

Stephen Katz Oral History Interview
National Institutes of Health
Conducted by: Kate Hallgren
June 18, 2018

KH: Great. So good morning.

SK: Morning.

KH: My name is Kate Hallgren and I'm a senior historian at History Associates, in Rockville, Maryland. So today, June 18, 2018 I'm interviewing Dr. Stephen Katz at the National Institutes of Health in Bethesda. Before we begin, I'd like to read a short introduction. Dr. Katz has served the public through his research and leadership roles at NIH since 1974. He was first hired as a senior investigator at the National Cancer Institute. By 1980, he was serving as Chief of the Dermatology Branch of NCI, a position he held until 2001. In 1995, he was appointed Director of the National Institute of Arthritis and Musculoskeletal and Skin Diseases, a position he maintains today. Dr. Katz, I am so happy that you can be here.

SK: Thank you.

KH: So I understand that you were born in New York.

SK: I was.

KH: Do you have any memories of the city?

SK: Oh, I do. I lived there for close to eleven years. I have good memories of New York City. I went to school there. I went to a parochial school. It's called a yeshiva. We studied Hebrew half the day and English half the day. English, history, math, etc. Then my family moved to Washington, D.C., and then from there, three years later, we moved to Bethesda. So I actually grew up in Bethesda. I went to high school at Bethesda-Chevy Chase High School.

KH: Very close.

SK: Very close.

KH: Do you remember enjoying science classes when you were younger?

SK: No. Not at all. I had no interest in science. I was a very good mathematician, I think, but I had no proclivity to science.

KH: Were there any teachers who had an influence on you?

SK: No. None. I had a very, I would say, negative experience in both junior high school and high school. I graduated from high school with a general diploma, which was just between an academic diploma and a vocational diploma because I didn't have enough credits to graduate with an academic diploma. My major course was shop, either metal shop or wood shop. I had to repeat some classes in high school. I was a rather ignominious student.

KH: Did you enjoy working with your hands?

SK: Yes. Yes. I enjoyed seeing the product.

KH: I've read that you wanted to join the Coast Guard.

SK: Correct. During the last year of high school, friends of mine had decided, well, let's go to the Coast Guard for six months rather than going into the Army for two years. The three of us signed up for the Coast Guard. The only problem I had was that I was only seventeen-and-a-half. When we went to the Treasury Building downtown, we had to sign up for the Coast Guard, and my father said that he didn't care what I did, but he's not signing me up for the Coast Guard.

My other alternative was to go to college and in those days, it was easy to get into the University of Maryland. You have to realize that I had a brother, unfortunately, who recently died, who was at the other end of the academic spectrum from me, and he was sixteen when he graduated from high school, nineteen when he was in medical school. He was three-and-a-half years older than I am, but I think about seven years ahead of me in school, so I had a classic sibling rivalry in retrospect, and he was my ultimate hero.

KH: So when you went to the University of Maryland, you had a little bit of your brother on your mind.

SK: I knew that if I didn't do at least acceptably, I would end up going into the Army. At that time, the Vietnam conflict was starting, and so fear . . . I'd either have to go into the Army or I'd have to go out to work. I did pretty well in college and I finally read a book and I found that I enjoyed learning.

KH: So you turned it around?

SK: I turned it around. Yes. I was meant to be an optometrist. My father, my uncle, my cousins, my great uncles, were all optometrists, so I was destined to be an optometrist. That's why I took physics early on in my college career, but my last year at college, when I was ready to apply to optometry schools, my brother said to me, he said it in words other than this. He said, "Don't be a jerk. If you want to examine eyes, go to medical school. Don't go to optometry school." Disappointed my father for a little while because my father and I were very close, but it was a good suggestion on his part.

KH: Did you have any additional courses to complete once you decided to go to medical school or did you have all of the prerequisites?

SK: No. It happened that I had all the prerequisites because the prerequisites for optometry school are pretty much the same as medical school. I think I didn't have calculus, which was something that I didn't really know that I needed, but for medical school, I had all the biology and physics that I needed so I could apply to medical school.

KH: Did you enjoy them when you were taking them in college?

SK: Yes. Yeah. I enjoyed those science classes. I was not passionate about them, but I enjoyed them.

KH: Do you remember laboratory experiences in college?

SK: I do. Again, I enjoyed them. I was not passionate about them. My passion started in medical school.

KH: Okay. I want to ask you about that next, as you can imagine. So, you went on to Tulane.

SK: Right.

KH: What was that experience like?

SK: It was a great experience. I was on the upslope of learning and I devoured it. I loved learning new things. I thought biology of the human system was fantastic and I just couldn't get enough. So as a medical student, I really, really enjoyed learning new things and I was totally engaged. I'll say as an aside, I gave the commencement speech for my medical school this year, fifty-two years after graduation, just about two or three weeks ago. I hope that I transmitted to the students, I think I did, what Tulane medical school did for me in terms of opening up the world as a really small place to live and as people have the same issues all over the world and I had lots of opportunities at Tulane that have lasted me a lifetime.

KH: Like what?

SK: Like in 1965, spending a long summer in Kampala, Uganda through the auspices of Tulane, where I was able to do research. It was my real first exposure to any type of research. In fact, I thought to be a pediatric nutritionist after I finished that summer but I was not really prepared to walk the walk or talk the talk. But I did like the idea of research. It just sort of occurred to me that that is interesting. I worked with really interesting people and it was the first time outside the country and I got to meet lots and lots of people. I had an interpreter who helped me do the studies on the effect of urbanization on marginal malnutrition. That was, I think, my first published paper in the *Journal of Tropical Medicine and Hygiene* and it was just a wonderful experience.

There was a woman named Dr. Grace Goldsmith, who set the whole thing up. She was not well-known in our university, but she was worldwide known, very well known for vitamin B deficiency. None of us knew her, but I was referred to her because I really wanted to work in England, and when I met her she

said “Well, I don’t know anybody in England, but I know lots of people in Kampala in Uganda,” and it was great. It gave me quite an experience.

My father didn’t like the idea because he thought I might be eaten by the cannibals, but actually when I was in Uganda, I made many friends who were also from Kenya, and when I sent him home pictures of a synagogue in Kenya, then he knew everything was civilized in East Africa. It was a great time. East Africa had just been liberated from the British, so Tanzania, Kenya, and Uganda had really a triumvirate of leadership. It was a great experience. Made the world a small place for me.

KH: That sounds very exciting. What about that research excited you the most?

SK: Oh, I think learning new things. For me, it was a great thing. I think that learning new things, it turned me on. It didn’t make me into a researcher, but it did give me that exposure that when you learn something that nobody else knows, it’s exciting. Is that what spurred me onto a research career? Not really, but it certainly spurred me on to my career as to all of the international things that I’ve been involved with.

KH: I want to get back to that, but I also wanted to ask you about going on after Tulane to Los Angeles for your one-year internship there, as I understand it.

