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Transcript

Linda A. Sherman. Ph.D.
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Williams: This is an interview with Linda A. Sherman for the American Association of Immunologists Oral History Project. Dr. Sherman is professor in the Department of Immunology and Microbiology at Scripps Research California Campus. She was the president of the American Association of Immunologists from 2014 to 2015 and is a Distinguished Fellow of the AAI. We are at IMMUNOLOGY 2019™ in San Diego, California. Today is Saturday, May 11th, 2019, and I am Brien Williams.

Dr. Sherman, I'm asking you first to tell me a little bit about your background, where you were born and grew up, and something about your parents, maybe.

Sherman: Well, yes. I was born in Brooklyn, New York, where I grew up, and my parents were Holocaust survivors who had moved to the U.S. in 1947, after the war. My dad had been at Auschwitz and my mom had been in a work camp during the war in Europe, but they survived. Their families didn't, so we had no relatives. I had no aunts or uncles or grandparents. It was really just me and my sister, and it was a little lonely, but we had some distant relatives who had, in fact, sponsored their coming to America, so there were some people around in Brooklyn we knew.

But it was a very great time in Brooklyn. It was booming. It was after the war and there were lots of children being born my age, so I had lots of friends in the neighborhood that we were growing up. There was lots of young families like my parents, so there were lots of kids, and we had lots of freedom. It was before parents were afraid of letting their kids walk along the street, and I would definitely feel free to go anywhere I wanted from probably the age of five on. So we had a pretty interesting youth, where we, on our free time, could just walk over anywhere we wanted, a community center nearby, we could go and play games, and there were libraries nearby, so it was pretty good, very free.

Williams: What was the process of your parents getting their footing in the new country?

Sherman: Well, that was interesting. So my parents were sponsored to come to the U.S. by some relatives who had left Europe. I guess they were descendants of relatives who had left at just the beginning of the twentieth century, and when my dad came, the only thing he really knew how to do was—well, he didn't really know how to do any of the professions, but his great-uncle who had sponsored him to come was in the garment district in Manhattan, and so he worked in the garment district. There was lots of strikes where he would not be able to go to work because the union was striking, and they didn't have very much money.

My mom's family had been in the leather business and had leather factories and made leather goods, and so she knew a bit about that and she decided they needed something more stable than my father working in the garment district, and so she and my dad opened up a shoe store and they just became small businesspeople. She did the buying for the store, and I would accompany her sometimes to Lower Manhattan, where she had to buy the goods for the store, and then often I would

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be in the store on the weekends just at the cash register doing the sales and making change and working there. That was pretty much how they got their footing. I wouldn't say they were very successful. I mean, they were successful, but it was a small business, so it was what it was.

But I think that gave me a love for numbers and math, working at the cash register [laughs], and I just really had an affinity for math and science from a very young age, and also maybe because English was not my first language, it wasn't as comfortable. They spoke Yiddish in the house, and Yiddish was my first language, so maybe I wasn't as attracted to English and studies where I had to do a lot of reading. I remember I was sort of a late reader because I hadn't learned at home. So I think that was part of the reason I had an affinity for math and science.

But then I remember that I had difficulty with spelling and reading, and so I had a sort of embarrassing incident with that in second or third grade and I said, "This is silly." So I went to the library and just started reading a lot of books, and I think my reading aptitude enormously went up very, very quickly so that wouldn't be a problem anymore. [laughs] I sort of had to determine that was the issue and deal with it, which I did, so then I was okay.

Williams: So your parents' shop was in Brooklyn?

Sherman: It was in Brooklyn, yes.

Williams: Right. And at what point in their lives were you born?

Sherman: Well, they were—oh, gosh, my mom must have been thirty and Dad in his mid-thirties when I was born. I had an older sister, was born two years earlier.

Williams: But you were born in the States?

Sherman: Yes, we were both born in the States. My parents came over in '47. I was born in '50 and my sister was born in '48.

Williams: Right. You've mentioned your sister. What life story does she have?

Sherman: Well, my sister is interesting as well. She's very smart. She went to Brooklyn College and she got her Ph.D. in computers science, and she is the person who put the *Yellow Pages* online. That's how long ago it was. [laughs] It was before they knew about carpal tunnel syndrome, and she had to stop working after, oh, maybe fifteen years or so because she got very bad carpal tunnel syndrome and I think they didn't really know how to deal with it. She was doing programming and working with computers way before anyone else was.

Williams: Yeah, it surprises me that Brooklyn College had a computer program.

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Sherman: Computer science?

Williams: Mm-hmm.

Sherman: Well, I guess it was around for a while. That would have been in the early seventies when she was getting her degree, so it would have—that was a very good school, by the way. Brooklyn College is an excellent school.

Williams: So what about your schooling, where did you go?

Sherman: So I went to Barnard [College].

Williams: Well, before that.

Sherman: Oh, before that, I was at high school, and my high school, I think it was a very interesting high school. It was very oriented towards science, and our teachers seemed to all have Ph.D.'s, I'm not sure why. [laughs] They were really quite accomplished people, even in history and biology and physics. They were all really excellent. It was very easy to be able to do well in science with so much support around it, and I remember taking experimental biology instead of going to lunch because it was one of the things that I enjoyed doing and I didn't really like going to the cafeteria. I was sort of a nerd and that wasn't a comfortable place to be if you're a nerd [laughs], social aspects like that. So it was very easy to do well in science there. There was just so many—

Williams: Was this a PS [public school]?

Sherman: Yeah, [Samuel J.] Tilden High School was just a city—

Williams: It wasn't one of the specialized schools?

Sherman: No, not at all. The problem was—this was a long time ago. There was the Bronx High School of Science, which was very, very far from Brooklyn, where I lived, and the only other specialized school was Stuyvesant, and it was all boys. I was very upset in middle school because I saw the boys going off to Stuyvesant, who had my interest in science and math, and I couldn't, so I was a little peeved about that. So I wound up in our local high school and it turned out fine. I think they were very good. But, yes, I mean, had I been a boy, I would have been at Stuyvesant, but those were the days when there was boys' schools and girls' schools.

I was very lucky, I went to Barnard because I wanted to study physics and Columbia had an excellent physics department. In fact, most of my math and science courses were at Columbia, and I did get my degree in physics when I was there at Barnard, but I also had the advantage of being in Manhattan and being able to take wonderful philosophy and art courses and just going to the museums

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to see the art I would be writing about. It was just an excellent place to do your undergraduate studies in terms of having—what they always say is the city is your laboratory, and it was. You could just go anywhere, and it was wonderful.

