

**Teleconference Interview with Sami I. Harik, MD, FAAN**

**Interviewed by Douglas Lanska, MD, MS, MSPH, FAAN**

**Edited and annotated by Douglas Lanska, MD, MS, MSPH, FAAN**

**Interviewed for the American Academy of Neurology  
Oral History Project**

**Interviewed on June 9, 2014**

Sami I. Harik           - SH  
Douglas J. Lanska    - DL

DL: Today is June 9, 2014. This is Dr. Douglas J. Lanska speaking with Dr. Sami Harik for the American Academy of Neurology Oral History Project. This is a follow-up teleconference interview to be appended to an initial in-person interview conducted with Dr. Robert B. Daroff in Beachwood, Ohio, on February 22, 2014.

Sami I. Harik, MD, FAAN (born July 27, 1941), is a Lebanese American neurologist and neuroscientist who obtained his undergraduate medical school and post-graduate internal medicine residency training at the American University of Beirut in Beirut, Lebanon. He then completed a neurology residency from 1968 to 1971 at the New York Hospital, Cornell University Medical College in New York City, where he trained under [H.] Houston Merritt [sic] and Fred Plum [1924-2010]. From 1971 to 1973, Dr. Harik completed a fellowship in pharmacology and experimental therapeutics with Solomon H. Snyder, MD, DSc [born 1938], at Johns Hopkins University School of Medicine in Baltimore, Maryland. In 1973, Dr. Harik joined the faculty of the American University of Beirut School of Medicine, but in 1976, he moved to the Department of Neurology at the University of Miami School of Medicine in Miami, Florida, under the chairmanship of Peritz Scheinberg, MD, where he was a colleague of Dr. Robert Daroff.

In 1981, shortly after Dr. Robert Daroff became Chair of Neurology in Cleveland, Dr. Harik also moved to Cleveland as Professor of Neurology and Pharmacology at Case Western Reserve University School of Medicine. Dr. Harik later held joint appointments as Professor of General Medical Sciences (Oncology) and Professor of Neuroscience in Cleveland. He served as Vice Chairman of Neurology in Cleveland from 1986 to 1994 before being named Professor and Chairman of Neurology at the University of Arkansas

College of Medicine in Little Rock, Arkansas, a position he held from 1994 to 2010. Dr. Harik retired in 2012.

I was privileged to get to know Dr. Harik when I was a neurology resident and later a fellow and instructor at University Hospitals of Cleveland from 1985 to 1989. In my experience, Dr. Harik was an extraordinarily knowledgeable attending and as a clinician was typically competent, clear, decisive, and efficient. He often had uncanny insights concerning patients with complicated or obscure neurological or general medical problems. In addition, he took a great and sincere interest in his patients as people with problems rather than as neurological problems with people attached. He similarly took a great interest in how the neurology residents were doing and was always kindly in his interactions with them. He was never too busy to ask how someone was doing and always did so with a sincere and well-meaning interest and, moreover, was actually willing to listen to the answers provided. My wife, Mary Jo Lanska, also rotated as a pediatric neurology resident with Dr. Harik. She has fond memories of her interactions with him. As she recalled, “Sami was always very nice to me—always, always, always.” In addition, Dr. Harik has always had a great sense of humor.

Sami, in 1968, when you began your neurology residency at New York Hospital, Cornell University Medical College in New York City, Houston Merritt was finishing or had just finished his two-decade long tenure as Professor and Chairman of Neurology at the College of Physicians and Surgeons, and Director of the Neurological Service at the Neurological Institute of the Presbyterian Hospital. During much of that period, he was also Dean of the Faculty of Medicine and Vice President of Medical Affairs at the College of Physicians and Surgeons, a position he held until his retirement in 1970.

What can you tell us about Houston Merritt [1902-1979]?

SH: You had a very good introduction. The only correction I need to state is that I didn't train with Houston Merritt. I trained with Fred Plum. Houston Merritt was at Columbia [University] and we were down in mid Manhattan, south of Columbia.

My interaction with Houston Merritt was, basically, when we were attending grand rounds sometimes in Columbia—he came to the New York Hospital on several occasions—and when we consulted him in person. Probably, I could tell you some stories about these consultations. But unfortunately, I never trained officially under Houston Merritt.

