever. So he would buy a lot of those things.

COHEN: Was he feeding all the kids, or just you?

MORAES: All the kids, but he would notice if— Like for example, I had a huge predilection for this animal stuff, so he would buy it for me. The others would look at it, but I was the one that was really into it. So he was very attentive to how we were developing. He tried to feed us a lot of material. I think that was good. Now, early school I really cannot remember much about academics. I remember that I did okay. I was a decent student, maybe a good student, not an exceptional student. Then in secondary school, maybe, I got some good science teachers that piqued my interest a little bit more, maybe on the life science side. But early schooling, I really don't remember much about it.

COHEN: Okay. Now, in Brazil, when you say— You know, here there's elementary school, and then junior high or middle school; depends on where you are what they call it. Then there's high school. How does it work in Brazil?

MORAES: Right. I was trying to get this down. So I guess— Let's see. In 1973 I started what we call there secondary school. I don't know if that matches the secondary school here.

COHEN: So how old were you in '73?

MORAES: Let's see. I'm from '62, so I was eleven.

COHEN: You were eleven, so that would be like middle school here.

MORAES: Middle school. So that goes for four years, and then we have high school. So before that, I guess we just call it elementary or, I don't know— Different grades of elementary. Or if they call it something else, I don't know.

COHEN: So during those middle school years— Now see, here in elementary, you're with one teacher all day, and then in middle school you start having different classes. Is it the same there?

MORAES: That's what we had.

COHEN: Okay.

MORAES: That's when I started getting some good science teachers. Particularly one that I remember— Maybe during middle school I had only one life science teacher. There was a woman [Dona Cecilia] [who] was very good and stimulated my interest for that area. So after that, high school— So for middle school I went to a state school that, in those years, '73 to '76, was still pretty decent.

COHEN: Let's back up for a second. Was the elementary school public or private?

MORAES: Those were private, that I went to.

COHEN: Okay, so before we go on, why did your parents send you to private school as opposed to public school?

MORAES: I don't know. It might have been— I think it's both— Well, first, I don't think we have many good public elementary schools, but I may be wrong. God knows what was happening those days. It might have been convenience, to be relatively close to home, even though one of them was not that close. But I do know that what you call middle school was a state school but was considered good, so maybe that's why they put us there, and also my

mother was teaching there in [that] school. It was one block from our place, so it might have been a combination.

COHEN: Now, that can be— You know, middle school are the years that kids are the most awkward, so it could have been uncomfortable, maybe, having your mother at school. How was that?

MORAES: To me it was not much of a problem, because she was teaching girls only.

COHEN: Oh, girls' gym.

MORAES: But yeah, eventually there would be some awkward situations. I didn't have much of a problem. I think she was not disliked, so nobody ever said anything bad to me, and I never saw her, really. It was a relatively large school.

COHEN: Okay. How were those years for you, those junior high years?

MORAES: They were— I can't say uneventful, because I don't really remember. I remember I had some colleagues, I had a girl that I was madly in love [with who] never gave me the time of day. She was a very good friend of mine, but— So there was all this awakening for sexual feelings and all that. Also, I started changing social groups, I guess. I can't remember exactly who I was friends with mainly there. I remember a change, and maybe I was interacting more with people who were riding skateboards and things like that. So that was the link. So not people that were in school with me or in my building or something like that; I was going a little bit more out of my way and maybe being a little bit more daring in the kinds of activities I would do. Like skateboarding could be a very dangerous thing. For example, we would go to a faraway neighborhood that had very huge slopes, and I would come back holding to cars to get a ride.

COHEN: The kinds of things if you heard your daughter [Emily Kay Moraes] was doing them, you'd have a heart attack.

MORAES: Yeah, I wouldn't appreciate it. But I can't remember any really remarkable things that happened during those years.

COHEN: Now, you know, in this country people get put in groups. You know, there are the

brainy kids and then there are the nerdy kids and then there are the popular kids, and whatnot. Did that sort of stuff go on there?

MORAES: Yeah, it went on like that. I think I never fit completely in one group or another. As I said, I was a good student, but I don't think I could be considered [to be in] the brainy group. Also because my activities were, like, sporty and, quote unquote, "cool".

COHEN: Cool?

MORAES: Yeah, and I think that continued through high school, that pattern. So yeah, I was probably more interested in being in the cool group than being in the brainy group. There is a difference also between Brazil and here, [which] is that there you can make up for a bad track record, eventually, and here it is harder to do that. If your GPA [grade point average] is not good here, it is hard to make up for it. There, when you finish high school, you have to— To get into the university you have to go through an examination, and the best grades will get in.

COHEN: Regardless of the GPA?

MORAES: Regardless of your previous history. So maybe the pressure of always having to perform to maximum was not there. But still, I enjoyed school, but I was definitely not the nerdy type. I would skip school once in a while, I would do some bad things.

COHEN: Now, what about high school? Was that public or private?

MORAES: When I finished middle school, I went to this very good private high school [Bandeirantes], very good and very difficult, but traditionally people who graduate from there would get into the best universities, because they were really tough. So I did— There were three years, and I did okay for a year and a half, but then I just got tired of working so hard for my grades, and I did what a lot of people do there, and the parents don't like very much—that's to change to a school that is not as hard after some time. I think my brother did three years there, and I did a year and a half, and my sister did one year there.

COHEN: You got braver as the generations came through.

MORAES: And of course, I had to use very good arguments with my father as to why that was not such a bad idea. I don't think I was at risk of losing a year yet, but I think I used the

argument that maybe I could lose a year and that would be bad for me.

COHEN: Lose a year?

MORAES: Well, have to repeat a year because maybe my grades were not up to par. So I moved to this school [Objetivo] that also traditionally takes people from hard schools. It's not a bad school, because they have decent teachers, but they really do not demand too much in the sense that if you want to learn, you learn, and if you don't want to learn, you don't have to, you'll still get grades somehow. But they also offered one thing. They had these courses that were preparatory courses for this university examination, the entry examination. So this high school also offers these courses, and they were offered for free if you were a student there, so I took advantage of that. So I finished half a year and then my last year of high school; at the same time, during the evenings, I did this course that [was] a preparatory course for the university. So maybe I would have been better prepared if I had stayed at a tougher school, but I think I still got a decent preparation. Then I went for this big examination [where] you have to pick the school you want to go to. All the good schools are state schools or federal schools. The private schools are not as good in Brazil.

COHEN: The colleges, you mean.

MORAES: Colleges.

COHEN: Or universities.

MORAES: And of course you don't pay in the public schools, and they're better, so you really want to get in there. So some examinations are extremely competitive, like medicine, and others are also competitive, but a little less, maybe. So I had some interest in medicine, but not completely. I could do medicine, but I really liked biology and science in general. Like, one thing that I did when I was maybe twelve or something like that, I used to collect the information that comes inside the medicines. You know, all those huge names. I loved that kind of stuff, so I had an interest in these kinds of life sciences. So I did the examination, and I chose a course called biomedicine, and that is a very small course in a federal school [Escola Paulista de Medicine]. Only twenty students a year, and it's kind of biology, not medical school, and they emphasize the medical part and they emphasize research as a goal for the students, because it's such a small group. So I did that, I pass[ed] it, and—

COHEN: Okay, let's back up for a minute. I have a couple of questions about your high school and then about how you came to that decision. Why did your parents—? You know, here, to get

into one of those sort of fancy private prep schools, you almost have to start in pre-school and work your way up through the system, which clearly is different there, but was it hard to get into that high school, and why did your parents send you there?

MORAES: Well, they sent me there because they thought it was the best. How hard is it to get in? I think you can just get in. I think you have to interview, but I can't really remember what kind of requirements they had.

COHEN: Do you recall how expensive it was, or were you involved in that at all?

MORAES: Not at all. My parents were paying for it and—

COHEN: Because they had three kids.

MORAES: I bet it was expensive, but I can't give you a number. Also, with Brazilian inflation, it would be meaningless for us.

COHEN: Okay, so when you decided to do this biomedicine or to take the exam for that, at that point, what did you think you would do? I mean, did you have a picture yet of what—?

MORAES: Very vague. I mean, I would love to say that I knew exactly what I was going to do, but that was not the case.

COHEN: You'd be alone if you knew.

MORAES: So I knew that was an area that I enjoyed.

COHEN: Right.

MORAES: I was not even sure that I would end up doing research, because that course could also prepare you to do lab analysis, clinical analysis, and things like that.

COHEN: You mean like a lab technician in a hospital or something like that?

MORAES: Yeah, something like that, or you could open your laboratory analysis business and do contracting and maybe make decent money.

COHEN: Okay.

MORAES: So that was— But I was not sure, really; it was just that— There is a problem in Brazil that— It's better here, because there, at eighteen, when you enter college, you really have to define your career path. So college here gives you a buffer zone, where you can still change a little bit, and I think that's good. So there you have to choose, and you can't change unless you drop out and start again. You have only one major, and that's what you go for. So it wasn't clear to me what I was going to do, to tell you the truth. I just knew there was an area I liked, it was a good school, so hopefully it would prepare me for something that I could use and make a living.

COHEN: Okay, and why did you decide to stay in São Paulo? Because Brazil is a big country; it's got more than one university.

MORAES: It's a big country, but people don't move around as much as here, and for studies, it really wouldn't make sense for me to move out; actually, people come from outside to São Paulo, because that's where the best schools are.

COHEN: Okay.

MORAES: So that was a natural thing, and also people stay with their parents for a long time, until they get married, basically. So I guess until you are twenty-five, twenty-six, you still live with your parents. That is a common thing.

COHEN: Actually, it's becoming common here, now.

MORAES: And when you move out it's a big trauma thing. Your mother cries.

COHEN: So you lived at home. Now, see, here it's considered part of the experience of college to go live in the dormitories and things.

MORAES: And that's a nice thing. Yeah, I compensated for that, I guess, by traveling a lot without my parents, but during the scholar year I was there.

COHEN: Okay, so what was the university experience like for you?

MORAES: The university experience. So, as I said, the class was very small, only twenty students, so that gave a real unique character to the experience. We were like a small group; I got a girlfriend early on in our class, we would study in groups that were basically a third of the class, so it was interesting. The initial years we were together with the medical students, so we had to do all those gruesome courses: anatomy, etc. It was new and exciting and tough, but it went okay. I was also a decent student if I was motivated. If the course was not good, I would just find a way to get grades. I remember one in particular; that was mathematics. That's where we're supposed to have some kind of advanced mathematics, calculus there. The teacher was just terrible. I couldn't understand one thing that she was talking about. So I was cheating and getting answers from my colleagues, and I didn't care much about it. But other courses, like physiology, that were interesting to me, I did quite well by myself. But still, I think that in college I became even less in the brainy part of the class, because then I was really into surfing and I was traveling any opportunity I had. My social interactions became much more these weekend interactions with people that would travel and I would meet at the beach. You'll see later on that that had a very strong impact on my career, but yeah, so I would go to school with flip flops, a suntan, and I did okay. I would study hard still, when we had tough exams I would do the work, but I think at that time I was doing just enough to get by.

COHEN: Well, are you safe there? Here, if you don't maintain a certain grade point, you get kicked out of school.

MORAES: Yeah, I realize this now; I'm here on the executive promotions committee, and I see lots of kids here that don't get good grades and are kicked out of school, and they're pleading for their lives. I think there you could repeat a year. I definitely did not feel the pressure that I was going to be kicked out. Even though I was never in that position, my grades were always good enough that I was not even close to repeating a year. But I did not feel the pressure. I think I could repeat one time the whole year there. There are other schools in Brazil that are even easier. You have private schools, particularly, that are not as good; they have what is called dependency, that you can repeat only the course you failed without losing a year. So when you go for your new academic year, you just have to repeat that one course. In our school it was a big thing that they didn't have that, so if you repeated you had to repeat the whole year. So that was pretty much all the pressure I felt, that if I did bad I would lose a year.

[END OF TAPE 1, SIDE 1]

COHEN: So how long is the course in Brazil for the first degree, the bachelor's degree?

MORAES: Four years.

COHEN: So it's the same as here, except nowadays people take five or six years to do it, but generally it's four years. Now, I noticed on your resume that you went and did a master's after that, and that that took another four years.

MORAES: Yeah, that's common practice there, at least for this course that was geared towards research, that you do a master's afterwards if you want to be on research, and by that time, by the end— Well, let me back up a little bit. The last year is an internship year.

COHEN: Oh.

MORAES: So you work in the lab, and I went to a lab that was in biochemistry that, even though it had its problems, was pretty good. So after a year there I was more into research, and I decided to complete, so I did my master's right there in the same lab.

COHEN: Okay. Was that Helena [B.] Nader's lab?

MORAES: Yeah. It's actually— The big professor there was Carl [P.] Dietrich, and Helena Nader was his former postdoc that became his wife, eventually. So the group had a little bit of that dynamics of having a big boss, and the wife of the big boss in charge. That makes it a little weird sometimes, but still, they were publishing in international journals, and that was motivat[ing].

COHEN: When did you realize that you were interested in research?

MORAES: Well, I always enjoyed life science. Now doing research or doing something else— I'm not sure it came to me as something I realized or if it was just a consequence of my career. I'm very curious, and I have a tendency, I guess, to do research naturally. But I can't remember a defining moment that I thought, "Well, this is really what I want to do." It was just a progression of events, basically.

COHEN: But you did make a decision, because you went into the master's program.

MORAES: Yeah, I guess I did.

COHEN: Well, I don't know, some people don't. They just go to graduate school because they can't think of anything else to do.

MORAES: Well, maybe I have to try to remember a little harder, but sometimes I have the feeling that I was going through the movements, even though at that point it is hard to believe that I did that without having more of a clear mind. I was always ambitious, and by the time I finished this internship I really wanted to be publishing papers and doing good research, because that's my area. I wanted to do as good as I could. So, for example, when I finished the bachelor's degree, I took three months off and I came to the United States for vacation. I came alone, I came with one of these passes that you can buy if you're abroad that you can fly anywhere you want for two months. So I flew everywhere; one of my goals was to improve my English, and the other was just to experience the United States. But one of the places I went was Saskatoon, Canada, because there was a professor [Norman M. McDuffie] there who had gone on sabbatical where I was in Brazil, and he was the only contact I had, and I wanted to see if I could work with him for a year or so. So I actually could fly to Minot, North Dakota, and it was winter.

COHEN: One of the garden spots of the world.

MORAES: And then I hitchhiked to Saskatoon in the winter. A lot of people tell me that this is suicide, but I guess I was not thinking, and I was lucky enough that it was not that cold. So when I got there, this professor was surprised, because I didn't tell him that I was going to go, but he was also impressed that I had that kind of drive. He was changing his lab, he was doing something, and he said that it was not a good idea for me to go there, but I should definitely check other places, because it looked like I had the drive and I should be doing that. So then I used the same trick and went to labs of friends of friends and actually gave talks about the work that I was doing. My supervisors were actually pissed, because I didn't tell them that I was doing that. So I was a little bit naive in a sense, but I had a lot of drive to do these things during my vacations that I really didn't have to. So at that point I was really already going full steam towards my career, I think, even though maybe I was— It just came naturally with that one year of experience.

COHEN: So did you end up getting a job in the States?

MORAES: Well, I ended up in San Francisco, where I rode a bike for a month.

COHEN: Oh, okay.

MORAES: And then I went back to Brazil. I didn't insist on it, because once I went back I started my master's degree there, and after two and a half or three years, my supervisor sent me to Buenos Aires for this very nice course that is at a world famous place called [Instituto de Investigaciones Bioquimicas] Fundacion Campomar. That is where Luis [F.] Leloir was, that was a fellow that won the Nobel Prize for discovering how glycogen was synthesized. So he was very famous in Argentina, of course. And he created this whole institute with lots of good people, and they have this course that after you ended the course, you would stay working with someone there, and that was also maybe a defining moment for me, because I was lucky enough to work with this guy called Armando [J.] Parodi. He is really a role model for me, nowadays. I stayed eleven additional months with him there.

COHEN: Oh, okay. It says on your resume that you were there for three months. No, I'm sorry, seven months. Eight months.

MORAES: Yeah, it was eleven, total. It was a four month [course]—so maybe I miswrote it there—and after that there was lab training.

COHEN: Oh, okay.

MORAES: So overall, I stayed eleven months, and I just went back to Brazil because my supervisors were really pissed, because I was supposed to stay just seven months, and I was stretching and stretching because I wanted to finish a piece of work, and it was a whole life experience also. If you want I could talk about that; I don't know if at this point it is relevant for me to talk about what was happening on a personal level.

COHEN: Sure.

MORAES: Why not?

COHEN: If you want to.

MORAES: So when I went there, I went with a colleague of mine, a girl [who] was also a student in the same group of us in Brazil, and we were already having an affair or whatever and so it was a great trip. We drove to Buenos Aires through the coast and I had my car there and we rented an apartment. It was nice. The only problem was that she had a boyfriend in São Paulo.

COHEN: Oh.

MORAES: And after a while, for one reason or another, she froze and she came back before I did, and she went back to her boyfriend. So at that point I also became involved with someone that was working in Parodi's lab. I'm being candid now, because she was married, like, two months before that, but her husband lived in another city, and we were together all the time. So it was not one hundred percent platonic, but it was close to it. We really did not have much intimate contact at that time, but it was a very strong thing. So all these contributed to me stay[ing] longer and longer, and also the work I was enjoying a lot, because of the style of the lab and the way it was run, and Parodi would do work in the lab all of the time, and that was something that impressed me that I cannot do here.

COHEN: We're going to get to that.

MORAES: So I stayed as long as I could, and ultimately he finished the work after I went back to Brazil. And after a while, I think, he finished the work and he published a paper, and put me as a first author, and that was very important for my start, as we will discuss later. So that was a great experience, so I really didn't have to go to the United States at that point, so I was happy with that. But when I went back I had to finish my master's, and I did that in another few months.