SK: Correct. I thought to go into either ENT or dermatology. I was heavily influenced by my brother who was an internist first and then during his internal medicine residency decided to become a dermatologist, which I thought was basically laughable. I thought it was laughable because I didn’t know that people went to medical school to be dermatologists. I guess in retrospect, I’m wrong. I decided to do a medical internship and all the places that I applied to had medical internships.

Most internships were rotating internships, that is, they did a lot of surgery as well as internal medicine. I just wanted to do internal medicine, so I decided to go to LA County Hospital. They accepted me. There were about 180 interns total. Only twelve of us were medical interns, and that was also a great experience. It was a very active experience. In fact, if I didn’t have the Army facing me, all male physicians had to go into the Army in those days, I probably would have gone into general practice for a little while because I thought I was at the top of my game as an internist or as a general practitioner. But I had signed up for what’s called the Berry Plan, which was an agreement between the student or the doctor and the armed forces that you would go in after either one year of residency or three years of residency. I preferred three years of residency, and as a consequence, I did the Berry Plan after three years of residency.

Right, so I decided that I would rather do a full residency and then go into the Army, so the Berry Plan enabled you to make an agreement with the armed forces, not necessarily the Army. It could be the Navy, the Air Force, but the armed forces, that you would go in after three years. You were basically assigned to go in after three years, so I knew that after three years of residency, I would go into the Army for two years. I didn’t know where I was going, but I knew I was going for two years.

KH: So then you went on to Miami to a dermatology residency?

SK: Correct. I went to the University of Miami as a resident. Nowadays it's much harder to get a residency program, although I was not a bad medical student. Actually, I did quite well in medical school, but I went to the University of Miami because that was one of the great training programs in the country. Miami, Oregon, Harvard. Those were the three great dermatology residency programs. My brother had been at Miami and I had visited him on many occasions, and I became a dermatologist enthusiast because of what I actually saw. My preconceived notions were nullified by my exposure to my brother and his residency program. So when I was looking for a residency program, I looked at three or four places and something happened that never happens now. The head of the residency program met me on Friday.

I was vacationing in Miami during my internship and the head of the program, Dr. Harvey Blank, met me on Friday, interviewed me for a short while. He knew me because he knew that I visited my brother and I made rounds with him on several occasions. He said, "Well, Steve, we'd really like to have you here. Let me know by Monday if you want to come." Now residency programs in dermatology are really hard, sought-after nowadays, but in those days, it was a little bit easier, and of course, I did have the connection with my brother, so they had Katzs for six years. The day he left the residency program and the day he went into the Army was the day that I started my residency program. In fact, he left a car for me there.

KH: That's great. I'm sure that was very convenient moving to Miami.

SK: Yes. Very convenient, also convenient that my soon-to-be-wife's family was from Miami. That's not why I chose that residency program, but it just so happened that she was finishing her master's degree at Stanford and was going to University of Miami because of me to do her Ph.D. in Spanish literature.

KH: That's fantastic. That sounds fantastic. May I dig down? What about your brother's experience there appealed to you? You said it was a change for you?

SK: Oh, it was intellectually very stimulating. It was a tremendous variety of things that you did as a dermatologist and you did clinical medicine. You could really be an expert in what you were doing. You also did microbiology. You did histopathology. You did some surgery. That all attracted me to dermatology, and of course at the time, I never thought that I would do investigative dermatology. I thought I would be a real doctor and go into practice one day, perhaps with him. That really was my motivation, but it was a highly academic residency program. It turns out all residents were really expected to do some research project at the University of Miami.

KH: I was wondering if that was hard to balance, your research with your other training, clinical roles there.

SK: It was. I worked hard, but there were some things that were happening in dermatology nationally that were discovered in the area of immunology and it actually intrigued me, so when I started my project, it was really in the immunology of skin. It was a relatively new field and there was somebody from

Boston who came to Miami as an adjunct professor who was very helpful to me. His name was Theo Inderbitzen, the head of the Dermatology program at Miami, Harvey Blank, was also a very encouraging person. High standards, but very encouraging, so whatever field you went into, he would encourage what you did. So I worked long hours, but I loved my residency program, my co-residents, and my project, actually.

KH: What was your project?

SK: My project was to develop a program where we could diagnose patients with severe blistering skin diseases using immunological methods, which were just then coming to the fore, and it was exciting for me.

KH: That sounds very exciting.

SK: Yes.

KH: I understand that Harvey Blank had used an electron micrograph to photograph the varicella virus.

SK: Right.

KH: Right, so were your new methods involved with some of this equipment that was getting more and more sophisticated?

SK: No. Not at all. It was in a completely different area. He actually didn't care what area you pursued, as long as you pursued something. He and I remained friends throughout his life. He died in, I think, the late nineties. Even though I didn't go into virology, which was his field, he was fine with it.

KH: That's fantastic. What kind of equipment were you using?

SK: I was using immunofluorescence microscopy, basically, cryostats, microscopy, skin, mouse esophagus. It was really just wonderful seeing these patients and making definitive diagnoses. It's like anything else. If you see a lot of patients and you pay attention to them, you can become an expert at that.

KH: I think that's very interesting and we'll get back to that a little bit. I want to know more about your research, but I also want to go forward and ask you about your service at Walter Reed then, which followed.

SK: Right, so of course I had to go into the Army after my residency. There was no question about it. The only question is where I would go. You could, at that time, if you wanted to do research, and there were not so many people doing research in dermatology spend three years, for example, at Letterman Army Base. When I interviewed out there, I didn't like it at all and I decided to just take my chances on where I would be assigned. Fortunately for me, the residency program at Walter Reed was in danger of losing their accreditation. They had a big residency program, but there was a lot of inbreeding in that residency

program, inbreeding meaning that they only had people on staff who had trained there, so I was fortunate in that I was identified as a Berry Planner who was going to come in and bring some new blood into the program as an outsider.

So, the head of the program who I met at one of the dermatology meetings, happened to be a big dermatology meeting that was in Miami, I guess in December of 1969. When I met him, he said, "I want to tell you that we're going to take you to Walter Reed for your two years, but if you tell anybody, I'm gonna shoot you," because the word wasn't out in terms of what the assignments were. But for me it was a great thing because I was also coming home. This is my home. Washington, D.C., Bethesda is my home, so it was great for me and my brother had just established his practice in Silver Spring at that time, then moved to Bethesda. It was a great thing for me and I was basically brought in to bring an academic focus, an academic dimension to the residency program at Walter Reed in addition to being in charge of the outpatient clinic.

KH: Wow. That's a lot of responsibility.

