



The American Association of Immunologists Oral History Project

Transcript

Leslie J. Berg, Ph.D.
November 1, 2012
Worcester, MA

Interview conducted by
Brien Williams, Ph.D.

Transcription: TechniType Transcripts
Transcript copy editors: Bryan D. Peery and Elizabeth R. Walsh
Final edit by: John S. Emrich

© 2013 The American Association of Immunologists, Inc.

Publicly released transcripts of The American Association of Immunologists, Inc. (AAI) Oral History Project are freely available for non-commercial use according to the Fair Use provisions of the United States Copyright Code and International Copyright Law. Advance written permission is required for reproduction, redistribution, and extensive quotation or excerpting. Permission requests should be made to: The American Association of Immunologists, 9650 Rockville Pike, Bethesda, MD 20814-3994.

To cite an interview, please use the following general format: [Name of interviewee], interview by [name of interviewer], [date], The American Association of Immunologists Oral History Project. <http://www.aai.org/OHP> (accessed [date]).

Williams: This is an interview with Dr. Leslie Berg for The American Association of Immunologists Centennial Oral History Project. Dr. Berg is professor of pathology and the graduate director of the Immunology and Virology Program at the University of Massachusetts Medical School. Dr. Berg was president of the American Association of Immunologists from 2011 to 2012 and served as an AAI Council member from 2006 to 2011. She was awarded the AAI-PharMingen Investigators Award in 2001 and the AAI Distinguished Service Award in 2006. We are in Dr. Berg's office at the University of Massachusetts Medical School. Today is Thursday, November 1, 2012, and I am Brien Williams.

Well, a very distinguished introduction there. Tell me a little bit about your family background.

Berg: So my parents are both native New Yorkers, and they got married in the early 1950s and moved to New Jersey, the suburbs, where I guess most good New Yorkers moved when they had kids, and so I was born in New Jersey.

Then my dad was a very early person working in the computer field, so he has an MBA, but he was doing what they called data processing for a big company that made airplanes or airplane motors or something like that. Then he got recruited to Xerox, I guess it was around 1960, and they moved to upstate New York, to Rochester, the headquarters of Xerox Corporation at the time. So we moved there, and I lived in upstate New York till I was twelve years old.

My mother has a teaching degree. She was, I think, a frustrated child of her parents' views, especially her father's views, that women shouldn't really have careers except for being nurses and teachers. So she was, unfortunately, kind of pushed into the teaching profession, which probably was not a good fit with her. She told me once she always wanted to be a journalist, and it just was not okay with the family scenario. So she didn't really work much when I was growing up. She did some substitute teaching in the public schools and things.

So then in around 1969, my dad was transferred to Southern California to head up a company, a little company that Xerox had acquired that was in L.A., and the family moved to L.A. Then about two years later, that situation at Xerox, they decided this was a mistake and they didn't want this company anymore, and they tried to move my parents back to the East Coast. But that didn't work, so my dad left and started working on his own. So he was basically self-employed for the rest of the time I was a kid in middle school, high school. We lived in Beverly Hills, California. I went to Beverly Hill High School, which was an interesting place, actually.

Williams: So how interesting?

Berg: How was it interesting?

Williams: Yes.

Berg: Well, so Beverly Hills is its own city, it has its own school district, so it's not part of an L.A. school system, which is the reason my parents moved there. I remember when we moved there from upstate New York, my mother sitting us down and saying to my two brothers and I, "You know, you're going to have a lot of friends whose families have a lot of money and they have a lot of things that we're not going to be able to have, and, basically, live with it, deal with it. Just don't expect that all of a sudden you're going to be able to live like everybody around you."

So in school I had friends who had houses with tennis courts in the backyard and whatever, went on fantastic family vacations all the time. We were not living in the poorhouse, but certainly we didn't have chauffeurs or Mercedes-Benzes or any of those things. Beverly Hills had a very good school system, so I think that certainly served me well. People have the idea there were a lot of movie stars lived there, but it turns out that's relatively uncommon, at least in those days. This was in the seventies and eighties. Most of the kids were children of families whose fathers, a lot of them, were in the movie business, but they tended to be more the producers and the directors, not so much the actors. There were a lot of people whose families' dads were lawyers or bankers or whatever. So it was, I think, interesting in that there was certainly a lot of kids with a lot of expendable—whatever the word is—income that were abusing many pharmaceutical substances. There was a lot of money for drugs and things like that.

So I think it was a good way to learn to make your own decisions and learn some self-control, because there were certainly temptations and things around you that you could get yourself into serious trouble if you didn't manage to learn how to say no and avoid that kind of stuff. Then I think the other half of it was the academics. It was a great school, and there were a lot of kids who were very motivated academically, so I tended to hang out with that crowd and avoid the druggies and whatever, but it was definitely a part of the whole culture.

Williams: So your father was maintaining his own profession as an individual.

Berg: Yes. I think most of the time I was still in school and living at home, he essentially started a business where he outfitted doctors', physicians' practices with computer systems. So this was like the first wave of people, small, like, business computers, so they would have computer programs to do their billing and start keeping track of their patient records. He essentially would help them install the software and then train the secretaries or people at the front desk or whatever to use the software. Then, of course, the inevitable problems, things wouldn't work or crash or whatever, and he would go and fix them and get them back up and running and stuff. So he did that for many years, and, I guess, successfully.

Maybe I was an unusual kid. I never worried about it. As long as it seemed like he was doing okay and things were okay, I don't remember having a huge amount of information. I think, with my dad, even before he worked at home when he was working at companies, I don't think I had a really clear idea of what he did all day. Probably what I do all day, which is go to meetings. I mean, now it's more clear what you do in office all day. But, yes, it worked out okay. I think he was successful and, yes, did that for a long time. Since he was working for himself, I don't think he ever really retired in a formal way. I think he kind of slowly wound down as he got older, took fewer new jobs, and then sort of phased things out.

Both my parents are still alive. My dad is eighty-seven; my mother's eighty-two.

Williams: Still enjoying Southern California?

Berg: Yes, although my dad has Parkinson's, which he was diagnosed with in his seventies, and it's now gotten pretty bad, so he doesn't get around much. He's really not very strong, and he needs care. He doesn't have 'round-the-clock care. He's got someone that comes to the house during the day for six or so hours so my mother can get out and do something, because otherwise he can't really stay alone, and someone needs to be there to help him and stuff. But I think up until a few years ago when they were mobile, they certainly did enjoy Southern California.

Williams: Just a footnote. What about your grandparents?

Berg: So both sets of grandparents, their family roots are in Eastern Europe. They're Eastern European Jews, and both my grandfathers, one on each side, were born in Eastern Europe. One of them was born I think in what's now Poland but I think at the time it was like Eastern Russia or something, Western Russia, I guess. The other one, I'm not sure where he was born, but they were both born in that part of the world and were part of these big families with nine, ten kids that immigrated to the U.S. in the early 1900s. One of my grandfathers, I know, moved around 1915 or something. I think he was drafted into the Kaiser's army in the Hungarian Empire, and he didn't want to serve in the army, so that was the end of him, and he moved to the U.S.

Both my grandmothers were also part of Eastern European Jewish families that moved. They all lived in the Lower East Side in New York, but they were both born in the U.S., so they were part of families that their parents had moved, and a few siblings, and then they were born later. So they all lived in New York, and both my parents were born there.

One set of grandparents, my dad's parents, my grandfather was a tailor, and he worked at Saks Fifth Avenue and was in charge of the couturier salon where they

made the dresses. I guess the designs would come and somebody has to sew them into stuff. So he ran that workshop or whatever you called it. My other grandfather was a plumber and had actually bought some property in New York. He managed apartment buildings and things like that. So they both did well, the grandparents, sets of grandparents.

Williams: And your parents met in New York City?

Berg: Yes. So I think they were fixed up on a blind date and like, I guess, was common in those days, I think they were married about four months after they met. My dad hadn't served in World War II, and so he had been eighteen when—let's see. If he was born in '25, so I think it was around 1943, so that he started in the army, I think it was after D-Day, but while there was still fighting going on, and he was stationed in Italy and fought in World War II.

He became a medic, which he's not a doctor, so I think they trained people to be able to give first aid on the battlefield. So I guess the idea was the medics were supposed to be noncombatants, like he wasn't supposed to be shot at or shoot people, but, of course, he did get shot at. So there's all sorts of old family stories about things that happened on the battlefield, and probably you don't want to hear all those family stories, but, anyway, so he was very good at bandaging boobos when I was kid. I mean, when you fall off your bike and scrape your knee, you never went to Mom. You went right to Dad because he was very calm and professional about it and very good at bandaging.

Williams: Did you catch the science bug while you were at Beverly Hills High, or did that come later?

Berg: No, it started at high school. So I was interested in science, I think in sort of biomedical science, and I did a few things in the summers. Between school years, I worked once, like, volunteering in a lab at UCLA, which was kind of down the road from my high school, and I worked in a neurobiology lab. I think I did some other science-related program one other summer.

But I wasn't really sure, I think, when I started college what I wanted to do, because actually when I worked in this lab over the summer, I didn't really like it. I think it was the wrong kind of science. That particular lab was a lab that worked on sleep research. They did sleep research using cats, and so they did a lot of what's called electrophysiology, where they put electrodes in the brains and then when the cats go to sleep, they record their brainwaves and they correlate that with whatever, the body physiology, and other things. It turned out it just didn't capture my passion, I guess.

So I went to college, and I wasn't really sure what I was going to do. I was a biology major, but I had no idea what I was going to end up doing with that. I have to say that unlike kids these days, I don't think we worried so much in those

days about what you were going to do. I mean, you went to college to go to college, and things were going to take care of themselves. I think maybe the world is more complicated now, I don't know, but kids seem much more aware of their future and having concerns about it, what career they're going to have and what job, and I don't remember worrying about any of that.

Williams: Were several of your classmates also headed to Harvard?