Williams: So what did you see as a path ahead when you graduated from Barnard?

Sherman: Right, and that's interesting, because I studied physics. I applied to graduate school in physics and I was accepted in several graduate schools, including Columbia. I went to see my advisor at the end of my senior year and I told him that I'd probably go to Columbia and I wanted to do theoretical physics, and he was a professor at Columbia and he said, no, I couldn't do theoretical physics. At that time, there was just not very much need for theoretical physicists. They would wind up driving cabs instead of getting positions, and he just really couldn't see me as being one of the few people they would allow to do theoretical work. They would allow very few people. They needed more experimental physicists than theoretical ones and basically he was telling me I wasn't cut out to do the theoretical work, and that's what I wanted to do. I didn't want to do—the last thing I wanted was to push a button on a cyclotron and then go collect data and analyze it for two years. That was not my idea of science. I like thinking. My happiest place in college would be in the carrels in the library, where I'd just be there playing with my equations and trying to make proofs. That was great and that's what I could see my life being, but he was basically dashing it.

So I was sitting in this sort of coffee shop near by Columbia and a friend of mine who had been in biology and was going to medical school who I'd known from high school, in fact, who was one of the few people at Columbia I knew from high school, was passing by and asked where was I going to be going next year and what was I going to be doing and I told him what happened and he said, "Well, have you ever thought about biophysics?"

I really had never even taken a biology class in college, so I said, "No."

He explained to me that many of the people in the biology department at Columbia had been physicists and that it was not at all unusual for people to go into biology from physics. So he introduced me to the chair of the biology department at the time. I had never taken biology, but he said, "Why don't you come work in this lab for the summer and see if you like it in biology, and if you do, then you can start the graduate school in September."

So I guess I was able to do that, because I had been accepted by the physics department, so I sort of could have been enrolled at Columbia, but I don't know how you go from one department to another in those days. But it just all happened, and I did work in a lab that summer, in biology, and I remember I had to take organic chemistry, because even though I had lots of chemistry classes, I'd never had organic chemistry, and it was so easy compared to classes I had taken in college. [laughs] It seemed, "Sure!"

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And biology in those days, there was so little known compared to now that I hadn't missed much by not studying it. It wasn't like I was going to medical school and needed to know anatomy or anything. This was basic science and biology and there was very little known. They had just started learning about genes, you know. So it wasn't really a loss not having had biology in college because you could easily catch up just in graduate school at that point.

So I was doing molecular biology, biochemistry, working on DNA replication, and the person I was intending to do my Ph.D. with told me that he was taking a position at MIT and if I wanted to work with him, I'd have to transfer to MIT, so I applied and transferred to MIT and worked in his lab. That was a lot of fun doing DNA replication work, and that was all new. It was such an exciting time. It was at a point where we were just starting to do modern molecular biology. There had been a description by Tom Maniatis and a few other scientists at Harvard of restriction endonucleases, which would allow you to cut DNA at specific sites, and so you could clone DNA and you could start sequencing and working with it. This just enabled us to do so much we hadn't been able to do before, so it was a very exciting to be doing molecular biology.

While I was at MIT, there were a couple of astonishing findings, actually, in the field of immunology. [Susumu] Tonegawa had shown that the immunoglobulin gene had come together from two separate segments of chromosomes and two different sites, and so that it was a totally new concept that one gene, one protein had come from two different areas in the chromosome, and it just opened up all kinds of ideas about immunoglobulins and how they may have evolved and how they could be produced.

The other *extremely* exciting finding was production of hybridomas by the group in England and [Georges J. F.] Kohler and [César] Milstein, and I was very fortunate that I got to visit them in Cambridge before they published their findings and they were just talking about it. I was visiting there and they were telling me about it, and it just completely opened up new areas. Everyone back at MIT, including David Baltimore, who had just won a Nobel Prize, was talking about immunology pretty much, I would say, the way *The Graduate* had someone whisper in his ear, "Plastics," in *The Graduate*, the movie. Everybody was saying immunology, that was the field to be in, and so I just knew that that was what I wanted to go into for my postdoctoral research.

But there it was all over again. I had never had an immunology course. I did my graduate work in molecular biology and biochemistry. There was no immunology at MIT then. There was one immunology course and it was sort of on comparative biology. It was before Herman Eisen was hired, and he was the one who really started immunology at MIT. So there was none, but I decided I was going to do it, and because of my interest in hybridomas that I'd heard about, I went to a lab at [Albert] Einstein Medical School in New York, where there was someone who

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had been not making hybridomas, but wanted to, and he was able to fuse B lymphocytes together. Hybridomas is basically taking normal B lymphocytes and fusing them with B cell lines, and he had been fusing B cell lines in the lab.

So I went there and I made hybridomas, the first ones made there, with this technique called polyethylene glycol fusion. In fact, I think we were the first ones there, in the U.S., to do this, but I never published any of it because I didn't get the hybridoma-making antibody I was after. I was trying to get an antibody specific for MHC [major histocompatibility complex] genes, and I really didn't know how to immunize well enough to get the animal to make enough antibodies for it. So I was able to make hybridomas, but not with the antibody I needed or wanted.

But while I had been at Boston at MIT, I had arranged to do a postdoctoral fellowship at Harvard Medical School with Baruj Benacerraf, who was there in the pathology department, because I wanted to go back to Boston. I was really just trying to learn how to do hybridomas when I was in New York and wanted to be back in Boston. So after a year of doing this and learning how to make hybridomas, I went back to Boston and worked in the pathology department there, where I learned to do cellular immunology, working with T-lymphocytes, which is eventually what my lab started on and what my career was from there. So that's how I got from Brooklyn to there. [laughs]

Now, how did I wind up at Scripps, which was my first job? So I did my postdoctoral work at Harvard. I was there for a little while and I went to a meeting and met someone, Norman Klinman, who had once come to give a lecture at MIT while I was a graduate student, and I remembered we spoke about immunology for a few hours, and that was one of the reasons I became very interested in it as well. When I met him at this meeting again, we realized we liked each other and wanted to see each other. He was a professor at the University of Pennsylvania, I was a postdoc at Harvard, and we started commuting to see each other on the weekends.

