



The American Association of Immunologists Oral History Project

Transcript

Ethan M. Shevach, M.D.
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Interview conducted by
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Williams: This is an interview with Dr. Ethan Shevach for the American Association of Immunologists (AAI) Oral History Project. Dr. Shevach is Chief of the Cellular Immunity Section of the Laboratory of Immunology at the National Institute of Allergy and Infectious Diseases (NIAID). Dr. Shevach served as editor-in-chief at *The Journal of Immunology (The JI)* from 1987 to 1992, and he was awarded the AAI Distinguished Service Award in 1992. We are at the AAI headquarters in Bethesda, Maryland. Today is Wednesday, December 16th, 2015, and I am Brien Williams.

Thank you very much for doing this today, Dr. Shevach. Let's start with a little bit of your family background, where they came from and where you came from.

Shevach: Where I came from. So from what I know, my father was born in Poland and immigrated to the United States in the twenties, went to college at Boston University as an undergraduate, and actually received the first Ph.D. in psychology from Harvard University when psychology was separated from philosophy as a discipline.

My mother was born in Boston, the daughter of a Jewish educator. And although my father tried to find a job as an experimental psychologist during the Depression, that was quite difficult. Ended up as a Jewish educator, taught at the Hebrew Teachers College in Boston, and then became director of the Bureau of Jewish Education in Boston, sort of principal of religious schools, Jewish religious schools, after-school Jewish religious schools, for the entire metropolitan Boston area.

So I grew up in Boston, went to high school in Boston at the oldest public high school in the United States, Boston Latin School, founded in 1635, a year before Harvard University. And, unfortunately, when I was a high school student in the late fifties, early sixties, biology was not on the curriculum, curiously enough. So the focus at that time at Boston Latin School was still the classical education. One was compelled to take Latin from grades seven through twelve, French from grades nine through eleven or twelve, and in the tenth grade, as I was matriculating up there, one was encouraged to take either German or Ancient Greek.

Luckily for me, the Russians did one great service to high school education in the United States: they sent up Sputnik in 1957. High school curricula changed, and I was saved from taking Ancient Greek. I was allowed to take physics and chemistry instead, but not biology. So I graduated high school with absolutely no experience or no knowledge of the biological sciences, and in my senior year in high school really had no idea what I wanted to do.

I applied to a variety of colleges, and one day my parents suggested to me that I also apply to a novel program which was just being initiated at Boston University at that time, for their first class, which was a six-year program combining

undergraduate education with medical education. So one was accepted to medical school when one graduated high school, and if one maintained grades during college, during those two years of college, one was then allowed to enter medical school after two years of college and graduate with a total of six years. This included also summer school during most of the years, so one achieved about three years of formal college credits, but that was it, and the regular medical education program at Boston University. So I graduated medical school at a relatively young twenty-three years of age and went on to do two years of intensive training in internal medicine at Albert Einstein's main teaching hospital in New York City, Bronx Municipal Hospital Center.

And then the question was what to do with the rest of my life. And, well, fortunately, sometimes other factors come into play when one has to make career decisions, and the big career decision in 1967 when I graduated medical school was the Vietnam War. That was a big influence on one's career. And as a physician, to be honest, it didn't appeal to me to go to Vietnam. I was drafted by the army, as every male physician was drafted in those days, and was pretty likely going to spend a year as a general medical officer in Vietnam, though I didn't know that.

So the other alternative was to embark on a career in medical research and come to the NIH [National Institutes of Health]. And the NIH was a very cruel place to get an interview. It was a highly competitive environment for medical fellows at that time, probably two or three hundred applicants for each position. They would not tell you when they would interview you. You would be interviewed sometime between June 1st and June 30th of your final year of medical school, and you had to stay around and be available at their beck and call to come for the interview. Sort of strange. I got married at that time and wanted to go on my honeymoon, but I was restricted to where I could go so I would be available for this interview.

I had an interest in immunology during medical school. A couple of summers, I worked in the lab of an immunologist, the late Dr. Sidney Cooperband, and he had a sort of unusual influence on my career choice, that immunology was what I wanted to go into. I wasn't too successful in the lab during medical school, it was a very brief period of time, but I enjoyed what I did and I appreciated his guidance in directing me that immunology might be my choice of research specialization.

I applied to the NIH and I wasn't too selective in those days as to what I'd take, anything to avoid going to Vietnam, that included virtually all of the medical specialties that were available at the NIH, and my first choice was not to join the National Institute of Allergy and Infectious Diseases, but to become a rheumatologist, to join the National—it wasn't called NIAMS then, but the Arthritis and Rheumatism branch of what I think was NIDDK [National Institute of Diabetes and Digestive and Kidney Diseases] in those days or something like

that. They didn't want me, and I was fortunate enough to be selected by the NIAID, by Dr. Sheldon Wolff in the clinical program there, which involved a year of really clinical training or clinical experience as a clinical fellow, taking care of patients with unusual combination of immunologic, autoimmune, and infectious diseases, and then entering the laboratory for a two-year period of intensive training.

I enjoyed my clinical experience and I was very eager to get into the laboratory to start my research experience, and I had a horrendous year in a lab. I had a mentor who didn't adequately supervise me. I was extremely unhappy. I was ready to give up research completely and go back to clinical medicine.

I sought the advice of Sheldon Wolff, who was the boss, who was head of the group, and Shelly was a very interesting administrator. He was a very good judge of people. He later left the NIH to become Chairman of Medicine at Tufts and, unfortunately, died at a very early age. Shelly said, "Okay, you're a smart guy. I understand why you've had an inadequate experience. Others have complained as well. Find another job within the Institute, another lab that would be eager to have you," and he'd pay my salary. So that was a very generous offer, and I talked to several other people within NIAID to choose a lab.

I finally selected the laboratory of Dr. Ira Green, who was my initial mentor in the Laboratory of Immunology. Ira had the knack of sort of knowing what would work and getting results fast, reliable results fast, I should say. And Ira started me off on about six different projects, working with both human, mouse, and guinea pig cells, in vivo and in vitro, and after a year with him, I had probably the material for four to six papers, which I wrote. This was a tremendous boost to my ego, which I desperately needed at that time, and I began to think about immunology, think on my own.

I developed some interesting collaborations with other scientists outside of the Laboratory of Immunology, primarily Dr. Alan Rosenthal, and he and I, independently of Dr. Green, went on to make a major discovery, which is the discovery that both lymphocytes and macrophages had to come from the same strain, had to share the major histocompatibility complex to collaborate in culture and in the animal.

In those days, publishing a couple papers was actually enough to be asked to become a tenured member of the NIH. So in 1973, I assumed what really is now my present position as a tenured member of the Laboratory of Immunology, where I've been since 1973 with different titles, but with the same job.

Williams: Great. I want to go back and just ask you a couple of follow-up questions. When did the scientific light occur for you? When did you first—because particularly going to Boston Latin, you weren't getting exposure too much to science.