Berg: At my high school, two of us went to Harvard, and my friends, actually all of them—as I told you, I ended up hanging out with the sort of academically-minded group. So of my immediate friends, four or five girls, one went to Yale, one went to Princeton, one went to Berkeley, and I went to Harvard. Then one other boy in my class was the other person that went to Harvard, so there were just two of us. And a bunch of kids went to Stanford. I would say for most of being a West Coast school, that was sort of the Ivy League version of most people's aspirations. It was pretty rare that people were interested in going back east. I think because of my family background, having grown up on the East Coast, I don't know, I was tending in that direction.

Williams: Was there any particular reason why you chose Harvard?

Berg: Yes. So, again, one of these family lore stories. So when we were little, we would go on the typical American road-trip family vacations in the summer. One summer, I think I was probably about ten, living in upstate New York, we did the Boston, Cape Cod, New England trip, and I remember going to Harvard Square and thinking, "This is the best place I've ever seen, and I have to come here and go to college, because this is the most fun place I've ever seen in my—," you know. It's just so full of life. Harvard Square was full of college-age students and endless bookstores and cafes and just a hopping place, and I decided right then and there that was my aspiration, I guess, was to go there, go to college there.

Williams: So high school grades were pretty important to you if you kept that goal in mind.

Berg: Yes. I think so. I'm not really sure how much I remember. Of course, maybe I'm just forgetting, but definitely it was very important to me getting good grades in high school, but I don't actually remembering thinking, "I need this to get into Harvard." By that point in time, I think it was more just general feeling like I want to go to a really good topnotch school, I need good grades, and I need to do whatever other stuff.

So I was actually not valedictorian of my high school class; I was number two, and I was perfectly happy because I actually didn't like public speaking. I did not want to be the valedictorian. I did not want to have to give the little talk at graduation, so I was perfectly okay with that. But in our high school we had, you know, whatever grades, and then you got different kind of points, depending on what level the courses were, if they were honors courses or AP courses. So since

I was actually really uninterested in history, so I didn't take some AP history class that one of my friends took, and so she had like one more point or something than me. [laughs] So that was it for my being number one. So I had to settle for number two. But, yes, I did, I think I strived hard to get good grades.

Williams: So what words would come to mind to describe your four years at Harvard?

Berg: Words come to mind? Well, definitely eye-opening. I can't even think of individual words that describe it. What?

Williams: What was it like?

Berg: Oh, yes, I can talk about that. So there were a couple things that had a big impact on me. So, of course, there are always the friends, which are a big part of your life at that age, but in terms of educational or academic things, there are two things that really had a lot of impact. So one is I got involved with helping to teach a little seminar course with a professor who's actually an astronomy professor, who taught things like—it was almost a cross between philosophy and astrophysics, anyway, about the meaning of life and all this kind of stuff. I just thought this was really fun, so I helped him put together a little seminar course that I helped him teach one semester when I was, like, a junior or senior. So that was a lot of fun and that was interesting, but it wasn't really part of the long-term future.

But what happened, I guess, my junior year of college was I discovered molecular biology. As I mentioned, in high school I did some research experiences in the summer, and when I started college, I was a biology major, and I actually didn't like most of the introductory biology that you learned in college. It was all about how many seeds a plant has and what the name of it, you know, yuck. Descriptive, like, ecology and stuff. We used to have to learn things like when a forest fire burns everything down, what trees grow back first and then what grows second. I was like, "I don't care. I don't want to know this." I don't want to know what classifications of species and this kind of archaic stuff that reminded me of Old English Oxford professors and stuff.

Anyway, so it turned out that right about this time molecular biology was sort of getting off the ground, and there was over the decade or so before I was in college, or maybe twenty years, people figuring out the genetic code and how proteins were made and how the DNA encoded the sequences of amino acids for various proteins and how bacteria genetics worked and how they could regulate the transcription of genes. I mean, there's all this molecular stuff, and the experiments that people did to figure these things out were so intriguing to me, because they were a combination of real in-the-lab wet-bench experiments with some kind of theoretical—I don't even know what to call it—model. Like, well, if this is true and it works like this, then if we do this experiment, the answer A

will tell us it's this mechanism. The answer B will tell us, no, it's some other mechanism.

To me, what was the most exciting part of it was this combination of using your brain to figure out how you think things work and then designing an experiment which would categorically tell you yes or no, it's like that or it's not. It was not just poking at things and seeing how—you know, this descriptive kind of biology that was very traditional, where you looked at things and described them and write down the bark is brown and it's this rough and it's got this many stripes. I mean, who cares? [laughs] So I'm not that kind of scientist. I like to think about things and figure out models.

And all of a sudden I realize that there is this kind of biology. It's just something I hadn't learned about before. So that totally changed my life. Then I decided to go to graduate school. So I graduated from college in 1980, so that fall or whatever, I applied to graduate school. My father, being a good businessman, sent me articles from *The Wall Street Journal* in the mail in those days, "No Jobs for Ph.D.'s." Those were the titles of these articles he was reading, and he was mailing them to me. I'm like tossing them in the trash, right? I don't care. What do I care if there's a job? This is not about getting a job. This is what I want to do, right? I'll worry about that later.

You know, I've often wondered if—people worry about discrimination against women in science and other traditionally male fields, and I actually think there's something liberating, for me, about having been female, which is that I don't think it ever occurred to me to worry about being a breadwinner, having to have a job, a career that would provide for a family and make enough money to support a family. You know, it was not something I ever thought about. I wonder—I don't know, you're a man—whether this is something as a college-age male that is a part of your—it's just imprinted in you to worry about this, because I never considered that for a second.

Williams: While your father was mailing you all these warnings, did he have an alternative plan for you?

Berg: No, of course not. [laughs] I don't think so. So I would say his view of mailing of the articles was more of a "Go into it with your eyes open. If this is what you want to do, do it, but do it knowing what the prospects are." I think he was more worried that I would have some false sense of whatever would be down the road.

Williams: So what were the steps that took you to Berkeley?

Berg: So then I applied to graduate schools. So I applied to programs at places where they had good molecular biology programs, and, actually, it's like other people's stories, I guess. It always turns out there are influences of individual people on what decisions you make that just are completely serendipitous. So I had a

professor named Rich Losick who was a biology professor at Harvard who taught a very small seminar course I took my senior year that was some kind of molecular biology course where we read research papers and then met and discussed them. This course had five students and two professors, and three of the students were graduate students, and then there was me and another undergraduate. So it was fantastic, because you had the undivided attention, nearly, of two Harvard professors twice a week for a couple hours.

So when I was talking to this professor about applying to graduate school, he was the one that was advising me about where to apply, because I didn't know. I had no idea where good departments were. So I applied to MIT [Massachusetts Institute of Technology], I applied to Berkeley, I applied to Harvard, and where else did I apply? Those are the ones I remember. I don't even remember, probably like a couple others, but not very many, maybe five altogether.

So then what happened in those days was you applied, and if you got in, you could go visit, even though they didn't pay for it. You had to pay your own way, but you could go visit. They would arrange for you to meet students and faculty and find you a place to stay or something. So I went and visited MIT and Berkeley. I decided, "Okay, I've been at Harvard. I'm not going to probably go there."

MIT at the time was considered probably one of the best departments in the country, and I found the graduates students there so snooty. They basically said to me, "You got in here and you got into Berkeley, and you're actually thinking about going to Berkeley?" They thought it was a no-brainer. You got in here, you would come. There was just no two ways about it, that was their view. I just thought, "Screw this. I don't need any part of it. I've been at Harvard for four years surrounded by people who think they're the cat's meow. I don't need any more of this. I'm ready for some place where people are not so full of themselves." And I really think it was interesting, but I just kind of had a negative reaction to that.

So then I went to Berkeley, which, of course, it became a really, really well-known Biology Department. But right when I applied, I think, was just at the beginning of it turning around, so it wasn't up there at the top.

The reason my Harvard professor had recommended it is that he had a couple good friends, colleagues, whatever, that had just moved there and joined the faculty. They had both been at Cold Spring Harbor. One of them was this guy Michael Botchan, who became my Ph.D. thesis advisor. I remember Rich Losick telling me, "Oh, you should go to Berkeley because Botchan's there and Tjian's there," and a couple people he knew had just moved there and joined the faculty.

Anyway, so I went out to visit Berkeley, and there's something about its wackiness. It's a place that's unlike any other. At least in those days, it was full

of fringe people. So something appealed to me, I guess, so I ended up going to Berkeley and went to work for this guy Michael Botchan, who had been recommended by my Harvard professor. And that was great. I had a blast.

Williams: How come?

Berg: Well, so Berkeley is a great place, and I think after I finished graduate school, in the back of my mind I had always had this thought, “Okay, one day I want to end up here on the faculty, because this is the place to be.” It’s a beautiful place. It was full of people that are doing their own thing. There’s something about that place that it’s just so accepting of everything. People are not pigeonholed into little whatever, categories. The department I was in at the time had a lot of faculty that ended up—I mean, in our culture here, they would be considered nutcases, I think. Whether they were or not is debatable, I guess, but they were people that were into alternative religions and Indian gurus and weird stuff.

Okay. But why was Berkeley so great? So my thesis advisor, Michael Botchan, was just the best. Like all of us, he had his flaws, but he was someone who taught me a lot of really important things about science. One was, he always had a sort of passion and excitement for every new result. His well never ran dry. I mean, he just would get excited over everything that happened that was good. What I really, I guess, appreciated about him, he always had some big-picture model of how it all fit together. So whatever we were working on, whatever else information there was in the literature and from his colleagues, he had it all put together into some big “This is how it works.”

But the best part about him was when someone would do an experiment that was completely irreconcilable with his model, the model, out the window, gone. It might have been the best thing he’d ever thought of for the last month, but if there was one piece of data that said no, this is not the way it is, okay, that’s it, out the window. Then, of course, it would take him maybe till the next morning. He’d be running in the lab the next morning, “Oh, no! Now I’ve got it! I figured it out. Well, this other was wrong, and that experiment said this was wrong, but now I think it’s like this.”