SK: Well, it was. It was. I went into the Army as a captain. It was disconcerting for many people because of course I didn't know captain from major to lieutenant. I didn't know any of the stuff, but I went in as a captain and many of the residents there were either majors or lieutenant colonels, and the patients couldn't understand how a captain was telling a major or a lieutenant colonel what to do with the patient. So there some complaints, actually, and very quickly I was promoted to a major so there were fewer questions. I had a wonderful experience in the Army. It was just a great two-year experience, and I was also able to do some research. They assigned someone to me who had done a one-year Berry Plan, but he had done it in research, so he knew no clinical dermatology, and as a consequence, he could only work in my lab. I had a small lab in the Army and I continued doing the same research that I had been doing as a resident.

KH: On blistering skin diseases?

SK: On blistering skin diseases. That was my first interaction with people from the NIH who sought me out because they were studying patients with a certain gastrointestinal problem called celiac disease. Celiac disease is associated with a certain blistering skin disease, so they sought me out to do a collaboration. I collected a group of patients with what's called dermatitis herpetiformis, patients who I still study to this day. We did collaborative studies between the Army and NIH. We also developed a program which was a monthly combined rheumatology/dermatology conference, which was a great conference with John Decker, who was then head of rheumatology.

He then became head of the clinical center, and it was a wonderful collaboration that we had, so that's actually how I got to know NIH. Even though my father had been a patient at NIH in, I guess, 1964 or 1965, I really didn't understand what the NIH was even though I was a medical student. My father had a rheumatic heart, rheumatic heart disease and he was brought to the NIH on a few occasions for study, so I knew about the NIH, but I didn't really know about the NIH.

KH: Then when you were at the Army, you actually wound up helping to advance collaboration and research with the NIH.

SK: Right. That was my first exposure and in fact, it was those people with whom I worked who encouraged me after I did my postdoctoral fellowship, to come to the NIH.

KH: Yeah. So shall we go to London first?

SK: London first. I decided well, I like this research. I wanted to try my hand at research, but I knew that I had to become an expert. I just couldn't do it without becoming an expert and I knew that I had to do a postdoctoral fellowship. I had gone to a meeting in 1969 where I met two people, one from England, one from Australia, with whom I thought, well, it might be fun to work with them, particularly since those were interesting places to go. I wanted to go someplace where, if I did a fellowship and then decided to go into practice, I could look back and say, gee, I had a wonderful time. My family had a wonderful time. My wife was very receptive. She was not looking for me to make a lot of money quickly and was very receptive to our going abroad. She had lived abroad for a year and a summer, and she said she had familiarity with it, so we decided to go abroad.

I was accepted to actually three places to do postdoctoral fellowship. One was in Australia, and I just couldn't go there. I had two kids at the time, and I just couldn't do that to my in-laws. My parents were not alive, but my wife's parents were alive, and I couldn't take the kids that far away, so I decided to go to England and to work with John Turk. So in July of 1972 when I was finished with the Army, we took off and we went to London not knowing anyone except this one person who I barely knew, and I learned about the G.I. Bill, which I had never heard of. I learned that I was eligible if I matriculated for the Ph.D. program at the University of London. So for £6, \$15 in those days, I matriculated and ended up getting an additional \$400 a month for my fellowship, which was then I think \$12,000 a year which came from the Dermatology Foundation of America.

KH: Fantastic.

SK: Yeah. It was a great experience.

KH: When you were at the University of London, I'm just trying to –

SK: I was at the University of London, but University of London had many different parts and I was at the Institute of Basic Medical Science at the Royal College of Surgeons, which was where Professor Turk was.

KH: Were you directly a part of his laboratory?

SK: Oh, yeah. Directly a part of his team. My project was looking at this unusual evanescent, delayed type hypersensitivity response called the Jones-Mote hypersensitivity. Now we can interpret it in more

contemporary immunological terms rather than what I did with my Ph.D., but I matriculated for a Ph.D. at the University of London.

KH: Uh-huh. Was it hard to give up your blistering skin diseases for it?

SK: No. No because I knew that I wanted to balance the two arms of the immune system. One with blistering skin diseases had more to do with serum, and what I really wanted to do was I wanted to expose myself to working with cells so that I had a balance in terms of understanding the immune system. So that was a deliberative process and I only looked at places for fellowship where I would be working with cells. Even T-cells and B-cells were just coming to their own in terms of the knowledge base. Subsets of T-cells weren't known. Subsets of B-cells were basically known because of the different immunoglobulins they would produce, but that was coming into its own. That's why I thought that working with cells would be a very good balance to my knowledge base.

KH: That's fascinating. What was it like working in Dr. Turk's laboratory?

SK: Oh, it was wonderful. It was wonderful. We moved to London. We didn't know that we were going to be on the fifth floor of a hotel for the first week or so, and there was no "lift" as they say. We were sort of in dire straits to look for an apartment or a house. It happened that Professor Turk's wife was the head of a big group of general practitioners in a certain area, and she found the house for us. That was the first time we lived in a house, a big, four-bedroom house, so it was a wonderful social experience for us, and scientifically, I worked with a woman who, unfortunately, recently died. She was great. Darien Parker was really a wonderful partner for me. We had two technicians who helped us and the project was one that was an ongoing project that I picked up.

Most experiments didn't work, which we know that to be the case, but the hypothesis was right, and the question was how to go about it. Professor Turk became a very close friend. Actually, we lived almost a few blocks from the Turks, but it was a wonderful experience. Scientifically, it was a wonderful experience also because somebody came to his lab to do a sabbatical, somebody named Morris Reichlin. He was a very famous rheumatologist who came to John's lab actually for the same reason that I came to his lab, to learn about cells. We also became very good friends and to this day have been good friends. (Dr. Reichlin died shortly after this interview was recorded)

KH: Fantastic.

SK: That was forty-seven years ago.

KH: And just checking on my, okay, so I understand. Great. So then you mentioned before that when you went there, you still thought you might have some clinical work ahead of you. You weren't completely committed to research?

SK: Absolutely. Don't mean to interrupt you, but in fact, I was never committed to having a research career. I had an open mind. If I was not successful, if I didn't like it, I was going to go into practice. I loved

seeing patients. I loved the field of dermatology. I still do. I couldn't go into practice now, but I could have for a long time. I was actually asked to join someone's practice in Miami, but I didn't do that. I thought, well, if I don't like what I'm doing or I'm not successful, I'll go work with my brother who had a practice here in the suburban Maryland area. If not, if I liked it, then I'd go to some academic program, never thinking that the NIH was a possibility.

KH: So how did you make that transition back to the NIH?

SK: I made the transition because in December of 1973, after one-and-a-half years of fellowship, I knew it was time to think about getting a real job. In September of 1973, I got a letter from the head of dermatology here, Marvin Lutzner, and he said that a senior investigator was leaving, and they had a position open. Would I be interested? He said that the pay would be \$25,000 a year. This would be in 1974. Despite the fact that I was attracted to that job, there was no way that I would take that job for \$25,000 a year.