COHEN: Let me back up for just a second. What was it, aside from the fact that he worked in the lab, that made that lab experience so good? And this is one of the important things, I think.

MORAES: Yes. I mean, he was very inspirational for me, but let me see if I can tell you why, because I'm not even sure I know. The first thing is that he was liked by the people in the lab, and he was respected by everybody. He would publish very good papers, very good journals, more than the average people there at that very good institution. I think his productivity was one thing that impressed me and how efficient he was. He was not only doing experiments, but he had a typewriter, an old typewriter; in those days he was still using that. He was typing all the time, or working on the bench. It was just that his productivity made a big impression. At the same time, he had a good sense of humor, he was warm, he was smart, he was sharp. So it's a combination of all that, and, as we will probably discuss later on, there were other people with similar features that also became role models for me.

COHEN: Okay. Well, I know from looking at your resume, all of this put together took four years to get the master's. Now, here you could almost have a Ph.D. in that time, so what—? I mean, you said this is the normal way to do it in Brazil.

MORAES: Yeah, it's different. There the master's and the Ph.D. almost takes the same time. Usually a master's will take three years there, and the Ph.D. will take three to four years there.

COHEN: Okay.

MORAES: And the master's there, if you're in the basic department, is not really a review or course of study. It's really work in the lab, so it's very similar to a Ph.D. For the Ph.D. level, you should be more prepared, so things should move faster.

COHEN: So there's no coursework. It's strictly lab.

MORAES: There is.

COHEN: Oh, there is.

MORAES: But you need sixty credits for a master's, and you need ninety for a Ph.D., but those sixty count, so you just need thirty more. But you still need your thesis with lab work if you're in a basic department.

COHEN: Right.

MORAES: So it's kind of— If I have to compare, the Ph.D. here is split in two over there. But that is good, in a sense, because if you want to make a change, you can. Even though I would say that the majority of people just go from master's to Ph.D. in the same lab. But I didn't.

COHEN: That's kind of interesting, because at the undergraduate level you can't make a change very easily. You know, you enroll in medical school at eighteen, or in whatever, but you can make a change after the master's.

MORAES: Yeah.

COHEN: That's interesting. So you made a big change; you came to the States.

MORAES: Not only that, but I changed the topic of study also. Completely.

COHEN: Let's talk about that.

MORAES: That's a funny story, because it's completely circumstantial.

COHEN: Okay.

MORAES: So every weekend I would go to the beach, this place called Maresias that's two and a half hours from São Paulo. It used to be a very quiet place, because there were no paved roads, but in the last ten years it became really crowded, because they paved the road and all that. But it was a nice surfing place, nice waves and interesting people. So I would be there every weekend, basically, and one of these weekends I met one friend of mine—it was not really a friend, it was an acquaintance—[who] was a very strange person. Should I give his name?

COHEN: That's up to you.

MORAES: Yeah, why not?

COHEN: Sure.

MORAES: Well, maybe I shouldn't, so I can describe him better.

COHEN: Okay.

MORAES: So he's an M.D., and he has training in the neuromuscular diseases. He got his M.D. in Brazil, but he had traveled to France and to other places to specialize in neuromuscular disease, in particular the pathology of neuromuscular disease. He is quite a character, because he

was very much into drugs and alcohol to extremes.

So it was not unusual to wake up, go to the beach, and see him there with his socks on, [his] face in the sand, and things like that. He has a very powerful father [who] is in the medical school in São Paulo, and I'm sure he put a lot of pressure on him in different ways, and that probably shaped the way he is. But in any case, I was there and he was there and he just out of the blue said, "Do you know how to measure carnitine?" That's a substance in the muscle that helps to transport fatty acids. I said, "No, but I could, right, if I get a protocol or something like that." He said, "Well, why don't we do this? Why don't you go learn this in New York? I know this guy [who] is the best guy in the world on this and that, and you can learn, come back, and do it, because I need it for my studies." I said, "Well, all right, let's talk about it." So this guy that I was talking about is Salvatore diMauro. He is a very well-known M.D. researcher in New York at Columbia [University], and he is originally from Italy, but has been in the U.S. for thirty years. He is at the top of the neuromuscular disorders field, particularly mitochondrial genetics. So I don't remember the details, but I contacted him and said— Well, this friend of mine contacted him first, and he said, "Yeah, sure, if you can come up with the funds." I was a very "go get it" kind of person, so I said, "Well, I'll find some fellowship." The fact that I had a master's was good, because Ph.D. fellowships were relatively easy to get. So I applied for a Ph.D. fellowship, but now I was really jumping the gun there big time, because I was not accepted to any kind of Ph.D. program. The guy just accepted [me] to go there and do some work, and I really went over my head there, because I got a letter from his lab saying that he accepted me to go there, but the letter didn't really specify what kind of work I would be doing. So I used that letter to apply for my Ph.D. fellowship. Now, I was determined to do a Ph.D., but I was not accepted to one, and I really had no idea what it would take for me to be accepted. But anyways, I applied and I got the fellowship, and so this was in March of '87 when all this is happening, and that was at the same time that I met [Maria] Julia [L. Pace], my present wife.

So the fellowship was to start in September or October of the same year. So I decided to go, originally for one or two years. I was also fooling myself that I thought I could get a Ph.D. in one to two years. I really had no idea of what a Ph.D. here is. But anyways, I was always taking quite a lot of risks, I think. Maybe I'm a little calmer now, but at that age I was just going to go for it and see what happened. I didn't even think that I could get in trouble with the Brazilian government because of the fellowship I had.

COHEN: Did you get in trouble with the Brazilian government?

MORAES: No. Not yet.

COHEN: Not yet. Okay. [laughs]

MORAES: So I decide to go. On a personal level, I had Julia, [who] I had met a few months

ago, and was not sure what to do with her, but I asked her to come with me. She was working at a magazine producing pictures and things like that, and she was a little tired of that kind of job, and she said, "Well, I'll go with a round trip ticket." So I went and I started working with DiMauro. And there the group is interesting, because it's actually four groups, four PIs [principal investigators], that interact very closely, and they do a number of things, from biochemistry to genetics to morphology. So it was a good place to learn, because I could go everywhere and they have open doors, and if you're not shy and [you're] driven, you can really learn a lot.

COHEN: But you were not in the Ph.D. program at this point.

MORAES: No, I was not in a Ph.D. program.

COHEN: So you got money to be a Ph.D. student, and no school.

MORAES: I had a fellowship for a Ph.D.

COHEN: Right. Okay.

MORAES: So the first order of business for me was I had to get into a Ph.D. program. So what I did after a couple of months, to get acquainted with the system—not really with the system, but with the labs— There was a fellow called Armand [F.] Miranda, and he was the PI of one of these four groups and he had a primary appointment in pathology and there was a Ph.D. program in pathobiology at Columbia University. So I was doing some work with him, and I said, "Well, let's try to get into the program there," since I was already doing the work and things were starting to work. So I went to interview with some people in pathology, and it was a disaster, because I just said all the wrong things, like that I wanted to do it in two years— I don't know. It was just a disaster. One thing I had in my favor was that I didn't need a stipend, because I already had one.

COHEN: Yeah, they like it when you have your own money.

MORAES: Right. But I guess they thought I was so out of it trying to do it in two years that they didn't accept [me]. But that was actually good, because my second trial was with Eric [A.] Schon, and that was in genetics; that was a better program overall, and also, even though I like Armand and everything, Eric's a better supervisor. He is much more into the science than Armand was. So my second interview was much better. I had several things in my favor; the

first was the paper from Parodi that was already published, and I was first author. Not many applicants have that, and it was published in *Biochemistry;* that's a good journal. The work was going pretty well at Columbia, and I also had a paper that we were working on that I think was already accepted for the *New England Journal of Medicine.* Thirdly, I had my own money, so they really didn't see any reason not to accept [me]. So I was officially accepted and I started working and I had to take the courses. I could place out of one or two of the courses by doing a test, like biochemistry, [which] I was already trained in, and things like that. So it went pretty smoothly then, because the work was already on a roll when I got in, and I was lucky— We can talk in a little bit more detail about the disparity related to the work, because it was the beginning of a field, really, and I was very lucky to be at the right time in the right place. That started toward the end of '87 or the beginning of '88. There were a couple of papers published by different groups, one in England and another one at Emory University, showing mutations in the mitochondrial DNA associated with human diseases. So up to that point, that was considered something that was not possible, because mitochondrial DNA codes for very crucial things that— I don't know, there are enzymes of the respiratory chain that we need to produce energy. So you would expect that mitochondrial DNA cannot have a big mutation, but these papers described that you could, because you can have a mixture of wild type, normal mitochondrial DNA and mutated mitochondrial DNA, so you have a population genetics. So that really started a whole new field, and their group at Columbia— They had a huge number of patients in the freezer with these clinical phenotypes of mitochondrial diseases, but nothing was known about the genetics.

COHEN: Because this may be listened to by non-scientists—will undoubtedly be listened to by non-scientists—what do you mean when you said they had patients in the freezer?

MORAES: They had pieces of patients.

COHEN: Like tissue?

MORAES: They had mainly muscle biopsies.

COHEN: Muscle biopsies.

MORAES: Also autopsies sometimes, and sometimes skin cultures, but mainly muscle biopsies, because that was a muscle lab.

COHEN: Okay.

MORAES: So it was relatively easy to take a piece of this muscle, grind it, extract the DNA, and test for these new mutations, or even new mutations that were not even described. So it was a very productive time, because we found lots of different mutations, new mutations, and we could come up with new techniques to study those diseased muscles. So publication-wise, it was a very good period, and at the end of five, six years I could just take a bunch of papers put together, write an introduction and a conclusion, and that was my thesis. So it really didn't take me much work to do the thesis, because the work was going so well.

COHEN: That's great. Well, I noticed that you got another master's degree from Columbia.

MORAES: Yeah, but that can't be counted.

COHEN: That's a step on the way.

MORAES: At Columbia I think they get money from the state if they give you a degree, so on your way to a Ph.D. they give you a master of arts and master of philosophy. I guess if I quit at that point I would have those degrees. But it was not something extra.

COHEN: I see. It was just part of the deal. Okay. So now, compared to Parodi's lab, how was this lab that you were in? In what ways was it similar and different?

MORAES: Well, the characters were quite different. As I said, there were four PI's. Salvatore diMauro is like the father figure there that keeps everybody together. He is a very nice guy, loved by everybody; his lab is not the most organized, because he has so many commitments outside of the lab, and he has to rely on people in the lab to get things going. Then there is Eric Schon; that's the one that does the most basic science maybe there, and does genetics, molecular genetics. He is quite a character, also. His strong points are his enthusiasm for science; he is always beaming when he talks about science. His weaknesses are—I don't know—he has maybe too many crazy ideas, and if sometimes people don't know him very well, they might think he is a bit obnoxious. That is absolutely not the case if you get to know him. Sometimes, maybe, he talks a little bit too enthusiastically about things and without stopping to think too much, but he is an extremely generous person, and he teaches very well, so I learned a lot from him. Then there was Armand Miranda, [who] was a completely different character, a little bit more shy. He would get upset much more often than most people at all kinds of things, and sometimes he would go into a rampage [that] the world is against him. Still a very nice guy, very bright, but all those problems kind of stunted his professional development, even though he did very well and became a professor, and he retired a few years ago. Then the last one is Eduardo Bonilla, who is originally from Colombia. Very nice guy. He does the muscle

morphology and the brain morphology. He is also kind of shy, so he was never very prominent in the field, even though he should [have been], because he doesn't go to meetings. He is always working under diMauro's umbrella, so I don't think he gets the recognition that he deserves, even though on a few occasions he did some work separate[ly], but he is a low-key guy. So they are very different people. DiMauro kind of juggles all of them and keeps them working together, but the advantage was that I could move through all these techniques, and really there was no— I mean, Eric Schon, who was my official supervisor, would not stop me or tell me, "I have to control you more," or something like that. I was really free to do whatever I want, and I published papers where the senior author was all four, basically, and towards the end of my time there they would let me be the corresponding author. So there was a lot of freedom.

Now, how it compares with Parodi's lab: Parodi had more of a— He was more of a role model for me, because that's the way I would like to be, I think. It's not the way I am now, but that's what I'd like to be, and I find it remarkable that people can do that. Nowadays he is even more famous; he became a Howard Hughes [Medical Institute] investigator in Argentina. That's not easy to do.

COHEN: Wow, okay. Well, we're going to spend most of tomorrow talking about your goal here as a PI. You know, you hear these stories about how many hours doctoral students and postdocs work, and some labs are very driven and others are not. What was this time like for you?

MORAES: For me it was very natural to bust my ass, really. I guess my main problem was to stop working, not to work. Julia had to push me to stop once in a while. The environment helps a lot, also. New York is a good place to work a lot, because usually you live close to work, it's [within] walk[ing] distance, so it's very easy to go in on the weekends and things like that, and you have lots of foreign postdocs that are very motivated, because they have a short period of time to accomplish a lot, and they're working there all the time. That's something I miss here, actually, and you just said we're going to talk more about it later, but— I don't know if it was my motivation or the environment or a combination, but I was happy in the lab. It was really like a second home, and I could stay until midnight without any problems. Of course, I wouldn't do that all the time, but once in awhile I would, and I would be happy.

COHEN: Well, let's back up for a minute, then, because you said Julia was the one that would kind of encourage you to take some time off. First of all, how did you meet Julia?

MORAES: How did I meet Julia? That was another one of my beach incursions. So I was there without a car for some reason, and so I got a ride with someone back to São Paulo, and she also got a ride with the same person. So we were just sitting in the back seat for two and half [hours] talking, but that was soon before— Yeah, that was interesting. That was actually five years before '87. So it was like when I came to the United States after my bachelor's. So I just met her

there, got her phone number, but then I traveled to the United States for three months and I lost contact with her. So five years later, for some reason, it was a weekend and I was in São Paulo, I didn't travel, and [I had] nothing to do; [I] went through my book looking for girls, and there was that name that I always remembered, because she made a big impression on me five years before. I thought she was really cute and everything, and nice, so I just called her, and I don't remember what I said. Something stupid, probably, that I found her name and I didn't know who it was. But eventually we went out, I think a few days later, in the middle of the week. She was at the end of a relationship that was not working for her, and maybe I speeded up the breakup a little bit, and we started seeing each other.

COHEN: And the rest, as they say, is history.

MORAES: It's history, right.

COHEN: But you said that she came to New York with a round-trip ticket. She was not going to stay, and you ended up staying.

MORAES: She wasn't sure. Neither was I, because we met six months before. So we first moved to this sublease, a really cute place, an apartment in Washington Heights that is close to Columbia [University College of Physicians and Surgeons] medical campus.

COHEN: That's sort of the "in" place now.

MORAES: Is it? I don't know. It was not the "in" place, but also it was nice. Everything was new for us, and we were together, and we didn't know anyone else outside of work. So that really created a very strong bond, and— I can talk about this, because it's interesting also. So after several months, she was on a tourist visa, and we had to do something about it, otherwise she would have to go back. So one of the options, maybe the only one, was to get married, so she could get a J-2; I had a J-1 visa. That was for an exchange scholar.

COHEN: Exchange student.

MORAES: So we did get married in July '88, in the city hall of New York, without any— My parents were in town by accident. They just came to visit us, and we got married, and that's the funny thing, because then I had already made arrangements to go to Canada with my parents. So after getting married we went on our honeymoon with my parents.

COHEN: [laughs] That's a good story.

MORAES: So things were working out, and we got married, and it was okay. We had some rough times, maybe the first two years, four years, whatever. The usual couple thing, but we survived all that, and now we're pretty stable. And we have a daughter, as you know.

COHEN: As I know, yes. Okay, but I gathered this morning that Julia does some kind of physical therapy work? So did she go back to school while you were here?

MORAES: Julia loves school.

COHEN: She loves school.

MORAES: She loves school, so in New York she went first to Parsons School of Design, because that was related to what she was doing in São Paulo, but at the same time she also likes to exercise, so she starts exercising, teaching exercise, aerobics, personal training, etc. She became more and more interested in that, so she went to another school. She went to Columbia [University] Teacher's College for a master's in physical education and exercise physiology. So she did that, and meanwhile she was doing more and more of the exercise thing. When we first moved to New York, she babysat, actually, for almost a year. So she always worked, really, and then she worked in a store for a while, until she started teaching more aerobics classes and doing more personal training. So then when we moved here, she went to physical therapy school to get a master's. University of Miami has a very good school, and she got in, and she finished that, and she started working full time as a physical therapist for two years. Now, when we had our daughter Emily, she slowed down a little bit. She cut it to part-time, and now she is in between jobs, because she was tired of the place she was working at.

COHEN: Okay, so you ended up staying at Columbia, not only for the Ph.D., but for your postdoc as well.

MORAES: Yeah, well, it's not a real formal postdoc, I would say, because it was the same group, so I just stayed a little bit longer after my Ph.D. You can call it a postdoc, but I was just doing more of the same. Now, I don't know if you want to talk about this today, but then I made a decision: even though I was thinking about going for a postdoc, I had this job opportunity that I decided to take here, the main reason being that I thought that I had maybe not as much experience as I could, but I had more than a Ph.D. experience because of my training in Argentina and Brazil. Of course, I realized at that point that I'd have to make an effort to keep

learning, because otherwise maybe my training wouldn't be enough for a very good career. But I would say that the period I stayed in New York, of course we are always doing new things and new techniques, but it was not a real formal postdoc.

[END OF TAPE 1, SIDE 2]

COHEN: Deciding to come to Miami is really a big decision, because it means you're staying in the United States. How did that decision come about?