Then the opportunity arose for us to both get jobs in California, and, as it turned out, he had to go to California. His former wife was bringing the children to Berkeley, where she had gotten a job, and he wanted to be close enough to see his children. Scripps was a place that we could both be able to get—I could get a lab there and he had his lab there, so it was one of the few places. There were no nepotism laws about that. It's this research institute as opposed to a university. So we both opened our labs at Scripps. It was my first job. I was twenty-eight years old. I was an assistant professor. He was working on B lymphocytes and I started working on T lymphocytes, which is what I had trained at Harvard on. So that's how I got there. [laughs]

Williams: And you're also describing a romance.

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Sherman: Yes. We were married for thirty-two years, and he died of melanoma in 2010. His picture is outside on the History Board because he was a very well-known immunologist as well.

Williams: So how did that work at Scripps as competing immunologists?

Sherman: Not competing, never competing. We worked in two different areas. He always worked on B lymphocytes, which are the cells that make antibodies, and I always worked on T lymphocytes, which was the cellular immune arm. Several times, we tried to work together, but it was too many cooks in the kitchen kind of thing. [laughs] I think that didn't work out. We were both too opinionated. But I was very, very fortunate. I always tell my postdocs and students they should marry wisely. He was a bit more settled. He was a full professor, I was an assistant professor. We did not feel competitive with each other at all. He was very well known. He was a prize-winning immunologist. I was just starting out in a different area. People at Scripps didn't even know who I was. They just knew they hired Norman Klinman, my husband. They didn't know that they also had hired me, because in those days, assistant professors sort of worked in domains of full professors. They didn't realize that I wasn't part of his domain, I just had this little space and was doing my own thing.

So nobody really talked to me, and it was sad, because had I gone to a university—now, I had had several offers for assistant professorships—I think people would have interacted with me more, they would have tried helping me more. But everyone just thought I was there working for Norm Klinman, and I wasn't, so it was confusing, because they didn't realize I could be on my own. But had I been in a university, I would have been a young assistant professor who would have been engaged in teaching and would have been engaged in committees, but at Scripps, there was none of that.

So it was a very lonely time for me. I remember often crying at night because I didn't think people knew I existed at Scripps. They didn't. My husband, who was so wise, he said, "Don't worry about it. Just do your research. You have this wonderful period of time where you don't have to spend time teaching or doing anything else. Just do your research and pay attention to that." And, of course, he was right. It was so much easier to be able to build my lab and build my career without having those distractions, so it was great. I was able to publish some very good papers very quickly and get grants and things. Things moved very well, and it was good from that professional point of view for my career, but I felt lonely. I felt like I didn't have other assistant professors I could talk to and interact with, and that was definitely something I missed.

Williams: So you weren't working under anyone's domain. You were on your own.

Sherman: I was on my own, yes, but nobody really understood that because they just assumed I was in his domain, but we worked on two totally different subjects.

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There were other people outside of Scripps that I communicated with scientifically, so it was all right. It was just there was no one working in my area at Scripps. No one really understood what I was doing there, and that was true until the mid-eighties.

So I got there in ’78, and then what happened was—I guess it was about ’86—we got a new chairman for our department, Dr. Per Peterson, and he worked in my area, tangentially, and he understood what I was doing. He understood I’d been working alone on all these things and he was fascinated with my research, and that was really the only time my career started taking off in terms of, oh, he was promoting me. Before that, I was there and nobody wanted to promote me. It was really interesting, because I had been publishing quite well, but I didn’t have a lot of people in my lab. There were no graduate students there at Scripps, and who wanted to be a postdoc for a twenty-eight-, thirty-year-old person? Most of the postdocs were older than me. So it was just me and a technician doing all this research. Then I had been doing well and I went to the chair and I said, “I’d like to be promoted to associate.”

And they’d say, “Oh, but you don’t have a lab. You just have yourself.”

I couldn’t really say anything to that because I hadn’t published yet with any postdocs. I had just started getting some, but I hadn’t yet published with any of them. So I was just patient.

Then by the time Per Peterson came, he definitely kept promoting me, and understood my science and collaborated with me, and that was very helpful, because there was finally someone at Scripps working in the area I knew. In fact, T cell immunology, it started coming up at Scripps around that time. In fact, Norm was trying to recruit some other T cell immunologists so I wouldn’t feel lonely, and he recruited Jonathan Sprent, who is probably one of the most famous T cell immunologists in the world, a wonderful person, and Mike Bevan, who was there and we managed to recruit to Scripps because of Norm, too, the world’s best T cell immunologist, although Mike didn’t stay for very long for reasons that had nothing to do with this. Jon Sprent stayed for a long time. So I did have colleagues I greatly was appreciative of, and was very helpful in terms of not feeling alone anymore in terms of what I was doing.

Williams: Right. Just before we leave the period before Scripps, you were at MIT, you were at Einstein briefly, you were in Cambridge, you were at Harvard. Compare those environments. What were they like as learning—

Sherman: Well, I had just visited Cambridge. It was just a vacation thing.

Williams: Oh, I see.

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Sherman: Yeah. So Barnard was wonderful, and I was at Columbia for a year, and that was very good. It was a very small biology department compared to MIT, so there weren't many choices of who you could work with. A lot of their biology was up in the medical school, but I was downtown on the campus where their undergraduate school and their physics graduate school and biology graduate school were and there weren't that many choices of who to work with. But it was a wonderful—now, what was your question again? Comparing—

Williams: Just the ambiance of these places—

Sherman: The ambiance. MIT was tough.

Williams: —and some of the mentors that you interacted with.

Sherman: Yeah, so Malcolm Gefter, who I worked with for my Ph.D., was brilliant, he really was, and he was just very well known when I was working with him. He had discovered a DNA polymerase which they thought was the one responsible for actually replicating DNA in bacteria, and he was considered a young Turk. He was really very dynamic, gave excellent talks, really could understand what was important and what wasn't, and I think I got great training, excellent training, from him. He was very tough in terms of—gosh, he was really tough.

When I first started in the lab—and everything goes wrong when you first start in a lab. I was dialyzing some protein that I had gotten from bacteria that took several weeks to get to that stage and the dialysis tubing broke and fell apart, and then I was collecting fractions off a column, and the collector stopped working and I lost my material, and that was a separate case, and I went to him and I said, "When is this going to stop happening to me?" and thinking, "Oh—"

And he said, "When it means more to you than it does now."

Williams: Good lesson.