My daughter was born in London, so we had three kids, and it was virtually impossible to take a job at \$25,000 a year. Couldn't live on it even in those days. When we came back in December of 1973, I looked at several jobs. I looked at this job at NIH, where my collaborators from 1972 from the Army strongly encouraged me to come here even though I didn't really want to be affiliated with the dermatology branch, which didn't have a great reputation. I was not attracted to anything that was going on in the dermatology branch, but they convinced me that if you come here, I could collaborate with anybody I want, which is true.

I also looked at places like Hopkins, the University of Virginia, and the University of Miami, which everyone thought, there wasn't so many "everyones," but the anticipation was that I would go back to the University of Miami, particularly since my wife's family was there and I had such a wonderful experience as a resident. But when I came here, it was intriguing to me to think about working here and collaborating with such smart people. It raises the level if you work with really smart people. Warren Strober and Ira Green were very encouraging for me to come here.

When I went to the University of Miami for my interview, it seemed like they were sort of taking me for granted as a senior resident and anyway, it was an interesting experience. I had very good relationships with people on the staff there, but finally I decided, well, I'd come to the NIH. If I liked doing research, if I was successful, I'd come here for a few years and then I'd go to an academic medical center; I love teaching students, residents, and if I don't, I'd go into practice with my brother or on my own. Either way, it was okay with me, so I was not absolutely committed to an academic career even when I came here in September of 1974.

KH: Do you think that having these different options allowed you to take risks?

SK: Absolutely. Still to this day, risks, I think, are really important. Obviously, you can't take too many risks financially when you have a wife and three children. Although my wife was a professional, at that time she was not working, so I couldn't take too many financial risks, but I knew as a physician, I could

do almost anything I wanted to do. So the risk was which place to go. At the NIH, I was assured that if I came here, I could work with anybody I wanted. I was not restricted to the dermatology branch, which I had not a very high regard for.

Actually, when I left the Army in 1972, I did call John Decker, with whom I had a very good relationship, who was head of rheumatology here. I told him that I would consider coming to the NIH if I could work in rheumatology because there's a lot of crossover between rheumatology and dermatology. Rheumatic diseases and skin diseases go together, and he said, "No." He said, "I'd love to have you, but," at that time, "We're looking for an orthopedic surgeon," so he was discouraging of that possibility.

KH: So you decided to come here instead of the University of Miami.

SK: Right, to the shock of many people.

KH: And were you hired by Dr. Frank Joseph Rauscher, Jr.?

SK: Dr. Rauscher was the head of the Cancer Institute, but what happened was when I came here for my interview, "we," Dr. Lutzner, who was head of the derm branch and I, met with the scientific director of the Cancer Institute, Nat Berlin. Nat Berlin started the discussion by saying, "Steve, we really want you to come here." I had a pretty good record, and he said, "What will it take to get you here?" I didn't know you could negotiate with the government because I had been in the Army. No negotiating in the Army, so I knew he was asking me what price. I said, "Well, 30, 31, 32, 33,000." He said, "Okay. We can do that." So it was a 30 percent increase of what I was offered, and in consultation actually with my brother, we thought, well I could do it with \$33,000 year. That was my salary, and I decided that this is where I was going to come.

KH: And they really wanted you.

SK: Yeah. It's always nice to be wanted. Yes.

KH: So if I understand correctly, as the senior investigator, you had your own personnel, budget, and space for research?

SK: Right.

KH: I was thinking this was a big change from being a Ph.D. student, but maybe not.

SK: Oh, big change. Big change to run a lab. From being a postdoc to running a lab is a big change. It's a big learning process philosophically, emotionally, and actually in reality, it's a very big change because then you're responsible not only for yourself, but you're responsible for others. Fortunately, when I came here, there were people who were already waiting for me to be their mentors. Of course, we didn't use the term "mentors" in those days, but their supervisors.

So the first day I walked into the office, which was I think September 15 or 16 of 1974, someone named Ken Hertz stood up, shook my hand, and he said, "Hi, I'm Ken Hertz. I'm going to be your clinical fellow." I didn't even know what a clinical fellow was at that time. He said, "I'm going to be your clinical fellow," and then I hired a technician and Ken was very helpful in getting my lab started. Basically, that was my lab with a technician, and Ken Hertz.

Then shortly thereafter, I started seeing patients. My brother actually referred me the first patient who I saw, a woman who had blistering disease of pregnancy. So that was my clinical outlet, blistering skin diseases, which I studied for many years, still do. He referred me the patient with the herpes gestationis and we worked that patient up, and that was the first of many patients. I also had many patients who I had seen at Walter Reed continue to see me because their physicians would refer them to me because they knew I was studying dermatitis herpetiformis as well.

So my clinical side became enriched and there was a woman here who was not a dermatologist, wanted to be a dermatologist with whom I'm still very good friends, Peggy Crawford. She didn't like what she was doing in the lab, so she started helping me in the clinic. People loved her. I had to go back to England to defend my thesis in October, but November, December, the lab had four of us. I hired a technician. It didn't work out. I changed that shortly thereafter. In 1974, we interviewed several candidates for fellowships here, three of them. It turned out that all three wanted to work in my lab, but I found out that only one could work with me because they had to be split amongst the other labs, so I thought it was a little false advertising.

The person who I chose of the three ended up being a dean at Emory Medical School for about fifteen years, so it was a good choice. He was a great guy and he expanded my lab. Oh, and there was one other person who I didn't know was working with me. I happened to go to the cafeteria one day. We had clinical rounds on Wednesdays at that time, and I saw this Japanese guy. I had worked with Japanese at Miami, so I saw this Japanese guy in the cafeteria. I was by myself and I sat down next to him. I asked him what he was doing, and it turns out that he was told that was going to work with me as well, but nobody told me that he was going to work with me. He was Japanese, so he had this certain shyness that he was not going to tell me, but it turns out that he and I worked together for four years, and we produced lots of new, interesting things. Hideo Yaoita was his name, and so the lab became four or five people, I guess, in July of 1975.

Then another good thing happened. In April of '75, we saw a patient. I was always interested in seeing patients. The chief, Dr. Lutzner, was not interested at all in seeing patients, so I saw a lot of the patients. I was asked to consult with a patient who came in with a rash and a chemotactic defect. This was a six-year-old who had had disease for four years, and they were at the end of the line in terms of what to do. She had been treated with "everything." Well, we did biopsies in April or May of 1975, and the patient was scheduled to come back the beginning of July without a diagnosis, but my brother and I, on long phone calls, decided that this child had a rare disease.

I knew that this rare disease (erythema elevatum diutinum) responded to a drug that wasn't usually included when one says, "She was treated with everything." This is a drug called dapsone which was otherwise used as a treatment of leprosy. She came in over the July 4 weekend in 1975, and I would say the way to make a reputation in a hospital is to make a right diagnosis, and then to treat a patient who's got disease for four years and, over the weekend, gets 98 percent better.