MORAES: Well, towards the end of my Ph.D. I think I [had] already made the decision that I was going to try to stay here. I was doing well in my field; even as a student I think I was already being recognized as a force in the field. So I thought I had to seize the moment and continue to work here. Now, I could always go back, but there is a rule of thumb that if you stay more than two or three years, it becomes really hard to go back, just because of the way research is done here and done in Brazil. Things are very difficult over there; to buy reagents and all that, the critical mass, and other factors make research much more difficult. So towards the end of my Ph.D. I had already decided that I was going to look for a job, but of course my original thought was that I was going to go for a postdoc, and I was considering going to David [A.] Clayton's lab at Stanford [University] when this opportunity came up, so I took it.

COHEN: What went into your decision to come here? I mean, you alluded to the fact that you weren't sure you could—

MORAES: Well, the way this opportunity came up was that there was another fellow in the lab called Michael [P.] King, and he was looking for a job. The chairman of neurology here [University of Miami], Dr. Walter [G.] Bradley, was determined to start a neuromuscular group, and he wanted to hire someone doing mitochondrial diseases, so I guess he heard that Michael King was looking for a job, and he sent him a letter offering him a position. But Michael wanted a basic department; he didn't want a clinical department like neurology, so he didn't want the job, and he handed me the letter and said, "Are you interested?" So I said, "Yeah, let me look into it." And there was a meeting of the American Academy of Neurology soon after that where I met Dr. Bradley and we talked, and he is good friends with Salvatore DiMauro, [whom] we all call Billy DiMauro. Billy loves me and said the world about me, and Dr. Bradley offered me a job. It didn't take too much money to bring me, as [I was] a really junior junior kind of person. My startup was okay; it was not the same as people in a basic department will get. There was another recruit here [who] was a little bit more senior than I was, and I guess he got a better setup, but that was fine with me. Now Miami, as a city, is the last place I thought I would end up, because that's where Brazilians come to do shopping. So when you think about doing research in the United States, you don't think about Miami. On the other hand, it's a little closer to Brazil if you have to fly, and it's still the United States, so the way you do research, the

funding and all that, is still here. I don't know if [it was] a little reluctantly, but we decided to come, and so far it has worked out okay, so I'm happy with the decision.

COHEN: Okay, so you've been here it looks like about four years.

MORAES: No, seven and a half.

COHEN: Oh, I'm sorry. I was looking at something else. Yeah, seven and a half. How do you like Miami?

MORAES: Now I like it a lot because, I guess, of one thing: windsurfing.

COHEN: So you're still at the beach?

MORAES: Yeah. Miami is quite boring in terms of geography. There are cultural events also, but not that many, and I don't think I would do much of that anyway. I like to be in a place where there are lots of cultural events, but if I count how many times I went to those things in New York, I could use two hands. Of course I would go to the theater and things like that, but not very frequently, so it's hard to justify being in a city just because of that. Anyway, here you have to take advantage of what the city has to offer, and Miami, to me, is only one thing: the ocean. That is what you have to take advantage of. So a few years ago I started windsurfing, and I got the virus, I got addicted to it, and it's my hobby now. That's one thing besides science that I really enjoy is windsurfing, and if it's really windy and I'm not too busy, I will even take off in the middle of the day, but those are rare days. Not that I can take off, if it is windy.

COHEN: Well, we're going to spend, like I said, tomorrow talking about your life here.

[END OF TAPE 2, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Carlos T. Moraes**

INTERVIEWER: **Helene L. Cohen**

LOCATION: **University of Miami**
Miami, Florida

DATE: **14 March 2001**

COHEN: There are a couple of things I wanted to go back to and kind of finish up from yesterday, and then we'll move on to today's work. One of the things I didn't ask you yesterday was whether you had any religious training growing up.

MORAES: My mother [Dirce T. Moraes] is Catholic. She would go to church on Sundays; maybe not every Sunday, but quite frequently. So were her parents [Guerino and Isabel Torres], I guess maybe more her mother. My father's side was also Catholic, but my father [Waldyr Rodrigues de Moraes] never really cared much for religion, so he wouldn't go to church unless he had to. But he was pretty neutral in terms of what kind of education we got, so I guess he was okay that we would go to church. As far as I can remember, it was always a drag for me to be there. I just was waiting for the time that I could sit down or kneel— I hated to stand up—but that's all I can remember. It was really too long. So my upbringing was not very religious. My mother was not very imposing on that. She has her beliefs, but she never imposed too much on us. When I was a little older, maybe eighteen, I became part of a group that one might call even a cult, that— Let's see, how I can describe it. The name was Pro-Life or Pró-Vida in Portuguese. It was founded by a physician that— Initially there was a lot of emphasis just on doing—how do they call it?—paranormal studies or telekinesis and things like that, and that's how I— I guess my mother got interested through a friend of hers, and then we went and they had courses. So it was interesting. There was a whole philosophy, similar to Buddhist philosophy, I would say, but there were also some kind of fun exercises of trying to do these telekinesis things that maybe was all placebo effect, but we thought it worked.

COHEN: You mean like bending spoons and things?

MORAES: Well, we wouldn't get that far, but trying to close your eyes and push people around and that kind of stuff.

COHEN: Oh, okay.

MORAES: But it developed— I don't know if as the course developed that's the way it was supposed to do, but anyway it became more and more philosophical as you advanced through the courses. They also has some interesting exercises of breathing and trying to get your conscience into alpha waves so that you can experience different things. It was kind of weird stuff. So this was interesting. I mean, there were some breathing exercises that were very, very intense, so you really get completely high at the end, and that was following music, so it was a trip. The instructors tried to direct us to some kind of trips that I would not necessarily go into. Not that they would tell us what to see, but if we were in alpha waves— I don't think I remember the details, but maybe I could detach from my body a little bit. Stuff like that. But it became more and more philosophical; I became quite involved up to a point, and they had— The courses were paid, but they never really tried to extract money from us. It sounded like a legitimate kind of exercise, and they also did a lot of donations to charity and things like that, and organized things like that. So it was a valued thing, and it was interesting. My brother [Sergio T. Moraes] also got involved at a certain level, [and] my sister [Silvia Regina T. Moraes]; my father, completely skeptical, even [him] we convinced to do the basic course, but he was completely against some of the premises. Like, for example, they would say, "If you truly believe that you're going to have a parking spot in front of the movie theater, you will find one," and my father thought that was completely absurd, because if everybody [thought] like that, there wouldn't be enough parking spots. So he was always very skeptical about this. With time, I guess, I lost interest, mainly because they became a little bit too closed. They were stimulating too much socializing inside the group. Not that they would say, "Don't socialize outside," but there was too much emphasis on doing things within the group, and that became a little annoying to me, so I just lost interest. My mother and my sister had already lost interest before that. They, I think, are still very popular in São Paulo, and they grew tremendously. This guy that founded it was a physician named, I think, Celso Charuri or something like that. I can spell it for you later. He became kind of a god figure because he died very young when this group was growing very fast. So all kinds of things came out of that, like his energy was around, etc. etc. When he was going to die, people actually knew, blah, blah, blah. So there was that aura of a god that he got, and that became a little weird also, but the philosophy was not too weird.

As I said, it was very similar to Buddhism: respect life and do good things and good things will come back to you, so it was not a too crazy kind of a cult, but I think it can be considered a cult. There is another one in São Paulo that is quite— I don't know if "big" is the word, but it's called Rosa Cruz. *Rosa* is from the flower rose, and *cruz* is cross, and I think this is an international thing. It's almost like a masonic kind of [thing], and this Pró-Vida was similar in many ways to Rosa Cruz in terms of the philosophy. So I think that was as much into religion/philosophy that I got into. I also, during my late teens or twenties, read quite a lot of books by Carlos Casteneda. I don't know if you read [them], but I think his first book, in Portuguese, was called *Erva Do Diabo.* That's *erva* meaning "herb" and *diabo* is "devil". So he was basically describing experiences with the peyote, that is, mescaline. His other books became much more philosophical and less related to drugs, but [rather] his experiences in Mexico with this mentor of his called Don Juan. So I had an interest in that kind of philosophy, but I guess the more scientific I got, the less philosophic[al] I became, and I think in terms of religion, that's as far as I went. My mother still goes to church and does things for charity, for

the church, and I try— We have some discussions, but of course I never try to tell her that I don't believe in any of this. My father— You know, I just realized that he doesn't believe in any of it.

COHEN: So you don't practice anything now?

MORAES: No.

COHEN: Because one of the interesting philosophical questions is whether God and science— Or how God and science can coexist, if they can. Any thoughts on the subject?

MORAES: Sure. I mean, the definition of God is probably the key aspect of any kind of discussion related to God and something else. I'm sure one can define God in a nontraditional way that is just, you know, nature forces making all these [things] happen, but maybe most people consider God as being a non-random force. So I believe more in a random force, like evolution happening just because of the way it works, by natural selection. So, you know, if I have to use a definition of God that the church uses, I would say that I don't believe in God. If I have to say that God is something that has a purpose, it can be whatever it is, but it has a purpose, then maybe I also don't believe in God, because I believe in the randomness of how we develop as a planet and as human beings. But even if you believe in God as something that has a purpose and starts everything with a purpose, I still don't see why that would be incompatible with science. You can believe that the purpose used the laws of nature to build whatever the purpose was. Yeah, I can't see how some religious people would deny basic knowledge that we have in evolution and life science, so the ones that want to keep religion alive had better adapt, I think, to what we know, because it's very clear, for most reasonable people, how things happen.

COHEN: Okay, thank you. Another thing I didn't ask you about your childhood was whether you had these—here we call them extracurricular activities, like music lessons and art and that sort of thing.

MORAES: I guess when I was a little kid my parents would take me to, like, clubs and I would do things like judo or some kind of exercise, but I lost interest very fast in those things. When I was a little older, maybe thirteen or fourteen, I started doing Capoeira, [which] is a very interesting dance/fight. It's really Brazilian; it started with the slaves. I don't know if you've ever seen [it], but it's very nice to watch, because as a show it's like a dance, but it can be a pretty violent fight. That was great exercise and a lot of fun and I did it for a few years. There were some championships, but those are less of a dance and more of a fight, and I guess they scared me a little bit, and after a couple of those [where] I saw people getting hurt and I had to really fight, [which] is something I don't like, I dropped out. I guess at about the same time I

was getting more interested in surfing, and that became, I guess, my main hobby for many, many years, maybe from seventeen to the time I came to the United States. So almost everything I would do in my free time, I would travel and then try to do some surfing, [which] was very cleansing for me. If I was stressed out, to go into the water and just fight the waves, it was just great. With the lifestyle of a surfer I did some related things, like I started playing the guitar, but never got any formal training, so I just learned with friends and [we would] play together. The harmonica, also, we played. What else in terms of extracurricular activities? I think that's it. In São Paulo I really wouldn't do much, so my hobbies and my activities were—So after I was sixteen or seventeen my parents didn't have much influence on what kind of extracurricular activities I would have, and I just did whatever was fun for me.

COHEN: Now, you mentioned this beach you went to that was, like, two and half hours from São Paulo, but I actually can't picture where São Paulo is on the map. Are there closer beaches that you could get to when you were younger?

MORAES: São Paulo is in the southeast of Brazil. It's about 60 to 80 kilometers from the water, from the ocean. The closest town is called Santos, but that's a port city, quite busy and unattractive. But actually, when I started surfing, I would go to many different places. Maresias I actually started going to a little later as a more constant thing during the weekends. When I started surfing, I would travel to many different towns on the coast, all within two to five hours of São Paulo. Some of these trips were really very adventurous, because there were no paved roads, and I had to drive through the beaches or very muddy roads, and [there were] very few people, as you can imagine, because of the conditions. So it was really a great time.

COHEN: So at what age did you start driving?

MORAES: I started driving at eighteen.

COHEN: At eighteen.

MORAES: But I started traveling with my brother and friends before I could drive. Actually, there were some interesting experiences, also, before I started going to Maresias. Maresias is in the city of São Sebastião, [which] I already mentioned to you, and that's north. It's closer to Rio de Janeiro state. I also used to travel south, and there is a town called Peruibe, it's south of São Paulo, and after that it's completely undeveloped going south. There are no roads and no nothing. There is actually a national park there. We used to do some trips there [where] we would drive as far as we could south and then just take our surfboards and walk through trails in the jungle. [There are] lots of mountains there; it's not flat like here in Florida. So we had to cross two groups of hills to go two beaches away, and we used go to this one, there were some

fishermen living there, people that always lived there, and we would stay with them in very primitive conditions. But that's the spirit I was in at that age. I really wanted to get out of the city and be in this place, even though I was devoured by mosquitos, but I would adapt. I would adapt relatively fast to that kind of lifestyle, and I remember it was just fantastic to be in a place like that. Sometimes we would go even further. We had some trips where we had to walk, like, for two days, for example, to get to some very isolated place that was just beautiful. So those were great adventures that surfing kind of allowed me to do; I was part of it because our goal was always to find some nice waves, but on the way we would experience everything else that was there.

COHEN: Sure. Well, if it took two days to get there, these weren't weekend trips.

MORAES: No, those were vacations or holidays that were, like, five days in a row.

COHEN: Now, yesterday you said that there was an impact on your career from the fact that you did less studying and more traveling, but you didn't elaborate on that.

MORAES: Well, the impact I had—I think I mentioned it—is that— It's not that I did more traveling, less studying; [it's] the fact that I would go to a particular beach. That's how I met that friend of mine that wanted to measure carnitine.

COHEN: Oh, I see. Okay.

MORAES: So that had a major impact on my career, because that's what brought me to the States, and [it] was completely circumstantial and by chance.

COHEN: I guess you said it and then you told the story later, so I didn't connect the two.

MORAES: And that's how I met Julia, also, so it altered my personal life.

COHEN: Sure. When you went to New York, even though you had traveled a lot, that was really the first time you were living away from home on a permanent basis, right?

MORAES: Second time. The first was in Buenos Aires.

COHEN: Oh, that's right. Okay. Well, Buenos Aires is a big city as well, but what was it like landing in New York for you?

MORAES: I was always very adventurous in this kind of thing, so I was never scared of those kinds of challenges. So it was very exciting. Everything was new; I had been to New York already, but that was a different setup now, and I remember that maybe the first day I went to the top of the building there that was at Columbia University, and I saw the Hudson River, the George Washington Bridge, and those— It was just very exciting to be there, and I was looking forward to whatever was going to come up. So as you said, I had traveled quite a lot, so I did not have any problems in adapting to a different lifestyle. That was easy.

COHEN: Well, New York can be a little bit of a culture shock, though. I know; as I mentioned to you, I lived there, and I know that for me there were a few things that took some getting used to, like the fact that the superintendent of the building does nothing unless you bribe him. Were there any culture shocks for you, or was it pretty much—?

MORAES: Amazingly enough, maybe for someone coming out of São Paulo, it is less of a shock than coming from someplace in the United States. One of the things that always struck me as quite different here in the United States is that everything is about money. Even though São Paulo is a big city, it's different. People do things sometimes just to exchange favors, or poor people will give you things for free, and here, particularly in New York, it's all about money. Everybody is trying to make a living. Everything is very expensive, so I understand that. So that was a bit of a difference, but still, it was not too hard to adapt. I was in the Columbia housing, so I guess I didn't have to bribe the super, but I also didn't get much. We paid a lot for housing there, and you don't get much for it.

COHEN: A couple of other things you mentioned yesterday—

MORAES: Maybe that's why I never got anything done for myself.

COHEN: Maybe.

MORAES: I didn't know I had to bribe the super.

COHEN: Just a couple of more things from yesterday, and then we'll move on. You mentioned that this [Armando J.] Parodi was a role model for you, and then you said that you had other role

models besides him. Were you referring to the few people in New York, or is there someone else?

MORAES: Someone else.

COHEN: Okay.

MORAES: Someone I met more recently, about seven years ago. A year after I was here in Miami, I decided to start a project, and I needed to collaborate with a fellow, and his name is Alex Tzagoloff. He is at Columbia, but not at the same campus. He is at the main campus of Columbia, in the biological science department. He is quite unbelievable. He is probably fifty-three, fifty-five, and has been one of the most productive people in the field of mitochondrial genetics, working with yeast. Until now, he spends most of his time working on the bench. He has a relatively small group, but all the work that comes out of his lab is very significant, because he defines very basic genes and how they work. He has all these yeast mutants, and I think NIH [National Institutes of Health] has recognized that; he has this so-called R37 grant, that's like for an outstanding scientist. I don't know exactly how they renew it, but I'm sure it's almost like automatic for someone like him. By the way, David [A.] Clayton also had one of those. So I went there and I spent two to three weeks in his lab trying to start the project. He came from Russia a long time ago, and I was just very impressed with his sharp sense of humor, strange at times. But the way he was always working in the lab, at that stage in his career, I find these things so hard to do that I admire people that can manage to do so. I'm finding it harder and harder here, and I'm much younger than he is.

COHEN: Okay. Well, we're going to go onto that in depth.

MORAES: Right.

COHEN: Okay, the last thing I want to ask you is, you made a very brief allusion to being in some kind of trouble with the Brazilian government. Was that over immigration or what?

MORAES: No. Actually I was not. I could have been, because I got a Ph.D. fellowship before being officially accepted to [a Ph.D. program], but I think I remedied that by being accepted to a program eventually. But I did not do things in the right way to start with. But that's the extent of those potential problems. The visa I never had a problem with. I don't know if you want to discuss later how I changed my visas and how—

COHEN: Actually, maybe this is a good time.

MORAES: Well, I came with a J-1 visa; that's the exchange scholar. Some students can come with that also. So I did my Ph.D. with a J-1, and then when I came to Miami, to be hired as a faculty member, I needed a green card, and I applied by myself for a green card using this outstanding researcher mechanism. That's a straightforward, fast-acting mechanism that they have, and you really do not have to be an [Albert] Einstein to get it. It worked very fast. I just put a dossier together, and they asked you to just demonstrate that your work has been quoted by others, you've done some important things, you have letters from different people from different countries saying how great you are, and you just put the dossier together, and within about a week or two they approved it. So it's really a very fast mechanism. So I had a green card for, I think, five years, and last year I decided to become a citizen. I can keep my Brazilian citizenship, so I have both.