Sherman: Yeah, it was a really important lesson, and I don't have the heart to tell that to my students. I don't have the heart to tell them that it's their fault, basically [laughs], because I never would have thought it was my fault, because it seems like these are accidents or things that—but it's the idea that you don't allow accidents to happen when it means enough to you. You just don't allow accidents to happen. And that's true. It's a tough one, but there were important lessons like that that I had learned from him. He didn't sugarcoat anything, and that's so important, I think, if you're very serious about what you're doing. I think nowadays we tend to treat our trainees with kid gloves a little bit, or maybe I try to because I don't want to be so rough on them as I was. That was a really tough training, and at MIT, there was a lot of people who told it like it was. It was a tough, tough place.

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But it was also very competitive. There would be times where I thought it was unnecessarily so. I would come in and I had been collecting fractions in the fraction collector overnight and one of the other people in the lab decided they needed it and moved my column off. I started working nights instead of days to avoid my colleagues. [laughs] Then one day, Malcolm came in and said, "You're not going to learn things from other people if you're not here during the day," [laughs] which, of course, he was right, and so I stopped doing that.

But it was a very tough atmosphere, and that's where I learned that people can be rough when they were insecure, and I think there was a lot of that that I saw at MIT and Harvard. Very, very smart people. I didn't really understand this till I moved to California and I saw very, very smart people who weren't like that. For some reason, I think some of those insecure people—and I don't know if this should even be part of this interview [laughs]—tended to need the name recognition of these great schools, and there was a little bit of insecurity, where they needed to have those schools' name recognition to support their view of themselves. I think part of that insecurity also played out in this competitive cruelty sometimes.

So maybe it was just that era in the seventies or so, but it just seemed that—I was just really glad to leave that competitive kind of environment to go to California, where I think it was less competitive. I mean, there were brilliant people there, and Harvard Medical School had some great people. I mean, Baruj Benacerraf was a Nobel Prize winner, and I worked with some very nice people in that department. Steve Burakoff was my direct advisor, and he was just wonderful and a very kind person. I learned a great deal about T lymphocytes from him. But there were other people in that department who really were rough. I had a great deal of respect for Dr. Benacerraf and Dr. Burakoff, but there were some others there that were rough. So when I moved to California and realized that not everybody was like that [laughs], sort of, over the years, things sort of fell into place.

Williams: But I've been intrigued by this question of science that was practiced on the East Coast versus on the West Coast and sort of wondering if there's really something there.

Sherman: Oh, I think people feel that, that there is a bit of a more competitive environment on the East Coast than there is on the West Coast, and I think the science is just as good, Berkeley and UCLA and Scripps and Salk [Institute for Biological Studies]. I think the science is just as good, but I think people just tend to be a little kinder to their colleagues and their trainees. I don't know why, but I think it's pretty well known.

Williams: Maybe walks on the beach.

Sherman: I do walk on the beach every— [laughter]

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Williams: And it helps, right?

Sherman: How bad can it be if, 7:00 a.m. in the morning, you start your day by a long walk on the beach?

Williams: Right. Exactly, exactly. So let's move to what you want the general public to know about the significance of the work that you've done and are doing.

Sherman: Right. So I have been working in basic science all my career, and it started out with trying to understand what it is that our immune cells, our T lymphocytes, see. These are the cells that are responsible for protecting you from viruses, and they'll kill viral-infected cells. They're also responsible for, as we know now, killing tumors, and immunotherapy now is all the rage because it's the first thing that can cure stage IV cancer. Well, that didn't come out of nowhere, the fact that we now have immunotherapy. I've shared in a little piece of that by working on tumor immunity and working on T lymphocytes for many, many years.

But we worked on a very basic level in mouse models and trying to understand what the immune cells can see and what they can't see, what they need in the way of help to get them to see things more efficiently, and I've been working on, basically, the question of what the immune T cells can see and not see based on self/non-self recognition; that is, you don't want to destroy your own tissues, and T lymphocytes have the potential to do that. That's autoimmunity. They have the potential to do that. You want the T lymphocytes to destroy viral-infected tissues and tumor tissues. So I've been trying to work on understanding what the antigens are that are seen in those situations. When you see self—I work on type 1 diabetes—what is it that's going wrong. Why are you seeing your own tissues and destroying the cells that make the insulin? And why is it when there's a tumor, why aren't you seeing it as effectively? Why is an autoimmune person able to kill their [pancreatic] islets, but the cancer patient not able to kill their tumor, and what the difference is in the self/non-self recognition and what we need to boost one and basically hinder the other.

I'm doing this from a variety of different perspectives, but right now what we're working on is a protein which is known to be involved in the autoimmunity, and if you have a particular form of this protein, you tend to get more autoimmune disease. I think it's important to understand how it's working and what it's doing, because it's one of the ways that the immune system's gone wrong, basically, in allowing people to get these autoimmune diseases, and yet if we knew more about it, we could use that for boosting the immunity to tumors. And, in fact, we're studying this particular protein as it works both in autoimmunity and in immune response to viruses and in immune responses to tumors.

So I think they're all related, the basic understanding of the immune system and the networks that allow your T cells to successfully kill or be hindered from

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destroying self-tissue or foreign tissue. The manipulation of the immune system that way is really the key to health and curing disease, and I think that's really what many of us in immunology are working on. But it's very, very important that people understand that these findings must be made at very basic levels if you're going to fully understand them. You have to do them in models that don't—a lot of people don't want to support science now unless it has to do directly with humans and supporting what we call translational research, research that is occurring in humans, and many people make arguments for that. Well, all of the success we're now seeing in tumor immunotherapy is based on experiments that were done in mouse models, every bit of it, and now we're using that knowledge and curing patients.

So people say, "Well, just stay working on humans." That's great, but we still haven't cured it completely and we still need to make more discoveries, and that's the basic science. If there's anything I'd want people to know, it's that I've worked on basic science all my life and there's still room to learn so much more that will allow these discoveries to be taken into people and cure disease, and we still have to support that basic science. That's really my pet peeve, is that it's becoming harder and harder to get funds for basic science as the NIH is looking to do more and more translational, and it's not that they shouldn't. They should, but they shouldn't do it at the cost of their future, which is the basic science. We just need more money to be able to do both.

Williams: Where's the location of that protein?

Sherman: It's in all cells of the immune system. It's a phosphatase PTPN22 and it's expressed in all cells that are derived from the bone marrow, which is all of the immune cells and all the cells that make up your immune system. In fact, the importance of it, whether it's in different diseases, it's probably playing different roles in different cell types, so in some diseases, it may be more important in B lymphocytes making antibodies, in others, it might be more important in innate immune cells, the macrophage and dendritic cells.