Shevach: No. Terribly taught physics and chemistry mostly. Probably during medical school when I spent some time—I enjoyed medical school. I enjoyed certain aspects of medical school more than others, mostly biochemistry and pharmacology. I can't say I enjoyed gross anatomy or looking in a microscope every day. And a little bit of time in the lab really influenced me that this is something I'd like to pursue for my life's career. I like getting results. I actually liked doing experiments in those days, and I was willing to—one of the advantages of graduating medical school when you're twenty-three and having to spend two years at military service, which is what my time at the NIH was, I could gamble. And since I was [unclear], I wanted to gamble in doing a couple years of research and see how I liked it. I have to say I had no financial obligations. Things were very different then. Tuition and medical school, I had no student loans. I was newly married, but my wife was working, actually mostly supporting me while I was a medical intern and resident. She made more than I did, in spite of my M.D. degree.

So I wanted to give it a chance. It appealed to me that research was the thing to do. I was influenced by a few people in medical school. Besides Sid [Sidney R.] Cooperband, the other was the head of Gastroenterology at Boston University, very famous gastroenterologist, Franz Ingelfinger, who later became editor of the *New England Journal of Medicine*. I saw his laboratory group was right next to where I worked in the lab, and I saw how he ran his clinical group and his research group, and it sort of was the spark that I wanted to explore. Needless to say, my initial experiences in the lab frustrated me greatly. I was sort of very upset that nothing worked and I wasn't being supervised adequately, and those are all very important things, and I'm glad that I was rescued.

Williams: Your father didn't encourage you to follow his career path?

Shevach: Well, I hadn't—and it's strange what your peers become. So most of my good friends in high school went on to become rabbis, Jewish educators. A few did other things, but I had a heavily—I wouldn't say religious. I had more of a secular Jewish upbringing. My parents actually wanted me to go to medical school. I think my father originally wanted to go to medical school, and in the Depression, again, wasn't accepted into medical school and then went into the field of psychology, which he also, I think, was frustrated by, that he never—he was one of the first experimental psychologists around, yet he couldn't pursue that as a career either.

Williams: He never did?

Shevach: Never did, yeah.

Williams: Do you have siblings?

Shevach: I have one sibling, a younger brother who's a year and a half younger than I am. He had a different career at Boston Latin School in that he liked Ancient Greek, so he avoided science completely, took Latin, French, and Ancient Greek at the same time, went on to major in philosophy at Harvard and ultimately got a Ph.D. in philosophy, and taught philosophy at the college level. So, different things appeal to different people.

Williams: So what was the NIH campus like in 1969 when you arrived?

Shevach: It was a terrifically stimulating place. The quality of the postdoctoral trainees, particularly the M.D.'s—and there were many, many M.D.'s. A great majority of the fellows were M.D.'s in those days, relatively few Ph.D.'s were in the lab, and everyone was really very ambitious. Particularly when I joined the Laboratory of Immunology, my peers all turned out to be unbelievably devoted scientists. No matter how early I came in to work in the lab, the fellow working across the hall from me—now I think he's chancellor of the University of California, Dr. John Stobo, who was professor and Chairman of Medicine in Johns Hopkins—Jack always beat me in every morning. And I came in pretty early as well, but he was always there before me. He went more into a clinical career and an administrative career.

But the other fellows in the lab were very bright, knew much more immunology than I did. Charlie [Charles A.] Janeway worked next door to me at that time. Don [Donald] Mosier was there. So it was a very well-trained, competitive, ambitious group who knew a lot, and people were pretty generous, for the most part, helping you out, starting out, learning things, getting you going, working with complex experimental animal models. It was a great period of time at the NIH.

Williams: So what major changes or developments have occurred since 1969?

Shevach: Well, things have changed. I mean, in a sense, we've many, many fewer—I've trained M.D.s in the past. I have a couple M.D.s, M.D.-Ph.D.s mostly, in my own research group at the present time, but there are many, many fewer M.D.'s coming to the NIH to basic science labs to explore training—at least I can only speak for the immunology field—in basic immunology.

Williams: How do you account for that?

Shevach: Well, there are lots of reasons that people propose. Research is, number one, less attractive. I think medical students learn from their mentors or their professors during medical school. If they're struggling getting grants, it's not an opportune thing. Debt is a very important thing. Many, many have student loans. Even though the NIH has a loan forgiveness policy for M.D.s that come to the NIH for training, I don't think that's enough. And I think the NIH doesn't sell itself well,

I think, or promote itself well, what a great place it is to do postdoctoral training in the basic sciences.

The other factor is a bit more subtle, in that fellows train in one of the big cities, say in Boston or New York or San Francisco, they're older, they're settled in their environment, and to take up and move and come to the NIH for training is tough. They may get equal—I mean, I can't say that the training at the Harvard Medical School area in Boston is any less good than coming to the NIH. So many have sort of stuck where they are and they don't come to the NIH anymore.

The NIH was a very unique place in the late sixties or in the early—really throughout the sixties and the early seventies, when one learned from people who'd been there what a great experience it was. When I was an intern, I had a resident a year or two ahead of me who had been at the NIH already, encouraged me to come. And many of my peers, my fellow interns and residents, also came to the NIH to do postdoctoral training, some in the more basic sciences, some in the clinical sciences. So it was a sort of forward feedback kind of thing, where the people ahead of you had already been there and said, "This is the place to go and this is where you're going to learn how to become a scientist as well as a physician." So lots of physician scientists were trained then. Now many fewer physicians want to be scientists, and the few that do have other places where they can also train and other reasons to stay where they are.

Williams: So how is the NIH staffing itself at the highest levels today?

Shevach: Well, that's a problem, I think, and the NIH is an aging community as well. One of the reasons I've stayed at the NIH throughout my career and one of the great boons to immunology is the immunology community at the NIH has remained strong over a period of forty years that I've been there. So there are always multiple seminars, many, many people doing related things, many people willing to help you out. For our weekly Immunology Interest Group Seminar, as it's called, we usually have 250 people. Very few other places in the country can boast that strength of immunology. And we've slowly replaced the group that's left or the group that's retired or passed on, but I think that there's sort of a lack of mid-level scientists within the NIH community in general who are willing to go on and stay. So people leave, and there have been incentives—I don't want to say "incentives." There have been reasons why people want to leave the NIH that weren't there in the old days.

Williams: Like what?