There’s something about most scientists find it very hard to give up their pet theories, and it’s a hard thing to do. If you’ve been believing for months or however long that things work a certain way and then the data start coming in that disagree with that, there are people who just can’t give it up. They find some way to discount the data that doesn’t agree, and they just say, “No, that experiment can’t be right because—” or, “That happened because of some other reason and it doesn’t really mean my model’s wrong.”

I think that’s okay to a certain extent if you’re being rational about it. There’s a little bit of that. You have to be somewhat selective if you think something’s relevant or not. But at some point you’ve gone from science to religion. It’s not

science anymore. If the data are disagreeing and you're discounting that because of your beliefs, to me, that's not science anymore. That's a different—we shouldn't get talking about religion. But, anyway, that's religion. That's not science.

So it was such a good learning experience to be around someone like that who would let the data inform his models and not the other way around, and to learn that when something happens that disagrees with your pet model of the week or year, you better darn throw that model out the window.

Williams: Did some of your work cause corrective action on his part, or not?

Berg: I'm sure it did. I think everything—yes, I think there was some of that. I think other people in the lab's experiments, my experiments, they were often—I mean, it happens all the time. You do experiments thinking you know what the answer's going to be, and then it comes out different. Half the time that happens, half the time you're right and you did predict the answer. Half the time you're wrong. So I'm sure that it was some of my experiments.

I think that was part of, I guess, the second thing about Mike that was really, really special, was that he treated the graduate students in the lab with as much respect for our ideas and thoughts as he did the postdocs and his colleagues, faculty colleagues, and colleagues across the country. So if you had an idea about something, he would be happy to sit down and talk to you about it. He wasn't the, "Oh, you know, I don't need to talk to you. What could you possibly say that would be of interest to me? You're just a graduate student."

So I think I got spoiled because I was used to him taking me seriously and listening to me and being interested in what I had to say, and wanting to know what I thought about a seminar we had just been to or some talk we had heard or some paper I had read. It was a shock to discover after leaving his lab that not everyone was like that, because, yes, I was spoiled. So he was a great mentor because I think he encouraged us students to think about what we were doing and to have opinions and argue for them, and not just be a technician and a pair of hands doing experiments.

Williams: How early on in your study there did you come to your thesis topic?

Berg: I think probably after maybe two years. I think for the first year I did things that—so I have to preface this by saying when I went to graduate school, although I had worked in this lab when I was in high school, this neurobiology lab, which was a completely different kind of science, I actually never worked in a research lab when I was in college. I never did any research, so I had no skills. I had absolutely no experimental skills. I didn't know how to do anything. So, that's very unusual. Nowadays, people don't go to graduate school if they don't have lab experience and work in the labs as undergrads or technicians or something.

So I think the first year I spent just bumbling around, being clueless, having nothing work, just doing things, and they were disasters. So that took a while to fix that problem. It's hard to know how much your memory exaggerates things. I just remember being a complete disaster.

Williams: Is that a critique at all on the program at Harvard?

Berg: Well, no, I think it was a critique on me not figuring out early enough what I wanted to do. So when years later I was on the faculty in that department at Harvard that I'd been an undergrad in, and we had plenty of undergrads working in the lab. I had tons of undergrads wanting to work in the lab, but they were people who had figured out early on what they wanted to do. They figured out either as sophomores or juniors that either they were interested in graduate school or M.D./Ph.D. programs or medical school, even. And I think I didn't figure all this out until I was a senior, and so it was too late by then to do anything about it, and so I think that was my fault. I think the opportunities were there. Plenty of other people were working in labs and doing research. I think it was my problem, not the school's problem.

Williams: So, briefly, what did your research work end up being at Cal?

Berg: So I worked on DNA replication. So Mike's lab in general was very interested in basically DNA tumor viruses and also kind of as a way of understanding how mammalian cells replicate their DNA, because viruses have to do it in the cell. So it's like a little microcosm of what the whole cell and all the chromosomes are doing. So his initial work when he first came to Berkeley when I was involved, it was all studying the replication of a small DNA virus.

So he had originally worked on a virus called SB40, and when I started in his lab, he'd just started this brand-new project that ended up being most of his lab for many years, working on bovine papillomavirus. So papillomaviruses are viruses that cause things like warts, but the famous papillomaviruses, the human papillomaviruses—there are two serotypes of them—cause cervical cancer.

So we worked on the cow virus, because at the time it was something that could be grown in cell culture, and the human viruses at that time, people hadn't really figured out how to grow them in tissue culture cells, and so they were harder to work on. So we worked on the one that was sort of more experimentally amenable, and they're all pretty closely related to each other. But this particular virus, papillomaviruses, generally most of them cause things like plantar's warts and things.

We would go to conferences, papillomavirus meetings, that were very small. It was kind of a small field, but some part of the audience, a third of the people at these meetings were clinicians who treated people with all sorts of kinds of warts, and they really reveled in grossing out the basic scientists like us who did

experiments on DNA and tissue culture cells, and we never saw the warts that the viruses would cause. We never really had anything to do with that, but they would show these really wild-looking pictures of the kinds of diseases these things caused. Yes, it was kind of an interesting experience.

So I worked on DNA replication. At the time, the whole genome of the virus is very small, and so it had just been sequenced, but there was really nothing known about what proteins were encoded by the virus, what the functions of those proteins were. So my work was we identified a number of the genes by cloning the cDNAs that were encoded by the virus and figuring out what proteins they encoded and then doing experiments to work out what some of the functions were. So I worked on one of the proteins that turned out to be important in regulating transcription of the virus and a little bit on one of the other proteins that controlled the replication of the virus. So we were trying to figure out how it was regulated, how the virus when it got in cells, how it set up shop and replicated its genome and became part of the cell and stuff like that.

Williams: So then when you moved to Stanford, which I think you did next, did you take that kind of interest in work with you, or is that a whole new set of . . . ?

Berg: No, I switched totally. I guess when I was in graduate school, probably around after three years or so, you start thinking about what am I going to do next. So I, I guess, put some thought into what kind of field I wanted to work in, and I think I decided to go into immunology with a bunch of weird criteria that landed me there that are strange to think about, because I didn't actually know any immunology. So I had never taken an immunology course.

So I knew about antibodies, which everybody knew about. That was about it. So I had been working on DNA replication, which is something that happens in the nucleus of a cell, right? I decided I wanted to work on a field that involved cells communicating with each other, not just one cell doing it, whatever, and how multiple cells in a system interacted. So there were a number of fields that I could have gone into, but one of the things that I realized at the time I thought would be really important would be to have a field of research which also had good genetics, because, for whatever reason, I decided that was really important in being able to figure out how things worked.

If you have a complicated system of multiple cells in an organism, you need genetics to help you figure out which are being able to make mutants that have defects so you can figure out what's important. So that made me think about what people call model organisms, right? So a lot of biology and biomedical research is done on things like fruit flies and yeast and these little things called *C. elegans*, which are little worms, nematodes, and stuff like that, and even plants. I thought, "Okay. This is where all the genetics is."

I finally decided, after I took some little mini courses they had when I was in graduate school, they were eight-week seminar courses in various topics, so I tried out a bunch of different things, and I finally decided I cannot work on fruit flies or these other things. I don't really care how wings are made. Like, I don't have wings. Why would I care how a fruit fly makes wings or antennas? Like, I don't care. So that was a problem, actually. [laughs]

So I decided I have to work on a mammal, right? And so that left mice. At the time there weren't really other choices. So there were actually almost no fields in 1986 that had good genetics in mice. There was some development in biology, and immunology was one of the few fields that had real genetics in those days in mice. So that, together with this, you have to have—okay.

So then the third thing I thought was, you can't do everything on animals, organisms, because it's too complicated. You need to be able to do things in tissue culture. So neurobiology was out because you could do genetics and you could have mice, but you couldn't do anything in a tissue culture. You can't put neurons in a tissue culture dish. Now they can do things, but in those days, they didn't grow, they didn't work, you couldn't study them.

Lymphocytes, which are the cells I studied in the immune system, are these amazing things that they work in an animal and they work in a Petri dish. You take them out, you put them in a Petri dish, and they do nearly the same exact things they will do in an organism. So somehow I figured all that out without really knowing anything specific, and I decided this has got all the features of a field that you could really get something to happen. You can really attack it at all these different levels with all these different tools.

So I decided I needed to go into immunology. It's funny how you think about these things, that you make decisions for most of your life based on too little information and sort of gut feelings and things that after the fact seem like they don't make any sense. So, anyway, that's how I made my decision.

Then I just had to figure out what lab to go to. So once again, I had a boyfriend at the time who had a friend who was an immunologist, and so this boyfriend arranged for us to get together. I got advice from this guy, who must have been a postdoc, I think, at the time, about who really good people in immunology were. And that's where I applied for postdocs based on one guy's recommendations. I mean, that's crazy, isn't it?

All right. So we're up to 1986. [laughs]

Williams: And you're now in Palo Alto, having moved thirty miles away.

Berg: Right. So then I went to Mark Davis' lab, and that's where I was saying I got a big shock when I realized not everyone was like Mike Botchan, because Mark

was about as different from Mike as almost anybody. I think the only thing he had in common with Mike at about that time was he was also a very young kind of beginning faculty member when I went to his lab. But unlike Mike, Mark was at the time, I think because he was so young and he had become this overnight success—this guy had cloned the genes for the T cell receptor—was this big Holy Grail of immunology for many, many years, and he ended up skyrocketing to fame, getting a job at Stanford, setting up a lab, millions of people wanting to come work for him. So when I was in his lab, he'd been there maybe two, three years at the most, and there were over twenty, twenty-five people in his lab. It was chaotic and it was a blast.

