

Interview with Doctor Stanley Fahn

Interviewed by Doctor Michael S. Okun and Lauren E. Klaffke

Interviewed for the American Academy of Neurology Oral History Project

**Interviewed at the 67th American Academy of Neurology Annual Meeting
at the Walter E. Washington Convention Center in Washington, D.C.**

Interviewed on April 20, 2015

Stanley Fahn - SF
Michael Okun - MO
Lauren Klaffke - LK
Audience member - AM
Peter Koehler - PK

LK: Welcome! Thank you for joining us this afternoon for our oral history interview with Doctor Stanley Fahn as promoted by the AAN Oral History Project.

My name is Lauren Klaffke and I'm a Ph.D. candidate at the University of Minnesota in the program in the History of Science, Technology, and Medicine. I'm here on behalf of the History Section of the American Academy of Neurology for the Oral History Project.

The Oral History Project began in 2011 aiming to preserve the careers, perspectives, and journeys of neurologists who have had major impacts on the field. Inviting both oral historians and [unclear] physicians to conduct these interviews, the AAN selects three physicians to be interviewed each year. The collection now includes eleven interviews of researchers and clinicians. The oral histories are fully transcribed and posted on the AAN website. Today, we are opening the project and the process to the public.

I am joined by Doctor Michael Okun, interim chair of the Department of Neurology at the University of Florida College of Medicine and co-director of the Movement Disorder Center. Together, we will interview Doctor Stanley Fahn. I now welcome Doctor Okun to introduce Doctor Fahn.

MO: Thank you, Lauren.

Doctor Fahn is the H. Houston Merritt Professor of Neurology and the Director of the Center for Parkinson's Disease and Other Movement Disorders at Columbia University in New York. Doctor Fahn completed his undergraduate work at the University of California-Berkeley and his medical degree at the University of California-San Francisco.

He then went on to complete an internship at the Philadelphia General Hospital and his residency at the Neurological Institute at Columbia University. Now, something that many people may not know is that he also completed a fellowship in neurochemistry research at the National Institutes of Health. He currently serves as the scientific director of the Parkinson's Disease Foundation [PDF]. He served as a member of the Scientific Advisory Board of the Dystonia Clinical Medical Research Center at Columbia University. He has previously served on multiple boards including the Medical Advisory Board for the Myoclonus Research Foundation, the Scientific Advisory Board for the American Parkinson's Disease Association, and he's chaired the Medical Advisory Board of the Committee to Combat Huntington's Disease. He currently chairs the Parkinson's Community Research Advocacy Council of the Michael J. Fox Foundation for Parkinson's Disease Research and he is, also, the founder and the voice behind Parkinson's disease as he began the World Parkinson Congress [February 2006], the first time that Parkinson's patients were brought right next to clinicians, researchers, and people caring from the community.

Welcome, Doctor Fahn. This is a public part of the interview. You are number eleven of the Academy's Oral History Project. This is a very important project for us and it's a privilege and honor to have you here. The tone is going to be conversational. We'll ask you some questions and we just ask you to informally respond and give us your thoughts. For the last fifteen minutes of this session, we're also going to allow the audience to ask you questions.

SF: Thank you both, actually. I'm the one that's honored to be here. I must say, I appreciate the introduction, but there's one correction I have to make and that is I'm director *emeritus* of the Parkinson's Disease and Movement Disorder Center at Columbia. I stepped down as director almost two years ago. I had over forty years being director. Long enough. It's nice for somebody younger to take my place. I'm still active. I still teach. I have a lot of fellows I train. I see patients. I do some research, not as much as before but I still do some. I'm still working [sounds like INH], so I do that. I'm glad that my successor, who is Doctor Un Jung Kang, was one of my former trainees. He became a professor at the University of Chicago in movement disorders. They advertized that my position was going to open up because I'd be stepping down from it. He was one of the applicants and he was the one they selected. That's pretty great to have him there. I just want to make sure that correction is made. I'm no longer doing administration. Maybe that's good.

MO: This is a history project. Lauren is an expert and you and I are the amateurs in this project, but we definitely want to get this as factually correct as possible.

SF: Good.

MO: I'm going to ask you a series of questions. Again, this is just a conversation.

First, how did you come to focus on Parkinson's disease and movement disorders?

SF: If you don't mind my going backwards in time... How did I focus on neurology, first? I like the nervous system very much. When I was a first-year medical student, I was the only guy in my class that got one hundred percent on the neuroanatomy exam. I always liked anatomy. I liked the way the brain was organized. I liked the physiology. I liked biochemistry. I looked forward to learning more about that. So I knew something about neurology, but neurology was a very weak field. There were no treatments. People who knew my interest said I should go into neurosurgery. I hesitated what I was going to do. I finally decided I'd better take a rotating internship—they had rotating internships in those days.—and I would bide my time for making my decision.

I was in my first month of rotation and I happened to be on psychiatry service at Philadelphia General Hospital, which you mentioned. I got fascinated by the brain, again, the psychiatric point of view. I had a psychoanalyst who was my attending psychiatrist that month. I realized then and there that I wanted to go into neurology. So I began to apply for neurology residencies and I got into neurology that way. Fortunately, Houston Merritt at Columbia took me in in the class of 1959.

I didn't know I was going to go into movement disorders. There was no such thing as movement disorders when I was a resident. But I liked all of neurology. But I knew from the very beginning that I was interested particularly in research and neurochemistry and trying to solve—hopefully solve—problems related to dying neurons. Why did neurons die? I was thinking more of a field like Alzheimer's disease then. Alzheimer's was not even a popular term in those days.

After my residency, I went to NIH to get my research training. I spent three years learning about and doing research in neurochemistry on the sodium pump. That was sodium-potassium, ATPase, which was a very popular enzyme at the time. It was just discovered. I did basic science research. I worked on the electric organ of the electric eel. That was what I was going to do: neurochemistry research.

It turned out that I was starting to look for a position after I'd spent three years at NIH. I was offered a place at Duke [University]. Mel Yahr [Doctor Melvin D. Yahr] from Columbia, one of my former attending neurologists at Columbia, came down. Maybe he was coming to NIH for other reasons, but he asked to interview me. He had just received a big grant from NIH to form a Parkinson Research Center. It was an information research. They were going to accumulate all the world literature on Parkinson's disease as part of their project and do research.

All of this was—I'm talking now about 1965—because of a major philanthropist, William Black, who started the company called Chock Full o'Nuts which makes coffee still to this day. His controller and good friend developed Parkinson's disease. William Black came to Houston Merritt who was the professor of neurology at Columbia. He was also the dean of the medical school at Columbia. He came to Houston Merritt and said, "I have this friend and he's got Parkinson's. What can you do?" Houston Merritt said, "We can't do much." So Mister Black decided to found the Parkinson's Disease Foundation and gave a million-dollar grant to Columbia as an endowment for research

and contributed five million dollars to build a new research building at Columbia. He stipulated that one floor of that building had to be for Parkinson's disease research and to house the Parkinson's Disease Foundation that they'd just started.

Mel Yahr was picked by Houston Merritt who said, "You're going to start doing Parkinson research." He applied for a grant to NIH and got the money. Now, he had to staff it. He started staffing it with very strong people: Dominick [P.] Purpura, who went on to be dean at Albert Einstein College of Medicine, and many other famous people. Then, he had a couple small labs left and he had to fill them. He came to NIH and asked if I would take one of the labs. I said, "Well, yes, but I don't want to focus necessarily on Parkinson's disease. I want to do basic science right now." He said, "That's okay as long it's the nervous system or solving problems with the nervous system." So I took the job. I started in August of 1965.

In September, or maybe it was November, of 1965, Melvin Yahr and his colleagues there started the very first international symposium on the biochemistry and pharmacology of the basal ganglia. Among the speakers were Arvid Carlsson, who eventually got the Nobel Prize for discovering dopamine in the brain, and Oleh Hornykiewicz, who didn't get the Nobel Prize but might have deserved an equal [unclear] for discovering low dopamine content in the brains of people dying with Parkinson's disease. I went to that and, of course, I attended all of the sessions and when I heard Oleh Hornykiewicz speak and he showed the data on low dopamine in the brains of people who died with Parkinson's, I said right then and there, "I'm going to switch from the sodium pump and sodium-potassium, ATPase, to working on the biochemistry of the basal ganglia. So I set my focus to the basal ganglia.

My next-door lab mate was Lucien Côté. We teamed up with Malcolm [B.] Carpenter who was the professor of neuroanatomy. We dissected animal brains and measured not only dopamine but GABA [gamma-aminobutyric acid] and choline contents and their enzymes in the different forms of the basal ganglia and, then, Mal Carpenter lesions in different areas. We looked at the change, what happened after the lesions. What we discovered was the substantia nigra, which is the site of degeneration in Parkinson's disease and has a lot of degeneration of those neurons and normally would hold all this neuromelanin, which is thought to be dead tissue, was really the hottest source of enzyme activity, making enzymes for GABA and for dopamine. It turned the substantia nigra from a dead tissue to the hottest tissue in the brain, practically.

Just at that time, also, George C. Cotzias discovered that a high dose of L-dopa could turn Parkinson's symptoms around and help people in a wheelchair to walk again. That was in 1967 that he wrote his first paper. So Mel Yahr and his team started a double blind trial on levodopa therapies for Parkinson's disease based on George Cotzias's work.

I started to get involved. I was going to the clinic and, then, in 1968, [Lewis P.] "Bud" Rowland, who left Columbia to go to the University of Pennsylvania, asked if I would go to the University of Pennsylvania with him and work in his research unit. He had a clinical center, also from NIH. So I left Columbia to go to Penn. I started my own clinic,

but instead of calling it Parkinson's disease, which Doctor Rowland wanted me to do, I decided we needed a different name. It's not just Parkinson's, those areas of involvement, but other types of abnormal movements. Doctor Rowland and I sat down and figured out what shall we call this class of diseases? We called it Movement Disorders. So that started Movement Disorders. That's a long explanation for what you asked, but I thought I should give you the history leading up to it.

MO: In your career, you've seen many shifts in the field. You've seen shifts toward looking at dopamine as an important transmitter and the importance of neurochemistry. You've seen recent shifts of looking at synuclein and protein and protein processing in the brain. Do you think we're going to see more major shifts in the pathogenesis of Parkinson's disease and, if so, what shifts do we have to look forward to in the field?

SF: Well, certainly, we're going to see more shifts.

Who would have dreamed about alpha-synuclein? We'd never even heard of that protein before the gene for it was discovered to be involved with Parkinson's disease. We're learning things all the time. There are genetic research people, basic scientists finding out what genes are doing and we're tying it in with diseases. The Lewy body, the pathological hallmark of Parkinson's disease, was discovered by Doctor [Frederic Henry] Lewy—Lewey was his name in German—a hundred years ago. In 1912, he discovered this and wrote a paper showing his conclusions of people dying with Parkinson's disease. And, now, the Lewy body was named and that was the pathological [unclear]. That contains the alpha-synuclein. That's how alpha-synuclein got tied into Parkinson's disease, and, then, finding out that Lewy body contains the protein also. That's just one.

Now, we have at least twenty PARK genes, PARK1 through PARK20, plus many other genes that cause Parkinson's disease, which aren't so labeled. All these are shedding light. Each one is going to have to be properly figured out. In doing it more and more, they're learning about autophagy. That's a central role. Alpha-synuclein is degraded through that route. That route is blocked. Alpha-synuclein accumulates. That leads to abrogation. That leads to cell death. All these things were unknown even a dozen years ago. There's so much happening. I just feel lucky to live in an era when all of this is happening and seeing all these discoveries in front of my eyes.

It's bound to change. I don't know what for sure they're going to discover next. Obviously, we can't predict. But people are already working on ways on how to block alpha-synuclein from moving from one cell to another, how to block abrogation or remove the abrogation [unclear] antibodies [unclear], how to analyze in the living brain the presence of alpha-synuclein through imaging, various PET [positron emission tomography] scan devices. There's so much. Alpha-synuclein is a key central player in Parkinson's disease, but, at the same time, there's going to be other things, as well. We already know *some* genetic forms of Parkinson's disease don't go through alpha-synuclein. They go through mitochondria. Some of these are autosomal recessive forms of Parkinson's disease. Almost every year, we're learning something new. It's just been a remarkable set of discoveries.

MO: Who was the one person in your life who had the greatest influence on who you are now today?

SF: I can always start with my parents, certainly, and my wife [Charlotte]. My wife keeps my feet on the ground and keeps me in touch with the rest of the world around me; whereas, I would focus almost entirely on my own work.

But as far as professional... Lewis B. Rowland was one of my young attending neurologists. He was assistant professor when I was a resident.

Houston Merritt was the chairman and Houston Merritt certainly had an influence, but he was so busy being a dean. We only met with Doctor Merritt once a week at his rounds. But he was a real person who knew neurology just like that. He could come up with any diagnosis just by hearing the story as he examined the patients. He was a superbly good clinician.

“Bud” Rowland was one of these researchers, young attendings. He had a lab at Columbia. In my third year, I had a research project in mind and I needed a place to do it. He let me come into his lab. Every night after work and after patient rounds and all the other work through the day, I would spend another two to three hours working in the lab starting my project and would take the subway back to my apartment at night, stopping off for a couple slices of pizza and a diet Coke to nourish me. And that was it. Then, when he came up with a patient with McArdle disease, which has phosphorylase deficiency, he wanted to work on that and asked me if I would drop what I was doing and work with him. He said, “This is a really important project.” I said, “Sure.” So I worked with him. He taught me how to work with biochemistry research. That spurred me on, of course, to go into my biochemistry fellowship at NIH. I still credit Doctor Rowland. He’s the one that took me to Penn when he went to become chairman at Penn. He took me back at Columbia when he became chairman at Columbia in 1973. We are friends ever since. He’s been a mentor.

MO: Who has taught you more about neurology, the students that you’ve trained or the patients that you’ve seen?

SF: Certainly *all* of those and, to this day, it’s still the patients. But I also learned when I trained my fellows. When something new pops up, they’d tell me, and I’d ask them questions and we’d go look up the answers. I’m always learning from the patients. No day goes by that I don’t learn something from my patients. I must say, you learn neurology also by reading about neurology, studying the brain, listening to your mentors, going to the lectures. It’s an ever and non-ending of series of learning. Certainly, we neurologists all know, we have to keep learning. That’s why we’re here at this academy meeting. The American Academy of Neurology stands for lifelong learning. All of these are important. But on the day-to-day thing, it’s seeing patients, learning from them, reading papers, listening to lectures, going to meetings and talking to the presenters of the posters. Everything should be learning. You’re always learning something.

MO: How do you define success for the young people coming into the field?

SF: Success is what one sees in their own mind. What is it a person wants to do? How can you get there? How can you do it? I don't think one should set their ambitions too high, because then they'll probably be doomed to failure. Not everybody can reach the highest of achievements. Many achievements come almost by accident. I think they should [unclear] success, let's say, on I want to accomplish this in my life. Let's say, I want to make an advance of some sort. I want to push the frontiers back and just work on a little project. That little project may lead to more and more and bigger projects and pretty soon, you're growing with the field and becoming a leader. Others may want to be a success at having a successful practice treating patients, helping people have a good life again, higher quality than what they had with the disease. I think these things are a success, too. Success is what a person wants himself. Some people want to be successful making a lot of money. If that's what their success is, they feel that's successful. Everybody has their own term for what success is. I leave it to the individual. I think we all individualize, just like with our patients. No two patients are alike. They're all individuals and we treat them as such. I think that's the way some will look at success. Everybody wants to be a success: success in marriage, success with their children, and so forth. This is all part of life.

MO: What's the one thing about you, about your personality, that you would describe yourself as to people who don't know you well, people who don't work with you everyday?

SF: I know what people say about me.