The person who was head of Neurology at the American University of Beirut, Dr. Fuad Sabra, trained with Houston Merritt at the Neurological Institute in the mid to late 1940s. He learned a lot of his neurology from Houston Merritt and used to tell us stories about Houston Merritt. Houston Merritt was a giant of a neurologist despite his short stature. He is reputed never to have missed a CPC [Clinical Pathological Conference], because of his clinical skills when neurology was still a black box and we didn't have MRIs or CTs to tell you what the real story is. Oftentimes patients dragged their secrets with them to the grave.

When I came to the New York Hospital to train with Fred Plum and Jerry [Jerome B.] Posner [born 1932] in July of 1968, I heard of Houston Merritt and I saw him. I also saw him at meetings, principally of the American Neurological Association, which in those times were held almost always in... What's the name of that town in New Jersey that's now a Mecca for gambling?

DL: Atlantic City?

SH: Atlantic City. It was always in Atlantic City, and it was dilapidated, and there were no casinos. You walked on the rickety boardwalk. Houston would be there in the front seat all the time.

My first interaction with Houston Merritt was when Fred Plum invited him to be a visiting professor at the New York Hospital.

We had admitted a patient a few days earlier that came in because she was unsteady on her feet. She was short and had funny looking features. Examination showed only ataxia. Sometimes you would get a Babinski [reflex] and sometimes you wouldn't. Sometimes she had exaggerated reflexes. But the thing that was very unusual about it is that she had nystagmus. Her nystagmus was vertical, but the quick component was upward. So I presented this to my chief resident who was Marcus Raichle [born 1937].<sup>1</sup> He said, "Yes, that's very unusual," because in his experience, vertical nystagmus is almost always—more than 99% of the cases—upward, [that is] the fast component up.

I went down to the library to read about the problem. Before I went in I didn't know what to look [for]. I wanted to look under vertical nystagmus and see what did they say about which direction the rapid movement should be. But before I went in there, there was a big table on which they had recent issues that arrived in the past two or three days. One of them was the—I forgot the name of the journal now—most likely the *Archives of Ophthalmology*. It was an article by David Cogan [1908-1993], who was one of the best-known neuro-ophthalmologists of his day. He wrote two classic monographs on neuro-ophthalmology.<sup>2</sup> One of them was on the afferent side, the sensory side, and the other

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<sup>1</sup> Marcus E. Raichle (born 1937) is a neurologist at Washington University School of Medicine in Saint Louis, Missouri, where he holds appointments as professor in the Department of Radiology with joint appointments in Neurology, Neurobiology and Biomedical Engineering.

<sup>2</sup> David Glendenning Cogan, MD (1908-1993), former Director of Ophthalmology at the National Eye Institute, National Institutes of Health, Bethesda, Maryland.

See also:

1. Cogan DG. *Neurology of the Ocular Muscles*. Springfield, IL: Charles C. Thomas, 1966.
2. Cogan DG. *Neurology of the Visual System*. Springfield, IL: Charles C. Thomas, 1948.

one was on the efferent side of the motor control of the eye movements. I had his books and I always looked at his monographs when I had a complicated case, which was mostly neuro or ophthalmological. In that article, David Cogan—I don't know whether he had any co-authors with him; but as a matter of fact, we should look into it and amend the statements that I make accordingly—I believe, had 23 [sic, 27] patients of so-called downbeat nystagmus.<sup>3</sup> He called it downbeat. He said that those patients, the vast majority, had hindbrain abnormalities and the most important hindbrain abnormality was an Arnold-Chiari Malformation. So, gee, that is exactly what I wanted to look at. It had everything.

So I reviewed the findings on the patient and called in Marc Raichle. He agreed and we said, "Let's present this to Fred Plum in morning." So we presented to Fred. Fred didn't know much neuro-ophthalmology, I believe. He said, "I don't know what it is." I said, "Downbeat." He said, "What's this downbeat, upbeat?" He looks at me like I was coming from outer space. [chuckles] But, then, I gave him the copy of the paper. We had Xerox paper, and he read it, and his face flushed, and he said, "Gee, Sami, that's a great discovery...you and David Cogan. How did you find this?" I said, "I was passing by the library and, perchance, I had a look at it." He said, "Houston Merritt is coming here as a visiting professor in a couple days. Why don't you present this patient to him?"

In the meantime, we did the plain color films, X-rays. We measured the McGregor line, and the Chamberlain line,<sup>4</sup> and all these kinds of lines, and see whether the odontoid process goes up above the line or not.