Shevach: Salary and restrictions on certain activities. Between about a ten-year period, 1995 to 2005, NIH scientists were allowed to consult for private industry on their own time and were allowed to be paid for this. And I did it to a certain extent. I enjoyed doing it. I learned a lot. The companies I consulted for valued my advice. And then one day we were told that this was totally forbidden, and there

were multiple reasons for that. It was basically banned in 2005 and never happened again. So at the present time, we're not allowed even to consult for free for private industry. [laughs]

Williams: This is something that came from Capitol Hill or—

Shevach: It came from Capitol Hill. So there was a famous series of investigative articles by a reporter for the *Los Angeles Times* that claimed that NIH researchers were spending *all* their time consulting for private industry, as you might imagine. But there were strict rules about what you could and couldn't do, and some of the rules were really quite reasonable. If you were away from your office consulting for a company, you had to take a day of vacation. That's quite reasonable. The government shouldn't pay you when the company's paying you. And you always had to ask for permission to do this in writing and formally have it approved, and a few people weren't doing that.

So the director of the NIH was called before a congressional committee and was embarrassed, I have to say, when he said, "Well, here are the lists of NIH scientists that are consulting for industry," and the congressional committee had a different list which was given to them by the major pharmaceutical companies and was considerably bigger than the list of people who had asked for approval.

So Dr. [Elias A.] Zerhouni, who was the director of the NIH then, said, "We can't enforce the rules. We'll just ban everything." And that's created other problems as well, including even doing useful things like being paid as an editor of a journal. That's still allowed. That's the only thing that's allowed. Editing is allowed as an outside activity.

Williams: Are foreign-born scientists—can they be hired by NIH?

Shevach: Permanently, I think they have to be U.S. citizens or have a green card. There are many more foreign scientists than there used to be as tenured faculty now at the NIH.

Williams: Well, I was thinking that that might be also a source for compensating for the lack of American—

Shevach: Well, but that's not good. I mean, yes and no, I guess. I mean, we train lots of foreign-born scientists, I have to say many more foreign-born scientists than American scientists. I have mostly foreign-born scientists in my lab at the moment.

Williams: Is that a change that has occurred since you came here?

Shevach: To a certain extent. The M.D.s, again, are now mostly not going into basic research. And over the years there was a curiosity at one point where it was

easier for us to hire foreign-born scientists than American scientists. So an American scientist required what was called an FTE [Full-time equivalent], a federal government slot, to hire, whereas a foreign-born scientist didn't, and we had a limited number of these so-called FTEs and we were a bit more liberal with non-FTEs. So at one point it was much easier to hire a foreign scientist than an American scientist. They've changed that situation so it's the same now.

Williams: What about medical students who choose the Ph.D. path?

Shevach: Some of them come to the NIH, not all. But again, they've been in one city, being trained for six to eight years doing their M.D.-Ph.D., so they're somewhat harder to move. They may move laboratories within a given city, but they're harder to move. Not all stay in research.

Williams: Who wants to leave San Francisco, right? [laughs]

Shevach: Well, that's right. Boston, you can leave. I left. But, you know, that's okay. It's cold.

Williams: So I'm curious about one thing here. Distinguish between the roles of the Laboratory of Clinical Investigation, which is where you started, I believe, and then moved over to the Lab of Immunology.

Shevach: Okay. So the Laboratory of Clinical Investigation was composed of about ten separate laboratories, but many of the senior people had clinical projects that involved admitting patients to the NIH Clinical Center. So I did my clinical training on the eleventh floor of the Clinical Center.

The Laboratory of Immunology, for the most part, first of all, although it has many—actually, many of the members still are M.D.s, of the senior people, not all, none of us have clinical responsibilities where we're actually taking care of patients within the Clinical Center. That doesn't mean people don't do research with material from humans, but active hands-on clinical medicine care, the Laboratory of Immunology doesn't do. We could. I mean, one could—I could have made my career—I elected not to. I decided that, I would say, it would be a distraction taking care of patients. So the competition with immunology is with Ph.D.s, and patient care involves a fairly big time commitment. Even with good clinical fellows within the NIH, I wasn't really interested in doing that, and I have to say I was more attracted to doing research in experimental inbred animals and asking questions in well-defined—even though I'm an M.D., it's much—well, it is easier to do experiments on experimental animals rather than the diverse human population.

Williams: So one of the challenges of this oral history project is to be able to keep scientists from delving too thoroughly into every stage in their career, and I keep making the point that you all have huge publication records, and so that's all been laid

out. So what I am interested in is in what accomplishments do you want to be, at this stage in your career, most remembered for?

Shevach: I'd have to say it would be not as editor of *The Journal of Immunology* [laughs], but for major advances I made over the years, some of which I've been recognized for, some which I probably haven't been recognized for appropriately.

Williams: Tell me what the—

Shevach: So there have been probably two major areas that—I've worked in multiple areas over the last forty years. The initial area I worked in was the discovery with Alan [S.] Rosenthal of what is now called MHC restriction in cellular interaction. We did these experiments actually in 1972. We published them in 1973. They involved the restriction between an antigen-presenting cell and a lymphocyte. It seemed surprising—we were surprised, actually, that the antigen-presenting cell and the lymphocyte had to come from the same strain of animals. You couldn't take antigen-presenting cell and non-lymphocyte, non-T cell, from one animal and mix them with the cells from another. This was discovered in the lab and was published and received lots and lots of interest in 1973.

Another group discovered, I have to say, the same thing in 1975. [Rolf M.] Zinkernagel and [Peter C.] Doherty, they discovered the restriction between the antigen-specific killer cell and its target cell in killing viral-infected cells. They happened to get the Nobel Prize in Immunology [Ed. Nobel Prize in Physiology or Medicine]. We didn't. [laughs] So a little bitterness there, I have to say, to be honest, and we did it first and other people even did it before us. So, okay, that's life.

The one great advantage of working at the NIH, at least in the laboratory where I worked, is we're not directed into any given specific area. So over the years, one might have said when the HIV epidemic came along that all immunologists should have gone into the HIV field, and even though Tony [Anthony S.] Fauci, the director of NIAID over all these years, was greatly interested in that area, there was no pressure put upon us to change what we were doing. In fact, NIH has allowed me to work in several different species, on all kinds of topics over the years.

And about twenty years ago, I had a sort of spark that I should do something different, and I was intrigued by a paper published, and I must say—in *The Journal of Immunology*—and ignored by everybody else, perhaps, except me, describing a new population of lymphocytes called regulatory T cells, or suppressor T cells sometimes, a paper by Shimon Sakaguchi. One thing I like to do is actually sit in my office and read papers and think about things, and the NIH allows me to do that. And I read his paper. I said, "Hmm. Sounds like this is something new and different. I wonder if he's right." I always wonder if somebody's right. That's one of my characters. I'm a critical character.

Everything seemed rather interesting, and the most important thing in that paper was the characterization of these cells involved the expression of a certain cell-surface antigen called CD25, which is the receptor for interleukin-2, this interleukin-2 alpha chain, and that was the key observation in the paper, that 10 percent of CE4 cells that expressed CD25 had this unique property of suppressing other cells.