[chuckles]

SF: I'm working and doing this and that. Some will say I'm humble and, of course, I'm not humble today. I'm sort of blowing in this now. In fact, I should just add that this last weekend starting with Friday night, there was a dinner and, then, Saturday, an all-day symposium and my former fellows came to Columbia in New York City to share with me. It goes to your head—no question about it. So I know what they said about me. They said a lot of good things. I would say probably I'm more on the lazy side. If I didn't have to work, I probably would just like to read books and maybe putter in the garden, do things like that. I don't have time for all of that. So my book reading is done commuting from my home to the hospital and I listen to books on tape. I do a lot of American history reading that way. I'm reading about the Civil War and the Revolutionary War and all the good biographies, as well. I would say I'm probably more laid back than most people think I am.

MO: What are some of the most significant changes in neurology that you saw in your career when you woke up and said, "I never thought I would see that happen in my lifetime?"

SF: Even in my own field, I would say that, let alone the rest of neurology. Certainly, in Parkinson's disease, the revolution of levodopa therapy for people with endless worsening disease over time who get invalidated in a wheelchair and, now, you can make them walk and you can delay the disability. That itself was revolutionary. The whole field of Parkinson and all the rest of movement disorder, we have treatments for: dystonia, choreas, tremors, tics. Then, I look at general neurology and what's happening? There are more papers on MS [multiple sclerosis] today than there are on Parkinson's or movement disorders. Hopefully, there will be good results, not achieved yet perhaps. for stroke, even though attacking stroke early does help people and some day, maybe, dementia, which is a scourge in humans. If we live long enough, we get dementia. Maybe there will be a cure some day. We don't see it yet, but I see progress all the time and I can't keep up with all of that. It's very hard for a person to keep up with everything. I must say that you can take almost any field and you will see some progress that you wouldn't have expected, wouldn't have thought of twenty years ago, let alone fifty years ago.

MO: What advice would you give to students and to doctors early in their careers who are interested in neurology? What are going to be the key pieces of information that you would pass on to them that are going to be important for them in their career?

SF: I think part of it is what does the person want to do with their life when they go into neurology? Do they want to be a clinician and take care of patients, which is a very noble task? How do you cope with the current climate of patient care and regulatory systems, insurance companies, all the other problems that we old timers now feel changed our lives *tremendously*, not necessarily for the better, that are difficult to cope with? These students coming in are coping. Then, there's the person who goes into neurology who wants to do research and how do they become an [unclear], how to get research training, and how to guide them along the way. I think we need both types of people. One of the things I liked about Columbia neurology from Houston Merritt was it wasn't all research and it wasn't all clinicians. It was really a combination. We all worked together. We can't do clinical research without the clinician. The clinician has to recognize the patient and has to get the patient to the researcher. We do a lot of our genetic research, for example, because the clinician sees the patient and gets the patient interested in research and gets them going into a research project. The same thing with clinical trials. We can't do this if we don't work as a team. Neurologists can do almost anything and still be successful. They just have to know what is it they want to do. Many don't know.

When I interview candidates for fellowship, almost everyone says they want to be an academic clinician, doing some research, some teaching, and seeing patients. That's good. So how do you choose between them? They all say the same thing? Only some are really going to do that. The great majority, maybe not the ones in our program, most people going into movement disorders, would say, "I'll probably go into clinical movement disorders." There's a *big* need for clinicians in movement disorders. There's so much specialty you need to know, that special knowledge to get your patient better, so it's very good for them. We need them! I don't downgrade them for that. If they don't

want to go into research, it's fine with me. They don't have to do research. If I can convince them to do research, that's all the better. I leave it to them. Where is their heart? Where is their mind? What are their family needs and all the other things? These are all important. Again, like I do with my patients...individualize. What is it that person wants? What does that trainee want? What's that person in neurology want? And then guide them along the way.

MO: There is no single person in the field of neurology that has trained more movement disorder fellows than you have. Can you reflect for us on what their accomplishments and what their impact has been on the field?

SF: I'm very proud of the fellows I've trained. Like any parent of their children when they do something good and, hopefully, better than you have done in your life, I'm very proud. I have great people that I've trained. They spoke, by the way, at the symposium on Saturday on dystonia, Parkinson's disease, essential tremor, psychogenic disorders, all kinds of fields in movement disorders. I don't want to single anybody out. There were terrific people talking that day and there were more that could have talked but there was not enough time on the agenda for them. So there are a lot of excellent people that I've trained in most of the fields in movement disorders

MO: Do you regret passing up any particular area of research or any research project during the course of your career?

SF: No. I think if I had it to do over again, I'd probably do the same thing. I wish I was a faster writer. It takes me a long time to write. I don't get as many papers written as I would like to write. I have to ask my fellows to do the writing. I might come up with ideas for a project but I don't have enough time to do them all, so I recruit other people. I'm glad about the ones I've participated in and the ones I was able to get other people started on. There is nothing I regret though.

MO: Do you think we care for patients better now based on the body of work that you and your colleagues have added to the field of neurology and to the field of movement disorders?

SF: Oh, it's probably more appropriate for others to say that than me. But I think the patients get better care in general.

MO: How far do you think we've come in movement disorders research and are you surprised at how far we've come in such a short period of time?

SF: Certainly, when something brand new that's revolutionary comes up, we're always surprised and amazed. Of course, in the last dozen years, probably the greatest advance has been in genetics, already in dystonia work, the DYD25, and in Parkinson work, the PARK20. We still don't have the gene for Tourette syndrome. We still don't have the gene for essential tremor. We have a lot to do. We have one Huntington disease gene that we know of and we have some look-alike types of diseases about which we know the

genes. There is still a lot of disease out there that we don't have genes for and that's just genetics.

How about the biochemistry? We're *way* behind in the biochemistry still in all of this. We need more brains. I tell patients, "One thing you can contribute after you pass away is to donate your brain. Let us study it."

For example, just take the problem of freezing of gait in Parkinson's disease, the late development of people with Parkinson's disease. It may occur early in other diseases like [unclear] syndrome such as progressive supranuclear palsy, PSP. But freezing gait does not occur early in Parkinson's disease. But it does occur in the great majority of people. It limits their walking. They walk and they'll fall. If they can't walk, they have to use a cane or a walker or a wheelchair. We haven't got the foggiest idea where in the brain that problem is and what the neurotransmitter problem is. Look at the research opportunities for the next generation of investigators. And that's just one symptom. Dementia is an even bigger symptom. Almost eighty percent of people with Parkinson's will get dementia sometime by the time they reach the upper seventy years of age and we can't do much about it. Why is that? Maybe the new developments and finding out the spread alpha-synuclein aggregates and how they spread from one neuron to another may stop that and we could treat it.

I'm looking forward to a great thing. In my lifetime, I saw great things happening. I think after I'm gone, there will be many more great things. The younger generation is going to look forward to some wonderful things happening in neurology.

MO: Do you think we'll see a cure for any movement disorder soon and, if so, which one or ones?

SF: I remember when I first went into the field, I heard experts tell me that the cure for Parkinson's disease was five years away. Well, five years have come and gone many, many times and we haven't seen it. So, I don't get into that.

The way I see the research going in Parkinson's disease—just take that one alone—is I see the understanding about how it becomes a problem protein, how it becomes a rogue protein and what scientists are thinking about the control and what helps and what helps Parkinson's is going to help in Alzheimer's, help in ALS [Amyotrophic lateral sclerosis], and other diseases and what helps in them is going to help in Parkinson's. We're going to learn from each other's subspecialty. I see this happening. When is that going to happen? I don't know. It can't come soon enough, of course, if you're the patient. They want it yesterday. But it's going to come. It may not come in my lifetime, though.

MO: If you were starting your career today, what area of movement disorders would you pursue?

SF: I like all areas of movement disorders and I try to pursue them all. I'm a generalist in movement disorders. I don't specialize in just Parkinson's disease or just dystonia.

Those are the two I probably work the hardest in. Even when I train my fellows... I've trained child neurologists in movement disorders. I tell them when I interview them, "You might want to be in child neurology movement disorder, but you're not going to learn movement disorders and how to treat conditions unless you work with adults with Parkinson's disease and how to work with levodopa, see all the complications and how to deal with that." There are children with Parkinson's disease, also, even though people don't seem to recognize [unclear]. You have to know how to treat adults before you can treat the children. They learn a lot about the basal ganglia by treating adults and, then, they go on to the child. So you have to be a generalist first before you can subspecialize. That's what I try to do with my fellows and, then, if they want to, focus their research on one thing at a time. They have to learn the whole field.

MO: If Stan Fahn had received a letter saying he was denied admission to medical school, what do you think Stan Fahn would have ended up doing?

SF: Good question. Probably applying again.

[laughter]

MO: Tell us about the excitement that you felt when you were waiting for the results of the Parkinson's disease transplant trial.

SF: Well, it was a double-blind trial. All the subjects, forty subjects, knew it was double blind and we weren't going to un-blind it until the very end when the last patient went through. I guess we were anxious to know what it was. We really didn't know [unclear]. We did see dyskinesias increase and we suspected maybe those were [unclear] but we didn't know for sure. When we saw the results, we were really impressed. We had proof of the principle that some people, especially young people under sixty, a fairly high percent of them, did get better with fetal tissue transplants. They were able to lower their dose of levodopa or they were able to not take levodopa at all, just depend on their transplant and maybe a few other minor drugs and function. So we had proof our principle works. We also learned from that study that the people who did the best, that didn't need the levodopa, were getting dyskinesias without levodopa, what I call runaway dyskinesias or persistent dyskinesias. That surprised us no end. When we went into this, there were papers out there suggesting that there was less dyskinesia, that a continue supply of dopamine will get rid of the dyskinesias. So we purposely didn't lower the levodopa dose in the study and let people have more and more dyskinesias because we were locked into the same dose. We didn't know then. If I were to do it over again, one of the things I would say about every subject, whether they're the placebo subject or the actual implanted subject, is "We should try to lower the dose periodically and see if we can get away with it." That would be another outcome variable. If I did it over, I would do that. Anyway, I was impressed with the results of the study.

I also knew that the dyskinesia is going to impact the stem cell therapy. Why won't they get dyskinesias also when we start doing stem cell therapy? I think there's a real problem. I think maybe one solution is start to implants of cells, maybe stem cells, stem

cells modified that generate dopamine, and put those in people early in the disease. Delay levodopa. What are all the complications with levodopa? Maybe those people won't need it and we can keep them going at a high quality of life for many, many years before they get into trouble. I think that's something that we should rethink now with the more we know about the results of the implant studies.

MO: Take us back and describe your first reactions when you sent patients to have parts of their brain burned or stimulating electrodes put in and you saw children walk again, tremors disappear, things that you had sought treatments for throughout the course of your career. Take us through your thoughts as you saw this evolve.

SF: Parkinson's disease, by the way, is a leading disease of neurology in which major advances have been done. We've already talked about levodopa. We could have talked about [unclear] before that that helped people. But brain surgery, stereotactic surgery for Parkinson's disease was another leading event in neurology. It was [unclear] people with Parkinson's disease. In those days, it was putting in lesions, to try different lesions throughout the brain. I'm not going to go through the history of all of this.

Eventually, it was Irving [S.] Cooper making a serendipitous discovery. He was trying to do something for another disease [unclear], a typical [unclear] injuring a blood vessel, getting a little stroke in a very sensitive region of the brain and the tremor stopped. He began to explore that and he realized there are targets with tremor [unclear] Parkinson's disease and he developed chemopallidectomy. Then, he did thalamotomies. That was sort of the start of it. Then came stimulators. [unclear] stimulation and much of the contributions of Alim [-Louis] Benabid who was the first to really apply this. Even before that, in Tokyo, [sounds like Nare-uh My-osh-ee], who was a neurologist doing stereotactic surgery himself with patient's with Parkinson's disease, discovered that he can control tremor better in the nucleus of the thalamus called the ventral intermediate nucleus. He would put electrodes in there and record. He would read the signals for tremor in his reportings and he knew that if you put the target in a lesion there, or stimulated it first to make sure he was in the target, that stimulation stopped the tremor. Then, he would go ahead and make the lesion. Benabid did one step further. He found the target of the tremor, but instead of making a lesion, he left in place the stimulating electrodes and showed that just stimulation alone without destroying the tissue also caused results of improvement in the tremor. He did that and [unclear] Parkinson's disease and other nuclei with the subthalamic nucleus.

Simultaneously, people were going back and doing lesions of the pallidum or dystonia and they found [sounds like An-throw-par] disease of the pallidum and that was effective. Then, dystonia was a target and the pallidum still is a target and stimulation there works as well. You couldn't really make the lesion in the subthalamic nucleus which is a major target today because we all knew from our experiences in clinical neurology that lesions of the subthalamic nucleus often caused amyloid balas and amyloid chorea [unclear]. So if you made a lesion, you might change Parkinsonism tremor [unclear] to chorea. But if you put the stimulators in, you can adjust the stimulation parameters, increase or decrease. If you can regulate it better, you could control Parkinsonism without causing

chorea. So this became a major target. Of course, the GPi [globus pallidus] is another major target, as I mentioned already. These were revolutionary. They deserved the Lasker [-DeBakey Clinical Medical Research] Award. Mahlon [R.] DeLong shared the Lasker award with Alim Benabid for the development of the subthalamic nucleus as a target for Parkinson's disease. Mahlon DeLong did a lot of the physiology showing the subthalamic nucleus played a role in many of the symptoms of Parkinson's working with animal models. Mahlon went on and did the physiology of the patient undergoing deep brain stimulation. This is a major event and he well deserved the Lasker Award for this big advance in the treatment of Parkinson's and other movement disorders, certainly dystonia tremors and, now, they're looking at Tourette syndrome, tics, [unclear] myoclonus, maybe others.

MO: When Stan Fahn can't sleep at night, what's on Stan Fahn's mind? What worries him?

SF: You worry about things in life, I guess, your family and how they're doing. First, you have your children and, then, you have your grandchildren to worry about. Hopefully, things take care of themselves. Basically, I don't let too many things worry me. I try to lead my life... If I have deadlines to meet, that's what worries me probably more than anything. If I have a deadline and I'm late, I've got to get up and get that project going, finish that paper. My wife says...don't assign me any more chapters because I'm always going to be late getting it done, because I've got too many things to do and I'm a slow writer. So it takes me a while to do it. That's what keeps me going: another deadline to meet.

MO: This is, now, the part of the interview that will be public. I'll remind you that this is an official historical recording. This will be recorded for posterity for the Academy Archives. Now, is your chance to ask Stan Fahn questions. I'm going to remove this microphone here and if you'd like to ask a question, I'm going to ask you to come up and briefly ask your question directly to Doctor Fahn.

AM: [unclear]

SF: [Walter] Birkmayer was one of the early people to give L-dopa. Actually, he was the first. He was a clinician that worked with Oleh Hornykiewicz in Vienna [Austria]. He had access to the patients. If you read various biographies and historical remarks by, let's say, Hornykiewicz in particular, he had to influence Birkmayer to give L-dopa. Birkmayer was still impressed that it had to do with serotonin in Parkinson's disease and he should try giving 5-HDP [5-Hydroxytryptophan] precursors of serotonin. Hornykiewicz [unclear] prepare L-dopa for injection intravenously to patients by a mechanism that was done previously by a psychiatrist named [Robert] Dedrick, who was treating drug-induced Parkinsonism. As you know, it all started with reserpine, if we want to go back in history. Reserpine was a drug from a plant in India, which calmed people down and lowered blood pressure. The mechanism, as we now know, depletes monoamines. That's how it lowered blood pressure. It was the main antihypertensive drug. But one of its side effects was it induced Parkinson's. Carlsson's work on it was

he wanted to know why does reserpine cause Parkinson's? They didn't call it Parkinson's. They called it the sedating effect. But the term neuroleptic came from reserpine causing Parkinsonism. That term neuroleptic exists today applied to dopamine receptor blocker agents to treat psychosis, the antipsychotic drugs. He showed reserpine induced Parkinsonism by lowering brain dopamine levels and if he gave L-dopa to the animals, he restored them and they perked right up. This is the awakening effect, Oliver Sacks's book *Awakenings*. He awakened these rabbits. L-dopa came into being at that time. He didn't do the next step which Hornykiewicz did: looking in the brain cells measuring [unclear] was down. Hornykiewicz encouraged Birkmayer to do this. They did their study and they reported it. But for injecting, the success rate was not great. Other people couldn't duplicate the work. It wasn't highly successful. It really wasn't until George Cotzias who gave L-dopa for the wrong reasons... He wanted to repigment the brain. L-dopa was one of three things he tried. He tried a couple of other things: peptide and phenylalanine, a precursor for L-dopa, on the way to L-tyrosine and L-dopa, that step. He tried those first but they didn't work. In fact, the phenylalanine made people worse with their Parkinson's. But he found L-dopa worked. Why was he successful orally when other people were not successful orally? Pat [Patrick L.] McGeer, a brilliant scientist at Vancouver, came up with the idea of trying L-dopa orally in high doses, but he only gave it for a short period of time and not in high enough doses. He only had the L-dopa. Of course, that's what Cotzias had, too. But Cotzias persevered. He had a government hospital, Brookhaven National Laboratory in Long Island. He was the head clinician there. They brought the patients in and he gave them small doses, kept them as in-patients for a long time, watching for side effects, built the dose up until he found this *miraculous* improvement in Parkinson's symptoms with L-dopa at high doses. So he was successful. Everything stemmed from that point on in the success of treating with L-dopa.