But he was very different than Mike in that Mark is probably one of the most brilliant people I've ever met, but in a way that at the time was more difficult for me to appreciate. He's not very talkative, so, unlike Mike Botchan, who I guess I take after, who could talk to the wall all day, like I said, he talked to the students as much as anybody else. He'll talk about science to anybody. Mark was very reserved that way, and we sensed he felt—now, whether this is true or not, I don't know—that his own ideas were enough. Like, he doesn't really need anybody else's ideas. He's very self-confident in his own ideas and his own thoughts about where the science should go, what should be done, and so it was a shock all of a sudden to me to feel like my opinions and thoughts and ideas were not only not being solicited, but were just irrelevant to him. He didn't need to talk to me. He didn't really think he would learn anything or find anything, get anything out of it. Why should he talk to some dumb postdoc that doesn't know immunology?

So one of my goals, again, in retrospect, of my postdoc, I think, was to convince Mark that I was worth talking to. I mean, I think that was, like, really annoying me, that he didn't think I was worth talking to. By the end, we had a very good relationship, and I think it all worked out for the best, but it was very frustrating to go from feeling like you were really part of the driving force of a lab, which all of us were when I was in graduate school, to someone treating you like, "Well, keep your mouth shut and go pipette. That's what I need you for. I don't need you to think." I think I'm over-exaggerating a little bit, but I certainly felt like that. So that was an adjustment.

I think the other adjustment was that Mark was much more—I don't think he's a very formal person, but he wanted more distance between the people in the lab and himself. I actually think maybe that was just because he wasn't that much older than us, and so maybe it was more important to him to have that boundary be clear. "I'm the boss. You're the peon. Don't you cross that line," whereas Mike Botchan, who was just one of these people that had no boundaries, he would borrow money from graduate students when he forgot it to buy lunch. It didn't matter that he made ten times more money than us. [laughs] So Mark was very different, Mark Davis. He was very much more wanting that separation.

Williams: I'm just curious, is Botchan still at Berkeley?

Berg: He is, yes. I saw him about a year ago.

Williams: And Davis is still at Stanford?

Berg: Yes. So I think Mark Davis, my perception of him has changed. Now, maybe the guy himself hasn't changed, but I think Mike Botchan is exactly the same as he always was.

Williams: So you spent four years there.

Berg: Yes.

Williams: Which, I guess, as you tell it, each year got a little bit better.

Berg: Yes, yes, it did. Well, first of all, I learned some immunology. That took the first year. So Stanford, in those days, in the late eighties, was absolutely the best place to learn immunology. It had a very big and very active immunology faculty, a lot of very well-known, world-famous immunologists. As a consequence of that, all those labs recruited really, really good people from all over the world, and so there were postdocs that were my colleagues at the time in all the immunology labs at Stanford that many of them were from the U.K. There were a bunch of people from other parts of Europe, Switzerland, and a few people from, like, Austria and Sweden. I mean, there were just people from everywhere, and they were all really, really smart and really good. I learned a huge amount from all my colleagues. So many of the people, especially the Brits, they were trained, classically trained in immunology and knew all the history and all the literature and all the stuff that I knew nothing about.

So we had this journal club, the postdocs, and I was the kind of ignoramus asking all the dumb questions, because I couldn't even understand what the experiments were half the time. I'd be like, "Well, why? What is this technique, and how does this work? Why are they doing this? Why are they—," blah, blah, blah. They were all extremely patient and good to me and answered all my questions, and so I learned a huge amount from all my friends.

The best part about those years, I think, besides learning immunology and everything, was certainly a huge number of the people that were my colleagues as postdocs at the time all became faculty members, and I still see them all all the time. So that bunch of people, unlike the people I knew in graduate school, because it was a different field and they kind of all spread out and did different things, I don't necessarily run into all of them as much. But the immunology peers I made, friends and colleagues I had as a postdoc, they're all still immunologists, and so I see them all all the time, and that's really, really a great, great community that we all have. It's nice to have other people getting old with you, so you're not alone. [laughs]

Williams: It sounds like once again Harvard Yard—

Berg: Yes, back to Harvard Yard.

Williams: —lured you, yes. Talk about how that came about.

Berg: Yes. Well, again, there are all these things that influence decisions that are oftentimes unusual or unexpected, things that afterwards you think, “Why was I thinking about this?” But, anyway, so, yes, I was looking for faculty jobs, and I think because I had been a molecular biologist first, before I became an immunologist, I think I had a bit more affinity for basic biology departments than I did for medical school departments. So I looked at some jobs that were in medical schools, but I was kind of attracted to schools that were college campuses, which is more like Harvard is. So that was part of it. I liked that aspect of being on a college campus that had all different kinds of departments, not just medicine. But part of me also was a bit, I guess—maybe the same thing that took me to Berkeley when I got into MIT, which is make your own path in life, kind of like don’t go with the expected whatever thing, a bit of the rebellious part of me.

Several of the jobs I looked at, and one or two offers I had, were places that had very, very famous, well-known senior immunologists. So, for example, Yale Department of Immunology, where Charlie Janeway, who was one of the biggest guys in the field, was a professor, and when I interviewed there, he had all sorts of wonderful, exciting ideas for things we could do together if I went to his department and joined the faculty there. And I remember thinking, “I’m just going to get lost. I don’t want to become Charlie’s little sidekick.”

Because when I was a postdoc in Mark’s lab, we had generated T cell receptor transgenic mice. There was one other lab doing this at the time. So there were two of us that had these mice, and they were really hot commodities, right? So he wanted to get his hands on these mice and do experiments with them, and I remember worrying about that. How am I going to establish my own identity and be my own scientist and have a research program where it’s clear that it’s coming from me if I’ve got this huge monolithic guy in my face all the time, well meaning, perhaps, and not meaning to do anything negative, but just it’s very hard to—you could just imagine, I could imagine, getting kind of overwhelmed by that situation and feeling like I was not able to really establish my own program and be independent.

So I ended going someplace where there were no immunologists because I didn’t want anybody telling me what to do. I did not want anybody, I don’t know, butting in, I guess. It turns out, in retrospect, now I know from having moved from Harvard to a place like UMass, where I have wonderful colleagues, I now know what I was missing, but I didn’t realize it at the time. I think at the time I

thought, “I could do this myself. I don’t need anybody. What do I need? I don’t need any other immunologists around. What do I need them for?” [laughs]

I don’t know if it’s shortsighted, but it was certainly a situation that, again, in retrospect, I realize was foolish. But that was what I felt at the time. I wanted to go and do it on my own. So I probably went to one of the few places in the world where, man, you’re doing it on your own. Nobody’s helping you or giving you much of anything, I mean, not in the sense that they don’t give you resources, but it’s all like they throw money at you, they throw a lab at you, and then “See you in five years, and sink or swim.”

Williams: What do you think was behind Harvard’s thinking in terms of hiring you in order to move into this field where they hadn’t gone before?

Berg: Well, so it’s not exactly true they hadn’t gone there before, but it turns out now, of course, then having been there you learn all the past history. So Harvard had a—I wouldn’t call it a tradition, but they had had a series of previous assistant professors who were immunology people, because they needed someone to teach the undergrads immunology, and most of those people, it was like a practically revolving door, right? So one would come in and last five or six, seven years, and then out the door and the next one would come in.

So they had overall, in general, most of the time someone like that on the faculty who would fulfill this teaching need that they had, but I think for all the reasons that I then came to appreciate, it’s a difficult situation to put yourself in, and people either ended up not being successful enough to get tenure at Harvard, which was my situation, or they chose to leave because they felt there were better environments for them to be more successful in.

So I think that was a lot of it, was that it is hard. It’s hard to do it on your own. I think I was pretty successful there. I wasn’t a superstar, but I was successful enough to be able to get jobs afterwards and have a reasonable number of choices.

At the time that I was leaving Harvard or planning to leave, my husband—we haven’t even gotten to him—who I met when he was a graduate student in Mark Davis’ lab when I was a postdoc, he was a postdoc when I was an assistant professor, and so he was finishing his postdoc right about when I was coming up for tenure, and so I was planning to leave anyway, regardless of whether I got tenure or not, which was nice, because I think it emotionally protected me from being too involved or worked up about whatever was going on with the tenure process. But I think it helped also me thinking. “There are better places for me to be. This is probably not the best environment I could put myself in.” So I knew I would be leaving anyway, so I wasn’t, like, really overwrought about the whole tenure process.

But it had its pluses. Okay. So being at Harvard meant I had great graduate students, really good, smart, motivated kids, and it was very easy to recruit postdocs because I was at a big-name institution in a city people wanted to live in, in a very nice place. So it wasn't all negative. There were some parts of it I don't look fondly upon, but there were other parts of it that were really good, and I have no real regrets about it.

Williams: Do you want to talk at all about the sort of downside?

Berg: Yes. So the downsides were that because it's a very broad Biology Department, most of my colleagues really didn't know or understand very much about what we were doing, the actual experiments and work, and so their measure of my success was did they open *Cell*, *Science*, or *Nature*, which were, like, the only journals we had in common, and see my name in those journals, and that was it. That was all that mattered. So if I published a paper in *Cell* or *Science* or something, they felt I was successful. If I didn't, I was not successful, to their standard.

So it was a very simple formula, and it was no secret, so you knew what was expected and you work hard and you try to accomplish your best and be successful, but I would say that I didn't quite reach the bar that was set for what their view of what success was. So as a consequence, the negative part about that was that when you would run into your colleagues in the hall or at some lunch meeting or whatever, there would be the, "Oh, how's it going?"

And what they wanted was like a sound bite of your latest *Nature* paper. "Oh, I just published a paper in *Nature* that said this." I had a really hard time turning my work, which was immunology, and it's very hard to put into a sound bite for a non-immunologist, so I had a really hard time coming up with good one-liners that would satisfy their need to feel that I was doing well.

Williams: And why weren't you publishing in *Cell*?

Berg: Yes, good question. So we had a couple papers, one in *Nature* and one in *Science*, when I was an assistant professor, so I wasn't a complete disaster, but I think the field that I was working in at the time in immunology was, like most fields, I would say there's probably nothing special about it, but as a beginning assistant professor, it's often hard to compete against very well-established labs run by famous people, and one of the things you learn right away when you leave—so I as a postdoc had been in the lab of someone very famous, and so my papers as a postdoc went flying into great journals. It was almost laughable how easy it was, *Cell* papers, *Nature* papers.