So it's been very hard to understand how it's working in these different—it has effects in so many different diseases, but it was first learned about because it was identified in autoimmunity. People who have this gene tended to get more autoimmune disease. But our feeling was, no, you don't maintain change in a protein—this alternative allele is basically a mutation that came up and was kept in the species. You don't maintain anything in the species unless it's going to be beneficial, and so it's not there to cause autoimmune; it's there for some other reason that must be beneficial. So that's when we started looking at what it's doing in viral immunity and what is it doing in tumor immunity, as well as trying to understand how it's affecting autoimmunity.

So it's an interesting protein in that it functions in so many different cell types, but that makes it very frustrating to try to understand. First, you have to identify

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in each disease, which is the cell type that it's working in, most importantly, and then what is its actual function. So it's a tough one, from that point of view, but I think there's a lot of lessons we've already learned in a few years on that.

My earlier work had been in many different things. I'm not sure if you're interested in my earlier research. This is really just the past decade that we've been working on this protein. Earlier than that, what we were working on was—wwe have been making mouse models as long as we've had our lab. The first mouse model we ever made was to try to understand, basically, graft rejection, which is because of different major histocompatibility molecules between different people. We each have our own MHC molecules, and if you get a graft from someone that's mismatched in that molecule, you'll reject it. We were very interested in what the basis for that was, because we had realized that even though you're rejecting another person's tissue, if you're a mouse, you weren't recognizing human cells as being foreign, so we thought there must be some molecules missing between species which is allowing them to understand that, "Don't bother making a response to this."

We didn't think it was the histocompatibility molecules because they were highly variable in mice, from one mouse strain to another, and within people, from person to person, so there was so much variability, if you took all the histocompatibility molecules and threw them in a basket and pulled them out, it would be hard to tell a mouse one from a human one because they're all so very different. So we thought it must have something to do with the T lymphocytes that see them that could tell the difference.

So what we did was we made a mouse that had a human histocompatibility molecule and asked whether or not it would now be seen in the mouse as foreign if we put a graft from that mouse on another mouse, and, basically, it wasn't seen as foreign. There was something different about it, but , as I said, we didn't think it was the variable part, that they look the same—they were so variable between the species, it couldn't have been—and the variable parts of the histocompatibility molecule. So we started to consider that it might be the parts of the molecule that are conserved that are different between mouse and human, something that had to interact with it so that every human had to see that part on another human histocompatibility molecule in order to respond to it.

In fact, what we did was we sort of started chopping apart the molecule and putting the mouse domains that were conserved onto the human ones, and then the mouse T cells could see it. They needed a part of the MHC molecule which was actually unique to the mouse which was not in the variable portion that was causing rejection, but it was something that told the T cell, "Yeah, this is an MHC molecule you should see." That was, on the T cell, another molecule, which is part of the co-receptor CD8.

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This is far more technical than you want to hear, so I'm going to stop on that subject. [laughs] But, basically, we were trying to piece apart what was being seen on an MHC molecule that allowed the tissue to be rejected, and then we identified the variable portion that was important. Actually, the reason that you made such vigorous responses against foreign tissues was because the variable portion, we knew then, was able to bind peptides in them, which is how they present antigens, but what we showed was when you do an allograft rejection, you're seeing that peptide, and there's thousands of different peptides on the cell surface that you were seeing. So even though it was just a liver cell that you were rejecting, there were thousands of antigens on that liver cell that were different from your liver cell because the MHC molecules had different peptides being bound to each of the MHCs. So we were basically describing what was being recognized on MHC molecules in allorecognition and graft rejection.

Once we made this mouse that was able to recognize human MHCs, because we put the portion on that, allowed it to interact with the mouse immune system, we realized that, oh my gosh, we could immunize this mouse and it had basically the human part that was able to bind antigens, and we could identify human antigens in the mice. So we started this for viral antigens and tumor antigens, and it just turned out to be a very useful mouse model for identifying human antigens, because now we could immunize the mouse and it would respond as if it would see the human MHC.

So we did that and it led us very much into tumor immunology and trying to understand self/non-self recognition in tumor immunity, and we worked on that for a number of years. That also led us into the autoimmunity component, because we really wanted to understand what goes wrong when you see tissue you shouldn't, and made some mouse models to study that. So one thing led to another, but it all had to do with T lymphocytes and what they were seeing in self/non-self recognition. That was sort of the common theme, and it's still our common theme now with this protein that we're working with, PTPN22.

Williams: Describe your current lab, how many people and so forth.

Sherman: So it's really small right now, two postdocs and one technician and myself. One problem we've always had at Scripps is we're a research institute and we don't have a big endowment because of that. There's no hospital that people want to donate to. There's no undergraduate school that alumni want to donate from, so we've never really had much of an endowment. We're just a research institute, and all of the money that pays for our lab and our salary has to come from our grants. So whereas in a university, usually you're paid because you're teaching and then the money you bring in for grants goes towards your lab, I've always had to have, oh, at least three grants to be able to run my lab. So the first grant would pay my salary, the second grant would pay my technician and for my mice, and the third grant would be the reagents and the postdocs.

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But about ten, twelve years ago, the NIH budget dropped precipitously and I could no longer get three grants, and so the lab shrunk down, because I still have to pay my own salary. That's a unique something to a place like Scripps. I don't think there are very many places like that, and I never would have started a career there had I known that. Scripps is now looking to change in the future so that it won't be a problem, but for this period right now, we're in sort of financial despair for having to both pay our own salaries and for the lab. So the lab has shrunk down to about half the size it used to be, so instead of four scientists—I've always had a small lab. We always had four scientists and a couple technicians. Now I have two scientists and one technician.

Williams: Do you fear that because of this economic situation, that Scripps might move more towards translational?

Sherman: Well, that's exactly what's happened. That is exactly what's happened at Scripps. So we hired a new president [ed. Peter Schultz, president of Scripps Research] a few years ago largely on the basis of the fact that he felt that he would be able to bring money in several ways. One, we thought he would get philanthropists engaged. But also he had been both in academia and industry and he had a company, a nonprofit called Calibr, which does translational research for hire, so that companies hire them to develop products or they develop products and then license them to companies. So the royalties from that would come back to Scripps and that would be a stream of money then that would allow us to have a big endowment.