Then we even pushed it a bit further. That was a bit unfortunate, because we almost lost the patient. We thought we would look at the fourth ventricle in greater [detail], and we proceeded to do a pneumo-encephalogram, which is archaic, you know. You don't hear about it anymore since the advent of CT and MRI, but that was the only way you could look at the hindbrain, fourth ventricle, and the ventricular space. We wanted to inject a few cc's [cubic centimeters] of air into the fourth ventricle and do a tomogram whereby you move the head back and forth to both sides. They you get a good view. During the procedure, the patient started to herniate, so we had to call the neurosurgeons. They bailed us out by putting in an intraventricular catheter in the lateral ventricle on one side. We saved the patient.

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<sup>3</sup> Cogan DG. Down-beat nystagmus. Arch Ophthalmol 1968;80:757-768.

<sup>4</sup> Chamberlain's line was a line drawn on lateral skull radiographs that connects the posterior edge of the hard palate to the posterior lip of the foramen magnum. The McGregor line—a line connecting the posterior edge of the hard palate to the most caudal point of the occipital curve—is a modification of the Chamberlain line that was used in the evaluation of basilar invagination when the opisthion (the posterior margin of the foramen magnum) could not be identified on plain radiographs. If the tip of the dens is greater than 3mm above Chamberlain's line, or if the tip of the dens was more than 4.5 mm above McGregor's line, it was considered indicative of basilar invagination.

Anyway, the patient did well and we presented her to Houston Merritt. Being naughty boys, we kind of cleansed it of everything that could give him any... We kind of glossed over several features that may give him a clue... We were astonished when Houston said, "Gee, that's a simple [open-]and-shut case of a *forme fruste* of Arnold-Chiari Malformation."

DL: [chuckles]

SH: I asked him, "How did you come to this conclusion, Dr. Merritt?" He said, "Come on, what else could it be?" [laughter] I love that. So whenever anybody asked me about why I came to such a conclusion, I didn't have a good answer. Houston Merritt was a great *sans clinique* at the bedside. He knew the diagnosis just by taking a history and doing a very incomplete neurological exam. But when you asked him why he came to that conclusion, he never was able to do as other people, such as [Derek E.] Denny-Brown [1901-1981], and Ray [Raymond Delacy] Adams [1911-2008], and Fred Plum were able [to do], each one to his own strength. Denny-Brown would go into physiology. He was a powerhouse in primate physiology to do experiments on chimps and other primates. Ray Adams was a great pathologist who came to it from the neuro-anatomical and neuro-pathological pathway at the autopsy table. Fred did more of the physiological [studies], [and was] kind of a maverick in a sense. To stupor and coma, he gave order to where there was none. It was mostly based on physiology. Houston Merritt was purely on clinical sense, taking a history, and seeing a lot of patients...his uncanny ability to really pitch and hone them to the proper diagnosis that was invariably correct. But he could never explain, so there is nothing that a medical student or resident could write about it, you know. You'd just have to add it to your repertoire and go on and on. I found that to be very interesting.

The other story I knew about Houston Merritt was when I was a second-year neurology resident. We had a patient come in with a cardiac arrest. He was brought in by the ambulance service. By the time he was in the emergency room, his pupils were fixed and dilated. He was a young man—he was a boy actually. He was like fourteen or fifteen years of age. He had Orthodox Jewish clothing. He was brought to the emergency room. At that time, [for] anybody who came in in a coma, neurology was called. I happened to be in the emergency room, so it didn't take much time. We tried to revive the guy. We inserted an endotracheal tube and attached him to the respirator. But the pupils were fixed and dilated. The patient had a cyanotic heart disease. I think he had tetralogy of Fallot. Something happened to him. I'm not clear as to exactly what happened. Again, we only had x-rays. I don't think we ever got an autopsy on the patient.

The patient was transferred from the emergency room to the floor. Within hours, there was a huge number of Orthodox Jews that were relatives of this boy. It turned out that he was the only son. He was surrounded by nine or ten sisters, but he was the only male of that family. His father was a very prominent rabbi or leader. He carried himself like he was the head of a tribe. I felt intimidated. On the floor where we admitted the patient—there were no ICUs [intensive care units] in those days—there was a room in which we put all the respirator patients. There wasn't a neurology floor. We were the ICU in those

days. In the corridor, there were literally more than fifty men with Orthodox Jewish clothing, habits or garb. They were all whispering and talking to each other. The father was there. It was a stressful reaction for me.