COHEN: Dual.

MORAES: And the reason I decided to is because there are some strange laws that do not give too [many] rights to green card holders. For example, not that I'm going to shoplift, but if I'm accused of shoplifting, I could be deported without trial.

COHEN: Oh, I didn't know that.

MORAES: And any other petty crimes like that, even if they were committed ten years ago, they could be grounds for deportation without a trial. So you, as a permanent resident, really don't have all the rights that citizens have. So I thought it was a good idea to get it.

COHEN: Plus you probably wanted to vote here in the Florida election for the president.

MORAES: Yes, and I did vote. Probably my vote went to Pat Buchanan, but I tried to do my best.

COHEN: [laughs] Well, you know, about two years ago, maybe not quite that long, there was actually an article—I'm trying to remember if I saw it in *Science* or *Nature;* I think it was in *Science*—about the influx of foreign scholars and whether this was good or bad for American science. So, being one of them, what do you think about that?

MORAES: That's an ongoing debate, I think. In favor of allowing scholars to come, I think, is the shortage of people here. I have a lot of trouble hiring postdoctoral fellows, for example, and if I can get a good one, [it is] almost certain they're going to be foreign. So there are not many applicants that come from the United States. So sure, some of the top positions will be taken. There will be more competition for American citizens, but I think there is a demand for it right now, and until— Competition is one of the hallmarks of why the American system works well, so curbing competition might not be necessarily good. So that's my view: Keep it open as the United States keeps everything open, and it works. It's a pity that not more Americans are going into science, for example. I think there's a real shortage, and I talked to my colleagues, and unless you're running a big lab at Harvard or Stanford, it's really very hard to get good American postdoctoral fellows. Students [are] easier to get, but even students— I'm on a steering committee for a graduate program here, and the best candidates are foreigners, so also at that level there are not many Americans that are willing to go into a science career. Maybe because of all the pressures involved in it, the uncertainty about funding—and I'm sure we're going to talk more about that—and the low pay relative to other jobs. Maybe now with the market downturn this will change; we'll see that it's not so easy to make money.

COHEN: Well, in academics, but in biotech[nology] these days, it's easier to make money .

MORAES: It was, right? Yeah, it is, but it's still risky. But I think we see more Americans going through those channels just because maybe it's riskier, but it's much better pay. Americans have a need of quick satisfaction.

COHEN: Instant gratification.

MORAES: Right.

COHEN: Yeah. Okay, well, let's talk about being a PI [principal investigator] then, okay? So there are many things you do, and I kind of want to go through them one at a time, and actually, since you've talked about the money situation, let's start there with the chase for money, which mostly involves grant writing.

MORAES: Okay. I am in a clinical department, the department of neurology, and that poses one extra problem, that is, the lack of institutional support for my salary. So I'm expected to bring in all of my salary from grants.

COHEN: One hundred percent soft money.

MORAES: One hundred percent, right. If you are in a basic department here, I think they will pay between 50 percent and 70 percent. They expect you to bring as much as you can, but they will still pay at least half of your salary. Not only that, but basic departments have a better infrastructure, shared equipments, and things like that. So I had to deal with these issues. So with the second issue, early on I tried to associate myself with the basic departments here and make as many friends as I can and get involved with the graduate programs and things like that. That has helped me in terms of recognition by colleagues that do the best science here. In terms of salary support, I had to go and get NIH funds, and I have been very lucky, I think, with NIH, because for my first application for an RO 1, I got funded. I could have gone to the R29s; those were beginners' grants, but they're judged very similarly to RO 1s that are more for established investigators. As much as they eliminate nowadays R29s, so I just applied for these grants called RO1 s, [which] are research grants. So I got funded by the National Eye Institute less than a year [after] I came here, I think. So I had a little startup. I had my salary guaranteed for a couple of years, plus a technician. So I got funded by that, and I think the same year I got funded by the Muscular Dystrophy Association, MDA. That will not pay my salary, but that will pay for a technician and research, so I could start to expand. When I started, there was someone that was a little bit more senior who was recruited at approximately the same time that I was. He was working with dystrophin and dystrophin-related problems and gene therapy of muscular dystrophy. He was not as lucky in terms of getting grants, and after, I think, four years or so, he had to leave the department because he had no funds.

COHEN: That's scary.

MORAES: Yeah, that's scary, but in this particular case, I benefit[ted] from it, because all of his space became mine, because we were kind of together, and a lot of his equipment became mine also, because they were neurology startup equipment.

So yeah, it was not good for him, but his bad luck worked in my favor, even though I had one less colleague to collaborate with or talk to at least. But later, maybe five years ago or so, I got another RO1 from the National Institute of General Medical Sciences. We can discuss more about the specific projects later. So I never had much of a problem in terms of bringing in my salary or funding people in the lab. I never had to fire someone because I was out of money. That doesn't mean that it's not going to happen. It's actually likely to happen, because now I have two additional RO1s, so I'm very heavily funded now, I think, with four RO1s. I've never had that many.

COHEN: You have four now? Oh, wow. I saw three on your resume and a couple pending.

MORAES: Yeah, well, one I've heard about, but it's not active yet.

COHEN: And you have two others also.

MORAES: Besides the one I mentioned, I have one that just started from NCI, the National Cancer Institute, and now I just heard that I got one from the National Institute of Neurological Diseases and Stroke.

COHEN: Right, but you have two non-NIH grants also.

MORAES: That's the Muscular Dystrophy Association, and I had one from the National Parkinson's [Disease] Foundation. That kind of expired, but I keep carrying it forward, because they let me do so, and I have quite a bit of money for supplies now, so I do not have to use that money. So I was lucky in terms of not having the problem of not having money. That doesn't mean that the anxiety was not there. I have always been very careful with money in all aspects of my life.

[END OF TAPE 3, SIDE 1]

MORAES: Yeah, I can even say that I was probably cheap in most aspects of my life. My family never had too much money, and my father was quite strict in limiting our luxuries. So we had a stipend after a certain age, and I had to work with that, and then whenever I could I worked to make some money. But I was always very careful, and that reflected right into the way I run a lab. So I always tried to save as much money as I could. I was frugal with spending, and I tried to get a bargain whenever I could. So I always had that money, but the fact that—This way, because I'm anxious about not having the money, and seeing what happened with that more senior investigator, that increased the anxiety levels quite a bit. But I also learned that you cannot let your life be run by that kind of stuff, so you just try to forget about it. And usually that's the good thing about the United States; if you have good ideas and you can do some work, there are quite a lot of opportunities out there for you to find money. As you get more experience, in theory, you should be better at getting grants and things like that. So now I'm trying to spend money more than thinking about how to make more money. I will definitely not be writing a grant for at least three years now.

COHEN: Yeah, because you've got quite a bit of time left on almost all of them still.

MORAES: Yeah, the oldest one is one year old.

COHEN: Yeah. A couple of things about the grant writing process: First of all, it occurred to me last night that you could have been a Pew Latin [fellow] at some point, because the Latin [fellows] are funded for their training with the intention to go back to their countries.

MORAES: Right. I wouldn't qualify for a number of reasons. The first one is that I had to be in Latin America when I applied, or at least I had to be doing a Ph.D. in a lab in the United States, but I had to be committed to go back.

COHEN: Right.

MORAES: That was not my situation, and by the time I heard about Pew I was already a faculty member, so I didn't have the option. That's actually a nice experience program.

COHEN: Well, except it does commit you to go back, [and] I don't know if you knew at the outset whether you wanted to go or stay.

MORAES: Well, once I decided to get a job here, I was committed to stay.

COHEN: So when did the Pew— You were in the '95 class, so you were already here a year or so by the time you got your Pew, right? And you were already funded from NIH, so was the Pew important to you?

MORAES: The Pew was extremely important to me. Not because of the money; the money is really not that much. I think we had— When we had [it] it was $50,000 per year. Now they increased it significantly, I think. It always helped, of course, particularly because it's a flexible source of funds, and we always need those. But the way the Pew helped me was to pair me with the best people in science; not only the other scholars, but during the meetings, the people that would deliver lectures, and just the whole environment was very rich, and that had a major impact on my career, for me to set some goals of where I want to go. I don't know if I'm going to reach those levels, but at least I have those standards, and that was very important.

COHEN: One of the things that occurs to me— You know, when you have to write—whether it be grants or papers, and we'll get into paper-writing a little bit later—you have to do it in English, and I know from my own experiences of speaking other languages [that] speaking it and writing it well are two entirely different things. How easy or difficult is for you to write in English?

MORAES: This is really interesting. The more senior colleague I had was very good at talking about his projects and getting people excited, but he couldn't write a decent grant. It's amazing how detached speaking or even being fluent talking about something is from writing. I have several American colleagues [who] cannot write very well, even though they know their areas very well and all that. So for some reason I think I write better than I speak. And I think that writing is not so much the grammar, but the ability to make things simple and clear and concise; I think that turns on reviewers. And also the ability to use images to illustrate your thoughts, and I think that early on I got good training just by working with other postdocs in New York on how to use computers to do graphics and things like that, and how to put text and graphs together. I think that helped me. We've been talking for a while, you already know my limitations in English, but when you write, spelling is easy to check. Grammar, I'll give to a colleague that is a native English speaker, and it can be corrected. Some mistakes might still be there, but I think from what people tell me, I have the ability to communicate things in a simple and clear way. So I never had a problem with writing, but I also always make an effort to give it to someone to review and correct, and I get lots of corrections. I think there are a lot of people that also think that they write well, so they don't really bother getting input from other people, and I think that's a big mistake, because there are always things that you just don't see. So everything I write goes through a number of revisions, and even when I feel confident that it's great, I know that it's not, and someone should look at it and correct [it], and I think that helps my final product.

COHEN: Well, actually, I found your papers quite readable, which is not always the case.

MORAES: Thanks, and those are quite specific topics.

COHEN: So do you enjoy the writing? Is it something that's pleasant for you?

MORAES: Yes, I do enjoy it. I learned a lot in New York. DiMauro was a good writer, and Eric Schon was a good writer. With Eric, we used to sit together in front of his computer and he would kind of write it, but he would engage you in such a way that you learned just by being there. Of course, you would be suggesting words and things, and you feel that you're really participating. He would use your words when you suggested one, but it was a very efficient way, because he was writing, basically, and you were just there helping. I've been considering this technique instead of letting students just write, and then you have to rewrite it because it's pretty bad. That's a valid method also, and maybe you can learn more from that, but I'm wondering if you cannot learn more first by just being engaged in the process with a senior person, and maybe at that point then you go and write your own, but you already have the basis and you see how a senior person would do it. So I'm trying; on some new papers that I have to write, maybe I will use this technique with the younger people and see if it works.

COHEN: As long as we're talking about grant writing, I want to finish up this area by talking about—since we have an opportunity to learn from you about Brazil—what is the situation for a scientist in Brazil in terms of funding? Where does the money come from? Is it soft money, hard money? How does it work there?

MORAES: Well, I have been away from the system for a number of years, but from the contacts and information that I have, I can give you a good idea, I think. If you are in São Paulo, you will not have much problem in terms of getting funds, and that's because the State has a law that dedicates a certain percentage of the gross product to this research foundation called FAPESP, Fundacão de Amparo a Pesquisa do Estado de São Paulo.

COHEN: You'll give me that later.

MORAES: Yes. So people that do decent research have absolutely no problems getting money from FAPESP, and they actually help you in important things and getting things fast. So money, I don't think, is a big hurdle for good groups there. What I think it is is the critical mass and the pressure you have to produce that is really not there.

COHEN: The pressure here to produce, you mean.

MORAES: Well, in Brazil there is not much pressure. Of course, you can be self-motivated and do it, but there is a tendency that if you produce too much there, people in the university are probably not going to like it too much, because you start to become too good.

COHEN: You make them look bad.

MORAES: Right. So that kind of negative pressure exists there, and some groups, some universities can deal with it better than others, and there are some centers in São Paulo that are very good. There are some centers in Rio de Janeiro and also in another state, Minas Gerais—that's also in the southeast—and in the south of Brazil. But if you go outside of São Paulo, things get harder, because investigators do not have state money, at least not a significant amount, and they have to rely on federal funds. Now, federal funding in Brazil has seen many ups and downs. Once in awhile they come up with a program that infuses lots of money on the federal funding agency that's called CNPq [Conselho Nacional de Desenvolvimento Cientifico e Tecnologico], but it's always full of bureaucracy, lots of delays between awards and receiving the money, and that problem is compounded by inflation. Now it's not so much, but in my time you would get $50,000 in Brazilian money, but they'd take two years to give you the money,

and that amount of Brazilian money now became $15,000. So that was always a problem, and people struggled, but they give fellowships. I came with a federal fellowship to do my Ph.D., and they try to improve, and they're trying to keep people there, also, by giving fellowships now that do not allow you to do all of your Ph.D. abroad. You do part of it, just like a couple of years, but you have to go back and defend your thesis in Brazil. So they call that sandwich fellowships, and I guess that retains more people there, even though eventually people leave for a postdoc and they might not go back anyway. But they have all the problems of a heterogeneous third world country. You know, they have some areas of Brazil that are extremely rich, other areas that are extremely poor; the money is somehow distributed, and there are many problems related to [those] inequalities.

COHEN: The last question about money, then, is: I know you have tenure now, but what does that mean if you're on 100 percent soft money? What, if anything, does it mean?

MORAES: Right. I can use, as an example, a colleague in the department that has tenure and ran out of grants. What happens first [is] that your salary goes down quite a lot, because your tenured salary is just a percentage of your salary.

COHEN: So there is some salary guarantee once you reach tenure.

MORAES: Right.

COHEN: Okay.

MORAES: So your salary will go down if you lose your funds, you lose everyone in the lab because you cannot pay technicians or students, and there is a lot of pressure from the department chair. In this particular case, the department chair either tries to find some kind of administrative job for this person to do, help with the website or do something, but the pressure is very high for this person to regain funding. Of course, the chairman will start to rethink the decision of supporting tenure for different people if people lose their grants, because the idea of giving tenure is that that's not going to happen, but it does happen. In a way, it reduces a little bit of the anxiety, because you know you're not going to starve, but if you have a desire to keep a competitive research lab— You know, if you lose your funds, it's the same as your group dying, basically.

COHEN: Well, some places will provide you with some bridge funds for a year or two until you can get refunded. Do they do that here?

MORAES: Well, that's the other problem of being in a clinical department. That kind of money is really not there. I guess the chairman could make a plea with the dean and try to find some special arrangement, but it's not very likely to succeed if the person is already not bringing indirect costs. So if you're at a more high-powered center and the institution really believes that you can make a comeback and they have the money, that would be great. But here in a clinical department it's hard to see that happening.

COHEN: Well, since we touched on paper-writing, let's go there for a few minutes. You said you're rethinking how you do this, but it sounded like right now what you do is you have the students or the postdocs write the first draft.

MORAES: Right, and then I go through it, or sometimes you have to basically redo the whole paper, so that's why I'm questioning if that's the best way, if they're really learning just by you telling them, "Don't do it like this." So at least for the initial ones, it might help for them to see how to do it, because they really don't have much of an idea if someone never— Or if they wrote only one paper and it was a struggle there, they don't even remember how they did it. So this is like teaching. What is the best way to teach?

COHEN: Right. Well, you said you've got a lot of foreign postdocs and some foreign students, but then you have the issue of language again. I mean, actually, some of the scholars I've talked to, when they came here as students, they didn't speak any English at all. So can most of your students manage a first draft?

MORAES: Yeah, they do. I mean, I wouldn't have a fellow or a student who does not speak any English at all, mostly because I wouldn't be able to recognize the person's ability to do science and things like that.

COHEN: Right.

MORAES: It's a common problem of how to judge fellows from China, for example. It's hard for me to tell if someone is really good or not, just because of the inability to communicate sometimes. Like as a student now, for example, I have an Indian [Sarika Srivastava] girl who speaks perfect English.

COHEN: East Indian, not American Indian.

MORAES: Yeah, East Indian.

COHEN: Okay.

MORAES: I wish I had an American Indian. We could get a fellowship very easily. I have a Greek guy [Ilias Kirkinezos] who also speaks English very well, and I have another rotating student [George Theodore] that is Greek also. As fellows I have one woman from Venezuela [Francisca Diaz], and she speaks English very well. I have a German guy [Markus Woschenck] who speaks English very well, and I have an Argentinian girl [Corina Van Waveren] [who] speaks English very well.

COHEN: So you have no American students at all.

MORAES: No. Now I have a technician [who] is from Spain [Jose Oca-Cossio] who speaks English very well also.

COHEN: Okay.

MORAES: I had only one American student, postdoctoral fellow, that was actually a bad experience. He was the worst one I ever had. He was a very bright person, probably one of the brightest I ever had, but he was just not committed enough for this kind of job. So, like, between experiments he would be reading novels, so his mind was not focused or concentrated on the project. So he stayed four years with me, because of my inexperience and inability to fire people; I didn't fire him, but I think after a year and a half I knew that he was not going to work out. In four years we had published a single paper, so that's pretty damaging to my program. Again, he was very good for certain things, but he was just not good for this kind of job, because his heart was not there. In research, you have to dive into it, you cannot do it half-assed.

COHEN: You can't wade in. You have to dive in.

MORAES: Right.

COHEN: Well, that brings us to another thing that PI's have to do, which is lab management, something that you're totally unprepared for, as a rule, when you first get your lab. I wanted to ask you— First of all, you mentioned yesterday that one of the things about Parodi was that he was well-liked. Is it important to be well-liked as a lab manager?

MORAES: I think so. Do you remember yesterday that we were there on my floor and there was a woman screaming?

COHEN: Yes. [laughs]

MORAES: She runs another lab on that floor.

COHEN: She's another PI?