All of a sudden when I was little old me just on my own and I would send a paper to a journal like that, they would get rejected. And I remember thinking, "Well, if I was in Mark's lab, this paper would have been right in." So part of it is politics that those journals, there's a bit of name recognition that goes into not only from

the editorial staff, but also the reviewers. You maybe pay more attention to a paper that you see coming from a person that's well known than someone you've never heard of.

As a postdoc, even though you got a little recognition for the work you published, really it's associated with the lab. My work as a postdoc was Mark Davis' work, right? And that's what people would think of it. "Oh, the Mark Davis lab did this." Right? So all of a sudden when you're on your own, you lose that little, I don't know, icing on the cake or whatever of having Mark's name at the end of the author list of your paper, and so it got tougher. It was a shock. No one tells you that beforehand, like, you don't realize you've got it easy now because you're in Mark's lab. You don't figure that out until all of a sudden it's in your face, like, "Oh, yeah, I guess it's different now."

So I think there was a little bit of that. I think also just being a small, smaller lab at the beginning and trying to—and your manpower at the beginning are beginning graduates that don't have a lot of experience so they're not as fast at doing things, they're not as technically adept, and it takes a while to get them up and running. So there's a lag time when you go out on your own and set up your own lab, and it's hard to compete with big-name labs full of experienced people.

Williams: Were you happy with the research activities that you were doing at Harvard?

Berg: Yes, actually, I think in spite of my believing that I was doing it on my own, in fact, truth of the matter is I was definitely influenced by my environment, I think, like we always tend to be.

And I shouldn't say that no one cared about me, because I had one colleague who was extremely helpful and supportive of me. His name is Ray Erikson. He's still there at Harvard, and he was a guy who worked on cell signaling. He had worked on Rous sarcoma virus in the early days and was the guy who showed that the *src* transforming gene of this virus was a tyrosine kinase enzyme, a particular kind of enzyme, and so he worked on kinases and signaling. And I'm sure that it was because of him and talking to him and hearing talks from people in his lab that I decided to do what I did, which was to try to clone tyrosine kinases from T cells, and that's really how the whole rest of my scientific career evolved.

The idea was to try to figure out how T cells really work and how T cell development really happens and how T cell activation happens. We needed to understand how the proteins in the T cell that transduced the signal from the T cell receptor, how that all works biochemically. So nothing much was known about it at the time I started my lab, and so we had this—and I kept hearing about tyrosine kinases and thinking, "Oh, there must be tyrosine kinases in T cells, and I bet they're important. And there must be ones we don't know about yet," because only one or two had been discovered at the time. So we set up this kind of

molecular screen for genes that encoded tyrosine kinases and cloned some kinases, and we still work on them.

So that was the beginning of it all and, like I said, I'm not sure that's the kind of experiment I would have thought of doing if I hadn't been in an environment with a colleague that worked in this area. He worked on a completely different—he didn't work on immunology at all, so it was a different system, but it was, I'm sure, influential.

So I felt like we were doing good stuff, and I think, you know, it was good. I don't think it was meaningless. I think it's work that some of it is—I guess if your definition of things being important is did they end up in textbooks, you know, some of the stuff we did ends up in textbooks, so that's going to stand the test of time, I guess. [laughs]

I think there were things I did wrong that probably contributed to, like you said, in the end after six years there not having been successful to attain the stature that was required, for example, to be successful to get tenure in that department. If I could go back and do them again, maybe there'd be things I would do different in the lab particular, not so much experiments but—I think there was—yes, what would we have done differently? I think there were some things we maybe would have tried to get done faster by working with, collaborating with another lab instead of trying to do it myself. I think that trying to do it myself might have been part of my downfall. I don't know.

Williams: Was there a continuity from Stanford to this work you were doing at Harvard, or was this a break and a new venture for you?

Berg: Some of the projects we worked on at Harvard were definitely like a direct, I don't know, sort of next set of steps from the stuff I did in Mark's lab at Stanford, but cloning these tyrosine kinase genes, that was all completely different, and that was part of—so it's that project started as what I call a whim in the sense that it was a shot in the dark like, "Oh, let's give this a try and see if it goes anywhere." It wasn't the main thing in the lab. It was a little side project. I had a rotation student, which are beginning graduate students that come and try out your lab for a couple months before they make up their mind about where to do their thesis work, and this student came and I gave her this project and I said, "Let's see if we can get this to work and we'll see what happens afterwards." And I actually had started doing the experiments. This was when I used to actually work in the lab when I was an assistant professor. So I started this project and then this rotation student came and I handed it to her, said, "Here's what I've done. I've tried this. I've tried this. I've tried this. I think we need to adjust here and fix this," and whatever.

She got it to work and cloned this first gene, like, in weeks, and I'm like, "Wow, this is great," and it was exactly what we were looking for. It's actually the main

project my lab still works on, is this one protein that she cloned the gene for after two weeks or something.

So over the next several years, I think the lab shifted. We cloned a couple more of these genes, and so we really shifted to working on these signaling proteins and doing less and less of the work that was more like what I had done as a postdoc. I think that's a pretty natural progression for many people, but it's a really important thing to do because you've got to get away from your old lab.

Williams: So talk about the transition to here.

Berg: So then after I was at Harvard and when my husband, Charles, was—oh, man, I was just thinking about this story that I should tell you about, about the tenured part, about something that my chairman at Harvard—okay. So Rich Losick, the guy that influenced me when I applied to graduate school years ago, turned out he was still in this department in Harvard when I went back as a faculty member, and they have a rotating chairmanship in that department. Every three years, someone else takes over. He happened to be chairman when I came up for tenure, and as much as I loved the guy and as much as he had a huge influence on my life, he's one of these people that sometimes says really tactless things.

So when Charles, my husband, who at the time wasn't my husband, he was a graduate student, he finished graduate school, he moved to Boston where I was at Harvard, and he started a postdoc at MIT. He's a scientist, but he works in a different field. He's a developmental biologist. He works on zebrafish. So he was doing his postdoc at MIT, and then we got married, and then we decided to have a kid. So we got married. I was thirty-seven, so I was, whatever, thirty-eight or something, thirty-nine, I guess, and so my first pregnancy ended in a miscarriage, and, anyway, then I got pregnant again. Then it took another year or something, and then I had my first son when I was forty.

But so that was the year right before when I was pregnant with the kid, before he was born was right when I was coming up for tenure, and Rich Losick was in my office, and he was having to break the news to me that the department wasn't going to put me up for tenure, which was not a big surprise. But, anyway, he said to me, "You know, Leslie, if only you hadn't had that miscarriage, then you would have had a kid, and you could have gotten a year delay on your tenure decision." [laughs]

And I remember thinking, "Oh, well, that's a really good reason to not have a miscarriage, so you can get a year extra on your tenure clock."

Anyway, and he didn't mean to sound like a horrible person. I mean, really he didn't. He meant it with the best of intentions, but it's one of those things that you hear someone say, and you're like, "I can't believe he just said that."

Anyway, so that was Harvard, yes. That was a typical, typical Harvard experience.

Williams: So talk about the transition to coming here.

Berg: Yes. So we were looking for jobs, Charles and I. He was finishing his postdoc and looking for his first faculty position, and so we applied to a lot of jobs. I think at the time I wrote to everybody I could think of to tell them, "I'm looking for a job if you know about anything or anything in your institution."

So Ken Rock, who was still then at Harvard Medical School, had been just recently hired here as the chairman of Pathology, and he hadn't actually moved yet. He was still at Harvard, but he found out, I don't even remember how, that I was looking for jobs, and so he contacted me and asked me if I'd be interested in interviewing here, and so I did.

It turned out there were other departments hiring, and then Charles got interviewed for a position in a different department. So in the end, I think, we didn't have a huge number of choices, but there were three or four places, I think, that we had both had offers that we could have gone. It was a tough decision, because there were places better for me and not very good for him and there were vice versa. So this place actually was kind of a little bit of a compromise in the sense that it wasn't either one of our best job we could have gotten, but it was the best combined job. [laughs] Both of us were happy with the jobs and the offers, and it was the best combined situation. So we both accepted the jobs here, our jobs here, and I have not a moment's regret. It's been fantastic. I mean, this has just been the best place. I love it here.

Williams: Why?

Berg: Because of my colleagues. I think also after Harvard it was such a different experience. So here it's the complete antithesis. I'm in a department that's just immunology, surrounded by immunologists and people that actually work in fields that are close enough to my part of immunology that we have a huge amount in common. We collaborate like crazy, so I have more than half the projects in my lab are things that we're doing with one of my colleagues down the hall or one of my colleagues downstairs or someone else. I mean, we talk about science all the time.

I can go down the hall and tell someone my latest crazy idea, and they have an opinion about it, they want to hear about it, they disagree or whatever. It's just like fun to come to work every day. It's just really great. And, like I said, now I know what I was missing all those years at Harvard when I was—so I had my lab and I had wonderful people in my lab that I could talk to, but I don't think I really knew what I was missing, what a pleasure it is to also have colleagues that you

can really interact with and share ideas with and work together with. You know, it just makes it fun. So that's been really great.

Then the other thing that's been really great is having a chairman that appreciates me. So unlike when I was at Harvard where you're an expendable cog in the works, you come, you go, you're whatever, I don't know, I never really felt like I was particularly important or valuable or whatever. And Ken certainly, I was his first recruit, and so part of the appeal of coming here was he said, "We have five or six more positions to fill, and you'll be part of helping to recruit people and part of deciding who we get and who comes and all that kind of stuff."

So I feel like that I've been part of the—it's nice to be able to surround yourself with colleagues that you helped pick and want here, people that you enjoy interacting with and talking to and have not just scientific things in common, but feel like they're people you enjoy as people as well, and so that's been fun.

And Ken is, you know, he the world's best chairman. I mean, he's just absolutely incredibly supportive. He's incredibly fair. He's honest to a fault. Just no baloney with him. You never get brushed off like he's too busy to talk to you, or you have some problem, he just tries to make you go away so he doesn't have to deal with it.