I went up to the father and I explained who I was. I explained what the situation [was]. He asked me very nicely what the situation was and I told him that his son had a cardiac arrest and, by the time he came, there was no pulse, and we tried to do everything to revive the heart, and we did, and he was breathing on his own now, but the pupils were fixed and dilated, and I was not obtaining any brainstem signs, and didn't get a corneal [reflex], didn't get the doll's eyes, and the calorics were absent and so on, and he was not breathing on his own. We had to keep him on the respirator. I told him that the prognosis is not too good. The guy said, "Look. I knew he had a congenital heart disease. He was sickly all his life. I don't hold anybody responsible. I'm not blaming anybody. I think it's all in God's hands." A very religious guy, he believed in fate and what is done cannot be undone sometimes. But he said, "I just have one request from you." I said, "What might that be?" He said, "I want Houston Merritt to come and see him, because I believe he's the best neurologist in the world and he's here in New York City." I said, "Yes, he is. I know Dr. Merritt. But Drs. Plum and Posner wrote the book on coma."<sup>5</sup> He said, "That may be the case, but I have had a lot of experience with Dr. Merritt and I believe only in him. I want him to see my son. If you don't want to call him, then please permit me to call him myself." I said, "Okay. Let me discuss it with my chief here and I'll get back to you."

So I went into Plum's office. He was there. He was in a good mood. He said, "Sure, go ahead. Why don't you call Houston?" I said, "What do you mean call Houston?" [chuckles] "He's not a friend of mine. Do you want to call him?" He said, "Sami, come on, I'm too busy now. Just call him. He would love to come. He *likes* it. He basks in it. That will be a good experience for you. You'll see the master at work." Fred and Houston had a very good relationship, I believe. He looked at me coming from a third-world country and he said, "My secretary will help you track him down."

DL: [chuckles]

SH: So we went out there and Fred asked the secretary, "Get Dr. Houston Merritt for Dr. Harik." I was waiting and eventually I got to Houston Merritt's secretary who got me... I said, "Dr. Merritt, my name is Sami Harik. We met before when you came and I presented to you that patient," and so on. He said, "Yes, yes, I remember very well." I said, "There's a patient and he's in coma. I believe he has no brainstem functions. He's brain dead. But his family insists that you come and see the patient." He said, "I'll be delighted to see your patient, Dr. Harik. I thank the family for their trust in me." I said, "What time would suit you, Dr. Merritt?" He said, "I'm a dean and I have all the time in the world. You're a busy guy, so you tell me what would suit you." [laughter] I said, "Would between three and four o'clock in the afternoon be good?" He said, "Yes. Let's

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<sup>5</sup> Plum F, Posner JB. *The Diagnosis of Stupor and Coma*. Philadelphia: F.A. Davis Co., 1966.

make it three sharp. Would you please meet me at the 68th Street entrance to the New York Hospital, since it's so big? I don't want to miss you. I'll be driven by my chauffer. It's a black Buick. If you would be there at three sharp, I'll be there." I said, "Sure, I will."

I went there about five minutes to three and at three sharp, the car stopped and Houston came out. I recognized him immediately. We went into the hospital. He engaged me, [and asked me] where I came from and so on. I told him about Dr. Sabra. I did medicine under him. Neurology was part of medicine in those days. He said, "Oh, yes, I know Fred very well." I call him Fuad; he called him Fred.

DL: [chuckles]

SH: We went out there. The family was still there. He shook hands with them. He shook hands with the father. I introduced the father and Merritt acted as if he knew the father from before. He said, "Okay. Dr. Harik and I will go in to the patient, and we'll exam him, and we will come out, and I'll give you my opinion." I said, "Yes."

So we went into the room and we closed the door. So I went in, and, you know, armed with a flashlight and the usual things and a little bit of cold water to squirt into the ears and so on. I did the examination and showed Dr. Merritt that there were no brainstem reflexes. There was no reaction to pain, noxious stimuli, and so on. I said, "Okay, Dr. Merritt, I don't want to take more of your time. As you can see, the guy is..." so and so, you know. Merritt just took out his key. He did the Babinskis. I don't know for what reason or what he got [by checking them]. [chuckles] Anyway, basically, he saw me through the exam and he didn't do anything else. I was kind of egging Dr. Houston Merritt to go out and talk to the family when we were done. He said, "Not so fast, young man," you know. "This is their son, they're only son. This is a great blow to them. We just can't come down here and leave in seven or eight minutes after doing this exam and just tell them that he's dead or dying. We need to show more respect. Why don't you just sit on the bedside here and let me tell you about some anecdotes about Fred Sabra when he was a resident here?" He went on and entertained me for around twenty minutes. Then, he said, "Now if you want, we can leave and talk to the family."