MORAES: Yeah. I think she believes that by scaring people, productivity remains good or high, so she keeps everybody on their toes. I don't think people in the lab particularly like her. They might be afraid of her, and also I don't think that being afraid reflects respect, either. So that's one way that [one] absolutely cannot run a lab. I think that if people in the lab do not respect you, it's going to be hard to get things done and to have open conversations and things like that. If you want to give them directions, they have to respect you to follow your directions, otherwise they won't. But the way people run labs, I think, is mainly your personality traits and how you run life in general. So I try to be collegial with everybody in the lab. With experience I am learning that maybe it's not a great idea to be too friendly; maybe you should keep a little bit of a distance, just because, as the lab grows, inevitably you end up with preferences, and maybe how you treat some people over others, just because it's easier to talk to one than the others, and that's something that I try to avoid, at least how it is perceived in the lab. I try to make it as fair as possible. And of course, if you socialize too much with people in the lab, that might exacerbate this kind of difference. So that's something I used to do more in the beginning, and I think most PI's do a lot in the beginning, and then you do a little bit less, just maybe because you become more professional and that's a more professional way to run the lab. So I think it is important to be liked. I think the only way that people will work hard for you is if they work hard for themselves. So I try to keep them motivated as much as I can, and I try to read what their goals are and try to help them define and try to give them the opportunity to reach those goals. That's how they work the best, but as an older professor told me, "You can take a brute diamond, like a diamond in a rock, and clean it and make a diamond out of it, but if you just take a rock, you can clean it as much as you want and it will never be a diamond." So some people are good for this kind of work, and then it becomes very pleasurable and easy to direct, and others you're just going to be hitting your head against a wall for years and years. And that's one thing that is one of my weaknesses, I think, as a lab manager, is maybe it is detrimental not being more honest with people that maybe should not be doing this kind of job, and maybe I will be even helping them by directing them to some different type of job. But I do have problems firing people or letting people go, and that's related to— I don't know. It's also a personality trait, and also this thing of wanting to be liked, and you feel sorry for people also, particularly foreigners. In many cases, they will have to go back to their countries if they lose

the job, if they are on an H-1 visa, for example. So that's an additional problem that is hard to deal with, but I can say here that the main problem of running a lab these days for me is finding good personnel. I spend a lot of time trying to hire people, and the percentage of people that I hire that are good is relatively small. Even though I try my best, that makes it very difficult to hire, so I have a lot of positions that are open that I cannot fill just because I cannot find qualified people. That's without question for me the main problem I have with running a lab nowadays.

COHEN: So you could have a bigger lab now, if you could find the people.

MORAES: Yeah, I think I have at least three positions that I could fill.

COHEN: Is any of that because this is Miami as opposed to Boston?

MORAES: Well, partially, but as I said, I've talked to people at Yale [University] and other places that are maybe not Harvard [University], but are still very good, and they have this same problem. So being in Miami, it's probably even worse. The best people you can get here, first of all, are foreigners for sure. Second, if there is someone that's coming, like a physician coming to do a residence here, and the spouse happens to be a scientist that has to come to Miami, that's when you strike gold and get the best people, [when] for some reason they have to be here and they need a job.

I am not unhappy with people in the lab, but few of them really would excel and bring new things to the lab. They would do things that I would tell them to do, but rarely will they reach the lab with new ideas and new techniques. But it does happen.

COHEN: Well, you said you've had a couple of role models. Do you pattern your lab after those people that you see as role models?

MORAES: Yeah, well, the couple were Parodi and Tzagoloff. I wish I could be like them, but I am not, but that's what I aim for.

COHEN: Well, you said that they were both at the bench, and when we get done talking about all these things to do, I think there probably won't be any time to be at the bench.

MORAES: Yeah. I mean, the administrative load I have is pretty large, and I don't know how they manage, so maybe that's what I have to learn.

COHEN: Well, I want to talk about your administrative things, but let's just quickly finish up the lab. How involved are you—? You know, some people are very hands-on mentors, and some people kind of let people do their own thing and just are open for questions. How involved are you in the day-to-day running of the experiments and the work in the lab?

MORAES: Maybe I would like to be a— Let them do it and let's just analyze the data, but I feel that I have to be a little bit more on top of them in terms of how to perform the experiments and things like that. Even though with the size of the lab I have now, it's not too big, but it's enough for me not to be able to do that too much.

So I have to trust them, but I still have to go through protocols to make sure that things are done right. Now, of course, with time I become more distant from the protocols, because there are new protocols that I haven't done many times myself, and that becomes a problem, so it's harder for me to troubleshoot, but that's the kind of PI I am. Particularly if something doesn't work, I would not tell the person to just do it again, I would try to go over the protocol and the details and see what happens, and see if there was some mistake that I can detect. So my limitation to be that kind of PI is really tying me up, so now I cannot do as much as I would like to. So maybe I am a little bit the kind of PI that—I wouldn't say stays looking over the shoulder of the fellows, but maybe in between letting them just do whatever they want and just checking the results and being over their shoulders.

COHEN: Well, you mentioned administrative stuff, so let's go there. How much administrative work have you been sucked into?

MORAES: This year it's starting to get really bad. One thing is papers to review; those come every week, but that's okay. That's not too much work.

COHEN: For journals, you mean.

MORAES: For journals, yes. The second thing is people that ask you to contribute chapters to books, and that is becoming a problem, because that is not really a priority in my research, and sometimes friends ask you to do it and it's hard for you to say no. That's something I should learn.

COHEN: How to say no.

MORAES: Yeah. For example, I think April 15 I have to have a quite large chapter written that I haven't started, and I'm sure I will not have any time to start before April 15. I have to do my taxes before that. So that's a problem, and I have to start saying no to those things, even though they are an opportunity to get junior people in the lab involved and get them to publish something that's important to them. So I like when it's a short paper in an area that involves one of the junior people in the lab; those are fun. But then the really bad ones are grant reviewing, and those are very painful to me, because I'm not very efficient. It takes me a long time to review grants.

COHEN: Are you on a study section?

MORAES: Well, I'm in a scientific advisory committee of the Muscular Dystrophy Associaton.

COHEN: Oh, okay.

MORAES: We meet twice a year. It's not too bad, because the grants are not as long. They're only eight pages instead of twenty for NIH. Last month I was invited to be an ad hoc in the mammalian genetics study section, so I had quite a few papers to read for that, and that took me a long time. Last week the scientific review officer called me and asked me to be a regular member of the study section. That means meeting three times a year and reading a lot of grants. I was very anxious about receiving that call. I was hoping that she would not ask me to do that, and when she did, of course, we have that huge sense of debt with NIH that you have to do it, but at the same time I was extremely hesitant because of how much time I have for research right now and how [much] worse it's getting. So I talked to her a lot about it, and we discussed back and forth, but she still wants me to be there, so she said, "Well, maybe we'll start in 2002 so that you can get rid of some of your other administrative responsibilities." That's just a dream, it's probably just going to get worse, but I agreed with it. Maybe I will be able to skip some meetings and not go three times a year, but I will have to do it. Of course, there are many advantages to being in a study section: you learn a lot, you meet people, but at this point in my career, I don't think the price I pay for it is worth it. It will take me three weeks to review these grants. If it's three times a year, that's just going to chop a few months out of my year. Even though you learn a lot, you also might make some enemies being in a study section, particularly if you are— I will be the only mitochondrial genetics person there, so people pretty much will know if I'm reviewing their grants or not if it's on mitochondrial genetics. That's not the major concern, but that's one of the negative things of being in a study section, because it's public record who is there. Of course, it's the whole study section that decides, and I think I'm a very fair reviewer, maybe on the generous side actually, and sometimes that's a problem. But that's not something I like to do. I really don't like to do that, just because I'm very inefficient. So then I am on the steering committee for the graduate program in cell biology, cell and developmental biology. There are quite a few meetings, and we decide who gets accepted to the program or not. I am in several committees of students in different programs, graduate students,

so we meet once a year to discuss their progress, and that's fine. That's part of the academic duties. I am on the executive promotions committee of the university, and that's for medical students that are recommended to be dismissed, and they can appeal to this committee, and that's once a month also.

COHEN: That sounds emotionally intense.

MORAES: Yeah, it's not too labor intensive, but it's quite sad actually, to tell you the truth. And there are just— Every single project you have, you have to write an animal protocol. This animal protocol is going to expire, and you have to renew it. Every year you have to write a report for one of the grants you have. So all these things add up in such a way that my days are filled with trying to fix those little things here and there, and I try to squeeze in time to talk to people in the lab, but for me to actually do experiments is becoming harder and harder, even though once in a while I try. When I have a little bit more time I start something, but almost always I have to give it to someone else to follow it up, just because I can't, because I have to travel or I have to do this or that. Travels— I have cut a lot of my travels, mainly because [Maria] Julia [L. Pace] demanded that I [do] so since we had Emily [Kay Moraes]. I never traveled too much, but I cut even more. I still go to meetings that I think are important. I enjoy going to meetings; you get new ideas and you see what is happening, and that is very important, let alone meetings like the Pew meetings, where on top of everything you have entertainment and a very interesting environment that is not only in your field. But that's how my days go. Some days I just don't know what I've done, you know? I just spend the whole day doing I don't know what, and also sometimes you have colleagues that come to you to talk, to discuss one thing or another, and that can take a big bite out of your day also.

COHEN: Now, I noticed that you also are involved in a couple of high school programs.

MORAES: I was. I'm trying to— I mean, I have not volunteered to accept summer students, just because I cannot handle it, so I don't think it even would be fair to them. I had some mixed experiences with the few I did. I had some that were quite nice and fulfilling, but I had a minority program once that really didn't work out very well, and that made me a little uneasy about these programs. There was a black kid from Georgia—they had some kind of fellowship where they spend a summer in Miami—

COHEN: The state of Georgia?

MORAES: Right. So the kid appeared the first day, and we discussed the project, and then he would just not show up at all. And at the end he had to write some kind of poster or thesis, and when he showed up, I was really pissed, but he wrote one at the end. I complained to the

coordinator, and the coordinator went to talk to him, and he accused me of being a racist and that's why I was so upset. So that kind of thing really made me uneasy about participating in these programs.

[END OF TAPE 3, SIDE 2]

COHEN: Okay, we are back on tape, and let's talk about teaching, then, because that can be the other big time sink for PI's [principal investigators]. Actually, it's usually a little less in medical schools than if you're in basic sciences. So how much do you teach?

MORAES: Right, that's one of the perks of being in a clinical department. I have no real teaching responsibilities, and, in theory, that's why they don't pay my salary. But because I am very involved with the basic departments and in particular with the cell biology graduate program, I teach a couple of lectures for them a year. I give a couple of grand rounds in neurology a year, but it's really not much. I don't really like teaching in a class setup. I like mentoring graduate students in a one-on-one kind of situation, but I don't like lecturing much. It's stressful to me to give lectures, for both classes and general lectures in my field. They are always stressful to me, so it's not something I look forward to.

COHEN: So for you it's good that you don't really have to do too much of that.

MORAES: Yes, it's very good.

COHEN: How do the students—? Well, actually, before I ask you that, do you do any teaching in the med[ical] school at all?

MORAES: To medical students?

COHEN: Yeah.

MORAES: Once in a while, but not on a regular basis. Sometimes in the neurology part, I'll talk about neuromuscular disease a little bit.

COHEN: Okay. Since you don't do a lot of teaching, I don't know if you're going to actually be able to answer this, but let's try. How do students here, American students, compare to

students in Brazil?

MORAES: Well, if you're talking about medical students, I think the ones here are pretty exceptional, they're very good. To be accepted to medical schools they have to be very good. And that's also true in Brazil; to be accepted into a good medical school you have to be at the top. One thing that we discussed earlier is the fact that here, there is a lot of pressure for you to keep doing an outstanding job, and maybe that pressure is not there in Brazil. So that's one difference I see. In terms of graduate students it's a little harder for me to compare, but from what I remember from when I was there, most graduate students in the school I was in were very motivated, and, maybe because of the more limited resources, they're quite resourceful, also, in trying to adapt things and maybe make a machine or make a device or something like that. It's really hard to compare. I don't know if I see this here or I don't. My first impression [is] that they're not as resourceful, the ones that are getting accepted, but the graduate students really depend a lot on the institution. If it's a more famous program, the students, I'm sure, are going to be outstanding. Here at the University of Miami, I don't think we get top top students, and within the university there are some programs that are more powerful than others, and cell biology is the one I'm more involved with. It's not a very big department, the program is relatively small. So students are good, maybe not exceptional.

COHEN: When you were talking about who was in your lab, it sounded like you had kind of an even split between men and women.

MORAES: Yeah, I think that's about right.

COHEN: It's pretty even.

MORAES: It changes quite a bit.

COHEN: Right. That seems to be pretty much the case nowadays, that there's an even split. That split is not, however, apparent at the faculty level in most places. What does the faculty look like here?

MORAES: Well, in neurology there are definitely more men than women; at research in neurology maybe it's a little bit more women, but in the faculty in general—I could get these numbers probably—I'm sure it reflects the rest of the United States in that women are underrepresented. Now, I'm not sure why, and I'm not sure if that's not changing now, because I see people coming in right now, and they seem to be more evenly split, so maybe that's a trend that is already changing.

COHEN: I realize that everyone in your lab is from another country, but in the graduate school and medical school in general, especially in a place like Miami, what is the breakdown ethnically, in terms of Caucasians versus Hispanics versus African Americans?

MORAES: In Miami I think African Americans are really underrepresented in academia. There are lots of Hispanics, lots of Cubans in Miami, and lots of Cubans work at the university. Now, at the graduate student level and the faculty level, I don't think they are as represented as in other jobs. I may be wrong though, but the Hispanic representation is very high in Miami at all levels of physicians. Even in the faculty I think it's overrepresented when you compare to other parts of the country. Now, there are lots of Anglos also, particularly in the faculty, and maybe they are overrepresented if you compare with the general population, but— Yeah, that's my impression. Which other group did you ask me about?

COHEN: African Americans and Hispanics: the American minorities, if you will, although I think the Hispanics are not a minority anymore.

MORAES: Not here.

COHEN: Not here.

MORAES: The African Americans, I think, are underrepresented here in Miami at the graduate student up to faculty level.

COHEN: How about in the med school, because in a lot of places they are represented in proportion to their percent of the population pretty well in medical schools, but not in graduate schools? How is it here?

MORAES: I'm not sure, to tell you the truth.

COHEN: You're not sure. Okay. You just said a few minutes ago that you don't actually know what you do during the day, but if you can, try to take me through a typical day in the life of Carlos Moraes.

MORAES: Well, maybe one of the good things about this job is that we don't have a typical

day.

COHEN: But, I mean, let's start in the morning. I know that you take your child to daycare, because—

MORAES: Yeah, so that's how I start. I take my daughter [Emily Kay Moraes] to childcare, daycare—

COHEN: Do you take care of her in the morning or help her get ready, or is that [Maria] Julia [L. Pace]'s job?

MORAES: Only if we are in a rush. Now that Julia is working on-and-off, I'm kind of slacking off a little bit on house duties. But usually Julia prepares her and she's ready to go, and I'll just drop her at the daycare—

COHEN: So you get up about what time?

MORAES: I get up at about seven-thirty in the morning, but I take a huge amount of time just trying to get myself together. I read the paper at breakfast, I go to the bathroom and take a shower that is usually long—

COHEN: Well, soon it's going to be very short, because of the water shortage here.

MORAES: Right. Have you watched *American Beauty?*

COHEN: Yeah.

MORAES: It's the high point of the day, but not because I'm masturbating, but because it's a time of relaxation, and I can actually think about experiments and things like this, so my shower is a high point of the day. It's downhill from there. So then I leave home usually a little bit before nine. At least twice a week I will have a meeting at nine-thirty, so I have to be here at nine-thirty, but that's the time usually I get here, a little bit before nine-thirty. At first I do what everybody does, check E-mail, and I sort all the E-mails I have. I look at my calendar and see what kinds of wonderful things I have to do during the day, and when there is nothing written, I am just pure joy. But there are not many days that are empty. Then I just try to struggle with my

priorities. Whatever is the deadline coming up first— If I have to review grants and if I have plenty of time, I try to do it in the evenings at home, but usually if the deadline is closer I have to do it in the lab. So I have all these little things that I mentioned before that I have to take care of. So then I will try to talk to someone in the lab that I know their experiments are either not working or are having a problem, so I try to discuss and see what's happening, or someone that did an experiment that I'm curious about. So I do go talk to people or ask people to come to my office. Usually I try to go to the lab. I look at cell cultures, see how they look, but they are other people's experiments. And immediately, little things, little problems occur, like we have to order this that broke— I don't have a manager in the lab that will take care of all this basic infrastructure. People would order things for research, but if it's something different, then usually I take care of [it]. That's something I have to learn to delegate also. What else? Then, if I have time, I will write either a paper or a review, whatever I have to do, or a grant if that's called for. So my day is very chaotic; I'm not very organized. So around one-thirty I take a break for lunch, and I have lunch with some friends from another building. We just go out and eat, and usually I listen to their problems, because they like to talk more than I do. Then I go back and do more of the same. There are some experiments that I have to do together with people, go to the microscope— I am associated with many things with other people in different departments and different groups, so [they are] testing a new microscope and they want me to write a piece of shared equipment grant that I have to write. It's really hard to tell what I do in a typical day, because there are all these little things—as a friend of mine would call [it], chicken shit—that you have to do, and at the end of the day you don't know how you spent the day.

COHEN: Yeah. So what time do you wrap it up usually?

MORAES: Well, usually my most productive time is after four-thirty, so usually I get on a roll either writing or doing something, and I go up to seven and I stop, because I have to go home and see Julia and Emily. But that's definitely my most productive time, because people [have] left and they don't come knocking on my door, and I get things done.

COHEN: So you go home at 7:00 and you have dinner with your family?

MORAES: I go home at 7:00, we have dinner, we cook whatever is fast. And Julia is not your typical housewife, she doesn't like to do any of those— She likes to cook, but she destroys the kitchen when she cooks, so I [would] rather that she doesn't.