There were certainly many other people. I just wrote a review about the treatment of Parkinson's disease from the time of [Doctor] James Parkinson through George Cotzias. It's in the January issue of the *Journal of Movement Disorders*, if you want to see that historical review. It has a lot of the names in there of people who worked on the L-dopa story. The names I would single out... If had the choice, I would have given the shared Nobel Prize to Carlsson, Hornykiewicz, and Cotzias. Together, those three made history bringing L-dopa to where it is today, *the* gold standard for treating Parkinson's disease.

As far as surgery is concerned, as dramatic as deep brain stimulation is, I would hope that we don't do surgery in the future. I would hope we would cure the disease before we'd get to the point of surgery. But right now, we're very thankful we have something else to help the patients, so surgery is one of the great things.

AM: [unclear] the Aspen Course is a comprehensive movement disorders review. I believe this is the twenty-fifth year. [unclear] celebrating its twenty-fifth year. I wanted to know a little bit of the history behind how this review started.

SF: The Aspen Course is one of my pet projects. As you said, it's our twenty-fifth year this year. I hope people will come. I left a few brochures out there but you can also get it online on the web.

So how did it start? I had the idea that people would probably come to hear some experts in treating in the field of movement disorders but how to make it successful as a CME [complete medical education] course and not have it too expensive. So I approached two of my colleagues to see if they would share this vision with me: [C.] David Marsden and [Joseph] Joe Jankovic. Just the three of us would cover the whole field. We'd keep the expenses down. They'd only have to support three people's living expenses and travel expenses. We would try to do this. They agreed. I thought maybe we should first do it in a nice place, a resort area. People might come in the summer. I thought of Lake Tahoe. I grew up in California and used to vacation with my parents at Lake Tahoe in the summer. But Joe Jankovic said, "No, that won't work. We should go to Aspen. Aspen is convenient,—he's right—"but to go to Lake Tahoe, you'd have to fly to San Francisco or Fresno or Sacramento and rent a car. You couldn't get there easily. Aspen is easy. You can either drive from Denver or fly directly to Aspen from Denver or other cities. So let's hold it in Aspen." That's how we started in Aspen.

Then, I had to get approval from my chairmen, which was Doctor Rowland at the time, and the Columbia CME Department. They were reluctant. "Oh, we can't afford to take that risk. What if it's a failure? We'd have all those expenses. We'd have to make the expenses up." So we made a deal with them. At least Joe Jankovic and I said, "We think we're going to be successful. We're not going to have a failure. If we run a deficit, we will personally cover the deficit from our own pocket." That's all that Columbia needed to know. They let us do it. Fortunately, we didn't run a deficit. We thought there was a need and we thought that people would come hear us.

We already had experience at the Unusual Movement Disorder Seminar at the Academy meeting. We started that. David and I did that for twenty years...well, he for eighteen. He died two years before I ended it. We did it for eighteen years in a row when he was alive and, then, I did it two more years. People would come from all over to hear David and me, see the video tapes and discuss them. We'd ask the audience to discuss them. We'd have all this conversation going into the wee hours of the morning. We would just continue until we ran out of cases.

We knew people were interested. So why wouldn't they come for something in the summer at a resort area? So, therefore, we wanted to hold it either the last week of July or the first week of August for people to get away and, then, they could take the rest of their vacation in the wonderful Rocky Mountains. That's how we decided on Aspen: the convenience, the location, and vacation time as well as putting up a little bit of risk. But we were confident. I guess it's like any entrepreneurial businessman who thinks he's going to have a successful restaurant or business and he just needs to borrow some money to get it started. So Columbia CME put up all the additional cash, made the contract with the hotel, and everything else. We had to get the money and pay them back. That's how we did it.

Thank you for the question. Go to Aspen if you haven't been there.

[chuckles]

AM: What would you say was your biggest scientific mistake, something that you overlooked for too long?

[pause]

[laughter]

AM: Maybe nothing.

SF: No. Even when I see patients today... Let's say I see a patient, you wonder if it's psychogenic or not. I'm sure I saw patients that I may have miss-diagnosed. It's hard to make a diagnosis...psychogenic. You don't want to make the wrong diagnosis. You don't want to [unclear] way and you give the benefit of the doubt. Let's treat them. I may have overlooked some cases. That's clinical mistakes.

Scientific? I guess maybe not publishing quick enough, delaying publication, maybe not publishing things I should have published, observations. I had a lot of observations. I'd try to get my fellows to publish them. I'm not sure. I certainly think more of my successes than my mistakes. I started the L-dopa study. It was one that I really felt was an important need. We have to find out what L-dopa is really doing. It was a mistake how we did it, I think. If I knew then what we know now today, I would have designed it differently and try to do that with a follow-up trial, which we never got funded for. Certainly, we make mistakes and I think we learn from our mistakes. The thing about medicine in general is we all learn from our mistakes. You see a patient and you try this and that and you learn. Just like by serendipity, you learn the good things you do, too. I haven't thought of that question. If I knew I had made a mistake...I'd shoot myself, if I'd missed something I didn't do.

[chuckles]

MO: This concludes the public portion of the oral history. This is the eleventh archival [interview]. This is the first time it's been done publically, the first hour as public and, then, Doctor Fahn will join Laura and me for a private multi-hour oral history, which will be available in addition to everything that you heard today.

I want to take a moment here to thank Doctor Fahn for spending an hour with us today.

[applause]

[End of the public portion of the interview]

LK: This is Lauren Klaffke. It's Monday, April 20, 2015, and I'm here today with Doctor Michael Okun and Doctor Stanley Fahn for part two of our interview at the American Academy of Neurology Annual Meeting in Washington, D.C. Doctor Okun and I are joined by Doctor Peter Koehler, leader of the oral history working group. We're interviewing Doctor Fahn for the American Academy of Neurology Oral History Project.

Thank you, again, for meeting for this portion of the interview.

In our public portion, we talked about how you became interested in neurology, but I'm going to take us even further back in time. I wonder if you can tell us about your early life and education and the influences of your parents and how you became interested in a medical career.

SF: That goes way back. First of all, I was born in Sacramento, California, fortunate to have been born in California, because my parents were immigrants.

My father [Ernest Fahn] left Germany when he was eighteen to escape anti-Semitism. That was 1928. His older brother already had come to California, so he decided to come. He got his visa and went out to work. He survived. He became a peddler and, eventually, developed his own business. I would tell you in California in the valley is farming as you hear about it today and the drought. There were a lot of farmers and he would buy their used burlap bags that they had bought seeds and stuff in. They needed to have clean bags with which to harvest their crops. That was before bulk transports. He would take the bags and go to the junk yard, sell them, and make some money. He would learn how to patch and sew and clean the bags, repair them, and sell the used bags back to the farmer. He, eventually, got into the burlap bag business. That's how he started. Another brother came over from Germany after that and joined him as a partner on this business.

Then, my father decided to go back and get married. That was 1931. He went back and wanted to marry a girl from the old country. He met my mother [Sylvia Schumer] at a family friend's wedding that they were both invited to, my father's family and my mother's family. My father saw my mother at the wedding and signed her dance card for every dance. He danced with her all night. That night, he told his parents he would like to marry this girl. He asked to take her home. She said, "No, I walked here with my girlfriend. I'll go home with her." But he wanted to marry her. Right away, he knew he wanted to. In those days, they had—I forget the name in English, the person who intervenes...

LK: Matchmaker?

SF: The matchmaker. My mother's family said, "Okay." But they tried to convince him not to go back to California. "There are only wild Indians out there. Stay in Germany. It's very civilized." He said, "No, there's too much anti-Semitism. I'm going back."

They got married in March 1931. My sister [Cecilia] was born nine months later in California. I was the second born in November 1933.

I grew up in Sacramento, sort of a middle class life. I went to school not too far away. I rode my bicycle to school, joined things. I was not a real great athlete, but I played sports after school in the park with all the neighborhood kids. In fact, one of my Sacramento friends remarked that Justice [Anthony] Kennedy in the Supreme Court lived in our neighborhood and he used to be part of our basketball players in the neighborhood. He was another Sacramentan, in other words. I went to school and I did well.

I went to [University of California-] Berkeley. I sort of knew I wanted to go into medicine, so I went to pre-med right away. I did very well in [C.K. McClatchy] High School, as well, so I had the grades. I got a scholarship called a non-paying scholarship. My father was able to afford college for me, so I didn't take any money. They called it honorary [?] scholarship, but they were very selective. So I got one of those. I took all the pre-meds. I wanted to go to UC-SF [University of California-San Francisco] and I was able to get in after three years. They did have a program. You could go in after three years. Your fourth year of regular college is the first year of medical school and you still get a bachelor's degree. A lot of my classmates who went into medicine did the same thing.

As I mentioned earlier in the first part of this program, I was interested in the nervous system even though I knew nothing about it when I went in. I didn't know anything about any medicine. There was no medicine in our family. Basically, the family was business people. They had no college education or anything like that. I had to learn from scratch what college was like and what medical school was like and learn that way. As I mentioned, I did very well in the nervous system anatomy and then all the other courses. I wasn't prepared any better than anybody else.

I still remember reading books in grammar school. I would go to the library a lot and read biographies of famous research physicians: [William C.] Gorgas conquering yellow fever in Panama. That was sort of my role model. I wanted to be that kind of a person and do something helping mankind. I thought through medicine was a good way to do it. That's what I sort of knew. I knew about doctors.

I had asthma as a child myself. I still remember the time—I was seven years old—when I got my first asthma attack. I really had a hard time breathing. My mother took me to the doctor. In the meantime, she had said, "We have to eat some chicken soup." I still remember that to this day. [chuckles] A seven-year old who couldn't breathe and my mother is trying me chicken soup to get me better. The doctor gave me shot of epinephrine, I guess it was in those days, and I was able to breathe again. I was hospitalized.

My family, who was looking for my health, even sacrificed to take me and my sister, my two-year-old sister, to Phoenix, Arizona, to recover from asthma and get the cure of the dry climate. My father drove us there and he would go back home. Then, in a couple

weeks or so, he'd come visit us for like a weekend and go back to take care of his business. We stayed there for, I know, a whole semester, the spring semester. I was put into a class and the class was already advanced further than in Sacramento. I remember the arithmetic class. They were having long division and I never had heard of long division where I grew up and I couldn't do it. Of course, I was dumbfounded. I couldn't solve this. I didn't know what I was doing. Then, of course, I studied right away and I was able to master it. I turned out to be pretty good in math at the time. I still remember those. That's how far advanced, a year ahead, the kids in Phoenix were over the ones in Sacramento. Those little things, I remember. I remember falling down breaking my arm. I even remember the drive down to get to Arizona. It was in the winter. It was like in December and we had to drive over the Tehachapi Mountains to get to southern California. L.A. [Los Angeles] is on the other side, south of the Tehachapi Mountains and it was snowing and we had no chains on the tires. It was slippery. My father drove carefully. We made it. All those little events pop into my head now as we talk about it.

I had very caring parents who looked after me. They supported me to go to college and supported me to go to medical school. They thought I was just going to be a practicing doctor like all the other doctors they knew. But I really wanted to do academics and research. Again, they were supportive. They never denied me anything I really wanted to do in that sense, so that was all to the good. So I still admire my parents for that. That was my early life. Then, going away to college, moving to Berkeley, meeting other people in the medical school was a growing up experience. How you progress, we see that in our own children, of course, when they go away and leave the nest. I've always been close to my parents. When my mother was widowed, I would try to call her at least once a week and make sure she was okay. It was a good family life.

LK: I was wondering how your family ended up in California.

SF: That is a good question. I told you my father's older brother had already moved out to California. He was really the pioneer. My father and mother were not actually born in Germany. They immigrated there. They grew up in what was then called the Austria Hungary Empire. Today, I know the area. It's Ukraine. I actually visited. I even went to the cemetery where the Fans were buried. My grandfather, that is my father's father, struggled to make a living. He would go off to Germany and peddle and come back. The family was Jewish so he came back for the Jewish holidays and for Passover. When my father's oldest brother turned ten, he would go out with his father and go to Germany with him and help run the business. I guess he started a junk shop or something. Then, when my father was ten—my uncle was now eighteen—he decided to bring all the family out there, so they moved to Germany. So my father was ten years old when they moved to Germany. My father was now going to take over where his older brother had helped his dad run the business. The oldest brother decided to leave and go to the United States. He worked in New York in the sweat shops. He decided New York was not for him. He had no money. He would work his way across the country. He would work on ranch lands and this and that, helping out at some stores. He eventually ended up in Oregon on sheep ranches helping out. He sort of learned the wool business. He moved to San Francisco and started to buy and sell wool. When my father was eighteen, his older

brother was already settled in San Francisco. My father didn't stop in New York, except to go through customs and immigration. He went right to California. He ended up arriving in San Francisco the day my uncle was getting married. My father was the only family member from my uncle's family to represent the family. My father had no place to stay, so he stayed on the sofa in my uncle and aunt's honeymoon period. That was odd.

[chuckles]

SF: In those days, when people didn't have any money, they all lived together and learned how to do. As my mother said, "Those were the good old days when you had nothing and you had to struggle and be close to each other." Eventually, my father had to get a job. I told you how he became a peddler and actually got his own business. That's how he came to California. The other brother came to California. They had one sister who was married with children and they got them out about 1938, early 1939—I was five years old—just before the invasion of Germany into Poland. They also got my mother's brother out and tried to get the rest of the family out. They got one sister, my mother's younger sister. She lost her other family. They didn't survive the war. They were lost in the Holocaust. But they got them out and my father's parents came out.

That was an interesting story, too, as long as we're doing stories of family. This is how life was in those days when Nazi Germany was rising. My grandfather who was living in Germany—the town was Dortmund—with this little store, a pawn shop, and their house. He was evicted to go back to Poland. He was not a true German, because he was born in Poland and was not a German citizen. So they sent him back. But they realized he didn't sign the papers to transfer his property to the German government. So he had to come back. To get him back, they sent my mother's sister who was in the family, related through marriage. They sent her to fetch him from Poland, bring him back to Germany. By the time they got back, the visas arrived that my father and my mother had sent to get his parents out and my mother's sister out. They got the visas, so they were able to leave Poland. They had to go to Genoa. They were on the last boat out before September 1 [1939] when Germany invaded Poland. They just escaped. I still remember at the train station when they came and we met them. It was really something. My [grand] father was very religious, had a beard and everything else. All of us were very secular by then. It was interesting to see them. My grandparents couldn't speak English. I couldn't understand them and they couldn't understand me. Nevertheless, it was a nice warm relationship. That was that story. It was how the family got close together, because of all of that.

LK: Was English your first language?

SF: Oh, yes. I only knew English. My parents would speak Yiddish at home, but only to themselves. They always wanted me to learn English. I never learned a foreign language at home in those early days. They only spoke Yiddish to hide something from my sister and me.

LK: [chuckles]

SF: My sister and I learned Yiddish only by listening to them, but we couldn't speak it. Even to this day, I only know very classical Yiddish phrases or words. I'm not very good at it. I can understand a little bit of Yiddish because of that experience.