Now, when I initially went to the fellow who was in charge of this patient, I told him, "I think this patient has erythema elevatum diutinum." Very unusual. He laughed at me. Then I said, "Well, and I think we should treat the patient with dapsone." He laughed at me again, so I wasn't taking that laugh. I went to his boss who I knew, actually, was a friend of Professor Turk's, and he asked me, "What would you do if it were your daughter?" I say I'd do the same thing. We treated that patient over that July 4 weekend. She was 98 percent better, and interestingly, she's been on that drug for forty-some years because if she stops the drug, the disease comes back.

A few years ago, I was called by her doctor to ask if the drug is associated with thyroid disease. It's not. I was telling the doctor who was in West Virginia, the family was from West Virginia. I knew that the mother and father had divorced, and I told the doctor. I said, "You know, that's a very unusual family. This woman really took her daughter to lots and lots of doctors in 1974 until we finally got the diagnosis and got the treatment." I said, "Do you ever see the mother?" To which he said, "I'm married to her." Good story, huh? (Laughter.)

KH: That's a great story.

SK: Good story. Yeah.

KH: How fabulous to be able –

SK: So we shared that treatment with Drs. Gallin and Fauci. They were young in those days, as was I, and it was a wonderful thing to really change this kid's life. She had really widespread not only skin disease, she had bad arthritis. She had some pulmonary problems, and this drug, which acts on neutrophils, helped her a lot, and still does.

KH: That's fantastic. Could you talk a little bit about what the culture was like when you came to NCI?

SK: I didn't know much about the organization of NIH, not what I know now. I know a lot about the organization of NIH. It was a very collegial place. It intramurally was very collegial. When I say "intramurally," the scientists here on campus, so when I actually started, we didn't have an immunofluorescence microscope. I used the microscope from another institute, but I didn't even know I was using a microscope from another institute. It was just all collegial. Could I use this? I didn't know one institute from the other, basically. I had no idea about the organization at NIH, but it was an extraordinary, collegial place with lots of really, really, smart people to interact with, and they were very receptive.

They were very receptive to me. I hadn't done my fellowship at the NIH. Many of the people who came here as fellows stayed on to become senior investigators. I didn't do that because I was in the real Army. They used to call them the yellow beret because you could come here for two years instead of going into the military. In those days, you could get out of the Berry Plan. But I didn't want to come here because I didn't want to be a part of that dermatology branch, which I ultimately not only became part of, but became the chief of, and hopefully, people felt differently after I became the chief.

KH: Well, I want to ask you about that. I also wanted to ask you about your work on the Langerhans cells. Did that work start when you came here?

SK: No. That work started in 1977. In 1976, I had met someone at a meeting in Lund, Sweden, a person by the name of Klaus Wolff, who is a world-renowned dermatologist from Vienna. I'd given a talk on dermatitis herpetiformis. While we were having lunch one day, he said, "I have this really bright fellow who I think would benefit from going to work with you, and his name is Georg Stingl," somebody with whom I'm still very, very close friends with. So we agreed. I met Stingl. He came to visit, and it turns out that Stingl had been working on Langerhans cells in Vienna and had just published a very interesting paper in *Nature* showing that they had cell surface markers that were related to the immune system.

So we embarked on that as a major endeavor in the lab, first with guinea pigs, then with mice, to try to identify what their immunological functions were, and we collaborated with many people around the NIH, some in NIAID, some at NCI. It was a win-win situation for all concerned, and we continued that work as did Georg Stingl continued that work when he went back to Vienna. He did go back to Austria and told me in his farewell discussion with me, we were very good friends, but one thing I didn't know is that he fell in love with my technician, and he was going back, and he was marrying her, and they were going to live in Innsbruck where he had a position.

So I had to get a new technician, but she was great. She was Laura Gazze. Unfortunately, she died after they were married for twenty-five years. Died from an unusual type of tumor. He and I remain good friends. He recently retired, but still does academic research. Brilliant, brilliant guy. My claim to fame is I have had many really smart people work with me and together we have forged new areas.

KH: I do want to talk about that too, but one result that fascinated me, I read, one of your articles about the bone marrow transplants on mice that was published in *Nature* in 1979. It said that 68 to 80 percent of the resulting Langerhans cells were of donor bone marrow origin. How did you feel when you started to get these results back?

SK: Oh, they were fabulous. That collaboration occurred with David Sachs, who was in the Cancer Institute, and that came about because somebody came to give a talk. We used to have an immunology series. They still do immunology talks and there's maybe fifty of us in a room. After the talk, Ira Green, who I mentioned earlier, said to David Sachs, "David, I think you should talk to Steve Katz," because they were interested in chimerism, David was. Then we got together, and we did these experiments to try to look at the origin of epidermal Langerhans cells, and that's how that came about. It was really very exciting.

KH: Some very exciting results. So by 1980, you were the chief of the dermatology branch?

SK: But I had been acting [as chief] since 1977 because the chief of the branch went on sabbatical. Maybe he went in '78. Again, I was not supposed to be the acting chief, but the scientific director, who was then Al Rabson, came to me and asked me why I didn't want to be the acting chief. I told him that I thought I should be the acting chief because they were building this new building, the outpatient clinic. I thought it was important to have an assertive spokesperson for dermatology, in terms of space. He said, "Well, Dr. Lutzner wanted Dr. Peck to be the acting chief, but it's my call, so I'm going to make you the acting chief."

When Dr. Lutzner asked to be on sabbatical for a second year, the deal was that he could go on sabbatical for a second year, but he would not come back as the chief of the branch. He was not so interested, I think, in dermatology per se. He's a very nice guy. Then I was appointed as chief. There was no search. Nowadays, you never do things that way, you know. You always have to have a search, national search, or international search, as we do for almost everything, particularly with branch chiefs and clinical directors, scientific directors, but it was nice to be just appointed as branch chief.

KH: And you were able to play a role in continuing to transform the branch.

SK: Right. There was a balance we had, but it became more immunological. We kept a few of the fellows who did great. Tom Lawley, who ultimately became dean at Emory, he stayed on as a senior investigator. Then we also had another great resource at the Uniform Services University of the Health Sciences (USUHS) across the street was just started. I think it must have been started in the early to mid-eighties, and the dean there was a friend of mine, Jay Sanford.

He wanted to have a dermatology program because "Scoop" [Henry M.] Jackson, who was a senator from the state of Washington, had put in one of the appropriations bills that a million dollars was to go to the USUHS University to develop a dermatology program, because his doctor, who I knew well, his dermatologist was Naomi Kanof. She got Scoop Jackson to put that million dollars there. So Jay Sanford needed to spend some of that money. We knew each other scientifically. He was an infectious disease person and he called me one day. He said, "Steve, I'd like for you to become the professor." The Sulzberger Professor, that's what it was called, so it must have been mid-eighties. It must have been '85.

KH: Eighty-nine?