And now she's more concerned with Emily, so she will prepare something for Emily. But she will cook; now that she's working less, she actually is preparing food almost every day, even if it's an improvisation. When she was working full time, I would cook most of the time, but something fast also, pasta or whatever. After that, some time with Emily, and Julia will go take a bath in the bathtub with Emily, and maybe that's the time I have to be by myself, and I will either read something I have to or watch a little bit of TV, because someone told me that

when you watch TV, you spend 15 percent less energy than if you're doing nothing. So it's a good time to relax, I guess. Then they will go to bed around ten, and I will stay up either reading something related to work or as a zombie watching TV and just trying to relax a little bit. Or I will be reading some windsurfing magazine or something like that, when they come. That's pretty much my day.

COHEN: And you go to sleep at what time?

MORAES: Around eleven-thirty.

COHEN: Okay. How about the weekends? Do you work on the weekends?

MORAES: Yeah, I used to, but since Emily was born, I am not working during the weekends. When I am doing experiments I usually pass by, because I have cells growing and I have to feed them, or if someone in the lab is traveling, I will pick up the slack by keeping the cells growing for this person. But these days I'm coming much less often than I used to.

COHEN: So what kinds of things—? I mean, I know you windsurf, but what do you—?

MORAES: I pretty much spend time with Emily and Julia, and I do things for the house, so I will go buy this, go fix that, and take Emily to the playground, to the beach, or something like that.

COHEN: So how often do you get to windsurf?

MORAES: Not often enough.

COHEN: Not often enough.

MORAES: Whenever it is windy, and for Miami that is not very frequent. Now, how often do we have a windy weekend? That's even worse. In the summertime there is absolutely no wind, so from May to September I don't windsurf at all, unless there is a hurricane.

COHEN: But you don't want to windsurf in a hurricane, do you?

MORAES: Well, you do.

COHEN: You've done that? Oh, gosh. [laughs]

MORAES: Usually they just brush by. So then during fall, winter, and spring, you have better chances, and I will go a little bit more often. Usually I go with a professor in physiology [John N. Barrett]. That's a guy that is older than me; he's, like, fifty-something. He has very old equipment, he refuses to buy newer stuff, but we go together, and it's really a great relaxation for me. To be— I mean, not only the physical activity, but to be dealing with water and air and basic earth elements is so refreshing. To be out there when the sun is setting just renews me, so I'm ready to work hard again. So that's really my stress management these days. Now, before I had Emily I used to work out quite a lot with a partner [who] is a faculty in neurology [Miguel Perez-Pinzon] also. So three to four times a week, we would go to the gym at the university and jog or do weights. I was in great shape, and that was eighteen months ago the last time I went. That was a good stress management thing also, because we would talk a lot; we would exercise our tongues almost as much. But doing physical activity— I miss it, because of the stress management and the feeling of being more fit.

COHEN: Well, having a baby changes a lot.

MORAES: Yeah.

COHEN: And actually, one of the questions that I have— I didn't ask you this, but one of the questions I ask sometimes is, why aren't women more represented on faculties? Actually, let me ask you that, and then I'll go back to this other question. Do you know? Do you have any thoughts about why women are not—?

MORAES: Well, definitely motherhood would make it very hard, because the system does not cut much slack for time off. So I do know several women that have kids, or maybe one kid, and can still remain very active in research, but I really admire them because the husband might help a lot, but still the heaviest load will always be on the mother, at least for the first two years, or year and a half, I guess. So I understand how they will be less competitive if they have kids, just because the system doesn't really care. I think a higher number of women would enter graduate school, it's just that they don't progress. I mean, the percentages don't reflect later on. So the reason for that I can just think of— Maybe a lot of women would still follow their husbands, and maybe the husband's career would take priority and they would have to adapt. I think that still happens a lot. Maybe some will just get discouraged by the cutthroat system and the

competition. Once they have a kid, I think their life perspective might change, and [they] say, "That's not really what I want to do." I'm just saying this by observing Julia and how she has changed in terms of career goals before and after having a kid. It was a pretty dramatic change that I think will happen more often with women than with men, just because it's instinctive and hormonal, I think.

COHEN: Well, the reason I asked that question when I asked it is that your work life changed when you had a child also. What kind of an impact, if any, has that had on your career?

MORAES: Well, I think if I work more efficiently there will be no impact, neither positive or negative. But I definitely will have to improve my organizational skills and efficiency so that I can keep working well, but I'm still in transition. I'm not putting in as many hours, but I still have that completely disorganized way of running the lab that makes it hard to be as efficient.

[END OF TAPE 4, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Carlos T. Moraes**

INTERVIEWEER: **Helene L. Cohen**

LOCATION: **University of Miami**
Miami, Florida

DATE: **15 March 2001**

COHEN: I always have a few follow-up questions. Yesterday, actually both days, we talked a little bit about how you came to shift gears and go to Columbia [University], and I was wondering, though, what was going through your mind? I mean, did you have any interest in changing directions or in this project? I mean, what was going on inside of you that made you do that?

MORAES: It was a lot of chance, I think. I was definitely getting to the end of my rope in the place I was. I was there for many years, I think four years, and I wanted a change. But this change really could have taken any direction, and I think the fact that I went to New York was pure chance. A little before that—I don't remember exactly when—I also interviewed for a job in a chemical company in São Paulo, [where] I would be doing quality control or something like that. So they had a test to get in— It was actually Dow Chemicals. Multinational. I applied there because during my trip in the United States after college, I met a guy that worked at Dow, and I wrote to him asking him about some job possibilities either in the States or in São Paulo, and he gave me the number of Dow Chemicals in São Paulo. I contacted [them] and I went to do a test, but I was probably ill-prepared for a chemical test, and I didn't do very well. But if I had done well and maybe there was a position, I don't know, my life would be completely different. So I think there is a lot of chance involved. I don't think I planned my career early on and worked towards it. I had this trend, this tendency to try to look for something better, and I was always very curious and very industrious. So maybe that eventually would [have led] me to a research career, but I don't know, maybe not. So if I had not stopped the work in the lab that I was in, I probably would have done a Ph.D. there. That's what most people do. They do a master's and then a Ph.D., and then they go for a postdoc, usually in a lab in the United States or Europe that is related to the work you did in your Ph.D.

COHEN: Right.

MORAES: And then people will go back and maybe be hired as faculty in that same group, and it just inflates. So there's not much stimulation for people to diversify, and maybe I saw early on that I didn't want to be just one more in that group that already had too many chiefs and not enough Indians. So maybe I would be out of it in one way or another, but the way I got out was

very circumstantial.

COHEN: Was going to the States part of it?

MORAES: Well, finding that friend that put me in contact with Billy [Salvatore] DiMauro and getting a Ph.D. fellowship and all that. If it was not for that encounter, I would not have done that, and my career would have taken a completely different path.

COHEN: Okay, well, let's talk about the work and how you kind of evolved from that into what you're doing now.

MORAES: Well, when I got to New York I was in this big group that specialized in studying metabolic myopathies, mainly muscle diseases, but also some nerve and muscle disease. At that time they were kind of focused on mitochondrial abnormalities. The mitochondria is this organelle inside the cell where energy is produced, and if you have a problem with the function of the mitochondria, usually you don't have enough energy; tissues like brain and muscle that require a lot of energy will suffer first, but you can have other tissues involved. So at that time, in '87, these disorders were diagnosed mainly because of pathological features, so you would take a muscle biopsy, for example, and see [that] the mitochondria was abnormal. There was a little bit of biochemistry showing that some enzymes could be decreased, but that was very inconsistent. So I think I mentioned that just briefly, but in '88 there were two papers, one in *Nature* and one in *Science,* describing for the first time mutations of the mitochondrial DNA. We had—and we even joked about it—all those patients in the freezer that were actually pieces of tissues from patients, but they were relatively easy to go back to and study retrospectively. So it was a very productive time, because we had a lot of work to do and almost anything we would do would be very interesting and new. So I was lucky enough also to have a postdoctoral fellow called Massimo Zeviani, [who] now is a major force in the field, and he is in Milan in Italy. He was kind of finishing his training there, but he got me like a pupil or someone under his wing, and he taught me basic molecular biology that was the basis of everything I did since that point. So we had an overlap of a few months there—Massimo and myself—and when he left, I was kind of in charge of the molecular biology of all these diseases; it was really a very lucky position to be in in that lab.

COHEN: Yeah, you were a brand new graduate student.

MORAES: Right, I was still in Ph.D., doing my Ph.D., but I was kind of the expert in there. Eric [A.] Schon was the molecular biology expert in the techniques in general, but not so much in studying patients the way we were doing, using small muscle biopsies. Of course, he could direct anyone doing that, but I was already doing it. I started with Massimo so well and was on a

roll—

COHEN: You were doing the muscle biopsies on the patients?

MORAES: No, analyzing the DNA defects of the muscle biopsies. So I was really on a roll and getting lots of data, and then I developed some new techniques to study these muscles; for example, if you cut a muscle section—a transverse section—and you look at it under the microscope, you'll see these big fibers, and those are cells, muscle cells. In mitochondrial diseases it's interesting, because some cells are affected and others are not. There's a mosaic. So we developed some techniques that I started [where you] could dissect those single fibers just using a thin pipette and then try to analyze the DNA in those isolated fibers, and that was very nice because it gave a correlation of the presence of a DNA defect with some abnormalities that muscle fiber had. So we could dissect abnormal or normal and see how the DNA was in those different ones. We could do that for a different number of mutations that were— Every month there was a new mutation reported, either in our lab or somewhere else. So [there] was a lot of good work that we could do, and it was relatively easy because everything was new.

So by the time I left— Well, a little bit before I left, I was a little bit bored with studying patients and describing new mutations, and I wanted to do something more mechanistic. So I started to get more involved with Eric Schon, [who] was actually my official mentor, and I started doing riskier projects that usually would not work, but in any case I would learn much more from that. So I did some projects [where] we tried to express some gene in a cell, something that was a little bit more mechanistic, with Eric Schon. I did some more molecular analysis with Eduardo Bonilla also, using antibodies and trying to define better how a mutation would cause a certain problem. So that gave me a broader background: molecular biology, biochemistry, immunochemistry, etc.

So by '92, '93, I was finishing my Ph.D. and then I had to make a decision— I don't even remember if I already talked about this. If it's repeated the editor [can] cut it. So I was thinking about going for a postdoctoral fellowship, and I mentioned that, but then I also mentioned how I received, from Michael [P.] King, this letter from Dr. [Walter G.] Bradley, [who] is the chairman of neurology here. So I decided to check it out, I talked to him, as I mentioned before, and I came for an interview. That went well, I came for a second interview with [Maria] Julia [L. Pace], [that] also went well, and I was hired. So in my work here, from the beginning, I used what I had from New York. I had some mutations that I had described, and there were these new techniques that actually Michael King applied in New York and I learned; it was to use these cells without mitochondrial DNA, to repopulate them with exogenous mitochondrial DNA, and that was very useful to study patient-related mutations, because we could put those mutations in cells that grow very well in culture. So I started doing that, and I applied for an NIH [National Institutes of Health] grant on that, and I was funded, because, again, things were very novel at that point. And I think in the first three to four years, that was the basis of my work, mainly studying mitochondrial DNA mutations in these cells that were repopulated with patients' mitochondrial DNA. I was also studying patients' tissues and still describing some new

mutations or new variations. I continued to collaborate with Columbia, particularly with Giovanni Manfredi, [who] is a very good friend of mine these days, and we still collaborate. He was with the Columbia group until recently, and now he has his own lab at Cornell Medical Center [New York-Presbyterian]. So mainly patient-related mutations. I also started to do some projects that involved potential ways to ameliorate the defect caused by these mitochondrial DNA mutations, and those are very difficult projects that I'm still working on, and they are very hard to work and to give the desired final result, but I'm insisting.

For example, expressing genes that are usually in the mitochondrial DNA in the nucleus. This is a valid approach, because we cannot insert genes into the mitochondrial DNA. There are no techniques available for that, but we can insert genes into the nucleus. So if we take a mitochondrial gene, then we have to recode the whole the gene, because the genetic code is different, and we have to modify and make a number of things and then express it in the nucleus. I have been trying to do that, other people in the field have been trying to do that with different genes, and it's a very difficult problem. I don't know how specific you want to get into the different projects.

COHEN: Well, this will most likely be read by non-scientists, so an overview is good, but I just had a question about what you were saying. If you modified the gene in order to put it in the nucleus instead of the mitochondria, how do you know that it has the same function?

MORAES: You modify it in a way that it still codes for the same protein. But it's just that a codon that's read, let's say, like methionine in the mitochondria is different from what's read as a methionine in the ribosomes, in the cytosol. So if you want to be a methionine, you have to change the gene for the final product to be the same. So other projects involved, again, these mutations in tRNA genes, [which] are the transport RNAs; those are small RNA molecules that are necessary for protein synthesis. There are some mutations there that cause human disease—

COHEN: Such as?

MORAES: One of the problems with one of these mutations seems to be that the amino acid that has to attach to the tRNA doesn't attach very well. So I also started to develop some projects to find this enzyme that attaches the amino acid to the tRNA, and try to overexpress it and try to maybe improve protein synthesis in these defective cells. So I've been doing these kinds of projects. But also a few years ago I started to do something that had more of an evolutionary spin, and that was to try to put in a human cell, mitochondrial DNA from different species. I tried putting from quite divergent species, like rodents and things like that, and, as was expected from some earlier work, the mitochondrial DNA from these species could not coexist with the nuclear DNA from a human cell. It's just that there are too many interactions between these two DNAs, and they were too different. So finally we did it systematically with different primates' mitochondrial DNA, and we found that chimpanzee mitochondrial DNA and gorilla

mitochondrial DNA could coexist and be functional with a human nucleus, but if you use mitochondrial DNA from primates that diverged from humans longer [ago], like orangutan or old world monkeys or new world monkeys, those could not function well with the human nucleus. So that was quite interesting, because it gives us perspective on the coevolution of the nucleus and the mitochondria: how these interactions have coevolved to produce energy.

There were lots of spin-offs from this work. For example, even these cells that we could— Mitochondrial DNA from a chimpanzee, for example, could coexist with a human nucleus, [but they] were still a little bit defective, and we characterized these defects in a little bit more detail. Those defects were quite specific, so they could mimic some defects that are observed in neuromuscular diseases—

Neurodegenerative disorders. So we would publish a series of papers using these cells as models for defects in mitochondria. More recently, we started to work also with some cancer cells that were described as having mutations in the mitochondrial DNA. We actually don't have anything published on that yet, but I have a graduate student [who has been] working on it for a couple of years now. So the group of Bert Vogelstein, that is a famous group at [Johns] Hopkins [University] working on cancer, described these mutations in colorectal tumors, mutations in the mitochondrial DNA. Those were somatic mutations, meaning that they were not present in the neighbor[ing] healthy tissue, only the tumor, but they had no clue if these mutations had any functional significance, and that's where our expertise could help. So we got these cells from his lab and Jim [James K.] Wilson's lab, and we have been trying to characterize how mutations in the mitochondrial DNA affect function, or not, and this is quite interesting. We have also started to work with apoptosis, or cell death. That's a very in vogue topic these days, and, again, the mitochondria is crucial in cell death, and we try to use our expertise in biochemistry and mitochondrial genetics and use the models we have to try to address some problems that maybe other people cannot or would not. So these are the main projects that are going on right now in the lab. I still do work with mutations in mitochondrial DNA found in patients. I'm still trying to characterize those. Also, I've branched out a little bit more into some neurodegenerative disorders, like amyotrophic lateral sclerosis, and we have a grant from the Muscular Dystrophy Association to do this work. We're trying to identify or characterize the mitochondrial involvement in some mouse models of ALS [amyotrophic lateral sclerosis] [where] the mitochondria seems to be affected early on. So we're trying to see how relevant that is to the development of the disease. Also, we're trying to create some models of other neurodegenerative diseases by knocking out some mitochondrial oxidative phosphorylation genes, these ATP [adenosine triphosphate] producing genes, and these might mimic Parkinson's Disease or Alzheimer's Disease, also—

COHEN: Parkinson's or what?

MORAES: Alzheimer's.

COHEN: Alzheimer's.

MORAES: So we are quite diversified, but still limited in the number of people working and their commitment.

COHEN: That's a lot of different projects for a small lab.

MORAES: Yes. I mean, that's also related to how I run the lab, I think. I try not to overlap too many projects between two different people. I like people to feel that they have their own project, and, of course, you can have several sub-projects in a big one, but then you have more chances of overlaps and political problems and things like that. Yeah, sometimes the spread can be a problem, but hopefully I'll be able to know when I'm spreading too thin.

COHEN: You know, because you sort of stumbled into this, it brings up something that interests me, which is: Was it just a lucky chance that you fell into something you liked, or did you like it because you started working in it and it became interesting to you?

MORAES: The latter one.

COHEN: The latter one.

MORAES: I think I could be working in anything related to life science and I would be happy about it. I find everything interesting. Even to have a clinical relevance is not a major thing for me; I could as well be working on something extremely basic with no direct relevance to clinical research and be just as happy. So I think I like it because I started working on it, but again, being honest, I think if I was in a different field I would like it just as much. If it's something that you see that you're making a contribution and you understand it well, I get very interested and I like it.

COHEN: Okay.

MORAES: Yeah, I hear some people talking about, "I always wanted to do diabetes research or this kind of research," and that's definitely not my case. I like life science, I've always liked life science, but there— And I even could be [doing] archaeology or something like that, that is not really life science. I would still be enjoying it, I think. What I really like about the research is the lack of a routine, and it's extremely challenging, because you can set up your own goals and

those can be hard to achieve, so you can make it as challenging as you want.

COHEN: So if you had to say what the thing that you liked the most is, would it be the lack of routine, or something else, if you had to say?