So that's what he's working towards, but I think the fear is that now—and this is true almost everywhere, but particularly at Scripps—that unless you're doing something very translational, that you're not contributing, you're not going to help bring in money, and that's really a shame, because I was the one who wanted to be the theoretical physicist. [laughs] When I was in biology, I just wanted to do basic science. I just want to sit there and think and come up with the next experiment that will help us understand something, and science just isn't like that anymore.

I think now people are under a lot of pressure to be drug-developing. If you're not working on something that has that potential to be "druggable" and use it, then why bother? You're wasting your time. That's such a shame, because I think that's just going to choke off basic research, and it's so shortsighted, because where will the next discoveries come from, the next big revolution that gives you the next ideas to develop new drugs?

But I think that I'm very fortunate to have been able to do science at a time when there was money in science and there was no pressure to do translational science, and so I just feel so fortunate that—and I feel badly for the young people coming up now, because they're going to have a much harder time getting funded and they'll need to do that sort of—I see people from my own lab that would have, in

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another day, would have gone into academia now going into biotech straight out of their postdocs, some straight out of their Ph.D.'s, and it's not giving them the opportunity to do that basic research.

I've always said the reason we're so lucky is we get up each day and we have an idea, we can go to the lab and test it. We're just so—not spoiled, but I guess we are, self-indulgent. We're just self-indulgent. We can do what we want, basically, when you have a basic research. You can test out your ideas, whereas if you work for a company, not only do you have to do what their project is, but then they can take that project and you're doing a different project because they've switched out, they've decided to drop that one. And it's great if you're interested in doing lots of different things, that's great, but if you're in science because of being so self-indulgent like I am [laughs], that's not how you want to work, and I think that's going to be harder for people to be able to do in the future.

Williams: I don't quite think "self-indulgent" is the word. It's more sort of a purity of vision or the opportunity to work very productively in lines that occur to you.

Sherman: I think there's a bit of self-indulgence.

Williams: Okay, all right.

Sherman: Because you want to prove your ideas right. There's a stubbornness. There's a selfishness. Believe me, it's not so pure. [laughs] We scientists are not so pure. We become enamored with our ideas, often a little bit too enamored of our ideas. No, I wouldn't say we're pure. [laughs]

Williams: That's a great statement. I'll have to remember that. Is the same trend occurring in other countries, the move towards translational?

Sherman: Yes, I think America is often the leader, be it good or bad. So, yeah, I think there's still some—well, one of my former postdocs who I just had dinner with last night is in Switzerland, and he feels he has a great deal of freedom in terms of being able to do what he wants. Compared to what we have in the U.S., he feels he's lucky. So I think it depends on which country and how the funds are being distributed. I don't know from country to country how it is. I think some, their budgets are much tighter than others and maybe the pressure there is greater.

But I would see that as a trend in the future, that people do want—and rightfully so—taxpayers want to see what they're paying for. I think we're showing them that all the time, but there's always that pressure. Congress wants to see where's that money going to, what developments. Well, I think the pressure's off a little bit right now because we've shown them the immune system can cure cancer, and it's the only thing that can cure stage IV cancer, so the pressure's a little bit off, but they want more, and, of course, we all want more. But, yeah, I think there will be that pressure in the future.

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Williams: You joined the AAI in 1981.

Sherman: Yes.

Williams: And what was your reason for doing so?

Sherman: Oh, I was so pleased to be able to get in. It was a great honor then. I think as a basic immunologist, all of my heroes were at AAI. They were all members of AAI. I would go to the meeting every year, and it was an opportunity, if I was lucky, to be able to present my research orally; if not, as a poster. But I was pretty lucky, and it was just a wonderful place to gather and meet other people in the field and form lifelong friendships with your colleagues. I still feel that this is where I want to come every year and see people I haven't seen in a while and catch up and hear some good science. I think it was just not even thought of not to do it back then. I think now there are more options. People don't feel it's important because there's so many other meetings, but the opportunities the AAI has provided to me in my career—there were travel awards that I got back then, even—and just the social networking and scientific networking, always very much something I look forward to each year.

Williams: And you became involved in a number of the committees.

Sherman: Yes, yes, and I was honored to be asked. Whatever it was, I'd always say yes. I think that's the kind of service that scientists look for in providing to their colleagues. We review grants for NIH. That's our service, and I think this is another part of what we need to do, is provide service for our colleagues as well. That's just natural for us to do that sort of thing. These are our friends and colleagues.

Williams: Right. So then you ran, I guess, for the Council. Is that a—

Sherman: Yes, I was asked to run for Council, and that was a huge honor for me, because I had known several of the presidents and they were my heroes. They were people I had always looked up to, and it's still hard to believe that I've been one of them. I am one of those people who I looked up to so much [laughs], so I guess some young people must look up to that, but it's hard to believe that I got to that stage and was able to do that, to serve on Council. I think we made some meaningful changes.

Williams: You want to enumerate some of those changes?

Sherman: Yes.

Williams: Now you're talking about as president?

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Sherman: Yes, councilmember and president.

Williams: Let me just finish here with the Council a little bit. You, in your statement about wanting to go on the Council, mentioned that you thought that NIH was funding bubbles rather than, I guess, basic research. Define the word "bubble."

Sherman: Oh, did I? I don't even remember that.

Williams: Yes, this is a direct quote, "Congress needs to know that the continuous growth in the science is very important versus NIH funding bubbles."

Sherman: I *really* don't remember that. Isn't that interesting? I'm trying to think of what I could have meant. Perhaps things that looked like they were great breakthroughs, but weren't and just sounded good. [laughs] But I really don't remember saying that. Doesn't sound like something I would have said, but I believe you. I just really don't remember that. Interesting.

Williams: Okay.

Sherman: But I'm sure it would have had to do with perhaps—I always say that NIH was always there to fund the science that, oh, was sexy-looking and everybody was doing rather than the good solid progress in a sort of meaningful and intentional way, and still happens. Something would look like it's new and shiny and everybody would sort of jump on that and they'd all be working on that, and sometimes it would pan out, a lot of times it wouldn't, but you always needed to sort of have a gimmick [laughs] to get your grant funded, and that's still true. Good solid science is hard to get funded. But I'm not sure what I meant by "bubbles."

Williams: I suspect that part of the situation is that the Congress tends to favor certain, quote, unquote, "sexy" areas, and then that is reflected in some of the NIH behavior.

Sherman: Yeah. They like to give money if you come to them with a shiny new possibility. They'll want to give money for that rather than for just the same old same old, but it's the same old same old which, I'm afraid, leads to most of the discoveries. I don't think we have much say in what NIH gets to fund, unfortunately. [laughs] There are a few people they listen to, but it wasn't me, that's for sure.