SK: Eighty-six or even '89 because Sulzberger died in '84, I think. He said, "I want you to be the Sulzberger Professor, and you'll work at USUHS. We'll pay you more money than NIH is paying you, but you can stay in your current position. I worked it out with Vincent DeVita (head of the NCI)." That didn't work out. I said that no, I wouldn't do that because it's just basically using that money to benefit me. Then he said, "Well, I know what your issue is." He knew that I had three kids. He said, "I'll pay each of

your kids \$20,000 a year for when they go to university.” I said, “Jay, I’m sure that’s not legal.” He said, “Well, let’s see what happens.”

We spoke to our lawyers. Of course, it wasn’t legal. Then ultimately, I became the Sulzberger Professor and I was paid a salary over and above my current salary for working extra time, which is an unusual circumstance in the government. But the reason I mention all that is because it enabled us to establish a laboratory at USUHS, so we were able to take some of our senior people here who were finishing fellowship who I really thought would expand the program and be beneficial to them to stay on because USUHS is right across the street.

So, this all transpired when John Stanley, who was ready to leave and was looking at jobs. Jay called me and showed me some space across the street. I thought it was great space, and then I called John when he was on vacation in Maine. I said, “John, don’t look at any other jobs because you’re going to stay here at the Uniformed Services.” That’s how we expanded our program. John Stanley went there at first, and then a few years later, he came back to the NIH. John ultimately became head of dermatology at University of Pennsylvania and Kim Yancey, who was from Georgia and finished his fellowship with Tom Lawley at the NIH then went to USUHS next. He ultimately became head of dermatology at Wisconsin, then at the University of Texas Southwestern. Now they have, as head of dermatology there, I think, Tom Darling, also came through our fellowship program. It’s a wonderful collaboration, win-win situation with the Uniformed Services of the Health Sciences.

KH: Were there any research areas that you had also identified as places you wanted to make sure you expanded into?

SK: No. My philosophy was, and this is a famous quote by Josh Lederberg that I can’t come to, but it’s a quote that talks about what you need to do to get a creative environment is get really smart people and let them do what they do. So basically, that’s what my philosophy has been. Retrospective, it aligns with Lederberg’s famous quote, but that’s what basically I did. That’s how I chose the senior investigators of the branch because we had searches, but it was basically getting smart people to do their thing that they could be excited about.

KH: Fantastic. That sounds like a lot of great collaboration.

SK: A lot of great collaboration. I was not a collaborator with them. They could do whatever they wanted. I was not necessarily a co-author with them, but we had a very exciting time because there was a lot of good science being discussed.

KH: As chief, you had other responsibilities too, right? How was that learning process?

SK: As chief, I would say I had very few other responsibilities. I had a responsibility to go to the chief’s meeting. In those days, I had much more responsibility for the budget of the branch, make sure we kept within budget, but other than that, there were no real administrative responsibilities. Now, I undertook many responsibilities outside of NIH. Outside of NIH, those responsibilities were with the Society for

Investigative Dermatology, the American Academy of Dermatology, the Clinical Immunology Society, all of which I became not necessarily an officer, but I became very active.

Going back to talking about the world being a small place I became very active in the World Congress of Dermatology, the International Society of Dermatology and the International League of Dermatological Societies, ultimately being on the board for twenty years and being president for five years, which was a very long term, and in 1992, in being the secretary general of the 18th World Congress of Dermatology in New York. So I was very active, I would say, administratively here at the NIH, but also on the world scene of dermatology, and investigative dermatology particularly. I was president of the Society for Investigative Dermatology in 1994/1995. I was very active in that area, but my job here was really recruiting good people, making sure we were within budget, and making sure that people were publishing and that when we had our reviews, we did well on reviews.

KH: As you got involved with the societies and associations, did you see your work as communicating scientific discoveries and disseminating them?

SK: Always. I always gave lots and lots of talks. I know our communication plan here at the NIH is much greater than that, but as a scientist, perhaps selfishly at first, but when I gave talks, it was a great source of referrals. So this little girl who had this rare disease, it was only by talking about her that I got other referrals from people around the country, and that's another great thing about this place is that you can get referrals from anywhere to bring patients in, to study those patients. It's a benefit to the research program and it's also a benefit, hopefully, to these patients. In terms of communication, I was not as aware of communicating with the public-public at that time, but certainly with the scientific and medical public. Really important, so I went as visiting professor probably, maybe I said this, ten times a year at the various academic centers and enjoyed that very much.

KH: That's fantastic. Did you enjoy teaching at USUHS?

SK: I did. I taught two courses in biochemistry. They didn't have a graduate program. I taught two courses in biochemistry and a few other courses, but in biochemistry, I taught courses where I could bring a patient. These were first year medical students, but I could bring a patient who had the disease and demonstrate what the problem was, both with porphyria, and also with xeroderma pigmentosum. So I could talk about pathways. I think I would give four or five lectures during the year. For two of them I could bring patients in who actually were there because of a fault in the biochemical pathway, biological pathway.

KH: How do you think that impacted your students?

SK: Oh, great. It's great, nothing like it. Nothing like it, I think, and not because it was me, but there's nothing like being a medical student and learning these intricate pathways and then seeing what happens if they go wrong.

KH: Then by 1995, you were appointed director of the National Institute of Arthritis, Musculoskeletal, and Skin Diseases.

SK: Arthritis and Musculoskeletal and Skin Diseases.

KH: Okay. I always stumble on that. Thank you. You were appointed by Harold Varmus?

SK: Right.

KH: Tell me about that process.

SK: So I had been recruited to various positions around the country. I wasn't interested in leaving the NIH. The only place that I really looked was at the Rockefeller Institute. I was recruited there in 1994. I didn't really think much about it, except one morning I told my wife that I was having lunch with somebody who was going to try to recruit me to New York. Our three children were living in New York at the time. She said, "Well, let's go for it." She got queasy after I actually went there, but I wasn't really interested in going there. Then they called me back and they made me a sweeter deal, but I wasn't looking for a sweet deal. I didn't really look at other places, although I went as a consultant to some of these search committees at Yale and at Hopkins, but I wasn't interested.

I was very happy here. I was very, very satisfied with what I was doing and the changes that were being made, the support that I got, and I knew if I made big bucks as I could have, I just would spend those big bucks! I was very, very happy here. I guess in the beginning of 1995 or the end of 1994, the director of the Institute of Arthritis and Musculoskeletal and Skin Diseases resigned. There was an acting chief, acting director, and two people from the search committee called me and asked me to apply and I applied, and frankly, I didn't have my ego in it because I actually knew very little about running an institute. It's very different than being an intramural scientist, so I applied, and I had a very good interview.

Then I had an interview with Harold Varmus and also his staff in Building 1, and that went well. Those all went well. Then he called me up in the beginning of June and he asked me to be the director. Of course, I didn't know what I was getting into, but it was a great honor, and it was really more than great. It was a fantastic honor to work with him, and that he chose me to lead the institute was a great honor for me. I started on August 1 of 1995, so I'm looking at the end of my twenty-third year as director of the institute, a great honor. I had great teachers at the institute that taught me all about how the NIH is run. After all, 85 to 90 percent of our dollars are in the extramural program.