MORAES: Yeah, I think the lack of routine is close to the top. The excitement and frustrations of research, I think, are also something I like. I mean, the lows— Of course I don't like the lows, but it makes the highs nicer when they come.

COHEN: Yeah.

MORAES: And also the unpredictability of the results—that motivates me. I enjoy that kind of stuff, particularly when I was working more in the lab, you know, when you're developing an X-ray and in those few seconds you start to see a band come up. You have some expectations, and the result might not fill those expectations. That makes your adrenaline pump; at least, it makes my adrenaline pump.

COHEN: Yeah, like a good wave.

MORAES: Right. So when they come and when the result is good, you have a lot of pleasure out of it. I like to finish some body of work. I have a lot of satisfaction getting a paper finished and published, but I also— And I think that's a plus. I'm not someone that likes to commemorate or enjoy too much something that I accomplished. Once I— I enjoy accomplish[ing] it, and leave it behind. That's how I see these things. Once I finish something, that's it, let's go to the next one. Any researcher can do that, because you finish a certain piece of work and, of course, you continue the same line, but you have accomplished something very concrete. And it's interesting also how a finished piece of work looks nice and organized, and when you look at the way you came to that point, it was not that organized. And even the way you tell the story it was not in the same time frame or in the same sequence that it actually happened. If you see my office, it's a huge mess, but when I mail something out, it's very organized. So I find that interesting also, that you can be juggling a number of things and still you make something very neat at the end.

Like this morning, for example, I had to cancel our nine-thirty thing. I had to get some preliminary results for a grant that a colleague of mine is putting to buy a confocal microscope, so it would be a shared equipment grant. Anyway, he told me those guys were demonstrating the equipment, and they were trying to install yesterday, so I had only this morning to do it. So I just got some cells, I ran there, I would try to do whatever we could, and the machine is very nice, so the guy actually could get some neat images. So after this interview I will probably go

back there, get those images, try to pack it together in the few pages that would be my part in the grant, and send it out and hopefully get funded. But, you know, it's a very chaotic process, [but] somehow you can make a nice final product, even something small like that. So I enjoy that aspect of the work.

COHEN: What do you like the least?

MORAES: What I like the least is the administrative burden. I already mentioned that I don't like to teach, and I don't teach much, so I'm happy with that. I like to have contact with students, but closer contact, more at the mentoring level than lecturing. But then you have committee meetings that someone has to do, I understand [that] and I do it, but I don't like doing it. We were talking earlier about not having a secretary. My office is a complete mess; I wish it was more organized, but I also feel that I really don't have time to organize. The way I do it is every three to four months my office becomes so bad that I have to stop everything and organize and clean everything, and then it's very neat, and a week later I'm already going through hell again. That's how it works. So everyday I get a number of faxes about things I have to fix, machines I have to fix, grants I have to review, papers I have to review, so I don't particularly like that. It's not that I dislike that, but that takes all of my time, and I truly dislike the fact that I am becoming more and more distant from the lab. I don't like that. The politics of science I actually enjoy. I find competition and collaboration something healthy and normal in life, so of course you have people that you like more and like less in the field. It can be more secretive or less secretive, but I think it's all fair game, and I really don't have a problem with that. I try to be very friendly with everybody in the field, and very generous, and I expect people to do the same. So I have a good time. In my field, I think people like me. They probably respect me at the level that I should be respected at this point in my career. So I like the politics of science. And once in a while you'll have a character that's a little weird and who behaves in some strange way, either because the person is too competitive, too egocentric, or too— What's the word? It's not low self-esteem, but too afraid to be scooped or something like that, and they will behave in a not very collegial way. But that's okay; it happens in every profession.

COHEN: Well, actually, a couple of those subjects are on my list of things to talk about, so for a few minutes let's explore this business of competition, because you had said the other day that that was why the American system works well, because of competition. What is the upside of competition and the downside of competition?

MORAES: Well, the upside is that you can't take too long to get something done, because otherwise you're going to read about it. So that makes things move much faster, and again, I can make a parallel with Brazil and my experience with Brazil, where the pressure is not as high. It's not as high to publish, not even as high to get funds, because you might not have many research funds, but your salary is guaranteed from the time you get a job, and things move much slower. Here in the United States, if you are too slow, you're not going to get funded, you're not

going to publish good papers, and you're going to perish. So that's the upside: it makes things move faster, so productivity is high. The downside [is that] it's very stressful and makes you work very hard, but that's the price you pay for being very productive.

COHEN: Have you ever been scooped on anything?

MORAES: Oh yeah, more than once. And usually I know that I'm going to be scooped, because I know that a person that is working on the project is slow, and you know that a competitor is working hard on it or has a lab that is more powerful and puts more resources there. So yeah, I have been, and of course I get a bit depressed, but not very depressed, because I think that's part of the game and I should be expecting that this will happen. And hopefully I will scoop some people also, so it's a give and take.

COHEN: Well, one of the fears that I think people have when they're starting out is that they're going to have all their eggs in one project, one basket.

MORAES: Well, I don't have that.

COHEN: No, you don't, but your fellows do, and if they get scooped, it can sink their whole career.

MORAES: Right. Well, I like fellows to have at least three projects, and those are usually— They can be completely different projects. Two reasons: one [is] because in research I think that one project— People have much more time on their hands than what they need for one project. A lot of experiments take time; some experiments take days when you are selecting cells or things like that. So people definitely have time to run at least three projects, and, of course, some will not work, but hopefully some will. So yeah, I definitely try to protect them in this way. And also, if I see that they are in all very difficult projects, I try to give them an extra one that is more of a certain thing. I think it is very sad if people stay in the lab for a number of years and cannot publish anything. It has happened in my lab—as we discussed—with that American fellow, but I did try. I gave him one more, like, bread and butter kind of project that he advanced to a point, but the cells got contaminated and this and that, so he couldn't finish that, also. So someone else is going to pick it up and finish that, probably.

COHEN: What happened to him, though?

MORAES: He became a senior technician at—

COHEN: So he didn't graduate.

MORAES: Well, he was a Ph.D. from UCSD [University of California, San Diego].

COHEN: Oh, okay.

MORAES: So he was doing a postdoc, but yeah, he was not very hungry.

COHEN: So now, what about collaboration, though? How important is that in your work?

MORAES: I think the best thing about collaboration is that it's fun. Not only that, but then you're discussing science with a peer and you can actually learn things. This is particularly important for someone like me that is here relatively isolated on this island of Florida. There are not too many groups, particularly in my field. There are some people in Gainesville or other universities, but here in the area [there is] not much, even though everybody at the university that works on mitochondria, I'm in close contact with and I'm collaborating with. Like the example I just gave about the shared equipment is part of that. I actually have started—and I'm proud of this—a mitochondrial club here at the university, so we have like six different PI's [principal investigators] and people from these groups that meet once a month, and I got some money from the dean for research to buy food, so we have meetings with food and very lively discussion. So it's different from a regular seminar, because we just interrupt the person who is presenting all the time, and we discuss mainly projects in progress, so people get a lot of suggestions. So I think these things are very important, mainly because you are talking to people who can teach you something.

COHEN: Is there any downside to collaboration?

MORAES: Yeah.

COHEN: Aside from writing book chapters.

MORAES: Right. It distracts you. It sometimes makes you less focused. I know people that the only thing they do is to run around trying to do collaborations, and they don't have their own thing. So in particular if you have some funds and you are relatively successful, people will

want to collaborate with you, even if it's to have your name on a grant, and that's usually okay, because you don't have to do much.

But collaborations in excess can be a nuisance for sure, and can distract you from your main goals. So definitely there is a downside to it. Particularly if you are doing it more for a political purpose and not so much because you are interested in that project. But like, for example, as a follow up on that project where we were mixing nucleus from one species and mitochondria DNA from other species, I started collaborating with a biotech[nology] company, Advanced Cell Technologies, that [is] doing cloning of animals, and they are trying to do interspecies cloning. So that's a fantastic collaboration, because we can see when you do an interspeci[es] cloning, you use the egg from one species and the nuclear donor from a different species. Now, the mitochondrial DNA is going to be in the egg, so you're going to have some kind of potential incompatibility there. They're trying to clone an endangered species of cow called *Bos gaurus,* [which is] from Southeast Asia, using the oocyte from a normal cow, *Bos taurus.* We published a paper together with them showing that these clones, at least these fetuses that they could obtain, had the nucleus from one species and the mitochondrial DNA from a different one, and potentially that can be a problem if you try to use this technique to rescue endangered species. So those kinds of collaborations are great, because that's not something we can do. But we can use our expertise to their benefit also, to characterize a very specific part of the project. So overall, collaborations are great if you're interested in them and you're not just doing it because the chairman wants you to do it or something like that.

[END OF TAPE 5, SIDE 1]

MORAES: To continue the collaboration theme, because I mentioned not just doing what the chairman asks you. I also do collaborate with my chairman [Walter G. Bradley], by the way, but it's on a topic that I have a lot of interest in, and that's ALS, amyotrophic lateral sclerosis. He, of course, pushes for me to do more work on that, and I like the topic and I am funded to do so, so I am glad to do that. Sometimes we also have other collaborations that I might have less of an interest in or less time to do it, but I can be honest with him and say that that's maybe going a little bit too far.

COHEN: Yeah.

MORAES: But that's one thing that I'm sure lots of young researchers have to deal with also: People that have some influence in their lives asking them to do something, and what to do then.

COHEN: Yeah, well, it sounds like if you can say, "No," that's a good relationship.

MORAES: Yeah, we have a good relationship.

COHEN: Okay. Well, I want to go back— We got a little bit sidetracked, although they were to topics I wanted to talk about, but I do want to go back a little bit and talk about the significance that this work that you're doing will have in terms of our understanding of the sort of basic processes that go on.

MORAES: Oh, tremendous significance.

COHEN: [laughs] Of course.

MORAES: Yeah, of course I would like it to be very significant, and that's what I'm always trying to do. [For] the part related to mutations found in patients, I guess the best payoff would be in patient diagnostics, treatments, and counseling, the short term. Of course, these mutations also help us understand how different genes work, because they are natural mutants. So that's building up on the knowledge of mitochondrial genetics and mitochondrial biology in general. The mitochondria was usually seen as, you know, an energy factory in the cell, but more recently it's becoming more and more important for different processes, like cell death, for example, and it might mediate a number of signals inside the cell. So it's like any basic research; I think it's hard to predict what kind of impact it ultimately will have. But the way it's working right now, I think, it's just another building block on our knowledge of those processes, and that's how most of science advanced, I think, with those little pieces. Now, if you're asking me if any particular result I have found or I had was a major breakthrough that would take the field to a different level, I don't think I have contributed at this level, even though some people might say I have, particularly at the clinical level, describing how a certain mutation would correlate with a certain clinical phenotype. We've published some relatively important papers in this field, but that would be coming from a different lab, I think, if we hadn't published within the year or two years. So in terms of something that really was a major leap, I'm still waiting to give that kind of contribution. That's my view. Julia would say that I'm always like that—that's my wife—that I'm never happy with the work; even though I said I enjoy the lows and ups, she says that I get stuck in the lows and forget that that is how it goes. But I'm always trying to get something better than what I had before.

COHEN: That's a trait of scientists, I think, isn't it?

MORAES: I guess, yeah.

COHEN: Although some people believe, and it sounds like you just said, that somebody else

would have published it if you hadn't. Some people feel there's an inevitability to all of this, that it really doesn't matter if you do it; somebody will do it. Do you think that's true?

MORAES: But someone does. I did it. No, yeah, that's how I see it. Again, that's how I see science evolving. Those little building blocks that lead to something. Then the questions, usually, I wouldn't say that they're obvious, but they're a consequence of that building up, and if you— It's very common to see three or four papers published on the same topic at the same time, because that's how the field is developing, and all these four papers might be breakthroughs. It's interesting, like maybe last year or two years ago, we published a paper where we put the rat mitochondrial DNA into a mouse cell. Why is that relevant? Because these cells were defective, and it would be the basis for it to produce a mouse that has a mitochondrial disease. It will be a neat model of mitochondrial disease. The same year there was a group in Japan that published the same thing, and a group in Australia that published the same thing. Exactly the same thing. So three papers coming out together, because that's how the field was developing. So yeah, there's an inevitability in these results, I think.

COHEN: I suppose some people would say there's a synergy of energy.

MORAES: You can call it that.

COHEN: Okay.

MORAES: And I try also, when I know that someone is doing the same thing I am doing—and this happened twice in my career and I was very pleased both times—to contact the person and publish together in the same journal at the same time, submitting together, because I think that increases the impact of both papers.

COHEN: And it avoids getting scooped.

MORAES: [That] also, and it's usually a friend, so then there are no hard feelings, also. So twice we did that, and twice they were accepted back to back, and I was very pleased.

COHEN: That's great. Okay. Where do the ideas that you have come from? Besides the shower.

MORAES: Well, besides the shower, I really don't know. It's amazing that— You see how my

day [is] today and how I'm running from one side to another. It's really hard to have good ideas when you are on this pace. The shower is a joke, but it's really serious; [it's] the only time that I can actually think and stop and relax. Of course, I can also do that some other times when I'm relaxing, but besides that I would say during meetings, talking to people, and of course every time you review a grant or review a paper, you get some information that might help you have some new ideas. But I have to say that my best ideas I have when I'm relaxed and I have time to think. That's time that's at premium now. I don't have much between family and work, running up and down. A few minutes every day, and it's a real problem, because, as we discussed, I have many projects going on, and I feel that some are neglected, at least at the point of my intellectual input, because I quickly review data with someone in the lab and just the obvious thing comes to mind, "Let's do this," but I'm not putting enough thought into these projects. And the person that's leading the project might not be experienced or might not have the same knowledge I have, and I have a problem with that. So that's why I try— That's why the administrative burden bothers me so much, because it's giving me less and less time for thinking about the projects and directing people better.

COHEN: Okay, well, we'll come back to that again, I think, before we finish today. But one of the ways that scientific advancement—I don't know if it's frequent—sometimes comes along is by serendipity. First of all, how important is serendipity in science in general, and then in your work?

MORAES: Yeah, I think it's very important. Chance is a major player in this field, I think, and that's why I think it's important to do experiments sometimes, even if they're a little bit of oddball experiments, because sometimes you get unexpected results, and that can lead to something interesting. It might not lead to anything, but once in a while. So I think it's very important, and I would say that, frequently, major breakthroughs will come like that. I agree with that. Has it happened to me? Yes, it has, I think once. Well, I don't know if I'd call it serendipity, but we were analyzing DNA from a patient that had a mitochondrial disease, and we had a problem with what we call Southern blots, when we run the DNA on a gel and then use a specific radioactive probe to detect, in my case, the mitochondrial DNA. But we could not see the DNA there, and the usual explanation for that is that the DNA is not good, it's degraded. So I repeated it a couple of times, and I was very junior in the lab—that was in New York—and more senior people would say, "Well, I don't know, there's something wrong with this." But maybe in the shower or somewhere or walking to work, I just thought, "Maybe that's the problem with this patient. Maybe the levels of mitochondrial DNA are really low and we cannot even see [it]." It ended up being true, and we defined a whole new group of patients that have this quantitative defect of mitochondrial DNA that were confirmed all over the world. So that was interesting. It was not chance, but it is an example of how unexpected results are dismissed somehow, and they can be very relevant, and they're just in front of you, and for some reason you don't see it because you're so focused on one expectation.

COHEN: Right.

MORAES: I guess that's an important lesson. Just keep an open mind and try to see the possibilities.

COHEN: Right. I often wonder what serendipity is anyway, or synchronicity or—

MORAES: Well, some people say that— How do they say? Luck comes to the prepared mind. So what is it, anyway? I guess it's really the ability to see an unexpected result and to identify something novel in that, because probably those strange results will happen all the time. You're not always ready to interpret them and see if they have any meaning.

COHEN: Well, we just touched very briefly on what might come out of your work, but are there any potential applications that you can think of that might actually happen?

MORAES: Well, treatment for mitochondrial disease. That's the most direct one. Lots of the projects are directed towards improving the defect caused by mutations in mitochondrial DNA. Those diseases have no treatment whatsoever these days.

COHEN: These would be, for example? The names of some of them?

MORAES: Well, they have usually strange names like Kearns-Sayre Syndrome, because Dr. Kearns and Dr. Sayre described it, or they're called MERRF, and that stands for myoclonal epilepsy with ragged red fibers. That's a muscle abnormality. Or MELAS, and that's mitochondrial encephalopathy with lactic acidosis and stroke-like episodes. So they have really strange names.

COHEN: So not real common.

MORAES: They are not common. They are rare diseases. But they occur, and treatment would be welcome. Not to mention the potential involvement of mitochondrial defects in neurodegenerative disorders, which is becoming more and more in vogue. Things like Parkinson's Disease or ALS or Alzheimer's Disease might benefit from our better understanding of how mitochondrial dysfunction affects the cell. So I guess those would be the most direct beneficial outcomes of my research.

COHEN: Those sound like prime candidates for gene therapy of some sort.

MORAES: In a way; the only problem is that we cannot do gene therapy with mitochondrial genes.

COHEN: Oh, because you can't get them into the mitochondria.

MORAES: Right, but we still could have genes that would have a beneficial effect even if we have to put them in the nucleus.

COHEN: Well, you know, applications brings up, then, the issue of patents. Do you have any patents?

MORAES: No, and that's really something that I'm not crazy about. Of course, if I discover something that's profitable, I would love to have it patented, but I think people are overdoing the patenting thing, and any variation of a result from anything— By the way, this is Helene coughing, not me. So don't transcribe that.

So I've seen people patenting completely ridiculous things, and that takes a lot of time and energy, not only from the investigator, but from the university, also. Also, it becomes harder for scientists to exchange material, and I have a problem with that. So no, I do not have any patents. I would patent only if I had something that I truly see the potential as something that has a commercial use, but if it's something that's going to be used mainly in academic labs and things like that—

COHEN: Like a gene sequence or something.