He went out and talked to the family. The family offered him money for his coming here. He refused adamantly. I escorted him back to the entrance of the New York Hospital. The chauffer was there and he took Houston Merritt.

That, I think, left a very indelible impression on me. Here is the world's best-known neurologist probably, a dean with a cushy job. He was maybe in his early seventies at that time, an old man. There was no monetary income for him. He is not going to run for elections. Why would he do such a thing? There is more to medicine. There's more to medicine than showing [by] the reflexes that the brain was dead. You just have to take your time. I learned so many things from him. I will forever remember Dr. Merritt as one of the giants [of neurology] based on these few interactions with him. He was a human being with a good sense of humor, and a very humane person. That's what I want

to tell you about Houston Merritt. I regret that I did not see more of him. I read his book [*A Textbook of Neurology*], which I didn't think was a great book,<sup>6</sup> but he was a fantastic clinician.

DL: Thank you for that.

When I interviewed Joe [Joseph M.] Foley in December of 2011, he said he knew Merritt as a medical student. What he said is apropos of what you told me in your anecdote. Joe said, "He was very good to me. He would give me all kinds of breaks, would even buy me lunch periodically. Back in those days, I had neither a pot to pee in nor a window to throw it out of. I was grateful for all the help I could get." So Joe really appreciated the warmth and kindness that Merritt showed him, even as a lowly medical student. Raymond Adams in an interview with Robert Laureno said, "Merritt was a rather easy-going, jolly fellow."<sup>7</sup> From what Foley told me and from others, too, it was clear that he had great empathy and, as you say, a real humanity. He really cared about people.

SH: Yes, he did. He did. Some people thought that he was a diamond in the rough, sometimes.

DL: Yes, I heard that, too.

SH: I didn't see that aspect of him, but I didn't see him enough. The man who gave the obituary for Houston Merritt is "Bud" [Lewis P.] Rowland and he cried. There were other people who thought that Houston had a... Well, let's not talk about it.

DL: That's fine. I did hear anecdotes from Bob Daroff and I've heard them from Bob [Robert L.] Ruff in the past, as well, about some of his idiosyncrasies. It was thought that some of those were what interfered with him getting the chair at Boston City Hospital.

SH: I don't know what happened. Sometimes, these people make their... That's very complicated, how people get promoted to what they did. What they got was a great guy. Denny-Brown was not as good a clinician as Houston Merritt or as Ray Adams. There were three people in the running for that and the two Americans lost. I don't know...maybe Denny-Brown got it because of his British accent.

DL: [laughter]

SH: He was from New Zealand.

DL: Yes, he was.

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<sup>6</sup> Merritt HH. *A Textbook of Neurolog*. Philadelphia: Lea & Febiger, 1974.

<sup>7</sup> Laureno R. *Raymond Adams: A Life of Mind and Muscle*. Oxford and New York: Oxford University Press, 2009.

SH: But he did his training in England. He did work with [neurophysiologist John Farquhar] Fulton [1899-1960]. He, clearly, had [a] more experimental side. He would spend like an hour or two explaining whatever the patient had, whether it was obscure or not. We've all been accused of the same thing, but Denny-Brown would get the Babinski to suit his clinical examination or what he thought the diagnosis was. [chuckles] I think that Houston Merritt did the real Babinski and he incorporated that into the diagnosis. Denny-Brown was a giant. Speaking about the paraneoplastic syndrome, he may be the first to really document that first paraneoplastic...the patient who had the neuropathy [sic, two patients]. I don't know whether it was a woman or a man. It was, I think, published in 1939 [sic, 1948],<sup>8</sup> long before [Walter Russell] Lord Brain [1895-1966] talked about the effect of ovarian cancer on the Purkinje cells and so on.<sup>9</sup> This was cancer of the lung and the patient had a neuropathy.

DL: Small cell, presumably?<sup>10</sup>

SH: I am not sure, actually when I think about it. It was cancer of the lung, but I'm almost certain...almost. But I don't know the subtype.

DL: At any rate, Joe Foley told me the same thing about Denny-Brown, kind of manipulating the toe to do what he wanted it to do regardless of the reality of the exam.<sup>11</sup>

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<sup>8</sup> In 1948 Denny-Brown reported two patients with sensory neuropathy in whom autopsy revealed severe neuronal loss in the dorsal root ganglia and previously undiagnosed bronchial carcinoma. Both were men, ages 59 and 71.