Then, I decided I'd take German and I took Latin because I knew I wanted to go into medicine. I needed a second language so I took German because my father and mother still spoke German at home. They could speak German and I could at least take my lessons home and speak with my mother in German. So I learned German that way, also. But it was always English. I was never good at other languages even though I did get a scholarship in language from high school, because I had gotten all A's in language classes. But it wasn't natural for me. Math was more natural. Science was more natural. Language sort of came because, again, I had all that home environment so at least I understood Yiddish and German, which is very similar, which helped me.

PK: [*unclear*] *auf Deutsch machen* [*unclear*].

SF: Yes.

[chuckles]

LK: Moving forward a bit, at Berkeley you said you did a pre-med track. You knew that you had academic interests, research interests. Is that something you developed while doing undergraduate work?

SF: It was all the schoolwork, reading, passing exams, socializing, learning how to be a social person, date girls. I joined a Jewish fraternity. My [father's] older brother who came to California first, his son who was about eight years older than me, had gone to Berkeley and joined the fraternity. He wanted me to join his fraternity, so I did. I broke in socially that way. Then, of course, I socialized with all my medical school classmates after that when I went to medical school. At Berkeley, I just took all the classes. I always made sure I took a gym class every semester. I would take different sports each semester. I would take handball one semester. I took basketball a semester, volleyball a semester, weightlifting. I figured I needed to do some weightlifting. I took that a semester. I did swimming a couple semesters. Of the six semesters I was there before I went to medical school, I had a gym class every semester, at least to get some athletics. I liked the athletics. I just wasn't a great athlete, but I certainly enjoyed playing the sports. I even represented my fraternity on the handball team. Because I took that one semester of handball, I could play handball with everybody and help out the team that way.

It was just one of those things. I knew I wanted to research because, as I said, I was reading and that was my role models, the Gorgas model, and other models, too. I didn't read [Albert] Einstein then. I didn't read the great scientists at that time. I do today and listen to books on tape about some of these great discoveries. I just read a book about Johnny von Neumann [*John Neumann and the Origins of Modern Computing* by William

Aspray] and his discovery of mathematics and the developing the computer and everything and, of course, the atomic bomb, his role in it. Those are all fascinating. I probably was not meant to be a great physicist or anything like that. My skills were other places. These subjects did fascinate me. I did do pretty well with science. I always was driven by medicine and advancing medicine through research.

LK: Earlier, we talked about some of your later mentors, but I wondered if you had any mentors in medical school who guided you. It seems like your early interests were in biochemistry.

SF: I still remember the time when I guess it was third year in medical school and I was in a neurology clinic, of all things. Here comes neurology again. I guess neurosurgery helped out with neurology or maybe it was in neurosurgery, at that point. Edwin [B.] Boldrey was the leading neurosurgeon at the University of California-San Francisco. He was quite famous.

MO: Bob [Robert B.] Aird, was he there?

SF: Bob Aird was the chairman of neurology when I was there. But that's almost one of the reasons I didn't go into neurology...because I thought that he was a rather weak chairman. I'm sorry to say this in public now. It detracted me. He didn't strike me as charismatic. When he stepped down was when Bob [Robert A.] Fishman was hired and he made that department great. Bill [sounds like Gillespie]—something else—[enter correct surname if remembered] who did neurophysiology, I remember him very well, also.

I still remember seeing a patient and Boldrey put me through the paces and was quizzing me. I was a student. He said, "Tell me the foramina that the cranial nerves pass through." Struck me! Wow! A question out of the blue. I'd never thought of that. I said, "I don't know." He said, "Okay. Think of each cranial nerve and term and think about where it might go." So he made me organize my thinking starting with the first cranial nerve, second, third and, eventually, I was able to name all the foramina. I knew them from anatomy days, but when he hit with a question without thinking in an organized fashion, how to segment the answer... Once you segment, then it was easy and I was able to get the answer. I thanked him for that. It made me think more logically. He was a model. That's one reason I thought of doing neurosurgery instead of neurology, because of the great neurosurgeons they had at San Francisco. Then, I saw my senses when I came back East and realized that neurology will have a great future. And, of course, I'm happy ever since. I made the right decision.

LK: You did your internship at Philadelphia General Hospital, correct?

SF: I did. First of all, that's a great hospital for general medicine training. Again, because I couldn't decide which specialty I wanted to do, I decided to apply to several different internships. One I applied to was a straight surgical internship thinking I was going to go into neurosurgery. I quickly squashed that one. I didn't sign up for that one

because I wasn't sure I really wanted to do neurosurgery. I wanted to take a general rotating internship and I looked at several good rotating internship hospitals.

So I drove east one summer between my third and fourth year with my medical school roommate who actually became my best man at my wedding. We drove across country. I got myself a scholarship. There was a mentor, a head of medicine, in our third year of medical school who was at San Francisco General Hospital. What was his first name? [Leon] Goldman was his name. He was a great intellectual giant. He eventually went into neurochemistry but he did medicine. He would meet with students in informal discussions. He talked about Woods Hole [Massachusetts] and the Marine Biological Laboratory. So I signed up for the cell physiology fellowship and I got a fellowship to go there in the summer between my third and fourth year of medical school. They had summer vacations in those days in medical school. I had a car. I drove back east with my roommate. He was going to go on to French Canada and learn French for the summer. I was going to go to Woods Hole. We decided to look at several places along the way, hospitals: Denver General, Minneapolis General. Then, I dropped him off and I looked at Philadelphia General, as well.

I decided that would be my number one choice and I got in. It was what I call a doctor's hospital. It was really an intern's hospital. Interns ran the hospital. They did everything. We would rotate from specialty to specialty. The five medical schools in Philadelphia had five departments of medicine: one by the University of Pennsylvania, one by [unclear], and one run by Jefferson, and so forth. You would be assigned to one of those when you went through medicine. You might have several sessions of medicine. So you'd get to meet the different attendings and so forth. But it was really that all the interns fell together at Philadelphia General. We called it ["Old] Blockley." That was the colloquial name for the place. They had tennis courts. They had doctor's quarters. You had to sneak your wife in. There was a matron out there who wouldn't let women into the building. So somebody would call her attention away and the woman would sneak in to the rooms of the doctors, even when my wife would come to visit. It was one of those fun places like being in college again. It was good.

I met my wife that summer I went to the Marine Biological Laboratory in Woods Hole, Massachusetts, Cape Cod. It was the first time in my life I had shellfish. It was so wonderful. My parents were a little bit kosher, probably a lot kosher early on, less so later on in life. So we never had that at home. I learned to eat that. I loved it. I still love it to this day. I didn't finish my research project that I started. I needed about another week to finish it. My mentor in the research lab at Woods Hole was a professor of biochemistry at NYU [New York University]. My aunt, the one that came out of Germany on the last boat, was living in New York with her family. I had a place to stay. So I came to New York for a week to finish my work in the daytime there. In the meantime, one of my bunkmates was a Columbia student. I asked him for the names of a nice Jewish girl in New York, that I was going to be in New York for a week. I got Charlotte's name and I called her for a date. She said, "No, I'm working. Could we go on a weeknight?" I said, "Just for coffee." She accepted. We'd go for coffee. I liked her very much and I took her out every night but one night that whole week. Then, we

corresponded and, finally, she came out to Berkeley. She was a student at Barnard [College]. She was in her junior year. She would come out and do her last semester of her junior year at Berkeley, transfer, and we got married that June [perhaps, Doctor Fahn would like to insert the year here]. I was a senior already in medical school graduating. I took my boards on our wedding day. The wedding was at my parents' home. Her family came out. My family was there. My medical school classmate was my best man and another good friend was holding what we call the *chuppa*, the wedding canopy. My wife and I went on a honeymoon for about two days camping in Yosemite National Park and trying to climb the sides of the streams with all the water spraying on you. It was fun.

Then, I drove back east in my car to get to Philadelphia to start my internship. In the meantime, Charlotte had to do her senior year at Barnard. In that first semester, the fall semester, she stayed in Brooklyn with her parents. She was living with her parents before that. She never lived in the dormitories at Barnard, always commuted by subway. She would come in on the weekends that I was free. On the weekends that I was not on call, she would come down to Philadelphia. We rented an apartment. Then, on the second semester of her senior year—that was the last semester therefore as my internship—she moved into Philadelphia and would take courses three days a week. Monday, Wednesday, and Friday, she worked on her last semester schedule. So I would get up very early, like four or five in the morning, drive her to the train station. She would take the train from Philadelphia to New York and, then, the subway to Barnard and, then, come back by train. Some of the nights if I was on call, she might stay in Brooklyn with her parents. So Monday, Wednesday, and Friday, she would go to classes, but the rest of the time, she was in Philadelphia.

The reason I got my residency—that's another story; she helped me with that—is once I decided I wanted to go into neurology... That was at the end of my first rotation in psychiatry when I realized the brain was really for me after all and I would go into neurology. Then, I decided where should I apply for neurology training? Of course Houston Merritt was well known. There were two programs in Boston. There was the [Derek E.] Denny-Brown program in Boston City and Ray [Delacy] Adams' program at Mass [Massachusetts] General. I applied there. They said they liked me, but they wouldn't take me unless I did another year of internal medicine. I didn't want to do another year. I knew what I wanted to do. I wanted to go into neurology. I tried to tell them that. I had all this internal medicine, and I also had OB-GYN [obstetrics-gynecology], and orthopedics. I knew how to deliver babies. I had a general thing. Houston Merritt asked me the same thing when I interviewed with him. I explained. I said even when I was on OB-GYN, there were pregnant women who had epilepsy and I had to know how to take care of their epilepsy. So I had a broad education, good advanced knowledge to go into neurology. And he took me. I also applied a couple other places like [Johns] Hopkins and Michigan, but I decided Houston Merritt's was a good place.

How did I even get in there in the first place? I looked at the place. I went and visited Columbia-Presbyterian Medical Center. I went into the Neurological Institute. I looked on the floors; there were no security guards or anything in those days. There was a first-

year resident seeing patients on the weekend on one of the wards. I introduced myself. I still remember it was Donald [L.] Schotland. Donald eventually went on and became an expert in the pathology of neuromuscular disease. He's a specialist in that field. He went to Penn with Doctor Rowland and stayed at Penn ever since. He was already a resident. I was an intern. He said, "In order to get accepted into the residency program, you're going to have to an interview. They're not going to take anybody that's not interviewed." But I was never invited for an interview. I said, "How am I going to get an interview?" He suggested or maybe I suggested—I can't remember now—I'd have to find a way to get an interview. So I asked Charlotte after one of her classes at Barnard, if she would take the subway up to the medical center and go to Houston Merritt's office and say her husband has applied. Could he get an interview? She did that. We were naïve in those days, you know. Houston Merritt was the dean of the medical school. She went to the department's office in Neurological Institute. He wasn't there. He was in the dean's office. But the secretary was there. I know this story later when she told me about it and, then, Bob Fishman told me about it, also. Bob Fishman was in there chatting with the secretary. The secretary's name I remember was Betty Fisher. She stayed as Merritt's secretary till he retired. She says she ran the department. In other words, nothing gets through without going through her, like a good administrator. [chuckles] Fishman said, "Pull out the folder." So she pulls out the folder and hands it to Fishman. Fishman looks through it and said, "Yes, give him an interview." So I got an interview. I did well in my interview, I guess. I got in. Then, of course, when Bob Fishman was asked to be chairman at UC-SF, there I was at UC-SF, a medical student graduate, he asked me about the program, and I explained about Bob Aird and how weak it was. But he really would turn it around. He and Doctor Rowland were good buddies. They were junior faculty at that time both doing research. They both trained under Merritt as residents and Merritt kept them. So Bob went on and made a great chairman at UC-SF. He made it the strongest neurology program in the country, even to this day. Rowland eventually did Penn and, then, Rowland, went back to Columbia and did Columbia. That's how I got accepted and while there, I knew I wanted to do research when I finished

I applied to NIH for research training. Then, I got my job, as I mentioned, in part one of this program. Melvin Yahr, who was one of my attendings, was a great attending. He was a very good neurologist, very good. I learned a lot from him. He did the first double-blind trial on L-dopa as it turned out and was doing that study while I was there. When I went to Penn with Rowland [unclear] L-dopa here, too. One of the private attendings, Gabriel [A.] Schwarz, already ordered some L-dopa out of his pocket. He would buy the L-dopa and we would go to a pharmacist who would package them into capsules. We would sell the capsules to the patients. That money would be turned into the pocket to buy more L-dopa. And we got more and more patients. All the patients that came to the hospital, we'd put in the fund to help research on Parkinson's disease. Gabriel Schwarz didn't take any money out for his private practice or anything. That was very good. The first paper [Schwarz GA, Fahn S: Newer medical treatment in parkinsonism. Med Clin No Am 1970;54:773-385]. [Lauren, this citation needs to be affirmed as correct. Thank you.] I wrote on L-dopa was with Gabe Schwarz. Then, of course, we did a few other studies while I was still there at Penn before going to Columbia.

LK: You brought up your research at NIH. I didn't know if you had any more to add to what we discussed earlier about how that research impacted your career.

SF: That's a good story, also. I knew I wanted to learn research. Which war was going on? Was it the Korean War?

LK: The early Vietnam War?

SF: I know what it was. The Korean War was going on when I was in college. I got into the doctor's draft, so I had a deferment until I finished medical school. Then, I became eligible for the draft. Since I knew I wanted to go into research, you could do research at NIH and be under the [United States] Public Health Service, a uniformed service. That serves as your military obligation. So I signed up to go to NIH. But I didn't know that at the time. I wasn't really aware of that. I decided what I really needed to do in research was learn biochemistry. I needed micro-biochemistry. I needed to do small pieces of tissue maybe from biopsies and other things so I could analyze them. The master of this was a biochemist [Oliver Howe Lowry] at Washington University-Saint Louis. And assay for protein was one of the things that made him famous and the most cited published authors in the world for the methodology of protein assay. I wrote to him. He said, "Well, you're in New York. One of the persons who trained with me is already a neurologist in New York. You ought to talk to him, Gerard [M.] Lehrer." Gerard Lehrer is a member of the Academy and I'd see him at these meetings. I don't know if he has passed away by now. [Doctor Lehrer died April 12, 2013] He was attending neurologist at Mount Sinai Hospital and Medical School in New York. So I came to him and chatted with him. He gave me an idea. He said, "If you want to learn micro biochemistry, another trainee from Saint Louis is now head of the neurochemistry labs at NIH." I think his lab was called Enzyme Chemistry or something. His name was [R.] Wayne Albers. "You should go to him and maybe you can get a fellowship that will satisfy your military and at the same time learn your biochemistry."

So I went to Wayne. He hired me. I got some papers in the *Journal of Biochemistry and Science*. I was doing pretty well.

In fact, that was why the head of physiology [given and surname?] at Duke [University] wanted to hire me to come to Duke. I told him I [unclear] neurologist. So he got Duke to put me in two departments. I would have a joint appointment in physiology and.... He was as sodium pump man. Eventually, he went to Harvard and became the dean of the medical school at Harvard. I've been close with him ever since. Ironically, he got Parkinson's. By the time he did, I was already a professor. He came from Harvard to come to see me at Columbia to help him with his Parkinson's. Again, how the circle goes around. He sees me give a presentation at a biochemistry/physiology meeting on the sodium pump. He was expanding. He would take me in and I would be in neurology. Of course, I declined. I visited twice and I just didn't like the south, and segregation, and all the other things.

Then, when Mel Yahr came around and offered me the job at Columbia, I decided to do that, even though I wasn't going into Parkinson's at that time. Again, just how fate works. I turned into a Parkinson's person, after all. I got in to Wayne Albers. I did biochemistry. I did reasonable well for the time I was there.

Eventually, I switched over more and more from the lab researcher—I did that for a while at Columbia—to patient care. I couldn't leave the patient's, you know. The more patients that wanted to come to me, I did less time in the lab and I had to have my lab assistants do it. I made my fellows go to the lab, at least initially. They did very well. I have a lot of excellent lab people who were former fellows. So I just got into clinical because there was a need and it sort of came back to me. I knew how to treat people and get them better and that worked out pretty well.