He never promises things he can't deliver. If he doesn't know if he can deliver, he won't commit. He'll say, "Let me look into that and I'll get back to you." Then he doesn't just forget and not get back to you like he didn't really want to deal with it, he's just making you get out of his office. He always follows through on things, and it's so nice to have someone like that you can trust that's your boss, basically. And I think he makes it clear that he appreciates whatever I do for the department, and we have a great relationship, that he knows he can ask me to be on some committee for some medical school boring thing, like, "Okay, can you please do this for me?" and I'll do it. And I get paid back by his being so supportive in other areas. So I think it's been great.

Williams: What was the status of the medical school when you all arrived?

Berg: It was a backwater, I would say. It was a quiet, not very important place. And things, man, how they've changed. I mean, it's not the most important place in the world, but compared to what it used to be, I think there are a lot of things that the medical school has grown enormously. I don't actually know the numbers, but there must be double the number of faculty as when I got here and a Nobel Prize and huge expansion, from our point of view, in the immunology that's here, especially a bunch of high-profile people in a field we call innate immunity that's really together with people in our department really put this immunology program, I think, on the map, and people have heard about it and know about it. When I was at Harvard, I don't even think I knew UMass Medical School existed. Maybe I had been here for a seminar once, but I thought it was the boonies, like

this is no place I'd ever imagine wanting to be. I think that certainly having interviewed here and visited, I realized that there was something here, but I think it was much different than it is now. It is much different now than it used to be.

The construction noise that we're having to listen to is just the latest building. It's the third of three buildings that have gone up since I've been here, which are one bigger than the next. So we're all hoping that they know what they're doing with building buildings in this particular day and age, but, yes.

Williams: What's the balance between clinician and research here in the medical school?

Berg: Yes, it's interesting you ask that. So this medical school has always been much stronger research-wise than clinically. So one of the reasons I think—well, I don't know, I mean, really what all the reasons are, but this is very young medical school, so unlike these big Johns Hopkins and WashU and Harvard Medical School, this medical school was founded in the late 1970s. So it's Massachusetts' only public medical school, so it doesn't have this long hundreds of year tradition of clinical medicine. So it was really primarily for a long time a medical school that did teaching and had very good basic biomedical research. The clinical part of it has kind of lagged behind, and I think right now, even, they're still struggling. This medical school still struggles to sort of get up to whatever standard they're aiming for in term of the clinical side of things, like clinical trials and clinical research. It's never been as strong as the basic science. So that's one of the things that was very appealing about a job here is that when I started here, the medical school was really run by the basic science chairs. I mean, they really made all the important decisions. They controlled a lot of the financial resources of the medical school.

So there's a lot of infrastructure that was devoted to supporting basic science. A lot of the financial resources of the school subsidized core facilities and shared research facilities. That was about the antithesis of Harvard, where nobody gave you anything. So I think that they're working on trying to beef up the kind of clinical research and clinical stuff, but it's always been powered—this place has been powered by the basic science.

Williams: When you came, did you have an administrative responsibility right from the start, or have you acquired that as time went on?

Berg: No, I didn't. I didn't have any. So the jobs, or, I guess, the structure of this place, they're academic departments, and I have no administrative position in the Pathology Department, and then there are graduate programs that are programs that a lot of which is involved in teaching graduate students and training Ph.D. students.

So the Immunology and Virology Program has a chair, and that's a rotating position, so most of us have at one point or another taken turns being chair. So

for three years I was chair of the program. That was a couple years ago. And now I'm the graduate director, so that means I'm in charge of all the graduate students. You know, there's plenty of other things that everyone does that are as much work administratively. It's not a huge burden, that particular job. So I think most everybody pitches in and does those kind of things. Mine happens to have a fancy title. [laughs]

Williams: So what's the balance between lab time or research time and administrative?

Berg: I don't think I spend too much time on administrative things. I would say the other big chunk of my time is I teach. I'm in charge of—myself and one other person, of the medical student immunology course, and so it's actually immunology and hematology. So that's a big chunk of time, so I do a lot of not only teaching in that course, but I have to run the course, so that's much more work than being the director of the immunology and virology graduate program, which is really not very much work. So how much time do I spend on the teaching? Probably about 20 percent of my time, I would say.

Williams: What's the status now of your research activities?

Berg: Well, they're going strong. I was thinking about when you brought up in your email the issue of what I feel like our big accomplishments were research-wise, but actually I feel like right now we're doing the best stuff. [laughs] So I think maybe I always feel that way, whatever's happening now is more exciting, because I don't know the answer yet.

I would say financially things are tight right now, so the lab is smaller than it has been for a long time. I think usually I have about ten people. I think the biggest my lab ever got was twelve or something, and now maybe I have seven, six or seven people. So that's a challenge, because when you have ten or twelve people in your lab, there's always a few people where things are working really well and a few people that happen to be struggling. Their projects are not going as planned or whatever, and you can kind of feel like there's always somebody to keep the ball rolling, and the smaller the lab gets, the dicier that is, that having to always make sure there's a few people whose projects are moving along. So we don't know how long this is going to last, this tricky situation with the funding, but right now I have some great people in my lab, so I feel like they're doing well and they're really cooking.

Williams: So I did ask you about the highlights of your career so far, and I think one way to look at that is what laypeople would like to know about what you've been doing.

Berg: Right. So I think what I see as the biggest advance or, you know, contribution is the work, which is still ongoing, and I'm not sure we have the final answers yet, but trying to understand how our T cells make decisions about what kind of T cell they're going to be and what kind of response they're going to make. So one way

to think about that is when you have an infection, depending on the nature of the pathogen, if it's a virus or a bacteria or a parasite, your immune system has to come up with a different response because you need a different response to clear different kinds of infections. And your T cells have to figure that out, so they have to know somehow or be told, "Okay, this is a virus. We need to make response A, which is going to get rid of a virus. If we make response B, which will get rid of a parasite, the virus won't care and you'll be dead."

So the signaling proteins that we work on, one of the ones, the first one that we cloned the gene for turns out to be important in how T cells make decisions, and that's what took us a very long time to figure out, I think, because the general thinking about signaling and T cell receptor signaling for many years was it served as very linear idea that—or I think of it as a light switch. Switch is on, switch is off. T cells off, T cells on.

Now we know that it's more complicated than that, and we always knew that there were different kinds of T cells, but now I think we have some insight into how the T cell receptor signaling is pushing cells into different pathways. So we think that this understanding how this one signaling protein is working and what its function is in a T cell is helping us figure out how these decisions are made by the T cell to make the appropriate kind of response when there's a particular kind of infection.

So I made some cartoon for a talk I gave last week where I was trying to convey the idea that this protein that we work on is not the light switch, the on/off switch, but it's telling the T cells not whether to stop or go but which way, right? You have other proteins that are kind of upstream that are the "stop" or "go" signals, and you activate this one upstream protein, you get to go, but then you hit my protein, the one we work on, and then that one says, "Okay, left turn, straight, or right turn."

So that's, I think, maybe it's very abstract. [laughs] But it's important in trying to understand how you can generate from this original T cell that was sitting there minding its own business, how you generate all these different kinds of effector cells that have to do different functions, and I think that this is just one piece of it. It's not the only part of it, but it's one piece of how those decisions get made.

Williams: And it resides on the T cell?

Berg: Yes.

Williams: Not on the pathogen.

Berg: Exactly, exactly. So there's different ways for T cells to get influenced by the pathogens and by their environments, and this protein is part of one of those pathways. So I think my general view is there are basically three kinds of signals

the T cell has to think about that are all together, resulting in whatever the T cell is doing. So there are signals with the T cell receptor, there are signals through these kinds of receptors called co-stimulatory receptors, and then there are growth factors or cytokines that come and impact the T cell.

All three of those pathways eventually have to work together to make an outcome, and so we study—the one protein we've been studying the most is important for the T cell receptor piece of that. So it's just one of the inputs, but it's one which I think was generally thought to be more of the on/off switch. Okay. If you don't turn it on, you're not going to go at all. If you turn it on, you go. And then these other two inputs direct the T cell. I think we would argue now that, in fact, the T cell receptor is also part of the directing traffic. It's not just the on/off switch.

Williams: This is a far cry from tree bark.

Berg: Yes, I know. [laughs] Exactly. Exactly. That was exactly my goal, and, in fact, I get very frustrated. My kids are now in high school, but when they were younger and learning science in grade school, it used to drive me nuts because it was so boring. I kept thinking, "No wonder kids don't want to be scientists if this is what they're teaching them in school." It's horrible.

Williams: I don't want to embarrass you, but I notice that you didn't become a member of AAI until '94, I think.

Berg: Yes.

Williams: I've noticed that most of the people I've talked to, early, early on in their careers become AAI members. How come?

Berg: Well, either because I'm younger than them, which I don't know if that's true or not, actually. So in 1994 I was an assistant professor, so I'm not sure when they started having these trainee memberships. I don't actually know if it was—it might have been that early and I was unaware of it, but certainly when I was—okay, when I was a graduate student, I wasn't an immunologist. So when I was a postdoc, which would have been the first time I could have joined AAI, it certainly wasn't anything most people knew about or did. So I probably never even thought about it. Then I became an assistant professor, and so a couple years later, I think someone must have suggested to me that I should join AAI, it was a good thing to do, and I probably did then.

Williams: So that was while you were at Harvard.

Berg: Yes.

Williams: Then quite quickly thereafter you became a member of the Education Committee.

Berg: Yes.

Williams: What did you do there?

Berg: Yes. So I ended up on the Education Committee because I was—well, I don't even remember what order things happened, but the part I remember was the AAI runs a couple of courses now in the summer, one of which was present in those days, and the person who was in charge of it about that time was a guy, Abul Abbas, was running that course. He was a professor at Harvard Medical School, and he knew I taught immunology to the undergrads at Harvard, and he asked me if I would teach in that course, which I started doing. I think I taught there one or two summers.