I had been asked to look at two other jobs at NIH, which I said that I was not interested. One was to run the general clinical research centers around the country. Ruth Kirschstein called me and asked me to consider doing that. I said, "I'm not interested." Then she called me before the Institute of Aging had a really good candidate in Richard Hodes, who's still the director. She called me and asked me to look at that portfolio. I told her I wasn't interested. She said, "You have to look at the portfolio." I looked at the portfolio. It was a lot of behavioral and social science, too much for my brain, and I told her that

although I might be interested in being a director of an institute one day, I was not going to put my hat in that ring.

But then when this institute directorship came up, Arthritis and Musculoskeletal and Skin Disease, that was something that I thought that my experiences could be translated to a bigger stage. I have had experience with the rheumatologists, with the clinical immunologists. I felt comfortable in that environment, in that venue and I thought, well, that would be a fit. If they want me, that would be a fit. I had a very good interview, and then all the other interviews afterwards were very good. I was delighted to be asked. It was an adventure for me, but where I didn't have to move very far.

Actually, I wasn't going to continue as branch chief, but the people in the branch, the senior investigators asked me to continue to be the branch chief because they thought I had some moxie, which maybe I did. We hired two new people instead of one new person, and they continued to do some of the organizational work that I had done as chief, people like Jonathan Vogel, who's since died unfortunately, and Mark Udey. They continued to do much of the scheduling for the branch because I just couldn't do it, but they agreed to do it because they wanted me to continue as branch chief.

That continued until I decided that people in the branch were mature enough that they could endure either getting somebody from outside of the branch, or somebody from within the branch to run the branch. I didn't have to do both jobs anymore, but I did continue running my lab until about 2015 or 2014 when I decided to give up my lab. But I continued the lab until, I think it must have been 2015. Mark Udey was chosen as the branch chief, and he was somebody who I had hired in 1989 from Washington University in St. Louis, so it was nice to see that he did that job until very recently.

KH: It was an honor for them to ask you to maintain that position even as you took on the directorship.

SK: Yes. Well, I think it was in their defense because they were concerned that bad things might happen to the branch because there was a new scientific director in the Cancer Institute who had no history with the dermatology branch. They thought at least in my role as director of another institute, nobody would squander the resources of the dermatology branch. It was a matter of survival, but I would say that we were all very good friends. We had lunch almost every day together and we had great respect for each other and their success was your success. Everybody's success was everybody else's success. It was a wonderful, wonderful environment.

KH: That's fantastic. For you, the extramural portfolio was the new addition when you took on the directorship.

SK: Absolutely. At first, some constituents thought that I was going to turn the institute into a dermatology institute or skin biology, skin disease institute, which was ridiculous. They thought it was an arthritis institute, there should be rheumatologists running that institute. Harold, who's an iconoclast anyways, wonderful brain, also just wonderful to work with. He said publicly, maybe it's a good time for a change. We should have a dermatologist running this institute. So he was not intimidated by them and I had a lot to do with that, with establishing myself as a scientist, as someone who had science as the

highest priority for the institute in no two ways, having clinical medicine as well as basic science as priorities.

I made it clear to all the constituents. We had many constituents. We have a rheumatology community. We have a bone biology community. We have the orthopedic community. We have the skin biology and skin disease community. We have the muscle community with regard to muscle dystrophies and others, so all of those communities, I had to make it known that science and science advances, new knowledge was my highest priority. I was not going to do anything to interfere with our commitment to outstanding science, and I think that by now, people understand that.

KH: How did you set their fears at rest when you started?

SK: Actions. It was clear that I evened the playing field across these various constituencies and by attending their meetings, by speaking to them, by answering sometimes very difficult and indelicate questions. I was not afraid of doing that because I knew that if we adhered to outstanding science, both extramurally and intramurally, nobody could argue with that. I think that's been born out over the years. I think the successful institute directors who have made that their mantra; that has been born out.

KH: I have a few different questions relating to that, but maybe you can just walk me through some of the NIAMS accomplishments or contributions that you're proud of and you want to discuss for the record.

SK: Well, for the record, there are many. I think in the portfolio that we have, one of the early investments that we made was in a public-private partnership to try to better understand and develop biomarkers for osteoarthritis or degenerative arthritis. This is the arthritis that affects twenty to twenty-five million people in the country as they age. Obesity is also a factor here. What we've done is, over the years, developed biomarkers in this public-private partnership for osteoarthritis, which means that companies can invest in certain drugs by using biomarkers rather than having to wait twenty years to see if a drug works. That's the importance of biomarkers. They work as surrogates for go/no-go situations for interventions.

The other area that we have done a lot of work in is in the area of osteoporosis. Not only osteoporosis in women, which is seen at an earlier age than in men, but also the study of osteoporotic factors in men, what we call Mr. Os, Mr. Osteoporosis, rather than the study of osteoporotic factors in women. So we study these patients for many, many years to see what their risk factors are. When you know what those risk factors are, you can better mitigate those risk factors, whether it's the drugs people are on, whether it's their visual acuity, those types of things are very important we now know in terms of susceptibility to fracture.

Osteoporosis is not a problem. The only problem with it is you get fractures, so what you want to do is mitigate those fractures or obviate those fractures. In the area of muscular dystrophy, we've, I think, moved the field tremendously forward from the first definition of the mutation that occurs in Duchenne muscular dystrophy, which is dystrophy of the boys who end up dying in their late teens or twenties. Now that's moved because of various types of better support systems for those boys, but I think we're

moving into a time where we will have some drugs or gene therapies that will really make a difference in terms of muscular dystrophy.

In terms of skin biology and skin disease, there are overwhelming advances in terms of stem cell therapy, iPS therapy for patients with epidermolysis bullosa and other skin diseases. In the rheumatic diseases, there's been a sea change because in the mid-1990s, the TNF inhibitors came about, and those were great until other pathways were discovered. Those other pathways led to the development of newer drugs that are far more effective not only in rheumatic disease, but also in rheumatic disease associated with skin disease, like psoriatic arthritis. So in all of these areas that I talked about that we cover, there have been major advances today compared to what we knew twenty-three years ago, and I like to think it's at least in part because of the investments that have been made, the judicious investments that been made in terms of good science leading to these advances.

KH: I noticed that over the 2000s and up to 2014, Congress has called on NIAMS specifically to focus on different areas, many of which you have mentioned, muscular dystrophy, lupus, juvenile arthritis. How does this sort of policy intervention or political attention influence . . . ?