LK: You became more interested in neurology and specifically found Parkinson's really interesting, I wonder if you could talk a little bit about classification and specialization in neurology in that period, what the field looked like.

SF: Even as a third year resident, the third year residents were asked to give lectures to the nursing students. It was sort of serendipitous, in a way. I was assigned—quote—extrapyramidal disorders, so I had to read about it and organize my thinking. I gave a lecture. I probably still have the notes today of that lecture I gave to the nurse students. Everything was known as extrapyramidal disorders and there were papers about this. I think [Samuel Alexander Kinnier] Wilson coined the term of extrapyramidal disorders in one of his lectures. He was the one who discovered Wilson's disease. So that was a term used for movement disorders. It's still used today. A lot of people still use the term extrapyramidal disorders. But it's really a misnomer. I don't want to go into it, but I've written about why it's a misnomer. It's better to use movement disorders as the term. Not everything is from the extrapyramidal system. The extrapyramidal system influences the pyramidal system. Everything goes through the pyramidal track ultimately.

I didn't know I wanted to do that. In fact, I was across the hall in Wayne Albers' lab at NIH from... I'm blocking on his name right now. [If remember, perhaps, Doctor Fahn will want to enter his name here.] He eventually went on and got the Nobel Prize for the metabolism of norepinephrine and epinephrine. I would chat with him. He brought in a lot of research fellows at NIH into his lab. I met a lot of these guys. They were all in the monamines and I wasn't. I was working in sodium ATPase.

When I returned to Columbia to work under Mel Yahr's research group, I still was doing sodium ATPase. It wasn't until I heard that lecture by Hornykiewicz that I began to really investigate monamines, first the biochemistry of the basal ganglia and, then, when I went to Penn with Doctor Rowland in 1968... After three years at Columbia, I went to Penn for five years. I had my own lab and research grants. I developed an assay method for how to measure levodopa in the blood. I would measure patients with levodopa, how it goes up and down and the other metabolites from dopa and I would correlate clinical outcomes with the blood levels. I was doing a lot of what we call today pharmacology.

So I was doing biochemical pharmacology. I did a lot of developing of assay methods then.

I started movement disorders. I gave the term to it [unclear] Doctor Rowland. I started giving lectures at Academy meetings. I was invited to give a course in neuropharmacology. There was a course in neuropharmacology every year at the Academy. Like there was a course in neuroanatomy, a course in neurophysiology, there was a course in neuropharmacology. It was run by [Thomas N.] Tom Chase, who was at NIH. Tom Chase was already into L-dopa. A lot of the subjects that people spoke about in this course at the Academy were really related to monamines and dopa and things like that. When I was asked to give that course, I said, "Yes, but the topics I want to cover are more than just neuropharmacology. I want the name changed to movement disorders so I can cover not only Parkinson's but dystonia and other movement disorders." I had a five-year cycle of covering all the movement disorders in five years of the course that I would be allowed to do. So movement disorders stuck. That's how the name hooked in. Eventually, I did the unusual movement disorder seminars. So movement disorders became a name itself and, then, eventually, a subspecialty.

I didn't start out in Parkinson's, as I said. It sort of grew because of Hornykiewicz and, then, I went to Penn. It was at Penn that I began to formulate. I was running my own clinic, how to classify movement disorders. Eventually, I saw people with dystonia and learned how to use high doses of anticholinergics to treat dystonia, how to use other drugs to treat choreas. I had my own patients. I was now my own boss, in a way. I was able to take things from the literature and my own experiences working with L-dopa, what was happening to these patients, and figure out how to do this. Then, I began to help classify things. I was asked to write an article, I remember now, for the Neurological Clinics of North America, on tremor. Well, I read about tremor and I couldn't find any great chapter. I had to come up with my own classification using whatever I had in the literature to help me. By being forced to do this, I was able to think about it and, therefore, write it up. A lot of these classifications get changed over time, get superseded by newer classifications, but, at least, it's a start. So I began to classify a lot of different movement disorders along those lines. I still have the old talks and slides about the different classifications of the tics, of the choreas, of the dystonias, and all these other things. That was just part of being a clinician. You just had to know how to categorize.

How I met David Marsden... I don't know if you're going to ask that question, but let me tell you.

LK: Yes.

SF: At Columbia, Lucien Côté and I were doing the biochemistry of the basal ganglia with Mal Carpenter. We were invited by André Barbeau to give a presentation at the International Congress of...I don't know if it was called Basal Ganglia or Chorea. He talked about chorea. Cotzias was there, too. He showed us videos, in fact. Lucien and I presented our work on the biochemistry of the substantia nigra, because we were now

getting all that data together and presenting it. It's still published in a chapter in a book that Barbeau published.

After that talk, David Marsden came up to us, introduced himself. He was a couple years younger than me, but he'd already been working on the substantia nigra. He was fascinated with the substantia nigra. When he was a medical student to get his thesis, he wrote a paper on the phylogeny of the substantia nigra. He showed that in small animals, you see the small nigra and it gets bigger and bigger as the animals phylogenetically mature, develop. It got only in certain animal species pigmented. Humans might develop it, but also in some elephants that live a long time, maybe some horses. He came up with the hypothesis that pigment was a result of some degeneration and it was a dumping ground, a garbage pit. It was sort of dead tissue and wasn't doing anything. Yes, they knew about degeneration in Parkinson's, but they didn't know what it did. That's when we presented our paper that this was a hotbed of activity. It made more dopamine than any other nucleus in the body, not that it's stored there. It's stored in the nerve terminals. The enzymes [unclear] there. David came up to us. I remember even that it was in Montreal [Canada], the Congress. It was the time of the World's Fair. It must have been 1967 or something like that. [1967 is correct]. The three of us, Lucien Côté, David Marsden, and I went to the World's Fair together that night after we met. I remember riding the roller coaster with them. That was a harrowing experience, the thrill of it all. [chuckles] We had a good time. Dave and I have been friendly ever since.

David was very maturing very rapidly in terms of his science and his skills. He was really super human in terms of his abilities in clinical medicine and neurology. He was already appointed head of neurology as a young man at the Psychiatric Institute. Then, he was asked to be the chairman of neurology at Queen Square. When he was being appointed to this position at the Psychiatric Institute, his labs weren't ready yet. He had some time on his hands and he came to the United States and visited different people. He visited us at Penn. I was at Penn and I had my clinic. He came with me to my clinic and here all these different movement disorder patients, some with Parkinson's, some with dystonia, some myoclonus. He and I would talk about the phenomenology. What are you going to call this movement? What's this? We started to agree with each other that this is what we're going to call it.

Then, when I got the chance as Education Committee Chair to suggest an evening course on unusual movement disorders, I took movies of those. There were no videos, just movies, super-8 movies, no sound. I was recording all of my patients in the clinic and Dave was starting to do his. We would show our tapes of unusual types of movement and how do you classify this. We brought a whole generation of younger people into the field of movement disorders with this classification scheme based on the movies we showed. We actually had film. Ultimately, we converted those into videos when videos came into being and supplanted the movies. We had movies in the original days. I had to splice them together, clip them out. It took a lot of time.

That idea for the unusual movement disorder seminar, which I said ran twenty years... It's still going on by other people. They would run all night when we did it. The reason

it started is when I got to Columbia from Penn in 1968, I was taking home movies of these people, and I decided it would be very interesting if the neurologists in New York might be interested in this field. Again, there was no field called movement disorders, at the time. There were several people around including Mel Yahr and a few other people that I recognized who were using L-dopa. Yes, by the time I got back to Columbia, it was 1973. L-dopa was out. So I invited them to my home once a year for an evening movie session. My wife would make a big salad. I would spend my day making French bread from scratch and chili con carne. We'd sit them down in our dining room, all these guests, and serve them this food. Then, we would break into the living room. I had a screen set up and a movie projector, and we would show mostly my movies, and we'd talk about these cases.

One of those years, I invited Oliver Sacks. He had his *Awakenings* [published in 1973] already and he had movies of his awakenings. So he volunteered to bring his 16-milimeter film of his patients that he took and he showed it. It was all on movie reel about his awakening stuff. Oliver and I have been friendly ever since, not close friends but close enough. I would go to his birthday parties. He would come to my house every once in a while. Unfortunately now, he's dying of cancer. His last book just came out, *On the Move [: A Life]*. It's his autobiography. I just got a copy about two days ago from him. We're going to his book publishing to view—I think in a couple weeks from now. I don't remember the date exactly—in New York. He was able to finish his book. He was given a short time. He has malignant cancer. He was able to get that done. Anyway, Oliver came and showed his thing. That was the idea, so I was doing that every year.

Then, at the Academy meeting, I decided with the Education Committee why don't we do sessions in the night, dinner sessions or non-dinner even, especially to show case reports and we would do the one on unusual movement disorders. That's how we started it. The Education Committee didn't want to have it open ended. They said, "Two hour time zone, only, thirty people, and six people because it's case presentations and you want to have discussion." Well, people wanted to come in and more and more people came in a crowded room. The next year, okay, let sixty people come. Let a hundred people come. Pretty soon, they ended up saying, "Okay, we're not going to put a lid on it. We'll still allow a lot of discussion, even though there are two hundred people in the room." We had a wonderful evening of exchange talking about patients. People brought their own video tapes or movies and showed them and we all discussed them. That's how movement disorders sort of took off. It was David and I bantering back and forth about how we disagree or agree on cases and the audience disagreed or agreed, also. It was one of these nice give and takes. We all learned from each other.

LK: Do you recall when those started at the Academy?

SF: It must have been around 1981 or so, I'm thinking. We can always check the records.

LK: Sure.

SF: David died in 2000. I did two more years after that. So maybe it was 1982 to 2002. I did twenty years before I stopped.

LK: We touched on this earlier. I didn't know if you had any further comment on it. At one time neurology was known as a "diagnose and adios" profession. I wonder how you viewed your role in entering neurology, what you thought about the therapeutic options available, and how you viewed what you wanted to do in the field.

SF: Yes, that's a very common expression, "diagnose and adios," applied to neurologists, and it had that reputation. That's why I wasn't sure I wanted to go into neurology, maybe go into neurosurgery. My colleagues and some of my professors were saying, "They don't do anything in neurology. Go into neurosurgery if you like the nervous system." Again, it sort of jelled in my first rotation as an intern. I'm going to do research anyway. This is a perfect field because nothing can be done. Maybe I can help get something done. It was a *wonderful* opportunity because there was nothing there. I looked at the other way. Instead of a glass half empty or half full, I looked at it as it could be filled up, not a doomsayer, that nothing could be done. Don't do it. No. This is a wonderful opportunity because nothing was available. Let's go do research. Let's figure something out.

Then, of course, the dopa story broke. That really broke everything open in neurology also. There was some epilepsy. Houston Merritt, in fact, became famous because he was the first to have a drug, Dilantin, for epilepsy. He did that when he was at Harvard before coming to Columbia. There was some stuff going on. That was in neuropharmacology with Tom Chase and [unclear]. There were a few drugs that were available for treating myasthenia gravis and some for epilepsy. There was Artane and things like that for Parkinson's, and, then, eventually, L-dopa. That opened up the field of movement disorders because you saw L-dopa caused chorea, caused dystonia. I had a patient with dystonia [unclear]. I could tell you that story, too. By this association of one disease with another, we came up with the idea of movement disorders as a group.

I lost track of your question again.

LK: How you viewed the therapeutic options and your role.

SF: Oh, yes. I felt there was no therapy. This was a way to get therapy. Still to this day, I think movement disorders [unclear] therapy. Fortunately, some therapy now may be quite good therapy in some cases. We haven't cured Parkinson's even now. We've gotten them a lot better. So there's a lot more to be done. It does show that things can be done.

LK: During your residency at Columbia, I wonder if you could talk about the environment there and maybe future leaders in neurology that you trained with, their influence on you and collaboration.

SF: One of the reasons I wanted to go to Columbia was Houston Merritt was a great man. He was already the editor of the *Archives of Neurology and Psychiatry*. Of course, he was already the dean. He had one of the first American textbooks. There were a few other textbooks before, but Merritt's textbook was already in like the third edition by the time I was a resident. He was a great man. He's not very voluble. He's very quiet. He really only talks when he's asked a question. He doesn't speak spontaneously. He's a reticent type of person. He's also a very great poker player. He knows how to extract money out of rich people.

LK: [chuckles]

SF: There are a lot of sayings about him. For example, one of his famous sayings—you'll probably hear about this a lot—is when one of his benefactors say, “What do you feel about tainted money? Would you accept it for your research group in your department?” Merritt replied, “The only bad thing about tainted money is there taint enough.”

LK: [chuckles]

SF: In other words, however they got their money, if they want to give it for research, we'll take it.

Merritt was a great clinician and a role model. We all stood up when he walked into the room. All the professors would come to him and show him tough cases, including the neurosurgeons. He would say, “Yes, go ahead and operate. This is where the lesion is going to be.” These were the early days when we didn't have the kind of imaging we have today and things like that. He trained all these people. I already mentioned Bob Fishman and Bud Rowland, but there were others. Jim [sounds like Ham-el] was a great mentor for me. He was a great clinician. He was not a researcher but he taught us a lot about neurology. [sounds like Are-min Buh-cal-ee], [sounds like Dine-uh Share-uh], Mel Yahr... [Sidney] Sid Carter was there. Sid was also a president of the Academy. So was Bob Fishman and Bud Rowland. Mel Yahr was a president of the ANA [American Neurological Association]. Houston Merritt was a president of the ANA. There were a lot of great people that Merritt trained. There were wonderful teachers. I learned a lot. I learned from my peers. There were very good residents. I learned from them, as well. To keep up with your peers, you'd have to study hard and do well. It was a very good environment. They encouraged you to go into research or do anything you want. Merritt was a very generous kind of guy. He was the kind of guy who grew up in a small southern town [Wilmington, North Carolina]. I don't know if he was Baptist or what. He excelled in medical school. I think he went to Johns Hopkins Medical School. Then, he did his neurology at Harvard and became a professor there, eventually.

Again, going back to the Nazi era, he brought a lot of refugees in and brought them into the department. He was criticized for bringing too many Jewish professors in at Columbia, which was thought to have some Aryan views of its own at the time. Houston Merritt defended [Harry] Grundfest and [David] Nachmanson, a great physiologist and

chemist, and many others. A great immunologist came. They gave him an appointment in the department. He was the guy that discovered gamma globulin for MS and CFS [chronic fatigue syndrome]. He only did the CFS work because he was in the neurology department. No one else would give him a job. Elvin [A.] Kabat, was his name. They were all immigrant people that did extremely well.

Neurology at Columbia wasn't just clinical. It was not neuroanatomy so much, but neurophysiology and neuropharmacology. Merritt made a big department out of it. It was a great environment. It was very open minded. I had a lot of great professors. Even the head, the guy who resurrected [unclear] dystonia was there, Ernst Herz, another refugee. He was in private practice but he would come and give attending rounds at Columbia. I remember I went on rounds with him when he came one day. He wasn't my attending, but I decided to follow him around and see his cases. I remember him from that. There were a lot of people who were teaching on the rounds with us were guys in private practice that Merritt would allow to come in and help with the teaching. Some were practicing psychiatry. They're called neuropsychiatrists. They did a little bit of neurology, but most of their patients were psychiatric patients, but they still loved neurology and wanted to be in neurology, but there was no way you could make a living being a clinical neurologist in those days. You had to do some psychiatry. That's why the boards of neurology and psychiatry are together still. All the neurologists were psychiatrists. Eventually, of course, they did separate out and the boards are more or less separate but not entirely so. You still have to do psychiatry boards if you're doing neurology and vice versa. If you're a psychiatrist, you have to do neurology boards because of that link way back. That link is coming back again. Neuropsychiatry, what is it? It's mostly neurology, the mind instead of structure, a lot of it: schizophrenia, depression, all those other things. They're all diseases of the brain.

PK: [unclear] [Wilhelm] Giesinger which wrote that in 1845 and he was right.

MO: When I took my father's oral history, he tells a story that you got into Columbia for dental school if you lived in the Bronx. The dean would not honor his letter because there was a quota for Jews in that day. It was sort of anti-Semitic. Did you face anti-Semitism during your career at all?