Then when he was ready to step down as director of the course, he asked me if I would be willing to become the director of the course. So I think somewhere along the way I must have got appointed to the Education Committee, and I don't remember if it was before or after this happened. So then I ended up being the director of that advanced immunology course for a couple years, and that usually is a—I don't think there's any clear rules about it, but people do it for a few years and then usually they get sick of it or it's time for a change or whatever, and so then someone else takes over. So that's how it all began.

And then after that, I think because of being part of that course, you get to know the other people that are involved in the Association, and I think the next thing that happened was they asked me if I would take over as program chair. I guess it was an obvious step from running this course, which meant inviting speakers and helping to make a program and decide what topics and all that to run an annual meeting.

So the job of the chair of the Program Committee is to be the, essentially, chairperson of the annual Association meeting. So that was the job that I did next for three years. So I think it was because probably Michele [Hogan?] knew I had done this course, and part of it is being—what's the word—reliable. Like they knew if I said I would do it, I would do it. [laughs] So there's always a lot of—you get, whatever, kudos for just being someone who if you agree to do a job actually does it and doesn't just blow it off and be irresponsible, so that's how I ended up becoming the program chair.

Williams: Then you went on the Council at some point, right?

Berg: Yes. So then that's a little bit different in the sense that—so these other jobs, someone could just appoint me. It didn't take any big—I mean, other people had to agree, but, I mean, there was not like a huge competition to want to do these jobs, right? So then going on the Council is something you have to—if you say you're willing to do, then there's an election and the whole membership votes. So the Executive Council, which is what you end up on and then eventually become

president, is a membership-wide election every year. So each year they have three people that agree to run to be on Council. So you have to be asked to do that, but if you do, then it's out of their control, like nobody can say who's going to win, but the people vote. So there there's obviously, some name recognition.

Michele has some weird statistics about the fact that for a very long time, I think they tended to have one woman and two men for election, and the woman always won. It's really interesting. Until one year they decided they had to not have a woman on the ballot, because there were already like four women on the Council and they were losing all their men. So there was something funny about that. I don't know if it's all the women members would vote for the one woman and then the male vote would get split or what. There are all sorts of theories about this. So I attribute that to why I ended up on the Council. [laughs] I was probably the woman on that ballot with two men.

Williams: Between the time that you're nominated and the vote takes place, do you conduct political activity? Do you campaign?

Berg: Not really, no. I think we're asked to write a little kind of position statement, and they put it in the newsletter, which people can read before they vote. I have to say that having been—of course, then for many years before I was on the ballot, voting as a member, I never read those things. So I just looked at the list of people, and you mostly know who they are, and you think who would be a good person for Council, who do you think would be sensible and a good advocate for the Association and make good decisions and things like that. So I never read those things. So I don't remember what I wrote in my couple paragraphs, probably nothing very interesting or significant. [laughs] But I don't know how many people read them, to be honest.

Williams: So there must be a whole cadre of the two-thirds of candidates who didn't win—

Berg: There are, yes.

Williams: —circling around. How does the morale work? [laughs]

Berg: Yes. So I think it must be tough, because it's like high school popularity contests, right? It's hard not to feel that way. So some people are asked to do it again, and sometimes people will win the second time. Sometimes people are on there twice and don't win, which I think would be even harder, right?

So, luckily, I mean, I think it's one of things most people—you just don't talk about it with the people. Like I don't hardly remember who most of those people are, and it's not like I would go to someone and say, "Oh, I'm really sorry you lost." [laughs]

But Michele is the one that has to call them and tell them, so it's one of the parts of her job that—there are several parts of her job that I would not want to have. Raising money, which she is phenomenally good at, is something I really, really don't like to do. I've had to do it for meetings before, and other things, and it's not my cup of tea. That, and it's like having to fire people, calling people and breaking that—of course, not like being a doctor and real bad news, but having to call people and tell them you didn't win and someone else is—I wouldn't want to have to do that.

Williams: So talk about the functions of the Council and the relations between the Council and the president and the executive director.

Berg: Yes, so it's an interesting situation because, as I'm sure you know, the Council and the president is a revolving door, although it's a very slow revolving door. So you go on the Council, and you're around for like seven years. You're on the Council, I think, four years, and then you're vice president, president, and then past president, so you're there for a while. But there's always one new person in and someone out the other end. The executive director, you hope—and in our case we've been very lucky—is someone who's been there for a long period of time and is the one constant.

My personal view is that—and I told Michele before I agreed to run for Council that she couldn't leave until I'm done. “If you have any interest in leaving this position and getting another job, you have to promise me right now you won't do it till I'm off the Council, because I'm not doing this without you.” [laughs] Because she is the backbone of the organization. I mean, the organization would be nothing without her. There are other professional societies that are nowhere near as successful and do as many things with the resources available as AAI, and it's all because of Michele, because she's so passionate about it and she's so energetic. She's someone who, for reasons that I've never been able to figure out, she's just like us. She's got a Ph.D. in immunology and was a scientist. She has the best business mind. So, being someone who is not that way, I just don't know where it came from. I don't understand how she figured all this stuff out, but she has such a good business mind, not only the financial aspects of running a business, but just how to get things done, how to motivate people, how to organize, delegate, and she understands the business aspects of running an association like that and getting things done.

So, for example, one of the things I've learned from her is that there's a price to everything. People may have an idea for doing something on the Council, like, “Oh, wouldn't it be nice if we could give out money. We have extra money. We could give out little things.” Well, somebody's got to have a process. There has to be a way for people to apply for the money, there has to be a review process, there has to be an administrative support for the—it doesn't just, like, happen, right?

So we're all, "Oh, let's do this." Like all the scientists. "Oh, let's start a new course. Let's have our meetings someplace," whatever. "Let's do a little of this.

And she's the kind of a voice of reason reminding us that this is—"Okay, if you really want to do this, we can, but first let's talk about how it would really work, and here's all the things that are involved in your idea. Are you still interested in doing it?" Then maybe the answer is yes, maybe no. But I think without her we would be flying off in different twenty directions with crazy schemes that couldn't be implemented, you know, or not very successfully. So she kind of has an awkward thing for her, like reining us in a little bit, I think.

Williams: So let's talk about your presidency. When you came in, what were your aspirations? What were your goals?

Berg: Yes, it's a blur. I don't know. [laughs] You know, I didn't really have a big agenda in mind. I think my view about being president of the Association was that there were a couple jobs that I had actually that were, for me, a bit of a—I don't know if "challenge" is the right word, but not things I would otherwise choose to do voluntarily, one of which was to go to Capitol Hill. So I knew this was part of—not so much part of the job, but that I felt it was an important thing to do as president and while you have the mantle of that position. So we did it, actually, twice, go to Capitol Hill, one time went around talking to senators' aides and congressmen's aides and things to lobby for—I know we're not supposed to use the word "lobby"—to encourage them to support biomedical research and things. So I think given the times we're living in and the issues with funding for science, I think that was the only really important thing I needed to do. That was *the* important thing I needed to do.

We also spent some time talking and meeting with various directors of NIH [National Institutes of Health] institutes about how they manage their portfolios. Part of, I think, the struggle that we all feel right now in the funding crunch is that there's certainly a lot of money going to science, but it's not going into basic science the way it used to. It's going to fund, some of it, more clinical or translational things, and some of it is going to fund big kind of projects that are almost like infrastructure, like sequencing the genome, kind of. Not that exactly, it's been done, but those kinds of projects which are not what people think of as investigator-initiated creative science.

So there's a bit of a tension right now, and I think what I felt, again, part of the responsibility of being AAI president was to try to use that position and our Association's whatever little power we have to encourage institute directors to think about whether they're really making the best decisions for the future of science and keeping the enterprise moving forward and all that.

So, to me, those were the important things that needed to get done. How impactful we were, I don't know, but we did what we could. Other than that, I

mean, I think I didn't have very specific projects in mind that I wanted the Association to do. I think that there's always the issues of the moment that need to be discussed and resolved. The publishing world is in an uproar, like what the future of print publishing will be and advertising and revenue and open access, and there's all sorts of stuff like this. We talk about these things on the Council all the time, and some of these issues have been going on for years.

So I didn't have particular, okay, I feel like this is—we have to now, like, have *The Journal of Immunology* to be open access. I had no agenda like that, so I really felt like the one thing that I can do as president that I can't do in any other format is the political end of things, because that's something I think that that position is really meant for, really. I mean, I think that's the one thing you can do when you're president. So that was my main objective, I guess.

Williams: When you went to Capitol Hill, did you go with a team?

Berg: I would have no clue what to do, so I must be the most unpolitical person in the world. This is just so far out of my sphere of normal life. So, thankfully, Lauren is the absolute best. She's the head of our Public Affairs Committee, Lauren Gross. She's a lawyer and she was a Capitol Hill aide for many years, and she takes us. She knows that we're complete ignoramuses about this, so she tells you what to wear, what to say, where to go. She goes with you, she holds your hand, she arranges all the appointments. She briefs you on who you're meeting with, whether it's which level of person or what she knows about them and their background, and how to talk to them. Because, of course, if it's a scientist, you'd talk to them differently than if it's a person that just came out of an undergraduate school as an English major, right? So you have to know that before you, whatever, meet someone.

So she just does it all. She organizes the whole thing, but her view, which I think is probably appropriate, is that she's kind of like—I don't know. You feel like one of those movies where the politician is this dummy, and there's someone in the back with the—you know. I mean, it's not that she wants us to not speak our mind or be real people, but she's behind the scenes pulling the strings and telling you and arranging everything, and then you're supposed to be there to represent the Association and be the figurehead or whatever, because I would have no clue how to go about it.

I was amazed at all these things, like phones ringing in the office of the congressman. I'm like, "Who's calling? What are all those phones ringing?" Well, people call up and want to tell their congressmen their opinion about something, some issue. I'm like, "Really? People call their congressman?" [laughs] It would never occur to me to pick up the phone and call John Kerry's office to tell the senator's office what I think about some—yes. I guess it's—

Williams: In your presidential message, you mention the funding issue.

Berg: Yes.