So I looked at the data in the paper, and I said, “How did he identify this?” He used immunofluorescent staining, fluorescence sorting, and separation of cells, and he said 10 percent. That seemed like a very big number. So I said, “What antibody did he use?” And it turned out the antibody he used to identify CD25 positive cells was made some almost fifteen years earlier by Tom [Thomas] Maleck, who was a postdoctoral fellow with me in my laboratory, and published.

I went back and looked at my old paper, and we said 8 percent of the cells, normal cells, expressed CD25. Eight percent, 10 percent; pretty similar. We were actually not interested in what those cells did; we wanted to know why they had CD25. We took a different tack to the research. But the numbers seemed right.

So actually one of the few times in my career, I said, “Let’s just repeat the experiments in this paper in *The Journal of Immunology* and see if Sakaguchi is right.” And a new postdoctoral fellow entered my laboratory at that time. I said to her, Angela Thornton, “Repeat these experiments. We have all the reagents.” For other reasons, we were interested in the animal model Sakaguchi used. Everything repeated, and we went on to show that these cells had suppressor qualities. In vitro, we made a very simple in vitro assay rather than using in vivo experiments. Angela developed these technologies to do this, and we stuck with it.

So a lot of objection came up at the time because there was a general feeling that cells that mediated suppression were not a unique population of cells, lots of historical reasons from the seventies and eighties. A tremendous body of research was done on T-suppressor cells in immunology that was probably completely incorrect at that time. Happens. So there was some opposition to us even taking on these types of experiments, and it was one of the few times where actually the External Review Committee that reviewed my laboratory, just as we were starting these experiments, gave me a very bad review and said perhaps we shouldn’t be doing these kinds of things.

But at NIH, you can get away with things as long as ultimately you’re productive. So we stuck with it, in spite of the observation, and these cells are now, twenty years later, one of the hottest populations of cells in the immune system, a population that we would like to manipulate clinically to either enhance their activity in diseases where you need more suppression or get rid of them where suppression is dominant. For example, in tumors, these are very prominent cells

in tumor infiltrates, and clearly suppress the immune response, both in man and experimental animals to tumors.

Williams: So what do you want the layperson to know about your achievements? What is significant for —?

Shevach: For almost my entire career, I've worked in experimental animal models, ten years working with guinea pigs. That's what we did, these studies on restrictions of interactions between cells, and for most of my career inbred strains of mice. And there's always some argument, what are we going to learn from studying a little mouse that's applicable to human disease, and even some objections overall recently that animal models don't reflect what goes on in humans. Well, humans are obviously much more complicated. I have exactly the opposite view. I think, for example, even in these studies of regulatory T cells, almost everything we've learned from the animal model has now been validated in man. So the studies were done in the mouse. 1995, Dr. Sakaguchi published his first paper in *The Journal of Immunology*. It took six years, 2001, for the first paper characterizing these cells, exactly the same way he did, in human peripheral blood.

So I think for the layperson to support, has to have the understanding that to really explore in depth a disease situation or a population of cells that can be manipulated to control diseases, to develop new drugs. We need an extensive program in animal research, using inbred strains of animals, and not everything we're going to learn from the animal model is going to pertain to man or work in man. There's some, I would say, trivial differences, believe it or not, between man and experimental animals. It's harder to purify regulatory T cells from man than it is from a mouse, but that's trivial. Most of the major things that one observes in a mouse hold up. The major things hold up in studying the human immune response, and we'll learn lots of things and develop lots of drugs.

Williams: Throughout your career, to date, have there been major distractions that you've had to deal with, or have you been able to pursue things with pretty good focus?

Shevach: I have to say I've been very lucky. I've been able to pursue things as I want to do them, as intensely as I'd like to do them without anybody interfering.

Williams: And have you as a scientist experienced any major disappointments or been led down a path—or taking a road down a path that didn't lead anywhere?

Shevach: We've all had that. I've had rather minimal ones. One obviously gets—sometimes one becomes seduced by a novel experimental finding. Postdocs make these novel experimental findings on a daily basis, and it's my job to sort out what's real and what's what we call a red herring, I guess, but even I've been seduced by a red herring a couple of times. But I think it's important for scientists to be self-critical, and I'm probably one of the most self-critical scientists around. [laughs] I'm critical of other people's work, but I'm also self-

critical. But I followed a few things that didn't pan out completely and had to abandon them, nothing of a major league nature, but projects that people were involved in that basically were artifacts in the end, and I think we've all had that experience.

Williams: Let's turn to the AAI for a bit. You became a member in 1973. And what attracted you? What motivated that?

Shevach: Well, one had to be a member. [laughs] AAI, then and now, was a major organization speaking for immunologists, sponsoring an annual meeting, which was very important for the field, and sponsoring, again, one of the major journals, if not *the* major journal in the field of immunology. So it's like joining the union. That's how I regarded it. If I was a worker in the field, I should join the union. It's not mandatory. [laughs]

Williams: I ask because you said you had to, and no one was telling you you had to.

Shevach: Well, I mean, everybody realized that if you wanted to be—you didn't have to, but if you were going to be consumed—if you wanted to submit an abstract to the annual meeting, you had to be a member. You didn't have to be a member to publish in *The Journal of Immunology*, but I think one should have the feeling that if one's in the field, one should become a member and support the field, so I was very eager to become a member, actually.

Williams: Have you seen any major changes in the organization since you joined?

Shevach: Well, it's much bigger. The field has become much bigger. The AAI's dealt with that. I wouldn't say there have been major changes, no. I mean, there are more activities perhaps, more interest, which I haven't been involved with, in lobbying Congress, things that the NIH scientists are encouraged not to do or are forbidden from doing. So I haven't been involved in that part of the activities. But in general, no.

Williams: So what drew you to *The Journal of Immunology* and [unclear]?

Shevach: Why did I want to be an editor, yeah. So one thing I learned rather early on in science is an experiment hasn't been done unless the results have been published, okay? So I think most of us, at least at some point in our career, at least when I was younger, I loved to work with my own hands, I really liked to do experiments, I liked to do experiments more than writing papers, and I think that's characteristic of many of us at certain points in our career. But you could have done tons of experiments, and if you haven't written them up, they haven't been done. Even though the results are great, only you know them. And I think the integrity of that process is very important to me, that you have to be able to write up a paper, make sure the paper has peer review, and ultimately gets published. So I was interested in the process.

At the time, Joseph Feldman, who had been editor of *The JI* for fifteen years, was retiring. The Council of the AAI was willing to take on—Joe Feldman was a permanent—that’s all he did. He was a full-time editor. He had retired from his laboratory to Scripps, and I think really most of the fifteen years was the full-time editor of *The Journal of Immunology*. I didn’t want to abandon my labwork, and the Council of AAI proposed a different format, that the editor would not be a full-time editor, would still be involved heavily in running his own personal laboratory, and would have, perhaps, deputy editors who would assist him in editing the journal and would divide up the work, so to speak. And this was a concept that other journals had advocated over the years, particularly *The Journal of Clinical Investigation*, which moved its editorial office and editors from one city to another every five years, and they actually move their entire editorial office, including the editors, all their staff, every five years, is the process they always had. That technically is probably tough these days, but that’s the way they still do it.