Williams: Did you appear before members of Congress?

Sherman: I've gone to the [Capitol] Hill. I've never testified before Congress, but I've spoken to several congressmen, yes, and gotten to know several of them from my districts and had some very meaningful conversations with them. But I'm from a Democratic district, so Scott Peters, I'll go into his office and I'll say, "Oh, we

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need this much more money for NIH," and I'll say, "Oh, we need six billion more this year," or whatever.

And he'll say, "No. Twelve billion." Like he has any power to get that, but he's so Democratic and so pro-research, it doesn't really matter.

But then I was able to speak with someone who was a Republican, Brian Bilbray, and it was really sad, his daughter, in her twenties, had melanoma, and this is when my husband was going through melanoma. I was talking to him about the drug that my husband was on and talked to him about that, and he was really knowledgeable of what was available and what was going to happen. I don't know what happened to her. I hope she got some immunotherapy in time and was fine after. I don't really know, but we did have a couple of meaningful conversations.

But I think I tended to preach to the choir, unfortunately, everyone I went to see, and the people who were against science didn't want to see you. So Darrell Issa, even though he's in Southern California, we tried to get to see him, but he wouldn't see us, and he's not very much pro-science. So you wind up preaching to the choir. There must be some who you have an impact on. I think what we could do, though, was give the people who were pro-science some of our talking points so they could be more effective in terms of telling their colleagues why it's so important to fund NIH.

Williams: So looking back on your year of presidency, what were some of the highlights?

Sherman: Well, I think it started before then. I started making a lot of noise [laughs], and Michele [Hogan] was a little put off by this, about the fact that our endowment was so large at AAI, and people were going through such tough times in the labs, keeping them running, that we needed to give more money to our members, we needed to have programs that were meaningful amounts of money. So it was when I was there I think we started, first, a small thing where at local meetings we would give just awards for postdocs and grad students who gave presentations, and that was a few thousand dollars per meeting. Then we enacted these fellowships, which was real money. This is money for salary for a postdoc in your lab or for a graduate student for a year, and this is a meaningful amount of money for these labs.

I think the idea of AAI giving back some of its endowment is something I still feel very, very strongly about. I think it's important for an organization like ours. We work for our members, we collect the money from our members in one form or another, and we should be giving back. Other than a few years of what we need to run ourselves, it should go back to the members, and I think we should be giving more even still. I'm afraid I haven't been looked upon as favorably by the administration of AAI because I keep talking about they should give their money

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away. [laughs] But, yeah, I'm really pleased that there were enough of us on Council that we were able to put that through.

Williams: You used a term in that regard that I'd never read before, "labs in need." Can you talk just about labs in need for a moment?

Sherman: Well, yeah. I mean, we are all—not all, but I think many labs have suffered in the past decade from there not being enough grant money to be able to do all the things that are required for running the lab. You need postdocs, you need postdocs' salaries. It's very difficult. It's very difficult to get enough money on your grants to—well, in my case, it's extraordinarily difficult. I'm an unusual case, as I said, because of Scripps not giving us any salaries, but most people will have maybe one grant. The average lab in the university might have one grant, and they won't have enough from that to pay for a postdoc. Maybe they can get the reagents they need and get what they need to do their experiments and maybe a technician, but maybe not enough to pay for both the graduate student and a postdoc.

So the smaller labs, which most labs are, are always in need of money, and the time now, especially a few years back when NIH was really hurting and people couldn't get grants—it's still ridiculous. It was at the level of, for an immunologist, maybe 12 percent of grants that you write can get funded. So there's many, many labs in need, and these are really excellent scientists who—the differences, everyone always says, between a grant that's funded and a grant that isn't is imperceptible. You can have a different group that would like this grant better than that, and another group would like this one. There's no rhyme or reason, often, within a certain range. If you're not funding the top 25 percent scientifically, then it's arbitrary, because within that, all of them can give you equally good results and lead to equally good discoveries. So there are many labs that are in need.

Williams: Talk about the importance of *The Journal of Immunology*.

Sherman: So I was always very proud to publish in *The Journal of Immunology*. When I started out, in fact, my first immunology papers were in *The Journal of Immunology*. I didn't even consider sending them anywhere else. There weren't very many immunology journals when I started out. So when I look back at my CV and what's published, there are many, many papers in *The Journal of Immunology*, and, to me, I call it our trade journal, trade like if I were, I don't know, a locksmith or a plumber. I'm an immunologist the way people—it's a trade. I've trained, I've apprenticed. It's my trade, and that's my trade journal.

The quality is extraordinary, because I know that the entire process—I've been an editor there for many, many years—we do such a thorough job in reviewing the science, and I don't think we're nearly as influenced as sort of these journals like *Nature* and *Science*, which they have criteria which the science has to be good,

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but, more importantly, it has to be science that's going to grab the audience, because they're for-profit journals and it's a different thing. We're not, we're not a for-profit journal. If anything, we probably lose money, but the quality is kept very, very high. The reviews are extraordinarily thorough and rational and they're looked at by the three reviewers who read the paper carefully and then a section editor that makes sure it's been done right, and then the editor then oversees that, the deputy editor. The level of care and quality is so high. It's much higher than any other journal.

So I'm really proud of what they do, and it's a wonderful journal. I still publish there and probably always will. I'm just sorry that there's so much competition right now, and not necessarily of that kind of quality. But, yeah, I think very, very highly of *The Journal of Immunology*, always have.

Williams: This is a self-serving, not personally, question. Speak about the staff of the AAI.

Sherman: Oh, my goodness, yeah. They're remarkable. They're so good at what they do. Michele Hogan, I don't think ten people could replace what one person does. [laughs] She's really extraordinary, and she does it effortlessly. She just makes it look so easy, but she's got an eye for talent, I can tell you that, because all of the people who work with her are extraordinary. I don't know how she picks them. And dedicated, they're all very dedicated and uncompromising, really, in terms of their quality. So I'd say it was an honor getting to know them. I may have given them a rough time [laughs], but I have the highest regard and respect for all of them, greatly. Yeah, they're a unique organization. I don't know any other of these scientific organizations at all, really, other than AAI, but I am extraordinarily impressed with what Michele has done with this organization for so many years. I've only really only known it under her leadership, and it's been extraordinary.

Williams: Do you want to define "tough times"?

Sherman: Tough times for scientists, you mean?

Williams: No, no, you said you gave them tough times.