SF: No, I never did. There was supposedly a lid at UC-SF when I went to medical school. But it turned out twenty-five percent in my class were Jewish. The Jews among themselves said, "That's the cap, because they're not going to take more than that." We felt we were the lucky ones that got accepted. I don't know if there's a cap today at all. I know right now if there's any cap... There's a lot more Asians living there now. They're the brilliant ones. They're ones that are in medical schools. There may be a cap on them for all I know. This kind of stuff goes on everywhere. I didn't feel it at Columbia. I know people like Mel Yahr sort of felt it and others, but I never did. One of the great things about Merritt is, despite never having grown up with a Jewish person, he just recognized talent and he just grabbed them. If they were there, boy, he was going to take them. That was very good we had a guy like that around. So I didn't feel it myself. I was taken I thought because I was able to do well.

I did well in medical school, the sixth highest grade point average in the class—maybe it was higher than that. Maybe I was second. It was pretty high. I was not the first. I was one of the six people nominated by my classmates for the gold-headed cane. That was an award given to the most promising physician in the class to graduate, who cares about patients as well as who would do well in research or whatever else. I was one of the six. I didn't win it. I was asked that at my interview, because one of the letters that came from UC-SF, one of my interview letters, was quoted by, I think it was, Jim [sounds like Hamel]. He looks at the letter and said, "What is this gold-headed cane?" I said, "I didn't really win it. So I \didn't mentioned it in my attributes." He said, "That's okay, but you were still nominated. That's an attribute." He sort of perked me up a little bit. Anyway, maybe that helped me get in, accepted to Merritt's residency program.

MO: By the way, there were more American Academy of Neurology presidents from Columbia University than any other place.

SF: Yes.

MO: Do you think that was a Houston Merritt...?

SF: Oh, yes, they're all Merritt's graduates. Merritt himself was never president of the AAN. He was an ANA guy. He wasn't the founder of the ANA. Of course, that's an old organization, but he was a leader. He used to sit in the front row with all Denny-Brown and Ray Adams, those guys. None of them were part of the AAN. The AAN was a new, Middle Western, young Turks organization.

PK: Competition in the early years.

LK: [chuckles]

SF: The AAN really took off. The ANA was leapfrogged over by the AAN. They're still very good. Sid Carter, Bob Fishman, Bud Rowland, and maybe a few others were president of both. And Timothy A.] Tim Pedley was president of the ANA as well as the AAN.

LK: I know you went to the University of Pennsylvania with Bud Rowland. I wonder if you could talk a little bit about how Penn compared to Columbia, your experiences there, the atmosphere, research and clinical work.

SF: When you're in New York, you think there's no place like New York. You probably know the cartoon—I forgot the name of the artist—where New Yorker's view of the world is they just see New Jersey across the river and California and that's it.

[chuckles]

SF: Certainly, when you went to Penn, it was opening your eyes up to how good neurology could be. Philadelphia was an old neurology town. It had *excellent* neurologists. When I rotated through neurology, my attending was a practicing neurologist who had actually trained at New York Neurological Institute. He was very good. They had good people at Penn in the Neurology Department, good Internal Medicine Department. You could see a lot of portraits of famous doctors and a lot of them were from Philadelphia. It was an awakening that there was more than just New York.

We loved Philadelphia. It was very hard for me to leave. When Rowland was asked, he told me when he came back from giving boards that he was being invited to be interviewed at Columbia for possible advising about the department. I said, "I know what that means. They're going to ask you to be chairman. Don't go. You're doing a good job here. I like it here." But he was asked to be chairman and he asked me to go with him back to Columbia. I had a hard time deciding. I couldn't decide. I really was growing. I had my own lab, my own clinic. I was doing well. I was doing courses at the Academy already. I was already getting involved with the Science Committee and, then, eventually, the Education Committee.

What led me to accept Rowland's job? One was money. I asked for three things. What he offered me on his own was... They had the support from William Black and the Parkinson's Disease Foundation and they had this endowment fund. I would be able to control the income from the endowment fund and have that for research. That was very attractive. Then, the three things I asked for... Mel Yahr was going to leave. He was the head of the Parkinson's unit. He was offered the job as chairman of neurology at Mount Sinai. So he would vacate his office. I said, "Could I have Mel Yahr's office?" "Yes." "Could I have Mel Yahr's parking space?" That's right there in front of Neurology. It's a very prized possession to have a parking space in front of the building. That was one thing and the other one was the endowment, those three things. I got them all and I accepted, finally, to go to Columbia. With the endowment, I was able to build up other... I had the labs there. We had more lab space. I had the scientists. I was able to put together a grant and do other things there. It worked out very well. Then, I became involved with the Parkinson's Disease Foundation, actually as scientific director. [unclear] some of the research ideas for that foundation, what it should support. All of that was for the good.

LK: What led you to return to Columbia?

SF: I returned because Bud Rowland was asked to be the chairman and he decided to bring five people with him. One was me. I went because of those three things [mentioned above]. Going to Columbia were those three demands, not for going to Penn.

Why did I go to Penn? I should have answered that first.

LKJ: Oh, okay.

SF: Why did Bud Rowland go to Columbia? Bud Rowland went to Columbia in 1967. He and G. Milton Shy were brilliant neurologists who came from Denver, then to NIH and then to Penn. Maybe it was to NIH and then Denver and then Penn, whatever. He built a strong built a strong department at the University of Pennsylvania. He was in neuromuscular disease. Bud Rowland was in neuromuscular disease. Milton Shy had this great reputation. When I was in my neurochemistry fellowship training with Wayne Albers, I would go to Saturday morning rounds every Saturday with Milton Shy. Milton Shy, the way he teaches—the Socratic method—is he would ask questions. He would point to a guy and give his name, “Answer this.” People wouldn’t know it and he goes on to the next guy till somebody gets the answer embarrassing all the people who failed before hand. I was, fortunately, one of these guys who could get some of the answers right. So I got to know Milton a little bit.

When Houston Merritt stepped down, his replacement was the head of NINDS [National Institute of Neurological Disorders and Stroke]. I just about got his name. [If recalled, the name could be added here.] He took over, but he was a very weak chairman and people were leaving. They decided to have a review of the department. Bud Rowland was asked to be one of the reviewers. I knew what that meant. They would ask him to chair it. Sure enough, they did ask him to chair it. That was from Penn. I know what it was. I’ve got my signals wrong. He was asked to step down and Milton Shy was brought in as chairman from Penn. Bud was going to leave and go to take the chairmanship at the University of Washington in Seattle. Milton Shy said, “Look, you leave your equipment in your lab and I’ll leave my equipment in my lab. You take over the chairmanship at Penn. I’ll take over the chairmanship at Columbia. They made a switch. So Rowland went to Penn in 1967. Milton Shy came to Columbia in 1967. Rowland said, “Once I get settled, I’d like you to come to Penn with me.” I said, “Well, let me think about it. Milton Shy has just started. Let’s see how we do.” I remember being interview by Milton Shy. He survived three weeks before he died of a heart attack [on September 25, 1967] of a heart attack at Columbia.

LK: Wow.

SF: He was starting to interview all the people. He remembered me from NIH. He asked me what I wanted to do. He asked me did I want to be a chairman. I said, “No, I never want to be a chairman. What I’d really like is to control my own lab, have my own source of money, be independent.” That’s the way I still feel today. I was looking forward to working with Milton Shy.

Then, he died three weeks later. Roger Rosenberg, who became president of the Academy and is here at this meeting, was chief resident at the time that Milton Shy died. There was another president of the Academy from Columbia, by the way. Milton Shy made a morning report every morning with the residents to the chief resident waiting in the neurological library. Milton Shy’s office was right there, but it was locked. It was late. Why isn’t he here? So they were worried about him, but the door was locked and they didn’t want to disturb him. Finally, time was going on and he wasn’t there. They

called the guard at the front desk to come up and unlock the door. They came in and there was Milton Shy dead on the floor. It turned out to be a massive heart attack. He had a family history of heart disease.

Houston Merritt was still dean of the medical school. He had to come back and be interim chairman again. That's when they were looking for another replacement. That's when they got the guy from NIH. I'm blocking on his name again. He was chair for five years. Then, when he was being reviewed, Rowland was already at Penn. He went to be one of the review panel members of Columbia's and, then, he was asked to be the chairman. That's when he wanted me to come and those were the stipulations. After Milton Shy died and I wanted to see how the new chairman was, I decided why don't I just go to Penn and work with Bud Rowland again. He wanted me to be his associate director of the research unit at the NIH labs there. I accepted that job. My relation with Rowland was really close after that. Then, when he went to Columbia, I went with him for the reasons I said earlier.

I got a little mixed up. I'm sorry about that.

LK: I got mixed up, too. No problem.

I have that you became director then of the Parkinsonism and Movement Disorder Division in 1973.

SF: Right.

LK: I wonder how you viewed that leadership position, what kind of environment you wanted to create, what your goals were.

SF: Just like at Penn, when I was asked to run the Parkinson's clinic, I said I don't want to call it Parkinson's Clinic anymore. It's got to be called Movement Disorders. When I went to Columbia, it was still the Parkinson's Clinic. It was Mel Yahr's clinic. Right before Mel Yahr was [Lewis] Lew Doshay, who wrote one of the early textbooks on Parkinson's disease. He was one of my attendings also at Columbia when I was a resident. I came back. I changed the name. I kept the clinic where it was: Tuesday afternoon. I changed the name to Movement Disorder Clinic. I would go to clinic and I would take the senior residents. They would come to the clinic. I would teach with them. Lucien Côté would go to the clinic and a couple of other people. Then, I would make rounds after that and see the movement disorder patients in the hospital with all the other residents and attendings. All the cases they wanted me to see, I would do that in the afternoon after clinic. Eventually, it grew. I got some money from a wealthy patient. I started a fellowship program and, then, I got fellows. I decided that if we would give a name to our center, we'd call it Parkinson's Disease and Other Movement Disorders. I used the word Other Movement Disorders, not just Movement Disorders, because Parkinson's is a movement disorder. I wanted to make sure it was all inclusive but have the name Parkinson's in there.

We renovated the space we had. I raised money from patients. I raised money from different foundations, as well, including the Parkinson's Disease Foundation. We also renovated the labs. I would be in charge of all the lab space that William Black designated Columbia had to give to the Neurology Department for Parkinson's research, so I was in charge. I had my own lab. I had other people in the labs. So I was able to develop it and try to make a cohesive thing for both clinicians and basic scientists. I wanted to emphasize basic science in Parkinson's disease and also expand to other movement disorders. I made my fellows, when they joined me, spend time in the lab, spend a year in the lab to learn the lab techniques. Bob [sounds like Berk] who only wanted to be a clinician was one of my first fellows. I said, "No, you have to spend time in the lab." He loved the lab after that and wouldn't go out. [sounds like Serge Shuh-bore-skee] loved the lab and wouldn't go out. Some people just came to the lab. Some would only do clinic. Eventually, I learned that not every clinician is a good lab person and I shouldn't do that, only if they wanted to have lab as well. Otherwise, they would become only a pure clinician. So I sort of evolved the program. I developed a training program and got more fellowship money and was able to get more fellows. Even if they didn't go to the lab, they still had to try to do some research, clinical research, at least and try to develop a program that way.

Many of the people I hired was because we had some money to hire them. Most of the people I hired were people that I trained myself. I saw how good they were and I wanted to retain them. Even Doctor Un Jung Kang, who has replaced me now as our director of our division, I asked him to stay on after. He was also a clinical person in the lab. But his family lived in Chicago. He wanted to go back closer to his family. He developed and grew in Chicago. It worked out and, now, he's back. Others stayed on at Columbia. Many have also taken other jobs [unclear]. [William T.] Bill Dauer went on to the University of Michigan. Susan Bressman went onto Beth Israel at Mount Sinai. I'd lose good people, too. We did train a lot of people. I tried to keep the best. Almost everybody in our division was trained by me. One exception is Cheryl [H.] Waters who was already a leading Parkinson's clinical trial investigator at USC [University of Southern California], whose husband was offered a job at Mount Sinai and she had to come back east. She asked if she could come to Columbia and I said, "Yes, you can work on clinical trials here as well as see patients and expand our clinical trial program." She was one of the few that weren't trained by me. It worked out well.

LK: How have your views on mentorship changed over the years? You're, obviously, mentoring a lot of people here.

SF: At the beginning, I guess I had more time I could spend with each of my fellows and guide them more closely. Later on, as I got busy with other things... I got involved with the Movement Disorder Society advising drug companies on research trials. I helped start the Parkinson's Study Group with Ira Shoulson, who was the head of it. He thought it would be good to have that and I joined him. All these things took time and I didn't have all that time anymore to spend as much as I would have liked to working with each fellow individually. So I tried to get fellows who knew what they wanted and all I had to do was advise them and maybe give them some other mentor. You should work with this

guy. You should work with that guy and helped them along that way, so divide the responsibility of mentoring with other colleagues.

LK: You had mentioned funding and getting donations from various patients and societies. I was wondering how you approached that. Is that something that you talked to them about or you often had people coming in and giving money?

SF: I'm a very bad fundraiser.

LK: [chuckles]

SF: Nobody asked me that in public yet. That's one thing I wish I could do better. I never did it. Houston Merritt was excellent. I learned what his technique was. He never asked for money for himself. He would tell one of his rich patients, "Look, so and so is working on this project. He could use some support. Why don't you support that?" And money would come in. That's what I would want my chairman to do for me is know some patients and get money for that. [sounds like Birch-krig] was extremely good. He could raise money the way Houston Merritt did. But I couldn't. I didn't think ethically it was possible that I could ask my own patients that I'm taking care of for money for research. Then, the pressure would be on them. I felt I cannot approach the patient, at least the patient I'm now taking care of. Maybe the survivor of a patient. Maybe if a patient died, I might be able to. But I couldn't do that with a patient. So I never did. What happened—I was lucky enough—was I had some wealthy patients who did very well. They were struggling and I would turn their medical illness around and get them better and they were grateful and, all of a sudden, I would get a check in the mail. They would say, "Who should the check be made out to?" They'd call me in advance. Of course, it was made out to Columbia University, but I was able to deposit it into a fund for research. That's how I built up my fellowship fund and built up some endowment. I was able to hire a scientist in the lab that way, too. I was able to take income from the endowment, apply to the salary and bring him in. That helped. It would always be patients contributing on their own. I never had to do it. That was how I did it.

For foundations, it was different. I would put in a grant to foundations like NIH. The Dystonia Medical Research Foundation, I felt like we could do a project for them when they said they were going to support some research centers. I put in an application. We tied for the best grant application. We got the funds and I was able to hire my first dystonia fellow who was Sue Bressman, my first nurse who was Carol Moskowitz—that was, I think, in 1981—and we developed our Dystonia Clinical Research Center. We discovered a lot of things about dystonia, because we had the money. We opened the genetics research in dystonia because of that. We found the first genes for dystonia. It was all because somebody wanted to do research and give money for it. We did myoclonus sort of similarly. We were unsuccessful in finding cures or anything, but we did advance the field a little bit in myoclonus. Again, I had a patient who had post hypoxic myoclonus and her husband would like to advance the field. So I set up a lab. I got some scientists to work on it. We were able to do clinical trials and develop a rating

scale for myoclonus so we could develop it. These were foundations that were set up by some people who were basically patients of mine.

LK: I'm going to change gears a bit here and get a little bit more into your research. I was wondering if you could talk about the development for rating scales for Parkinson's and Huntington's disease, how those came about, and who you collaborated with.

SF: Chronically, I can't tell you the order of which one came first. It might have been Huntington's first. Because of Nancy Wexler, there was a Huntington's project. NIH was going to get involved. I sort of got involved with Shoulson. We were on a committee. I said, "We need a rating scale." I worked with Ira and we developed a rating scale for that. That's been supplanted by a unified Huntington's disease rating scale, subsequently. But we had a rating scale at that time which was used.