Williams: Great for real estate agents. [laughs]

Shevach: Well, I don’t know how—yeah, I guess so.

All the scientific editors were in one place. Obviously, being at the NIH, this was attractive to me because I felt I could recruit others. I didn’t want to do this as a—I didn’t realize how big a job it was when I took it on, needless to say. But there were other very competent people I knew I could persuade to help me at the NIH, and I, before applying for the job, said, “Would you be willing to form a committee of editors, so to speak, that will assist in this process?” So that was a carrot.

I convinced the Council of AAI that the NIH would be a model place to actually try this out, and I think changing editors every five years is a very good thing for a journal. There are some journals that have the same editors forever. That sort of breeds familiarity, it doesn’t bring new life into a journal, so five years was a reasonable term that I was willing to put up with. I had been a—what did they call it—assistant editor [Ed. associate editor], I’d been a section editor of *The Journal of Immunology*, I was interested in building up the procedure of *The Journal of Immunology*. I felt I could do that and I felt that I could handle not only an adequate and a rapid but a fair review process. I knew the field.

The field was much simpler then, I have to say, 1987, probably think that one could have a broad knowledge of the entire field of immunology. I wouldn’t say that today. Absolutely not. I could say in 1987 that I knew all the major workers in the field, so I could pick subeditors and deputy editors and section editors, whatever we called them. I knew who was doing good work. I’m not sure you can, again, make those statements today. The field is much, much broader than it was and much more difficult now.

Williams: So when you were a section editor, what section were you—

Shevach: [laughs] Curiously, for reasons that were never clear to me, when Joe [Joseph D.] Feldman asked me to do this, it was the section of Clinical Immunology, even though I was probably the least clinical immunologist there. I did a variety of things, but for about a four-year period, I handled clinically oriented papers.

Williams: So you came in as editor-in-chief with an agenda?

Shevach: Yes, I was asked to come in with an agenda. I mean, the Council of the AAI, the office of the Association, decided that they wanted and willing to have a new process for running the journal, which has been maintained since then and done slightly differently, I have to say. So I thought it was a terrific idea, and they thought it was a great idea that all the deputy editors come from the NIH. We had one deputy editor not from the NIH, and I learned that very early on—that was very important—that one of the deputy editors would handle all papers from the NIH. So that was a key thing that I learned very early. So approximately 10 percent of the papers published, at least in the '80s, in *The Journal of Immunology* came from the NIH. I don't know what it is today, but in the '80s, right before I took over the journal, I actually looked, and 10 percent of the papers were coming from the NIH. So that's a big number.

And I didn't want to be stared down by people in the elevator coming up to my office saying, "What happened to my paper? How come my paper got rejected?" and things like that. So I completely removed all papers from the NIH from anything—I didn't see them at all. They were all handled by an outside editor, Dr. Frank Fitch, who ultimately became editor himself at the University of Chicago at the time. Then I could breathe a sigh of relief, just say I had nothing to do with it, nothing to do with anybody I met in the cafeteria or in the halls of the NIH. But all the other editors were at the NIH, and that actually worked incredibly smoothly, I thought. But like everything else, other people didn't think it worked that way. So when papers get rejected, one of the feelings was, "These people at the NIH, they're just too tough. They don't care about anybody else out in the field. They don't have to apply for grants like we do, and they're rejecting our papers."

And at one point while I was editor, the Council of the AAI suggested to me that I change things and bring in outside editors from other places, and I stuck to my guns, I have to say. I didn't want to do that. I pointed out to the Council that the acceptance percentage was exactly the same when I was editor as Joe Feldman had it for the previous fifteen years. I hadn't changed. I wasn't being tougher. We were publishing 40 percent of the papers that were submitted. That was maintained during my five years as editor. We never changed that. But there was a sort of bad feeling about everybody being at the NIH, and sort of the editors being exempt from having to compete for research grants, and we could sort of be

tougher, but we weren't. I think the editors were very fair and pretty objective in what they did. I was happy with the process. Not everybody else was. I took a lot of flak, I have to say, at some point.

Williams: So you maintained that?

Shevach: I maintained that. It was tough enough establishing the system where five or six people handle the editorial process. One of the original suggestions was that the editors meet once a week to jointly discuss papers together. This is how some journals had worked. *The Journal of Clinical Investigation*, I believe, works that way, that the editors would sit down together and go over things. I thought about that. There were just too many papers. There was no way. It would have taken a day a week or more for everybody to do that. It couldn't be done in two hours or something like that once a week. So all my deputy editors worked independently. I monitored what they did. I didn't want anybody accepting all papers and anybody rejecting all papers. That was part of my job, and to gently say—you know. I actually did very little of that. Most of them sort of subliminally had a 40 percent acceptance rate in the back of their heads, and we didn't manipulate that. That's the way it worked out over a period of five years, and the deputy editors all had significant expertise in their own subspecialty areas and they were picked for that purpose.

Williams: Are there changes that you brought to the journal?

Shevach: That was the major one and that was a big change, and I wanted to make other changes. So one of the problems with—I don't want to say "problems." *The Journal of Immunology* was obviously regarded as a place where you can get things published, a good, solid publication, but it's not a high-impact journal, and still regarded that way. Certain people regard it as a high-impact, but there are more prestigious places to publish.

The chief competitor was *The Journal of Experimental Medicine* in those days, and they had a very unique process of reviewing papers. You submitted a paper, and if your paper was not accepted, a month later you somehow got a form letter, without any critique, saying your paper was reviewed by the editors and didn't make the priority, was not enough interest, and you got it back with no outside critique. And they didn't send papers out, for the most part, papers from the outside for outside reviews.

That was a Rockefeller University journal; still is. Now they send papers out for outside reviews, but in those days they didn't. And the rumor had it—never validated, but I'll make it public [laughs]—that when the paper came in, someone looked at it, and if you weren't a member of the club, it was put in a drawer in someone's desk, and a month later it was taken out and sent back to you with a form letter.

So I didn't like that idea. I wanted things to be fair, and I felt we could be a bit tougher, maybe lower the acceptance rate. I learned quickly that probably was not a good idea. *The Journal of Immunology* is unique and serves the whole membership of the AAI, and the AAI, although it's immunology, is a pretty diverse group of scientists, covering all kinds of things and all kinds of species, people were publishing important papers in areas that maybe were not so high impact, yet the service to the AAI that the journal was to provide was to provide a forum for publication for all good papers in all aspects of immunology, even those that weren't perhaps of gigantic general interest or importance, but they were quality science. And I learned pretty quickly that that's what I had to do, and I couldn't become a very selective journal and say I was going to avoid certain areas of immunology that aren't so hot. I learned my lesson very quickly that the journal was sort of a diverse group of scientists, and I had to do that as well. Quality was important, but I couldn't be selective in subareas of immunology to choose for publication.