Sherman: Oh, well, as I told you—

Williams: You don't have to do that.

Sherman: No, I just was bugging them to do a few things they didn't want to do. [laughs]

Williams: Oh, okay. I noticed in looking over your bio, you are involved in so many other organizations as a board member and whatnot.

Sherman: Oh, yeah.

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Williams: How do you maintain your focus and your energy?

Sherman: Oh, no, I mean, it hasn't been so many at once. Yeah, these are not big, big draws on my time. When you're a board member, it's three times a year or something. It's not as time-consuming as you might think, so it hasn't been bad, and it'd generally be important things. I guess the most important was I was on the board of Scripps, and that was a tough time. It was when we were trying to do a search for a new president, and that was a very tough one. That was a few years back.

Williams: Do you recommend a career in immunology for people who are coming up in the graduate schools and so forth?

Sherman: Well, I think that what I've always said is it's such a difficult career that you need a great deal of self-confidence, but you also need to be doing it because you really can't do anything else and be as happy. It's got to be the thing that you know that unless you do this, you'll feel you've done the wrong thing, you're not as fulfilled doing something else, because it takes that kind of focus to stay at it because it's so hard.

When my kids were young, we had a nanny, as we are wont in Southern California, who was originally from Mexico, and she asked me what it's like going to the lab and doing what I do, and I said, "Well, I get to work and there's someone there who punches me for ten hours a day. Then I come home." [laughs] It's the continual beatings from the reviews you get on your paper rejections and the grant reviews that are rejections. They're always trying to beat you down, but you have to have a great deal of confidence and you have to be so driven in what you do to stay at it despite that. If you have that determination and need and desire, there is no better career than being a scientist.

And immunology is probably, I think, the most important field to be in, even if it sounds a little self-serving, because if you think about your body, you've got all these organs, but it's the immune system that's traveling through all of them, monitoring all of them, making sure everything's okay and communicating with all of them. And every disease you can think of, almost every, not quite, but almost every, is immune-mediated. We even now know coronary heart disease is immune-mediate, the innate immune system. Cancer is because of failure of the immune system. The immune system plays into any disease you can think of, just about. There are some genetically determined diseases where an organ goes wrong or something goes wrong that's not immune-mediated, but most diseases are something the immune system affects or causes. So I don't think there's any other science quite as important, biological science.

So, yes, I would definitely say it's a great life. I'm glad I did it when I did and not starting right now, but if you're self-confident and determined, I think you can make a great life with it, even now. Just choose wisely where you decide you take

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up your career and go to a supportive atmosphere, and I think you can do well, even now.

Williams: You mentioned children. How many did—

Sherman: I have two sons biologically, and then my previous husband, Norm, the one who's deceased and was an immunologist, had two boys from his first marriage, and we're very close to them. So there's those two stepsons and three step[grand]children. In fact, tomorrow, one of the stepgrandchildren and my son from L.A. will be coming to see me. My two sons, one's in L.A., he's a comedy writer [laughs], and the other one, he lives with me. He's developmentally disabled, and he'll always live with me. I remarried several years ago and have three new stepchildren.

Williams: Wow. Really?

Sherman: So I have a lot of stepchildren. [laughs]

Williams: Five stepchildren, yeah.

Sherman: Yes, I have five stepchildren and two children, yes.

Williams: Interesting. Your son who's the comic writer, we've been struck in talking to people about their offspring that not many of them go into—

Sherman: Science.

Williams: —science.

Sherman: Yes, yes.

Williams: Quite interesting. Many artists and musicians and so forth.

Sherman: Yes, yes. I think a lot of them, well, they see how difficult a life we've led, and they don't see the joy of it as much. Maybe they do see some of the joy of it, but maybe we don't show them enough of the joy of it. They see us come home and we're complaining, "This paper got rejected, that grant got rejected." They don't see the joy of us when we see our data. We're doing this because we love that, and we're designing experiments and we're getting the data, whether it supports it or not. I mean, that's what we live for, and communicating our science. I love giving lectures, communicating my science. They don't see that joyful part. They see us coming home and complaining. [laughs] But there are quite a number who have children who have gone into medicine or science.

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It's funny, because when I was a postdoc in Baruj Benacerraf's lab he started chatting about, "I never would have let a daughter of mine [Beryl Benacerraf] go into science. She's in medical school."

And I said, "Really?"

He said, "Yes." He said, "It's too hard a life. I would never have let my child do that."

Williams: Interesting.

Sherman: I was amazed, because I would have been honored had my son decided to go into science, but he was protecting her from it, in a sense. And she became a very, very accomplished physician, geneticist, and she's an excellent doctor. But that he would tell me that, he felt he could control the family, and he did. At first, I thought my son was going to go into science. He was interested in marine biology and geology and even did a year—no, six months—in Cape Town, the University of Cape Town, and published a manuscript on meteorology, and I thought, "Oh, my god, he's got a paper and he's still in college."

Then I said, "Well, are you taking your GREs?"

And he'd keep saying, "Oh, I think I'll wait a little while."

I didn't really know the extent he was doing comedy. [laughs] I had no idea he was working for *The Onion* on the side, and then he did that full-time. Then he was working at Funny or Die full-time. And, in fact, he made a video—when I was president, he made a video for AAI for after my President's Address. I was presenting about this CRISPR-Cas9 new technology of how to edit, and he made a video about using it to edit politicians so they'd fund more research [laughs], doing brain editing to get them to fund—and he made this comedy video, which was great.

Williams: Is he associated with any particular show now or—

Sherman: So podcasts are very big right now, and he's actually working on some podcasts with this new company that was recently started by Conan O'Brien and some other comedians. So he's doing a podcast with some other comedians in L.A. He's living in L.A. now—he used to live in Brooklyn—and doing that. So he's freelancing. He was working for Funny or Die and *The Onion*, but those companies have sort of—comedy's not doing very well these days. Most comedy now is politically oriented. It's a sign of the times, yes. So it's not doing quite as well as it was, and the kind of comedy he writes is a little broader-based, so he's writing these podcasts now. I think he'll be fine. He did what I told him; he married well. He married someone who does IT work at companies, so she'll

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always have a job. [laughs] He won't have to worry about that, and he'll be able to do his freelance comedy.

Williams: Is there anything we left unsaid here today or we covered it pretty well?

Sherman: Yeah, I think you got it all. [laughs]

Williams: Well, thank you very much.

Sherman: Yes.

Williams: Very good.

[End of interview]

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