In terms of Parkinson's, what was happening in Parkinson's, when L-dopa was coming out, there were different rating scales by different people. I went to some international conference once and there were complaints about this rating scale, that rating scale. There was no way to compare one study result with another study result. I had the idea that we needed a rating scale that's unified, that everybody agrees to, and we'd all use. When I was asked to help organize a meeting, bromocriptine was coming out. The drug company that made bromocriptine wanted to have a little meeting of some leading Parkinsonologists. I said that I would agree to do this but would you give us an extra day or two for a select number of these people to stay on so we could develop a rating scale for Parkinson's disease? That was the deal. They allowed that. I think we had only one day. I told these people in advance what the goal would be. Here were the rating scales and the literature. Let's work on a rating scale. I wouldn't let them leave to go to the bathroom until we agreed to some things. You had to force these people to agree. They all wanted to do their own thing. We finally got the thing. I called it the Unified Parkinson's rating scale. We finished it. We tested it out. It wasn't good enough. We had a second version and tested it and it wasn't good enough. We did a third version. That seemed to work. So the third version is what we accepted. We did a clinical trial, basically, videos and people examining the patients and the scoring of the committee members, and we put together this rating scale with the documentation of how valid it was and presented it at another meeting, and, then, got it published in a book, instead of a journal article. I still have the article in the book and still people ask for it. That became the Unified Parkinson's Disease Rating Scale [UPDRS]. That's been supplanted out by the Movement Disorder Society rating scale, which has some advantages, but it also has disadvantages. It takes longer to do, is more cumbersome and a little more complicated. The other one is a little easier and faster.

The Parkinson's Study Group was coming into being and they needed a rating scale to do their Parkinson's stuff. I told them about the UPDRS. Ira agreed to use it. So we did use the Parkinson's rating scale for all the Parkinson's Study Group studies and pretty soon, everybody started using the UPDRS. That's how it became standard.

The unified Huntington's disease rating scale, which I really didn't participate in... They decided to mimic and develop the one I did with Ira, expand it and make it the Unified Huntington's Disease Rating Scale. [Christopher G.] Chris Goetz did one on dyskinesia, the Unified Dyskinesia Rating Scale and so forth. I also got involved with a tremor rating scale because I was asked, "Wouldn't it be good if we had a tremor rating scale?" I asked Eduardo Tolosa and we would work on it together. I sort of developed it and he agreed with it. Then, we tested it out. Again, that was in a book, too. I, eventually, developed a later one for myoclonus. I even developed one for a couple other things, but they never took off and I never published those.

Rating scales are important, because if you really want to do studies, you have to quantify it somehow and you need some system to do that.

LK: This is kind of a broad question. How did you become increasingly involved in work on clinical trials? I'm specifically looking at your work on levodopa for NIH and, then, the controlled surgical trial for fetal tissue transplantation.

SF: Those are probably the bigger trials, but, actually, I got started before that. My first clinical trial was after I finished NIH training and I was three years at Columbia with Milton Shy and his successor before going to Penn. It was at Penn that I did my first clinical trial. When I was at Columbia, Mel Yahr ran the clinical trial on L-dopa. I wasn't part of the clinical research. I was in the lab. So when I went to Penn, I had the idea, look, here are drugs out there. These are neuroleptics that cause Parkinsonism. Why can't those drugs be used to treat chorea? L-dopa which treats Parkinson's causes chorea. So it should be the reverse. I said, "We should try it."

So I did a study on Perphenazine. I asked Gabe Schwarz, whom I mentioned, who was a senior clinical neurologist in private practice, "Gabe, what's the antipsychotic that's most likely to cause Parkinson's in all the drugs you've used?" "Oh, Perphenazine." So I said, "Okay." I got the drug company to give me some Perphenazine and a placebo and did a double-blind trial with different kinds of choreas including some Huntington's, some [unclear], and some other things. In those days, it was easy. You didn't have IRPs. [Lauren, please insert the definition for the acronym IRP here. Thank you.]. I just did the consent form and did it. I published that one. Even when I was at Penn, the Amantadine story was coming out from Boston. Let's try Amantadine in a double-blind trial. I think the drug company came to me. I said, "Let's design a study. We'll do a double-blind trial of that." So did a double-blind trial on Amantadine in Parkinson's disease. Shortly after that is when we wanted to do something on other diseases.

Eventually, the Parkinson's Study Group came out and the DATATOP [Deprenyl and Tocopherol Antioxidative Therapy of Parkinsonism] study was done. I really thought I knew clinical trials before, but I really didn't know it. This is a separate specialty that overlaps all clinical fields. Ira Shoulson was particularly interested in the specialty of clinical trials and the nuances and how to do it and all the technical aspects. I sort of learned by joining with Ira and developing the DATATOP project and meeting with all

these other people on how to do it. So we did DATATOP. One study led to another study and led to another study. We did lots of Parkinson's Study Group trials.

After the DATATOP trial was finished, what next should the Parkinson's Study Group do? I said, "We should explore L-dopa. What does levodopa really do in a patient? Does it really hasten the disease as some people think or does it slow the disease? We really don't know. No one knows how to design a trial." So I came up with a design. It turned out it wasn't the best design, subsequently, but it was good enough. Everybody thought it was a good design, so we did it. We got good results and until this day, it's the only double-blind trial on L-dopa and the dosage response curve. We did that study. That was called the L-dopa trial.

Then, when the fetal tissue transplant... The Swedes were doing it and no double-blind trials, nothing else. They were doing some human fetal tissue but, in the meantime, adrenal gland... Otologists take the adrenal gland from the patient and put it in his own brain and were reporting good results. The rating scale was, yes, they got better, didn't get better, got moderately better. I said, "We should do a double-blind trial if we're going to do anything at all." When President George Herbert Walker Bush forbid any fetal tissue work with federal money, there was a moratorium on getting NIH money for this. When Bill Clinton was elected president, we knew that law would be taken off the books. It wasn't a law; it was a presidential edict, I guess. I got a phone call from Curt Freed, who is a pharmacologist internist, who had done some fetal tissue transplants on Parkinson's patients even though he's not a neurologist. He said, "It's going to be allowed to go to NIH. I'd like to get a grant in to NIH. Could you work on this with me?" I had the patients and he had the technique on how to get the fetal tissue dissected out to get the dopamine neurons. I said, "Yes, but it has to be double-blinded. I'm not going to do any study that's not double-blinded, because we'll never know what the right answer is unless it's double-blinded." I got that lesson from my days with the Parkinson's Study Group and Ira Shoulson. I give him credit for teaching me all about clinical trials. So we designed this. I wrote the protocol of this. He wrote the protocol for how to get the tissue out and how to measure the dopamine production on the tissues and so forth. We'd see the patients in Columbia in New York. The patient would fly to Denver to get either the sham procedure or the real tissue. It would be blinded. We would have an excuse in case there was a mishap on the tissue, that there would be some false [unclear] and the plane trip would have to be cancelled. They couldn't go because the tissue wasn't stable enough in [unclear] dopamine. It turned out we had to do that once with a placebo patient, not a real patient even. It was built in and the patients knew that there might be in the protocol, so we'd keep the patient blinded. We figured out how to keep the patient blinded in the operating room, how to keep the patient blinded afterward with the MRI [magnetic resonance imaging] scans. All that worked out well. We even had a technique [unclear] how to assay for how good the blind was. It required the patients really being blinded as well as doctors. We did that study. It turned out on one hand to be positive in the sense that we were able to get the secondary outcome variable to be improved; that is, what we call the offstate would be less severely off. That was statistically significant in the younger group. We had even divided the patients in the planning. There would be an older group and a younger group and each one would

be analyzed separately to see if age mattered. The primary outcome group was a tougher one. We wanted to have the patients rate their own selves. It turned out they didn't rate themselves superior. It wasn't better than a placebo. So people say the drug study failed, but actually if you look at the secondary outcome, it was very successful. So we had proof of principles that it worked.

As I was saying in the first part of this interview session, we got dyskinesias, runaway dyskinesias in the people [unclear] really successful in. The tissue took and was growing dopamine, but you couldn't stop the dopamine from coming out and causing dyskinesias. That was a real problem and I think it's going to be a problem in future studies with tissue. It may even be a problem with the study with genes, when they do transplanting of genes. How do we control it? That could be a problem.

Anyway, I got involved with clinical trials that way. I still try to stay in touch with clinical trials.

LK: I read a blog post by a patient of yours named Judy Hazlett. She talked about your compassion as a physician and mentioned this instance when she was moving really slowly one day and you asked her if she wanted to run down the hallway. And she did and she felt wonderful. I was wondering if you could talk about your approach with patients and how that's changed over the course of your career, and what influences that?

SF: There is really no trouble with patients when you see patients. You learn from them. I try to find out what the problems are that the patients have, have the patient teach me any tricks they've learned. Then, I can help the next patient. I try to do that still to this day, teach some tricks the patient taught me, about how to overcome freezing. I had a patient who would jog when he would go into an off period and he found that if he jogged, he could work his off period off somehow or other. All I know is that the dopamine stored in his muscle tissue went into circulation again and got into his brain. No one would think about it if you weren't a patient and had done this. So you learn all these kinds of things. If you go into clinical practice at all seeing patients, you have to be compassionate; otherwise, why do it? Maybe some surgeons go in just because they want to operate and don't care about patients. They want to look at the x-rays and, okay, we're going to operate [unclear] without even seeing the patient. That's not what we neurologists do. We learn from our patients when we see our patients and find out what's wrong with them, what is bothering them, and how do we help them. Sometimes, we can't. All too often, we can't. Once we know what the problem is, we might have a solution for them and we can help them. That's what we do. I think it just evolves. It's an evolutionary process, nothing [unclear]. You just learn from your own mistakes, in a way.

LK: I wonder if you could talk a little bit about your leadership within the American Academy of Neurology. What stands out about that time in the different leadership positions you held. I know you were vice president and president. Do you have any comments on that?

SF: You don't start out that way.

LK: [laughter]

SF: What happened? I think I was at Penn and I was asked first to run the Movement Disorder Course [unclear] moved from Tom Chase [unclear]. From that, I was sort of asked to serve on the Education Committee and also on the Science Committee. I served on both committees as a member. I guess in each of these committees, I made suggestions of new things. For example, on Science Committee, there was talk year after year, "Should we have poster sessions?" The committee would say, "No one will go to the poster session. They all want to hear the platform sessions." It just turned out that I had attended a science session in [unclear] California, in neurochemistry. I was trained in neurochemistry in the NIH days. They did have posters. Ahead of the posters, they had wine and cheese. People, at the end of the day, would go see these posters in beautiful outdoor weather in the evening. The lights were on the poster boards and people would stand around munching cheese, chatting with each other over their wine. So I said to the committee, "I know how we can get people to go to the posters. All you have to do in the evening is offer wine and cheese or at a morning for breakfast posters, offer coffee, orange juice, and doughnut, and people will come. Food brings them out." Sure enough, they came and, now, the posters are ever successful. We have more and more posters. We have more and more scientific presentations. The more scientific presentations you have, the more people come to the Academy meeting. It goes hand in hand. That was when I was just a member of the Science Committee. I think Roger Rosenberg was the chairman, which, eventually, led up to his presidency at the Academy. I was also a member of the Education Committee making suggestions and so forth.

Then, one day, the Nomination Committee was making suggestions. They picked—I had his name on my mind and I just lost it—again, another Columbia trainee but he was at Boston University. He passed away a year or two ago. Who was president after [Melvin] Greer? Greer was also from Columbia. I'm blocking. He left Columbia to become chief resident at UC-LA. When Don Harter [unclear] became chief resident at Columbia, he was asked to be chief resident.

MO: [Joseph M.] Joe Foley?

SF: No. No. Joe Foley trained in Boston.

MO: We'll look it up.

SF: Okay. We'll put that in later. It was after Mel Greer. [Insert given and surname when remembered]. He took me aside at some meeting. Maybe it was one of these Academy meetings. He said he would like to appoint me as chair of the Education Committee. "Ohhh, it's a lot of work. I don't know if I want to do it." He persuaded me. He said, "This is really good. I think you're going to be good for it." So I took over the Education Committee. [Christine E.] Chrissie Phelps was just then a very new hire to

be assigned to education and science as a lowly staff person. Chrissie even reminded me of that yesterday when I saw her. I was, now, the chairman.

I had a couple of hurdles to face. One in the education session, the [Abraham Bert] A.B. Baker section was formed and they wanted to play a role. I was facing a conflict. The Education Committee was planning the courses and, now, the Baker section wants [unclear]. So I said, "You know what? A.B. Baker people, the chairman can be ex-officio member on the Education Committee and have a vote and say anything, also. So that brought them into the fold. There wasn't any conflict of who was going to do what. The other problem I had, which I complained about, was the previous chairman of Education was a fellow from the Mayo Clinic and he was picking the courses a year and a half ahead of time. Like the course for the 2015 meeting would have been picked some time in 2013, not after the 2014 courses. I said, "That is no good. The field changes. We don't know how good the speaker was. How can you invite somebody when they haven't spoken and invite them again? They might have been a lousy speaker. We're going to go through these courses already assigned, but, then, next year, we're going to meet right after the Academy course, a month later, have all the input, the feedback, of the data that was collected about the courses and the speakers and pick the courses for the next year." So we would have eleven months to hire, to get the speakers to accept, and so forth. To this day, that's how that's done.

Again, I was there to suggest we have other educational programs, breakfast meetings. Well, I think there were always breakfast meetings, but have dinner meetings, after dinner meetings, have case presentations instead, and that's how we started the Unusual Movement Disorder seminars. So I was good. I had a lot of time in my Educational Committee.

I was never invited to attend the board meetings, though, like they're doing today. I had to get the report from somebody else. Eventually, when they had the board meeting the day after the end of the Academy meeting, then, I was able to go and meet the board and make my own presentation. But, otherwise, I was left aside.

After I finished that, I was nominated to be vice president. I finished my term as vice president, two terms maybe, and I thought that was it. Then, one day, I got a call saying, "Would you like to be nominated to be president elect?" I said, "Sure." And there I was nominated. So you sort of grow into the job.

LK: We're getting close to two hours, so I'm going to ask a couple final questions. I was wondering if you could discuss the founding of the Movement Disorders Society and being co-founder of the *Movement Disorders Journal*. I believe you were involved in the founding of both of those.

SF: Correct.

LK: I wonder if you could talk about your goals in creating the society and the journal.

SF: I had been elected as the second vice president of the American Neurological Association one year and I went to their board meetings. This is way before I was a board member or Education chair or anything like that at the Academy. Somehow, I got elected to that position. I heard the financial reports and almost all their money came from their journal. They had a contract where half the profit would go to the American Neurological Association and the rest would go to the publisher.

I was thinking about this. I thought wouldn't it be nice if the people working in the field of movement disorders could get together as a group, form a 501(3)(c), whatever it's called, organization, and get a publisher to make a journal, and we would get half the profits? So I had this idea. At an Academy meeting in April, in I think it was 1986, I went up to David Marsden. I knew it would be tough, unless we brought the Europeans in. David was the leader, definitely the world leader but he was also a leader of the Europeans. The Europeans would never follow Americans. They're very politicized there. Here I am in public saying that. That was my feeling at the time. Maybe it's still true. I said to David, "How about this idea? We'll have our organization called Movement Disorders Society or whatever and have a journal." David jumped on it and said, "Yes, not only a journal, but why don't we do a video with the journal? No one has ever done that *ever*. This would be the first." I thought that was a brilliant idea. That's how innovative David is. I have to say that David was always one step ahead of me. [chuckles] He was the one guy in movement disorders I thought was superior to me. I learned from him, but he, also, learned from me, too. It worked both ways. So it was a mutual agreement that we would start this together.

The next big meeting was the World Federation of Neurology's section on Parkinson's that Mel Yahr was the head of. Every third year, there was a meeting internationally on Parkinson's. Mel Yahr was going to organize this one in New York, it turns out. That would be in June or July that same summer. So I developed a one page questionnaire with a few sentences on it. If there was a society and the dues were \$110 a year with \$100 going for a journal subscription and \$10 for the general dues for the organization, would you be willing to join? Mel Yahr allowed me to put that on a chair outside the entrance to the auditorium so people would pick it up and leave it back for me. I accumulated those. I got names and everything on it. There was an overwhelming, "Yes." I showed it to David.