Williams: You must have a very definite view of what makes a good editor.

Shevach: I have views the way editors should operate. One thing that I felt about the editorial process was that it should be impersonal. So I immediately—and my predecessor, Joe Feldman, if you didn't like a decision on your paper, you could call him up. So I couldn't have that in my life. I quickly learned that. I said, "You can write me a letter and you can complain, and I'll deal with the complaints or I'll direct them to the person that handled the paper." But I think it should be an impersonal, not a phone call, kind of operation. Now it's emails. That's also impersonal. But people still get on the phone and demand to speak to the editor.

So one humorous story is, in those days, *The Journal of Immunology* didn't publish its phone number in the journal, okay? So they knew I was the editor. People knew I was the editor, so they called Information and got my home phone number, and they would call my home, usually in some—it was frequently a foreign scientist who did this, and leave a message or get to talk to my wife, who wasn't overly enthusiastic that I was the editor of the journal in the beginning. So in some rapid conversation, "This is Dr. so-and-so from so-and-so and from so-and-so. I want to speak to Dr. Shevach."

Then my wife would say, "This is his *home* phone number. Please call *The Journal*." And we finally gave my wife the phone number.

The journal, I think in the second year I was editor, published the phone number for the journal office here at the FASEB [Federation of American Societies for Experimental Biology] complex rather prominently that that was the number to call. But I discouraged communication. I think one can complain to the editor, and my usual way of handling that was, "Well, you feel you've been treated unfairly. Suggest to me appropriate referees for your paper, and I'll get new

referees. I'll do my best to include a couple of your suggestions as referees.” Most authors were quite happy with that solution. They generally suggested what I regarded as pretty fair referees for their paper. I frequently used those referees, and sometimes the paper was reconsidered and they were right. They were perhaps treated unfairly or inadequately during the first review.

Things have changed now. I mean, now—well, a couple of major changes. The other thing I think about editing a journal is I think a science journal should be edited by active scientists who have their own laboratories and are actively engaged in science. So some of the major high-impact journals these days are edited by what's called professional editors, professional scientific editors. This is largely scientists who have decided to go into scientific editing as a career, but they either have just completed postdoctoral fellowships or decided that they didn't want to be active laboratory scientists themselves, and they're functioning as editors. I don't think that's the proper way a scientific journal should be run. I think a journal should be run much like *The JI* runs now, edited by scientists, by actively practicing scientists, with obviously some professional editorial help and things like that, but the people making decisions on papers and managing the journal should be scientists, active scientists themselves, and, unfortunately, the commercial publishers have discouraged that.

So, for example, *The Journal of Immunity* [Ed. *Immunity*], one of the high-impact journals today, was edited in the fashion I described to you that I picked for *The JI*. They had a group of editors at one institution or multiple institutions within one city for a period of five years, and that's how the journal started and that's how the journal functioned. And then the publisher said, “No, we want it edited by professional editors,” and hired professional editors who don't have laboratories to run the journal, and many of the publications today are run that way. I'm not sure that's the best way to do business, although we have little say in that.

Williams: Over your five years as editor, did you have any legal crises or big blowups?

Shevach: Not really. The issue of fraud always comes up. A couple of situations came up with certain rules that people violated. For example, the general policy of *The Journal of Immunology* and almost every other journal is that when one submits a paper, you only submit it to one journal at the same time, at one time. So my first experience was I get a call from the editor of *The American Journal of Pathology*, saying that we sent a paper to a referee and *The American Journal of Pathology* got a paper and sent it to the same referee, who notified both editorial offices. So that's a no-no. So I had to handle things like that. And we sort of banned these authors from publishing in *The Journal of Immunology* for a year. I contacted the vice president for research for their university; didn't really seem interested in punishing them. It wasn't regarded as a major sin. I think it should be. It shouldn't be done.

And there's obviously people saying—you know, plagiarism comes up as an issue. Not many times. Sometimes people just inadvertently or accidentally on purpose, whatever you may say, copied parts of the discussion of somebody else's paper verbatim. This is usually picked up by people rather quickly and we're notified. But nothing of a major fraud kind of episode. Over the years, I was lucky.

Williams: What about any major developments in the field that occurred and was published in *The JI* during your tenure?

Shevach: Well, one of the most interesting—so certain areas were ignored by certain journals. Probably the one area that I remember where the most of the original publications came to *The JI* was in the area of one of the cytokines, interleukin-12, which is a major cytokine influencing Th1 cell effector T cell development, and those papers on interleukin-12 were all published, many of them published in *The Journal of Immunology* for the first time, and nowhere else. That was it. I mean, other papers perhaps went elsewhere. It's hard to say in the beginning that this is a major—it may take five years to become a major observation, so you don't know.

I was not the editor when Dr. Sakaguchi's paper was published in *The Journal of Immunology*, and that has turned out to be a major development. But I haven't gone back and done—one does this by looking at citations, high-impact papers in *The Journal of Immunology*, and trying to fit that in with developments in the field. I haven't gone back and done that in the period when I was editor.

Williams: While you were editor, the journal celebrated its seventy-fifth anniversary, and I wondered how that was celebrated.

Shevach: To be honest, I don't remember. [laughs]

Williams: *Really?*

Shevach: My picture was on the cover, I remember that, along with the other editors at the time, but I'm not sure we had a big celebration.

Williams: That's interesting. [laughs]

It strikes me that there's been a proliferation of scientific journals and research notes and so on and so forth over the years. What effect has that had on the field and how do you handle it?

Shevach: Well, you can't handle it because it's all done by some of these online, completely online publications, so-called open-access publications, you know, which everybody can access for free, but they charge the author \$3,000 to publish a paper, so the author is paying for the online publication. I don't think one can

control it, and the field clearly hasn't controlled it. I get letters every day inviting me to become a member of the editorial board for some unknown journal, edited by some—if they have a name of an editor, I don't know the name, located in places like Rehoboth Beach, Delaware, so I'm never quite certain what a scientific journal is doing there.

It hasn't helped the field, but, of course, people search the literature very differently now. People in my lab either use *PubMed* or *Web of Science*, or, more often, they use Google to search the scientific literature. They don't pick up a journal. They have a specific question they want answered, and Google is not selective; it picks up papers with the right keyword in them, no matter where they're published. So the reader has to beware, has to be cautious, more than anybody, more than the editor of the journal in terms of is this good work. And you have to make up your mind, and we have lots of discussions about that in the lab. Is this paper published in some unknown place really a solid piece of work? It can be or it may not be. But it's buyer beware, in a sense. Reader beware.

Williams: What about clinicians? How do they access or get a clear picture of what's developing in the field?