Williams: Then also the question of downsizing, some aspects, related aspects. Talk about downsizing a little bit.

Berg: Yes. So I think there's a lot of concern now about whether—correctly so, my opinion—whether the expansion—so the expansion of biomedical science based on the doubling of the NIH budget brought along a big expansion in Ph.D. training programs for biomedical scientists. Those training programs were producing all the new Ph.D.'s every year, were predicated on the idea that there would be jobs for those people when they finished, commensurate with their education. So Ph.D.'s should be either faculty members or senior scientists or group leaders of companies or whatever. They shouldn't be technicians, otherwise why go to graduate school?

I think that certainly in the academic side, there's been a slowdown in the ability to hire all those people with Ph.D.'s as faculty or whatever, and the pharmaceutical industry and biotech industries also not in this huge expansion phase maybe that it was also in the recent past. So there's a concern that we're training too many people for careers that don't exist or that there won't be jobs for those people commensurate with their education.

On top of that, I think, now with less funding, the financial structure that supported all that training has also just eroded, because we pay for most of them from our NIH grants. So if you have fewer grants, you have less money to pay for graduate students. So the problem is that the money fell out very quickly, and so the whole process of the rest of it re-equilibrating has been much slower. It's human nature. If you had a big thing, you don't want to go down to a small thing, right? So people always feel that way about their labs, about their departments, about their graduate programs. So it's always easier to go up, get bigger, but no one wants to get smaller. So I think that's inevitable, but it's an unpleasant truth that no wants to really deal with. They always want it to be somebody else's problem, right? It's like, "Well, our school's really good, so we should have a big graduate program and let those other schools that are not as good take the hit." So I think it's also not very realistic.

Williams: In the current political environment, there probably isn't a lot—there are not many signals that things are going to change, except maybe to get worse.

Berg: Well, exactly. I think that's one of the things that's most disturbing to me, which is that—so in my scientific career, I lived through one of these times before, so when I was a beginning assistant professor, I started my job in 1990, and in early 1990s things were also horrible, and the pay lines were very low. The funding for research went to nothing, and it was almost as bad. I think it was probably about as bad as it is now, but it didn't last very long. So it lasted maybe two—I don't

remember because it's a blur now, but it was a couple years that the pay lines were very low, and then slowly things got better again and then things got really good.

But at the time afterwards, people sort of had this idea that, well, when things get bad it's always temporary, because I think maybe it had happened before. I think it happened a little bit again in the late nineties or early 2000s. There's a little dip and then things got better again. So there was always somehow this idea that, well, yes, when things will get really bad, but as long as it doesn't last very long, we can manage and then things will get better again.

I personally feel that there's absolutely no hope right now. [laughs] I just feel like there's absolutely no glimmer of light at the end of the tunnel that, oh, yes, if we can just make it through the next year or two, then all of a sudden things are going to turn around. I don't see that happening, so I think this is the future.

Williams: One of the aspects of that future may be that more and more science is going overseas, is that right?

Berg: Yes. So I think it's very unfortunate that many of the people that I grew up with, my colleagues who are my peers at other institutions, especially, I think, the people at what we think of as these private research institutes that are not academic institutions, not medical schools, but are freestanding research institutions, they usually exist on what we call soft money, so they have to pay 100 percent of their own salary out of their NIH grants, as well as all their supplies and personnel and everything else.

So it's even more a struggle for those people those places, and many—I've just recently heard of several longtime colleagues of mine who are moving to Singapore or back to Korea or Australia, and people moving to Europe, because for the first time in my whole life, the U.S. is not the place with the best resources. I mean, it's really shocking to think about, that we're losing, we're being out-competed now in terms of investing in biomedical research by the rest of the world. It's really sad.

Williams: Talk about the balancing of career and family life a little bit.

Berg: Yes. So I always tell people I'm not the role model you want to hold up in front of any of your graduate students. So I got married when I was thirty-seven. My mother's still shocked I had kids, because she insists—I don't remember saying this, but she says I never wanted kids. I do know I never wanted them when I was younger. I mean, I had no interest in kids, none, zero, not even a blip on my radar screen, the idea of having kids.

So I think what happened to me was at one point I realized when I was in my late thirties that not deciding—it wasn't that I didn't want them; I just didn't want

them now, ever. Then all of a sudden I thought, “Okay, now’s the time. I have to think about this and decide, because no decision is a no. If I do nothing, it will soon become a moot point because I will be too old.” So then I thought, “Okay, well, maybe I do want to have kids.”

So I had my first kid when I was forty and my second kid when I was forty-one, and, luckily for me, unlike, I think, some people who have issues if they wait that long to have kids, it was fine. I didn’t have any problems. And it’s been a blast. I think, for me, having my kids—so I was already an associate professor, I had tenure, practically.

Okay, so I had my first kid four months before I moved here to UMass, where I had my tenure position, so I think it worked out great, because I wasn’t really in the lab doing experiments anymore. I think trying to juggle being a bench scientist and a student or a postdoc, where I think I worked my brains out, with having a family would have been a nightmare. I think it would have been very difficult. I don’t think I could possibly have gotten done what I got done, and I had no interest anyway in having kids when I was in that phase of my career. But having kids when I was essentially already a professor and had an office and had a computer, it was no problem.

So I am all in favor of being old, having kids when you’re old, and there are only one or two downsides I’ve discovered to being on the older side of normal for having my kids. One is that my parents are useless as grandparents. They’re way too old. So I could never leave my kids with them ever, even when—they weren’t young enough to be able to be the kind of grandparents you’d leave your kids with for the week and go on vacation with your husband. So that was unfortunate.

On the other hand, my husband is younger than me, and his mother was nineteen when she had him, so she is the perfect grandmother, except she lives in Sweden. So she would have been great if she’d have been close by. [laughs] So we have this very strange sort of generational—I don’t know—whatever you want to call it in our family.

But I really think the most important thing for me in being able to juggle work and family is having a partner that is supportive of the work part of the work family. So I think that it works because when our kids were little, my husband and I split the duties. If the kid was sick and couldn’t go to daycare, he took half a day off, I took half a day off. It was never that it was always my—you know, the mom’s problem.

So I think if you have a partnership with someone where they’re in it fifty-fifty with you, you can do it. I think if you’re 100 percent responsible for every aspect of dropping the kids off, picking the kids up, cooking dinner every night, not being able to travel, forget it. It’s not going to happen.

Williams: I've been asking everyone what about recreational pursuits and how do scientists have fun?

Berg: How do scientists have fun? [laughs] Oh, yes, so I guess it depends on the season. In winter I hibernate, I think. The rest of my family are all skiers, but I gave up skiing when I was in my twenties, and I couldn't somehow start up again in my forties. It just turned out it didn't work for me. They all ski, so I actually have a—I'm, like, a craftsperson, and so I actually knit and sew and do crafty things. That's my winter recreation.

In the summer we do hiking and biking and as much outdoors things as we can. And we like to travel. So since my husband is Swedish and his family, all his immediate family, is in Sweden, every other summer we take the kids and go to Sweden, and we try to go somewhere else in Europe as part of that trip. So we're good travelers. So if I had endless time on my hands, I think I would do more of that.

Williams: I want you to think about anything we've left unsaid here today that you'd like to have as part of this historical record, and there's one other question that I feel like I need to ask you, and that is your observations on being a woman in science.

Berg: Right. Well, that was actually the only thing I thought of when you just said things we haven't talked about yet. So I think there are two aspects to that question that mean something to me. So I divide that issue up into the external and the internal. So I think externally it's a benefit. So from the outside world, do people treat me worse, do I feel that I've been discriminated against because I'm a woman? No. In fact, I think it's been an advantage because there's all this awareness of women in these kind of professions being underrepresented, and so there's all these mechanisms in place to try to promote women's careers. So I actually think, if anything, it's been a benefit.

So I think that the reason there's still an issue—so why doesn't the issue go away, right? I think it's an internal problem. So I think women, in general—now, there are exceptions to this rule, and I'm happy to tell you who I think those exceptions are, but for the most part, I think most women have, whether it's genetic or societally ingrained, have personality traits or qualities that are not the most useful in getting ahead in a career like this. I'm sure it's true in business too. But there's a certain amount of whatever you want to call it, like swagger, being self-promoting, aggressive, in terms of not aggressive scientifically but aggressive in terms of your career, pushy, demanding things, trying to argue, call people up and ask for favors or get yourself a speaking slot on a program. Being program chair for many years, I can't tell you how many people called me up and asked me could I give them a talk at the meeting. How many women called me and asked me? Zero. Never happened. It was always men.

So for me, I can say from my observations of my own career, it's my own limitations. I think it's my own personality, whatever you want to call it, that has been the limiting factor, not anything the outside world has done to me. So I feel like if I had been someone who could more easily be demanding about things like that or be pushy or be aggressive or be—so when a paper gets rejected, what's your response? Response A, you say, "Oh, this reviewer has some reasonable criticism. I can see why they rejected my paper." That's me.

Position B, "This person's an idiot. I'm going to call the editor of the journal and give him a piece of my mind and tell them that this person's an idiot and they should accept my paper anyway," and blah, blah, blah. So those people get ahead. In fact, it's a very successful strategy. It works. You push around the editor of the journal, and they cave 90 percent of the time, as far as I can tell. I could never do that. I just couldn't.

I had this talk with Mark Davis about this a couple years after I started my lab and was assistant professor, "I can't do this, Mark. If that what it takes, I can't do this, Mark."

He doesn't do it either, because he doesn't need to do it. But he said, "Not everybody has to do that."

I said, "But it works."

He goes, "Yeah, it does." [laughs]

But it works, and so I think that that's why. I think women are self-limiting. I think we're self-limited or whatever you want to call it, that it's hard for us to compete in an environment where those rules and those kind of behaviors are successful strategies.

Williams: And accepted.

Berg: Yes. So that's my view of it.

Williams: Anything else you—

Berg: No. I'm done. I'm done. [laughs]

Williams: Thank you very much for this interview.

Berg: You're very welcome. [laughs]

[End of interview]