The next meeting was in September in Hamburg, Germany...the World Congress of Neurology. I was invited to speak. I had a hotel room. I decided we should organize it and have some leaders. I said, "There should be three Europeans and three Americans." On the American side, we'll ask Joe Jankovic, Ira Shoulson, and myself. On the European side, we would ask David, Andrew [J.] Lees, and Eduardo Tolosa, the three people I thought would be good leaders. It turned out that Ira couldn't come that night. He had something else planned. So it was just five of us meeting in my hotel room and we put this together, the plan, and how we would do it. The idea was to have a journal. That was the whole idea. I would be authorized to go to the thing. We'd try to get a steering committee together. We came up with a bunch of names of other people, formed a steering committee. We came up with the names of a bunch of people for the steering

committee: [sounds like Mar-kell] was on it. [sounds like Coh-lahns] was on it. There were a number of people on it. We would meet the next night in a restaurant somewhere. We told them what the plans were. I would do this and would they do this? They elected me to be the head of the steering committee.

I was authorized to meet with Raven Press. I would go to the publisher starting with Raven Press and see what kind of deal we could get, if we could get a share of the profits. I happened to know the head of Raven Press because he got his Ph.D. at Columbia and I knew him before. He ran other books and stuff. My wife actually even worked there as head of their journal division. I met him at his house. He lived very close to me. We worked a deal. He would subsidize all the expenses for starting the journal. We wouldn't get any money until we paid him our debt back. You've got expenses. You've got to have somebody subsidize initially. Then, when they get their money back, we could start making a profit. So that's how it was done. The first dues, as I said, were \$110 a year. [chuckles] I don't know what they are now. We just said that if there were 100 people, we would do it. We had over 100 people and we started the society and we started the journal. That's how that got going.

We had to get a constitution and bylaws. I asked Gerald Stern, a very excellent English language person. I gave him the bylaws and constitution of the American Academy of Neurology and said, "Copy it after this. This is how we should organize it." We got that written and we presented it at the next international Parkinson's conference that was meeting in Jerusalem or Tel Aviv. I can't remember. We met there and we had a meeting of people who were now members of the Movement Disorders Society to vote on the constitution and bylaws. The only change... [sounds like Air-oh Coh-lahns] suggested we can't just elect them at the meeting that's going to be held every third year or something like that. We should have a vote by ballot, as well, offer a ballot votes by mail because not everybody can come to the meeting. So that was the change. We changed that. Then, it was approved and adopted.

At that meeting, I was elected president. David Marsden was elected president elect. Those terms were three years in those days. We thought we'd meet every three years with the World Federation of Neurology meeting. That changed later on, too, because there was merger with another international society of Motion Disorders, which had meetings every two years. We were combined and then we changed the constitution to have it every two years and a set of officers would change and we would merge the two organizations. That's how that came about. It became one group. We now have not only the journal but we have the meetings which were bi-annual and, then, became annual. That's now voted on every year in June, so the month of June is always the date for that meeting; the month of April is the date for the Academy of Neurology meeting. That was international in scope. Initially, the president would be taken from the two continents: America and Europe, back and forth. So I was the first president. David Marsden was the second. Joe Jankovic was the third. Eduardo Tolosa was the fourth. These were all founders, of course. Eventually, Andrew Lees became president. He was the fifth founder. We went back and forth. As we became mature, it wasn't necessary. We had

an Australian. We had maybe two Americans in a row, two Europeans in a row, whatever. The society has been thriving.

LK: My final question unless either of you have a question. In preparing for this interview, I noticed that you've written quite a bit about the history of levodopa and a few other historical talks on Parkinson's and other movement disorders. I wonder how you became interested in writing about the history and speaking on it and what the importance of that is for physicians.

SF: I always liked history. I still read history books and listen to book on tape on history. I think what happens is we reach a certain age... A lot of people don't know the early history. I'm always surprised. How come they don't they know? Again, I'm a lazy person. I don't go out of my way just to write something, but I am asked to write something and I accept. That's what gets me to write. So I accepted the task of writing for this 50th Anniversary issue, special issue when the Movement Disorders Society was asked to put something together. They asked me to help co-chair that. We picked the speakers and the topics and I took the task of writing the first chapter, "The Medical Treatment of Parkinson's disease from James Parkinson to George Cotzias," which is the title of the paper. I reviewed all that early history and tried to put it together. I've also done that for dystonia. I've done it for a lot of other things I got involved in. I like doing history. I think it's really important and it's a source for people to once in a while come back to and read about it, especially the new people in the field who don't know anything about it. It's a nice introductory source, so that's good.

There's one other topic I'd like to talk about, though, before we quit.

LK: Sure. Of course!

SF: That's the World Parkinson's Congress. That may be one of the more important things I've done. There have now been three of those. The World Parkinson's Congress came about because the then head of NIH, Doctor [Elias A.] Zerhouni, called together a bunch of Parkinson's specialists, including foundation heads. There were several foundation heads and Michael Okun is director for another one: the National Parkinson's Foundation. A number of these people were there. I was not invited or I was busy, I don't know. Other people went. Karen [S.] Marder in our group went. Robin [Anthony] Elliott from the Parkinson's Disease Foundation said that Zerhouni complained that we're not bringing patients into the field. So Robin said, "What can we do about bringing patients into the field of research and science?" I said, "Why don't we have a conference where patients are involved as well?" Now, there weren't conferences where patients were involved. At the Movement Disorder conferences, sometimes in whatever city, the day before might be a patient day where the patients would come and meet among themselves with a few speakers of the professionals talking to the patients. I've seen that at other societies and maybe the World Congress of Neurology and so forth where there would be patient days. Even the Academy or the ANA might have had them; I'm not sure. I was thinking of something different. I was thinking of a session where the speakers could also be patients and there would be patients in the room when the

doctors spoke. It wouldn't be just like a clinical meeting where it's mostly doctors or basic scientists, like the Society for Neuroscience is only scientists. I would want doctors who talk about clinical and the scientists who talk about science and the patient who receive all of this material to be in the same room together and hear each other and talk to each other in a language they can understand. I said, "We could probably do it."

We started from scratch. We had no money. We had to raise some money, again, from drug companies like we did with the Movement Disorder of Congress and see if we could get drug companies to support it. We would bring a program together, invite some people to help organize this with me. I put it together with good colleagues, planning the meeting. We would cover all fields of Parkinson's disease and patients would be invited and involved. It had to be a very short notice. We needed a professional company. We didn't have the current Academy of Neurology staff to help us. We had to hire a professional group to run the meeting for us. We had to pay the monthly bills that would come in. We would go out and raise the money. I hired a full time person to work with me directly who turned out to be the best thing I've ever done, maybe...Elizabeth Pollard, who is still there running the thing. We put this thing together. We needed a date and a place. It was going to be in 2006 in Washington, D.C. on president's weekend, so Congress was out of town. Washington, D.C. was dead, vacation time, school vacation and everything else. There were no hotels around here. The convention center was open. We could get it at a dirt cheap rate because it wasn't being used. [unclear] a year and a half in advance was all we had time to plan and raise the money. We signed a contract and booked it. I asked the Parkinson's Disease Foundation to help, if they'd be one of the supporters. I went to other foundations. I got contributions from Michael J. Fox, National Parkinson's Foundation, APDA [American Parkinson's Disease Association], PDF, plus other drug company donors, and so forth. So we were able to put this together.

We had the Congress here in Washington. Lo and behold, like 2,000 people showed. Then, we planned the next one and so forth. Now, we've had three and the fourth one is going to come up in Portland, Oregon, in October 2016. It's going to be every three years now. We figure every three years is about right. There would be new information available, not like every year when there's not enough information. Patients couldn't travel, maybe, that often, but every three years, maybe they could make it. It turns out that the patients and families make up about forty percent of the people. Scientists/clinicians and investigators make up thirty percent each. So altogether about 100 percent of the people are made up in those three groups. It really is good. Doctors and scientists learn from patients. Scientists never saw a patient. They're working on Parkinson's in the lab. They never saw a patient with Parkinson's. They get to hear what the patients' problems are and it's good for the doctors to hear what the patients' problems are. There are sessions on sexual therapy for patients and this and that. We doctors are ignorant about a lot of this stuff. We have to go to these sessions, too, and learn about all of this. It's really good. It makes us humble. It gets me upset because there are a lot of doctors, friends of mine, specialists who won't go because they don't want to talk in front of a patient or something or they're too superior to go that. I write them off and I just say, "Okay, don't go." I try to get people to go. Even if they're not giving a talk, they should

go and participate and help support the thing. The patient groups are really excited about it, so we get support from them, too. They're members of the committee. They're on the program committee and all the other committees. So they are actual vital contributors.

PK: Is it the first time a medical congress in which patients were involved?

SF: I don't know for a fact. I think Alzheimer's might be the first. That's what I heard, but I never went to one of those. I'd say, "If they could do it, we could do it." So that's when I decided we should do it. It's worked out very well.

LK: I'm glad you brought that up. That's great.

Any other questions?

MO: When you go to the World Parkinson's Congress, do you see a lot of your own patients who stop you?

SF: Oh, when I see them, we say, "Hello," and we chat, see how they're doing. Oh, yes, I see a lot of my patients.

MO: The most striking thing for me—I've attended all of them—is the number of my own patients who are there at the meeting.

SF: Yes.

MO: A number of patients now with the availability of the Internet and other things have read some things. So if you meet them, they can equate and can put two and two together and figure out who you were based on what they read somewhere in an article or on the Internet.

SF: Oh, yes, lots of [unclear]. They'll comment that the science is over their heads, but they still like hear what he has to say. How do you talk in a room of mixed people? Even clinicians can't understand them sometimes. They have to talk in language that people can understand. This is an important development in the research. This is what they found. How to explain it?

We're putting together a new journal with *Nature Partner Journals* on Parkinson's disease. I refused to do the editing, but I would be an advisor. It would be an open access journal [online only]. It's just getting started now the first issue is coming out. We would have commentary written by people who are trained in science but Parkinsonologists to write the abstract in lay language, an abstract for the non-specialist to read and understand. It takes a certain amount of ability to be able to translate into English what the scientists write. That's going to be in this journal. We want to do that. Open access allows the patients to get in without any fee for being a subscriber. It does put the burden on the author to come up with the money to get the article in, but many people do have grants and are willing to get maybe a publication in a journal like this. It

costs money, like \$4,000. It could come out a research budget or another budget they might have. Hopefully, it will be successful. We'll see. There other open access journals so we'll see if this one will fly.

PK: When your parents came from what today is the Ukraine and moved to Germany. Do you know to which city? Did they go to Berlin?

SF: No, no. They both moved to Dortmund. I told you my father was ten when he moved. My mother was still in her first year of life when her parents moved. My mother was six years younger than my father. They both moved to Dortmund, Germany, and grew up in Dortmund. I visited Dortmund when my mother was still alive. I went there to see some of the places. Dortmund has a good football team; that's soccer. They have good beer, Dortmund beer. It's one of the bigger cities in Germany.

PK: It's not so far from [unclear]. I know about many Jewish people who were emigrating from the Eastern European countries to the west. Most of them, I think, ended up in Berlin or [unclear]. Then, of course, [unclear]. I have a good friend who is a medical historian, who during the past ten years has been working on the Jewish immigration in the United States. These were really thousands of physicians who came here in the 1920s and 1930s. [sounds like Louis] one of the persons. He got the jump, I think, here as a [unclear]. Many of them had to change their profession.

MO: You were at the Neurological Institute in Columbia, once you settled down between the Penn, Columbia. This is a rare example. It's like Cal Ripken [Junior]. It's like the old baseball players; they stick with the same team the whole way through. Were there any times when you actually considered leaving to go do a chairmanship or something else or other offers?

SF: I was offered a chance to interview for the job at UC-LA way back. I told my wife I was curious to see what it was like. I was having a good time at Columbia. I liked it. But I should see what interviews were like, so I went out there, gave a lecture, and stuff like that. I interviewed with a lot of people. But my wife said, "Look, if you accept the job they offer to you, you're going alone. I'm not leaving New York for Los Angeles."

[laughter]

SF: So I knew that in advance. As it turned out, I wasn't invited anyway to be the chair, so it didn't matter. Then, any other openings that came up, would I be interested, I said, "No," right away." I wasn't going to leave. UC-LA was different because I am from California. UC-LA was a good medical school. It's my home state, you know. But my wife was a native Brooklyn girl and New York was her place. She wasn't going to go to the west coast.

MO: So many people these days in sports teams and in medicine have just a general philosophy that the grass is always greener instead of making the best of what you have and, then, continuing to grow it. Do you think a lot of your success was that you actually

stayed in the same place, dealt with the problems as they came, and worked through the issues, and you were able to be productive without the distraction of always thinking the grass is greener?

SF: No. I think the success is who is your chairman is. As my wife says, the second most important man in her life is Bud Rowland.

[laughter]

SF: I hit it off with Bud. I worked in his lab as a third-year resident. I put the McArdle paper together. He's a great writer with just one draft. That's how good he is. I didn't learn how to write from him, unfortunately. I still not a good writer. I did a lot of the biochemistry in that paper and we got that publication out. My first presentation at a national meeting was when I had just finished my residency and was now at NIH on my biochemistry fellowship. The paper was accepted to be presented as a platform presentation on McArdle's disease at the ANA. I presented it. I was nervous...my first presentation. We were close then. Then, when I was coming back to Columbia with Mel Yahr, Bud was hoping I would have been with him, but I wasn't interested in muscle disease. He was very accepting of me coming back and we've had a close relationship ever since. He was my mentor. He was like a role model. He was good clinician and a researcher with a lot of integrity, honesty, and hard work. Again, as a chairman though, he let people develop. He was always supportive. If you wanted to hire somebody and can raise the money, hire them. He wasn't restrictive in any way. No, you can't do this. Then, you wouldn't have a chance to grow. So I had a chance to grow. Again, with some wealthy patients giving money, I could hire somebody to at least get them started, get a lab space funded with equipment and supplies and develop the place. So I was able to develop the program. Writing grants, as well, we were free to do that with Rowland. Economic times are tougher now. You have to bring in money through patients and do all these other things. It's not as easy as it was then. Because of Rowland's great leadership ability... He developed the division system. Every division had a chief. The chief could grow their division to their abilities and he would let them grow. He wouldn't tamper. If you needed more space and you had the money, he'd go to the dean and fight for more space. Everything depended on the money. If you can pay for the space, you can have it, even if it's not in that building, then somewhere else. He would try to find the space for you. Space was always a big limitation at Columbia. That was one of the issues. But we were able to do it.

MO: All the years you worked at Columbia, did you ever get mugged or have any crime in the Washington Heights neighborhood?

SF: Oh, no, no. People were mostly very nice everywhere. Yes, sure, it's crime ridden. The crack capital of the world, it was called when we were there. But I never felt intimidated.

MO: The rumor from the people that you trained is that you would keep pretty late hours coming in and out of the Neurological Institute, and even, now, even today.

SF: Yes, I do. And I had that parking space right [unclear].

[chuckles]

SF: My car was always there. Even when I might be away for a weekend travel or whatever, I would come back and there would be my car.

MO: Do you still have parking space?

SF: Yes. I always said if any new chairman wants to get rid of me, all they have to do is take away my parking space. I'll leave like that.

[laughter]

SF: They'll know that's the sign that I want to leave. So far, they haven't done it.

The old joke... I don't know whether it's a joke anymore, because it's really true. Clark Kerr became president of the University of California, of all the campuses. He would tell donors, parents, "I have all these demands on me. Parents complain, 'Free sex on campus.' And students complain about free speech. 'We want free speech on campus.' The faculty, they demand free parking." The three freedoms.

[chuckles]

MO: We should probably let Doctor Fahn have a rest. You have gone non-stop here for three and a half hours.

SF: Yes. Okay.

LK: Thank you so much for your time.

[End of the Interview]

Transcribed by Beverly Hermes

Hermes Transcribing & Research Service
12617 Fairgreen Avenue, St. Paul, Minnesota, 55124
952-953-0730 bhermes1@aol.com