Shevach: In, say, basic immunology, I don't think they do, other than areas that eventually become of clinical interest and where papers get published in more clinically oriented journals. But most immunology is still—well, I'm not going to say—I think monoclonal antibodies, the clinical use of monoclonal antibodies and their usefulness in a variety of diseases has changed the view, has had to change the view of clinicians in terms of immunology as a science and contributing to particular areas of rheumatology and dermatology and immuno-oncology. So clinicians have to be interested in some of the basic science that's going on. Even the most common ad on television for drugs is for the drug Humira, which is a monoclonal antibody against TNF [tumor necrosis factor]. That's advertised several times nightly, so even the general public may soon figure out what it actually does, and it does work.

Williams: While you were editor, *The JI* was semimonthly?

Shevach: Yeah. We actually—I think it became that before I was editor. We kept it at that.

Williams: And I was amazed. I looked, I Googled, and in 2014 there were over 12,000 pages in the journal for the year. That seems like an astounding amount of information.

Shevach: It is, and those are the papers that got published. Remember, 60 percent didn't make it in. So it's a very big field. It covers a broad area. And it's not just—I don't know what percentage of authors come from the U.S. now, but it's always been a high percentage—*The Journal of Immunology*, although it's the official journal, The American Association of Immunologists has a healthy contribution,

probably 25 to 30 percent of the papers, I think, when I was editor, came from outside the U.S. So it's an important worldwide publication with a general reputation for excellence. So scientists from all over the world, some of whom are members of the AAI as well, are publishing papers, so it's a major resource. And if one looks at not necessarily what's called impact factor, which is citations to papers published in the previous two years, but to total citations to papers previously published in *The JI* since 1916, for that matter, when it was founded, it's a huge resource for the field. So people are constantly looking years later at older papers and quoting papers and reading what was done. So it's a big field. It always has been.

One of the first things that impressed me when I became editor was the volume. In 1987, the journal had a fax machine. Most academic departments didn't have fax machines, no one had the Internet, and every paper, the journal required you to submit four copies, hardcopies of your paper and hardcopies and glossy photos of your figures. So the first day I walked into the journal office here on campus, I was amazed at the quantity of paper that was sitting there facing me, literally piles and piles of paper which I had to handle on a daily basis, I have to say. [laughs] So the field is big and the journal is still being selective in what it publishes, so it's serving the community of immunologists worldwide, and that's why it's 12,000 pages and it was when I was editor as well.

Williams: Where does the bottom line fit into your editorship?

Shevach: Meaning?

Williams: Well, finances.

Shevach: Finances. So, luckily, the director, the executive director of the American Association of Immunologists at the time, the late Dr. Joe [Joseph F.] Saunders, and I had a very sort of interesting relationship. He managed the finances. I managed the editing. But at a certain point, you have to pay for every page you publish. That's how the system works. So you do that by subscriptions, primarily by subscriptions and page charges to authors, and there's a limit to that if you're going to balance out, if you're going to be in the black. And I didn't want to make a profit, the Association didn't want to make a profit at the time, and, luckily, that balanced out to about 12,000 pages a year. That's what we took in.

The AAI has a policy where every member must subscribe to the journal, but I think almost every medical or research library in the world subscribes to *The Journal of Immunology*, so people are paying healthy subscription fees. We charge authors or the journal charged authors at that time page charges, sometimes extra charges for color figures and things like that, and that balanced out. But if I was going to accept 100 percent of the papers that came in, that wouldn't have balanced out. [laughs] That was clear and that was made clear to me. And, luckily, over the years, occasionally, you know, one month we

published more papers than another, but for some reason—and I don't know how and it wasn't my manipulation—it balanced out in the end.

Williams: While you were editor-in-chief—and obviously there were great demands on your time—how did that affect your scientific career?

Shevach: I didn't think it did, but it did. [laughs] So it's very hard to evaluate. I took the journal for one other reason that we didn't talk about, and that is I wanted—my position at the NIH allows me to manage a modest-sized research group of about ten to fifteen people, but really I had no other—I think, much to my benefit, no other administrative responsibilities. So one of the sort of minor to major reasons I took over as editor of the journal is I wanted the management experience of managing something else, and, in a sense, managing a modest-sized business. The budget of the journal even then was in the millions of dollars. And I also wanted to be involved in hiring people, journal staff, dealing with journal staff on a daily basis.

So I managed my time. I was very lucky over the years in terms of people who worked here in the office handling the fundamentals of paper processing and things like that, and that worked very well. I was very lucky. Joe Saunders helped me a great deal in hiring editorial office managers who really worked very hard and helped me out. The journal staff liked to see me every day, so I came over here for an hour every day. But the NIH is only ten minutes away, so I was here from two to three every day, for the most part, one hour in the office and a little bit of commuting. And they liked that, and I guess—and I'm not sure I really had to do it, but I did it every day, for the most part.

Then I sort of worked at night. Theoretically, I was forbidden, because I was being paid by the journal to be editor, forbidden from using any of my working hours at the NIH to handle journal business. I usually did it at night, probably another hour or two at night, sometimes more. But the journal never sleeps. That's one of the problems. And it usually doesn't sleep during periods when you'd want it to sleep. So the biggest input of papers to *The JI* are two times of the year, right now. Between Thanksgiving and Christmas, more papers than ever come into the journal because people want to take a vacation at Christmastime.

The second period of time when the journal is busy, at least when I was editor, was July and August, summertime when everybody's on vacation. And if you think about it, it's only logical. People would finish their degrees. The academic year ends in June. One thing you want to do before you go on vacation is to finish your paper and submit it to the journal. So, actually, the journal was busiest at the times when I wanted to take a vacation as well, and I couldn't. I did, but it was tough.

So I suspect my science slumped some. It's very hard to know. I mean, your science always has its ups and downs. I've been in this for forty years. I've

obviously had my ups and downs during my career between very productive periods of time, very exciting periods, and less productive periods. That's the life of most scientists.

Williams: In '92 you were given the AAI Distinguished Service Award. Was that in recognition for your editorship?

Shevach: Yes, totally.

Williams: Was that a good thing to have happened?

Shevach: It's nice. [laughs] I felt I had done a good job, you know, in the sense I devoted lots and lots of time to the journal and I took it very seriously over the years.

Williams: How satisfying has your career been to you so far? We're moving away from the editorship now, except that's part of your career.

Shevach: Very. So I'm now seventy-two. I have no interest in retiring. My wife told me I couldn't retire, and as long as the work is going well, I probably won't retire if my health holds up. So it's been very satisfying. I really enjoy what I'm doing and I've enjoyed in the past what I'm doing, I enjoy it now. So I would say, you know, would I pick anything—you know, if the Vietnam War hadn't been going on in 1967, where would I be now? That's sort of the question you have to ask yourself. That's hard for me to answer. I'm not sure what I would have done if the pressure or outside influence hadn't compelled me to come to the NIH to at least develop my interests as a scientist. Would I have gone on to clinical practice, for example, taken a fellowship in rheumatology or internal medicine followed by a rheumatology fellowship and become a practicing rheumatologist? I suspect not, but it's very hard, very hard to think back on how you'd envision your life happening. So outside influences were very great on mine, and for the benefit. I mean, I thank the Vietnam War for sending me to the NIH. What can I say? [laughs]

Williams: Are you sanguine about the future of immunology at this point?