SK: We pay attention. Obviously, all of us have to pay attention to what the congress wants us to do. Sometimes it's really reinforcing what we are doing, so we have a major effort in trying to better understand lupus, lupus that occurs primarily in women, a much higher prevalence percentage-wise in African-American and Asian women, so it really has an element of health disparity. We still don't understand exactly why it occurs in women. We don't understand the disparity, but I can tell you that over the years, the treatment of patients and the support of patients has been tremendous. When I was a medical student, one of the first patients I saw was a patient with lupus who had central nervous system lupus, and it was clear that she was going to die very soon. Now that's not the case. Now it's much more of chronic disease. Systemic sclerosis (scleroderma), the same way.

The muscular dystrophies, we've made major efforts. We have the centers' program that's been in existence for fifteen years. We're currently trying to review that centers' program, and my philosophy has been that all programs that have been going on for some time should undergo review. Not the specific grants, but the program itself, so we have these muscular dystrophy centers called Wellstone Centers in memory of Senator [Paul D.] Wellstone, in whose office these were originated, actually. When he died in 2004, we decided to name them the Wellstone Centers.

We're currently looking at, should the program be as it is? Is it delivering what we wanted to deliver? Do we want more of a training program? Do we want more outreach in terms of this program? Those are the types of things that we do all the time. Very first thing I did as director was I asked a group to come together from around the country to look at our big centers' program. They had a big centers' program that comprised about 11.7 percent of our institute budget, and it turns out that we changed our centers' program as a consequence of that review. My job is constantly to look at what we're doing on a big scale to know that those investments are right investments. The congressional mandates are very few. They reinforce the paying attention to this or paying attention to that, but we are on it. We're always on it, and we are always very responsive to those requests.

KH: You also have written about the need for NIH to keep funding basic science, right? So is that a tension between a focus as Congress mandates on some of these specific areas and the basic science funding and research?

SK: For me, it's not a tension because for me, if you don't invest in basic research, then in twenty years, you got nothing to go on. If you don't invest in basic research, if you don't invest in training the next generation, there won't be any next generation and there won't be any basic research upon which to grow. That commitment, from my standpoint and from our institute, there's no questions about it. There's no real tension from my standpoint. There may be tension from the community, but from my standpoint, there is no tension. Good science will be supported whether it's basic, translational, or clinical. I think most of my co-directors feel the same way. I can't speak for them, but I can speak for myself, and that is the commitment to basic science has to be there, and I think as an institution, as the NIH, we have to have that commitment. I think Francis Collins feels the same way.

KH: I only have one or two more questions. One of them is about funding. I know in the 1990s there was an external board who asked NIH to cooperate more with private industry, to collaborate with private industry for funding. Then you know there's been criticism of collaboration with private industry and suspicion of that funding, so again, I'm asking, is there a tension in negotiating these demands as a director?

SK: I would say that they are not demands from Congress, but they're expectations that we at least consider them, consider public-private partnerships. Lately, some of those public partnerships, one in particular, the alcohol public-private partnership to which you're referring, has gone awry. That went awry not because of the inherent problem with public-private partnerships, but it went awry because of, I think, although it's not proven, the malfeasance of some of our employees. Now, the other public-private partnership that went awry was the NFL public-private partnership that NFL wanted to sort of have more input than they should have. Once we do a public-private partnership, the money is spent according to NIH rules, without prejudice, without bias, going for the best science that will deliver the answer.

As I mentioned earlier, the Osteoarthritis Initiative public-private partnership, we paid two-thirds with the Institute of Aging. The industry paid a third. In the AMP, the Advancing [Accelerating] Medicines Partnership, we pay fifty-fifty, and that's to look at and develop new pathways for the study of patients with lupus and with rheumatoid arthritis. Absolutely novel concepts. Those are much more basic research endeavors that are really precompetitive. Everybody wants to know the answer to those things. If you stay in that realm, it's much easier to avoid these types of conflicts. I have been involved with the Foundation for NIH as part of the steering committee from the NIH to steer what goes to the FNIH. I think the FNIH does a great job. We try to do a job at our end to make sure that what goes there is legitimate, is institute-driven, and is without prejudice or bias. Sometimes that goes awry. I hope that people won't think that that is the modus operandi at NIH because I think that's a rare occurrence.

KH: A theme of our interview today has been relationships, relationships between different scientists. Do you think that the ability to mentor, to be mentored and to have collegial relationships is integral to scientific success?

SK: Well, I do. I think it's been a big part of whatever success I've had. It's been a big part of my success. It's been a big part of whatever accolades I've gotten from various groups and people. I would be the last to say I've done it on my own. I think that I've been helped enormously from my colleagues, coworkers in the scientific community, from my tremendous colleagues as institute directors, and also from my family with tremendous support. All those interactions I think are important.

I think if you don't have those interactions, it's a much harder road to hoe than if you do have good relationships, so the relationships encourage interaction. They encourage leveraging. They encourage scientific and clinical interactions with other institutes because we all have parts of our institute that are very specific to what we do, but then there are many things that we do that can be done with other institutes, and that's when we collaborate.

I think that one of the things that Harold Varmus had in mind when he appointed me which he said was basically not only to be a good steward of taxpayer dollars, but also to play well in the sandbox, and to be fair. Those were the three things that he asked me to do. I think those are the three things that I've tried to do, but to play well in the sandbox means interacting with other institutes in a very positive way to identify common goals, not only with other institutes, but with other institutions, and that's where public-private partnerships come in.

KH: So I wanted to ask you about anything else I haven't mentioned that you might want to discuss.

SK: I would say the one thing that we didn't discuss is my family and the importance of family and family support in decisions that one makes to have a research career. Having a research career in the government means that you're going to make much less of an income than your compatriots elsewhere. There's no question about it. Many of my fellows, when they left, they would make double or triple what I was making. I never minded. It was their decision and it was my decision to be where I am. I think those priorities I set for myself, I set for my family, and the commitment to science and medicine.

They knew it, and as a consequence, although we never would have predicted it, my oldest son is an internist, also an epidemiologist who does research, much to his surprise, probably half the time. He lives in Tel Aviv. My second son, also a physician, works at Kaiser as a dermatologist. The dermatology couldn't get out of our system. My brother, of course, was a dermatologist. His son is a dermatologist, and my number two son is a dermatologist. Perhaps the most interesting person in my family is my daughter, who's a fitness instructor who is now pregnant with our first grandchild. My wife has been tremendously supportive over all these years. It takes a village. It takes a family. I never complained about my salary. Clearly, the salary is far less than the equivalent salary would be in most academic medical centers. I think that's it.

KH: My last question is, there's a young scientist starting out today. What's your advice to this person?

SK: Try it, you might like it. That's my advice. Of course, I always had this insurance policy of, if I didn't like it, I would be able to do something that I would love anyway. That was practicing medicine. I saw a lot of patients here at NIH, but not like I would have seen in private practice, which would have been a very different kind of a life. But if you had that option, if you go about it enthusiastically, and take risks, and listen to your mentors, and be honest with people, I think that that's a good way to go.

KH: Fantastic. Thank you so much.

SK: You're welcome.

KH: Thank you for your time.

SK: You're very welcome.

End of Interview