MORAES: Right, or, unless it's something exceptional, even a technique or device that is just going to be used in labs, I wouldn't even try to patent.

COHEN: Okay. Well, I want to move on and talk a little bit about some ethical considerations—we're going to wax a little philosophical now—because there are many in science, and I've thought of three areas that I want to talk about, but if you can think of any others, you're welcome to add to the list. The first has to do with, let's say, the way scientists behave. You hear stories about people who will give somebody a low score on their grant because they're in direct competition, or a reviewer who doesn't like a paper because he's going to publish the same thing. That's kind of one end of the spectrum— Well, I don't know if it's

one end, but that's one set of things. The other thing is just downright falsification of data. Have you, in your career, run into any of these kinds of things?

MORAES: I think researchers behave very well, usually.

COHEN: That's good to hear.

MORAES: My experience— Going to the first topic first, that is, if a reviewer will give a low score to a competitor or something like that, with my experience in study sections and review groups, I see that people really try very hard to be fair, and I try very hard to be fair. Also, if it's something in direct competition and I feel that I might be biased, I will probably decline reviewing such a grant, and I would like other people that are reviewing my grants to behave the same way. So I have seen people being mean, but I think they were trying to be fair. But of course this happens, particularly when you're reviewing papers and you're all by yourself in your office and if it's a competition thing. Again, I can only say from personal experience that I try to be fair. If it's a paper that's going to scoop me, but it's well done, I really find that it wouldn't be fair to turn it down. I might be a little bit more critical, just because I know more about it, and maybe unconsciously I am not happy about that, but if they have all the answers, I will just have to swallow it, I think. So that's my experience with that. Now in terms of falsifying data, that, of course, is a problem in science, and as the pressure mounts, it happens more and more. I really— It's hard to tell how frequently that happens, and in the lab, also with students and fellows, I never had such an experience, at least that I could detect. Now you're responsible for the lab, but in many instances you have to trust the results that are being presented to you. There might be some fellows or students that— I don't know. Let's say they have a series of ten values and, you know, they might discard some values that are off, but if they discard the lowest and the highest for every measurement or for every group, that's a fair thing, because that's a technical variation, probably. So I try to guide them so that they don't try to manipulate the data too much. I try to be fair. You can maybe adjust or modify your data according to the variability of the technique you are using, but you have to be fair. You cannot eliminate the high points for one group and the low points for the other group.

I have been lucky, I think, with people in the lab. I [have] never felt that someone was actually pushing the limits of honesty, but that's probably partly because of the low pressure environment we have. I can understand [how] if it's a very high pressure environment, someone might just come up with lots of good data out of nowhere. Particularly if it's someone with a weak personality or someone that definitely has to publish something or they will die. That, I think, happens much more in universities that are more "high-powered," quote unquote, where the pressure is much higher than here at the University of Miami.

COHEN: But as you said, because of the mounting administrative responsibilities and things, you have less time to supervise. So how do you maintain quality control in your own lab?

MORAES: Trust.

COHEN: Trust.

MORAES: No, besides all the distractions, I still am very controlling in a way. So if I have the perception that some data is not really right, I will go back to the raw data and I will see the curves and all that. So this is important to me, and that's why the administrative burden bothers me, because I will still be doing it. I will have to make my days longer, I will have to cut on something else, but I am still quite controlling. But yeah, I have to be careful to have enough time to do that.

COHEN: Well, I know you have lab meetings, because you had one yesterday.

MORAES: Yes. We have lab meetings once a week where everyone has ten minutes to give an update on their project. As the lab grows I might have to change that, because it becomes quite long, the lab meeting. But that's the way we have [it], so it keeps people trained on how to be concise and be clear in ten minutes, and also everybody knows what everybody is doing all the time. And I think also it motivates them to not come up to the meeting—actually, this is every two weeks—and just say, "I have done just this one thing in two weeks." That doesn't look very good. No, I'm wrong. We used to have [it] every two weeks, but now we have [it] every week, just because I was having less and less time and the lab meeting was becoming more important to me. Also because some people come to me a lot, and I like that, and others don't, and then the lab meeting is helpful, so at least I know where they are. And if I detect that they're going on the wrong track, I can go to them. So we also have a journal club for the lab every two weeks on a different day. Then we just alternate, everyone will present a paper in our field. So when the lab is done we have the mitochondrial club; that's once a month. I also participate in another club with biochemistry, that's an RNA club. People in the lab are not too crazy about that, so I usually go alone or with one more person. It's a very different area, but it's good, I think, to be exposed to these different things. That's how you get new ideas. If you always go to meetings in your field, you['re] always reading the papers related to what you do, you're going to have the same ideas that other bright people that run labs will have. If you want to have new ideas, you have to explore some other areas, either at the technical level or the concept level.

COHEN: Okay. Well, another ethical issue is [that] we can do all kinds of things now that we didn't used to be able to do, like cloning animals, and people, I'm sure, very soon. In Europe there's a big hullabaloo about genetically modified food. I guess my question is: Should we do everything just because we can?

MORAES: Good question. That's something that I see as inevitable, really. I think it's in the history of the human race that if we can, we will do it. In terms of genetically modified food, one can argue that we don't really need it, but someone can also argue that we need it badly, because in twenty or fifty years, we won't have enough food to feed everybody. I mean, that's another ethical issue that we can discuss; population control and how medicine is kind of creating a problem. So shall we do genetically modified food? I don't know. I just think we will do it because we can. I don't know of any example of something that we could do that we didn't.

COHEN: Well, I heard just on the news a couple of weeks ago that there are people actively pursuing cloning humans now.

MORAES: Yes, they are, and they will.

COHEN: They will.

MORAES: And what's wrong with that? We are playing God. How different is that from in vitro fertilization? As someone commented on TV that I saw, when that started, people thought it was crazy and anti-ethical, and nowadays it's seen as a common form of infertility treatment. People, I think, have a wrong idea about cloning, that you're going to create the same person. That, I don't think, is true at all. You are not going to be able to reproduce the environment that a certain person grew up in. So I don't really have a problem with cloning a person. It might have some tremendous benefits in terms of organ transplantations and things like this. You create a sibling that can save your life. I don't know. There are a number of ethical issues, of course, involved.

COHEN: Yeah.

MORAES: But some families might choose to do it. Is that wrong? I don't know; if the technology is there and that's their choice, maybe it's a valid thing. So what are my views on this? I like basic life, I like quote unquote "primitive life." I don't like what the human race is doing to the earth in general; you know, we are way too many. We are displacing other species, we are changing the environment because we have to produce food for so many people, and really, nobody is doing anything about this. All we do is to try to make people live longer and less people that will die, even if they are in very poor conditions, and that's just compounding the problem. So, you know, I see those things as much bigger problems than cloning a human or creating genetically modified food. I think those things are changing our environment much more and making a world that I don't particularly like. So my main ethical problems really are

with population control and all the effort we do to keep everybody alive.

Now this is a huge ethical problem also, and, as an example, [so] is the AIDS [acquired immunodeficiency syndrome] epidemic in Africa. You probably were not there, or maybe you were, during the reunion meeting. I don't remember. But at one of the Pew meetings, they described some shocking things about what's going on in Africa and how so many people are dying. But you can take an alternative view and think that these things develop when population is very high. Viruses will usually propagate when those conditions are created, and it's a way to control population. Now, you cannot see a child suffering and not do anything about it; that's our nature. So I'm pretty sure that we are going to ultimately control that, and this disaster is going to stop and the population will start to grow again, not only in Africa, but everywhere else in the world. And I'm really concerned with where we are going and what the world is going to look like when we are, I don't know, 12 billion. I'm also amazed that governments are not really being more active with this. China has been extremely criticized for doing something related to birth control. Of course, they're maybe already too late, because they have billions and billions of people, but at least they start[ed] trying to do something. Of course, religion has a major stake in that. They are completely against it, and they think we just should procreate until the earth explodes. But that's really my main ethical problem that I have, is how we're not controlling the population.

COHEN: But actually, in a way, biomedical science is contributing to that problem, because we're trying to find cures and ways to keep people alive longer.

MORAES: Of course it is, so what I do is against what I think— I don't know. It's compounding what I see as a major problem. Yeah, there is a duality there. Human beings are extremely curious. [phone rings]

COHEN: Let's pause for a second. [tape recorder off] Okay, we are back on tape.

MORAES: Yeah, I think I was talking about the duality of human beings, that— Like in my case, for example, I am very curious about the natural forces, and I want to understand them better. There is a lot of satisfaction in finding something that can help cure disease, but as I just mentioned, as a species I think we are going through a very dangerous phase of not paying attention to overpopulation and just trying to cure things and forgetting that there is a limit to how many people can live.

COHEN: Okay, there's one other ethical issue that I want to talk about, because you and I talked about it just a bit off tape, and that is the issue of animal use in research, because I know you have some mixed feelings about it. So maybe you could elaborate a little on that.

MORAES: Yes, I have very mixed feelings about it. Well, the one that— Who gave us the right to use animals for research? We did, right? So, I mean, you can always wonder, if some big giant from another planet comes here and thinks it can use us for research, what's wrong with that, right? There's nothing wrong with that for them. For us, it is a big problem.

COHEN: Right.

MORAES: We can argue for a long time about the difference between human beings and animals, and how we have a conscience and a memory, but to me we use those things as excuses to justify what we are doing as something that is right. Is it right? Well, we are saying that it's right, but that doesn't mean that it is right. I'm not happy about using animals for research, but I do, and I think, as I discussed with you, I do it with full knowledge that I am the one that is giving me the right to do so, and that's not a fair way of doing things. The argument for biomedical research and the use of animals is that we can save the lives of children, we can save the lives of many different people, you can owe your own life to animal research. And all of this is true, but still, we just go back to the same point I made before: who gave us the right? To me, that's the bottom line. I think we are pretentious enough to think that our decisions are final and fair because we say so, and that's the bottom line. I do use it, but I feel just like the way I described. I feel that I'm doing something that is not fair, really.

[END OF TAPE 5, SIDE 2]

COHEN: Okay, we are back on tape, and I want to talk a little bit—to kind of wrap things up—about how— How do you think you're doing in terms of meeting your own professional goals for yourself? No one else's, just yours.

MORAES: I have no idea what I'm doing. What am I doing to meet my professional goals? My professional goals are really to produce good science; it's as simple as that. Something that's exciting, maybe some new insights into my field. So to meet those goals, I just have to work hard, work smart, and have good help. The only way I can do that is do what I'm doing, just try to guide people in the lab to the best of my abilities, and hopefully I can do some experiments once in awhile. But I'm trying to really improve the quality of my research by not publishing very preliminary pieces of data, trying to make more of a full story, and those papers will have more— They do have more of an impact. So as I advance my career and I have quite a lot of publications, I am really de-emphasizing the quantity of publications and trying to concentrate on having better ones. So I expect the number of publications to maybe drop or stay constant even though the lab is growing, but hopefully they will all be in good journals.

COHEN: If you had to give yourself a report card now on your career, how are you doing?

MORAES: Well, it depend[s] on which aspect of my career. For funding I am doing extremely well, much better than I thought I would. I am really never completely satisfied with the research. I always think I should be doing better. I don't know if I'm being overcritical of myself or [if] I'm right. I think I'm right. So if you ask anyone in the university, they will give me a great report card, I think, because I think I'm liked by people, and I'm doing okay; I'm publishing and I'm getting funds, and actually, getting funds, for the university, is a synonym for doing very well. That's all that matters. But if I have to give [myself] a report card, I would give [myself] a B. That's because I think I could or should be doing better and have better ideas or have ideas that can actually work, because sometimes we have some crazy ideas that would be great if it works, but they're so crazy that they don't work.

COHEN: When you look at your personal life, and if you had to give yourself a report card—Based on your own goals, nobody else's goals, but just on your goals for your personal life, how are you doing?

MORAES: Oh, my God, is [Maria] Julia [L. Pace] going to read this?

COHEN: [laughs] Well, you have an editing option.

MORAES: Well, my personal life, I think, is more stable, and I have kind of accomplished most of the goals I had. I have a good wife that I love. I really wanted to have a child, and it was a fight with Julia to have one; because of her upbringing and problems she had with her parents—they got separated—and how she felt abandoned by her father and all that, she was very afraid to have a kid. She always mentioned that it was because her body was going to change, but I think it was way beyond that. But ultimately she agreed, and she went for it. And we have a daughter [Emily Kay Moraes] now, and that's an experience I wanted to have and I'm happy and it's great. Maybe I'm a little bit more social than Julia, even though I'm not extremely social. But Julia can— For her, happiness is to be left alone, and that includes not me. Reading a book— Maybe with Emily now. So our social life is pretty slow, and I don't see it picking up with time. We have friends, and we still see friends once in a while, but maybe it's also normal for where we are in our lives. It's not the same intensity as when we were a young couple. So I'm pretty happy with my personal life. Yeah, maybe our social life could be a little peppier, but it's really not a big issue for me, because I'm also not that peppy. So I would say I'm more accomplished there than in my professional life, in my own view, my report card, so I'll give [myself] an A.

COHEN: Have you ever thought about what you would do—this is your chance to just kind of

fantasize a little bit—if you weren't a scientist?

MORAES: If I weren't a scientist? I think I could do anything, really, and be happy. Of course, I would not like to do something that's boring, but, for example, design, graphic design. Not that I draw well, but if you have to use computers, I could do it, probably. I'm not very good in math, so I'd probably not be a good engineer or even an architect. When I was younger, I think I was pretty creative in putting things together, you know, making things. [I was] quite industrious. So I think I would not have a problem being something else, as long as I could express some creativity and explore different things. That, I think, is the main thing for me, that I enjoy. It's just to have one day different from the other. I don't know how many professions can actually do that, so I might be in a much narrower field than I think. So research fits very well this aspect of my liking. What else could I be doing? I don't know. I could be a lifeguard on *Baywatch.*

COHEN: [laughs] Professional windsurfer.

MORAES: Well, yeah. That would be nice. I would have to dedicate myself more. Move to a place with more wind. Yeah, it's hard to tell really, but I think I adapt easily, so I think I could be doing pretty much anything, and God knows if one different decision might have changed all that and I would be doing something else, like what I mentioned; if I was accepted to Dow Chemicals, I might be even doing something boring. I don't know. I'm very driven and I'm very ambitious, so even there I think I would probably be doing something, after some years, that would be kind of interesting.

COHEN: So if you looked in a crystal ball, what would you see five years from now?

MORAES: From my life? Work? Or anything?

COHEN: Both? Anything?

MORAES: More of the same.

COHEN: More of the same.

MORAES: Five years from now, definitely more of the same. My grants now are running for five years, so I'll be going towards the end of that, and probably going crazy trying to renew

them. The only thing I hope—and maybe I can manage—is to not be buried by administrative responsibilities, but I really don't see how I can stop that. It looks like a hopeless thing, even though I will try. But I don't see my life much different. Hopefully I will move to a house out of a condo, so we'll have a little bit more space, even though the idea of moving already turns me [off], because I don't have the time for that, let alone to look for a house. At work, moving here to a new lab in this building where we are doing this interview. That would be nice, I think, because I will have many more research groups around me, and I think that will have a positive impact on my research. So that's what I can hope for in the next five years, to maybe find some niches that are more original within my field and do things that turn me on, and continue to be excited and motivated about the work. And that's what I will try to do, and the crystal ball tells me that I will probably succeed in some things, and not others.

COHEN: How about ten years?

MORAES: Ten years, more of the same.

COHEN: More of the same.

MORAES: Let's see— I'm thirty-eight now, I will be forty-eight. I will be ready for retirement, probably [laughs]. I don't know. I don't plan too much in advance, so that's not something I think about. Actually, I don't even plan, like, in the year or so, and that might be one of my weaknesses. So I have no idea ten years from now. The crystal ball tells me that I will be around here, probably, or hopefully, with a nice group, people having fun at work, funding not being a problem, but I really have no clue. I will not predict anything, because I might bite my tongue.

COHEN: Some people have aspirations to be chairman of their department or that kind of stuff. Do you have any of those kinds of aspirations?

MORAES: Not me. As I mentioned over and over, administrative responsibilities are the worst nightmare for me. Now, I would love to one day be a member of the National Academy of Science. Will that happen? I think it's unlikely, for a number of reasons: the field I'm in, the university I'm in, the kind of work I do, because it's not so high impact. But it actually depends on me, and that's the bottom line, so I will always try to do the best I can, and if I reach that point, I will be extremely gratified and pleased. I wouldn't call that an aspiration, because I don't see it as a likely thing to happen. So I will go back now to my role models that were more senior investigators that were working in the lab, not with huge labs. I wish I could be like that, so that I will still [be] having fun doing research, maybe doing some work in the lab so I'm not losing contact completely, and interacting with the more junior people in the lab. So that's really my aspiration.

COHEN: Well, actually, I have asked all the questions that I have, and so I would like to, before we finish, offer you the opportunity to add anything you might like to add.

MORAES: Yeah, I don't know if I have anything to add. We've been talking for three days, and I think I've talked about anything and everything that has ever passed through my mind. Well, the one or two people that might eventually read this might be looking for insights on how people become researchers, and I think the Pew group is a successful group. I'm probably an accident in that group, even though I'm doing okay, actually. I think that the bottom line is that we're an extremely heterogeneous group, and people come to this position through different paths, and, as I said, some people might know what they want very early on, but in my case, and maybe in most of the cases, it was pure chance. That's definitely not a very romantic thing or inspirational, but I think that's the reality. But there's an underlying drive that people have, I think, that would direct your life to something like research. In my case, and in many of the scholars', I think it would be we're extremely curious, we're ambitious, and we enjoy the fast pace of science. So even though circumstances would take you to one place or another, I think those basic features will definitely help direct your path. And life in research is good as much as life is good in any other profession, I think. There are pluses and minuses, but I like it. I think that's it.

COHEN: Okay. Well, thank you.

MORAES: Thank you.

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[END OF INTERVIEW]

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