Shevach: Yes. I think at least from all the TV ads, immunology's really influencing clinical medicine in the ways that I would like to see it influence clinical medicine in terms of novel immunotherapeutic approaches. I mean, cancer is now an immunologic disease. Surely when I was doing my clinical training, it was not, and the immuno-cancer people have now proven that it is. So the applications of findings in the laboratory to many clinical situations are growing. For example, even with regulatory T cells that we work on in mice, we'd like to manipulate them in humans, and there's ongoing experiments in my lab and other labs where people are seriously thinking about how to do that. And I think advances will be made in all these fields.

So immunology itself has blossomed over the last twenty years, really. There were some disappointments. Initially, monoclonal antibodies were thought to be a tremendous advance; they were. But the clinical application, which was said to happen very quickly—remember, monoclonal antibodies were discovered in 1975. It wasn't until 1992 where people began to use monoclonal antibodies for the treatment of rheumatoid arthritis. So it's a long, long lag period between a tremendous basic advance in making a reagent, a drug, basically, or drugs or diagnostics, before it was applied to the clinic.

Now things are moving much faster, and major pharma is more interested in immunology and supporting immunology. Some pharmaceutical companies are only interested in small molecules. For many, many years now, biologics, the major research interest in almost every big pharma company. So immunology's had a big influence, will continue to have a big influence. And, unfortunately, we're producing very expensive drugs. That's the only other problem, who's going to pay for them. [laughs]

Williams: Do you recommend a scientific life to students and trainees that come under your influence?

Shevach: Yes, to people who have made it to ask me that. So you're asking the wrong question. The right question would be, what about my own children? Did I influence them at all?

Williams: That's my next question. [laughs]

Shevach: Okay, that's your next question. Okay, that's good. So let me answer that one first. So they had the opportunity to take biology in high school. I didn't. But they weren't terribly interested in biology, even though they'd seen what I had done. They'd come up to the lab and work in the lab with me occasionally on the weekends in my younger days when I was working in the lab and coming in on the weekends. I didn't push them or try to influence them.

So they go off to college and what do they major in? One majors in computer and electrical engineering, and the other majors in physics. So I can't complain. They're studying tough things. They didn't even want to think about going to medical school or going into biologic science. And today both are probably businessmen. I'm not even sure what they do today. They're in the computer industry in some careers.

So I had a chance to sometimes advise some of their peers while they were in high school. So one of the big questions that I was asked, should people become M.D.s or become M.D.-Ph.D.s? And I'm never certain how to answer that question. Some of my colleagues who are M.D.-Ph.D.s would say that's the best choice for someone wanting to go into a research career these days. I didn't do that. I was just a pure M.D. But life was simpler then.

So I still advise people to go into medical school and to consider research careers, but that's very tough. The people in my lab—so life in the lab has also changed. So when I started out as a tenured scientist at the NIH, almost every fellow I trained had the goal of becoming an independent scientist and running their own laboratory. That is probably rare today or down to, I'd say, 25 percent. People are realistic as well. Do they want to go off to a university, apply for grants, run their own labs? That was the major goal in the seventies, eighties, maybe early nineties. Not true today. Everybody's thinking of different things. The last group of people who left my lab have all gone to industry, and they seem quite happy. They're doing research careers in big- to middle-sized to small-sized pharma, even, to startups. So Ph.D.s in immunology are quite willing to do that.

Williams: That movement, does that have effect on the hardcore academic scientific community?

Shevach: Not really. I don't think so. No, there are plenty of people. I mean, we need more funding for the hardcore scientific community, I think. The budget for the NIH hasn't grown in ten years now, so there's a restriction on people becoming members at —, but I think people are still interested in doing that, but it's a smaller percentage.

And the other issue is are we training too many people. We're still training lots and lots of people and probably training too many people. That's been argued. I'm not sure. The answer may be, yes, what are all these people going to do. Luckily, most of the people who've worked with me have stuck with it. A few of the M.D.s have gone into practice. I recently had a very good postdoctoral fellow who decided he wanted to become a high school science teacher, and that's what he's doing, at least after he finished up in my lab. We'll see if he sticks with it. And I've had people go on to do other things. They're scientific advisors to the U.N. and things like that. So there are other opportunities where they want well-trained, critically thinking scientists with lots of lab experience and publications, even though they're not going to do that ever again, they're going to do other things. So there's room for lots of people in the field with lots of expertise in doing things.

But is the future bright? I'm not sure. I'm not sure if I was training today as a physician what I would end up doing. I'm not sure I would take the same path.

Williams: How did you balance family life and your professional life?

Shevach: Oh, always hard. You should interview my wife. She'd give you—I'd say I did fine. [laughs] My wife would say, "He's never around." And she'll see this video, so it's okay. [laughs]

So that's a problem. I'm an organized person. I don't advocate—I don't advise my postdocs to work twenty-four hours a day. And during my career I occasionally worked late at night, but very, very rarely, I must say. I like people who come in in the morning, know what they're going to do that day, and get it done in a reasonable period of time. I don't like people who come in at eleven o'clock in the morning, and, well, maybe they'll stay till midnight, but never really planned out what they wanted to do. So that's my personality. So I am organized, I always have been, and I've always operated that way. But your family suffers. I mean, it's not easy having a—it's not a nine-to-five job, that's for certain.

Williams: But your life, your own personal life, couldn't be just entirely offices and laboratories. So what have you done over the years to have fun, recreation, outside interests?

Shevach: Outside interests are, believe it or not, cooking, music, traveling, things like that. So I'm an avid classical music devotee. Over the past twenty years, twenty-five years, have become interested in opera, so if I'm attending a scientific meeting in some city where I haven't seen the opera, I go see the opera. No matter where it is, I try to do that. I like to read. I have no real specific hobbies, I'd say.

Williams: Anything else you want to add to this that I haven't covered?

Shevach: I think we've covered most of the things. I think editing a journal should be a part of everybody's scientific career, I think. If you're a consumer—and I'm still a consumer; I just had a paper accepted by *The JI* this week, which is good—one should be willing to contribute one's time and energy at a certain level, not just being a referee, but perhaps being a member of an editorial board, not necessarily the editor-in-chief, but taking a role in becoming involved in really what's part of—science doesn't exist, as I said, without publications, and it's not going to happen without people supervising that, and I think scientists make the best—not professional editors, but scientists make the best people to take that job on. And I'm still a member of the editorial board of a wide variety of journals, and I review lots of papers today.

Williams: Well, thank you very much for this interview.

Shevach: Thank you.

[End of interview]