Denny-Brown D. Primary sensory neuropathy with muscular changes associated with carcinoma. *J Neurol Neurosurg Psychiatry* 1948;11:73-87.

<sup>9</sup> Brain L, Wilkinson M. Subacute cerebellar degeneration associated with neoplasms. *Brain*. 1965;88:465-78.

<sup>10</sup> In Denny-Brown's second case, neuropathologist J. Godwin Greenfield (1884-1958) diagnosed the cancer as an "oat-celled" (i.e., small cell) bronchial carcinoma.

<sup>11</sup> A number of studies have examined the inter- and intra-observer reliability of the plantar response in various situations, and some authors have noted issues with variations in technique, and observer bias in elicitation of this reflex.

See also:

Cook C, Roman M, Stewart KM, Leithe LG, Isaacs R. Reliability and diagnostic accuracy of clinical special tests for myelopathy in patients seen for cervical dysfunction. *J Orthop Sports Phys Ther*. 2009;39(3):172-8.

Ghosh D, Pradhan S. "Extensor toe sign" by various methods in spastic children with cerebral palsy. *J Child Neurol*. 1998;13(5):216-20.

Kumar SP, Ramasubramanian D. The Babinski sign--a reappraisal. *Neurol India*.

It was based more on what he was trying to get at. Yes, I've heard that before many times. [chuckles]

SH: They probably have the same origin. [laughter] It's not a unanimity of opinion here but...

DL: That's very likely. That's very possible. Yes, I know what you're saying.

You mentioned, too, that Merritt was a little obtuse at times in terms of explaining himself. Bob Daroff said that of him. He left the residents or the medical students to try and figure it out. Most people who worked with him ultimately figured out much of it, but not always all. Some people found it frustrating. Joe Foley mentioned that many people found it frustrating when they couldn't see how he got from A to Z on his assessments. They couldn't see the steps and he didn't explain. He was very brief, often, with the patients when he was on rounds with them. As Foley said, "Denny-Brown was given to a better educational method, I think. He would explain in detail what was going on. But Houston Merritt so often would come up with a snap answer. He would be right, but it wouldn't be very explanatory for those who were there. I think he was that kind of guy. He was accustomed to knowing things, saying things that he knew, and not feeling the necessity to explain it."

SH: I don't know whether Joe Foley ever told you this. If you are interested in Houston Merritt, you ought to know this story. Houston Merritt would do grand rounds, unlike what we did in the New York Hospital in its heyday when we had one or two or three patients, I think, over one hour. He was invited when he was still young, younger, to Washington [University], Wash U, in Saint Louis to be a visiting professor. They had a full ward and I think they showed him something like twelve patients. Have you heard that story? Did Joe Foley ever tell it?

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2000;48(4):314-8

Kumar SP. The Babinski sign--a critical review. *J Assoc Physicians India*. 2003;51:53-7.

Landau WM. Plantar reflex amusement: misuse, ruse, disuse, and abuse. *Neurology* 2005;65:1150-1151.

Lanska DJ. Extensor toe sign and statistical methods. *J Child Neurol*. 1999;14(5):337-40.

Maher J, Reilly M, Daly L, Hutchinson M. Plantar power: reproducibility of the plantar response. *BMJ*. 1992;304(6825):482.

Miller TM, Johnston SC. Should the Babinski sign be part of the routine neurologic examination? *Neurology*. 2005;65(8):1165-8.

Singerman J, Lee L. Consistency of the Babinski reflex and its variants. *Eur J Neurol*. 2008;15(9):960-4.

DL: I've heard *a* story at Wash U, but not this one, I don't think, of Houston Merritt there.

SH: Do you want me to say it?

DL: Please. Yes.

SH: Okay. The first patient they showed to him was a woman, who was like twenty years old, who now came in unable to empty her bladder. She was paraphasic. Six months before, she had lost vision in one eye and, then, it returned gradually and so on. That was the history. So Houston Merritt came by. He took out his key and he got a Babinski response on one or both sides. Then, he tapped the reflexes and shone the flashlight into the eye. Then, he said to the young lady, "You have a common disease called multiple sclerosis. There are a lot of people who believe they have a cure for it, but as far as I know, there is nobody that has a cure. It's some immunological process that we still don't understand. When there's a cure about it, you're likely read about it in the *New York Times*. Good luck to you. I'll tell you that you will improve, but you'll never get back to what you were, to your baseline. Hopefully, the amount of improvement will be good."

And he said, "Next patient." The patient was unkempt and was found on the side of the street. He was homeless and he was an alcoholic. He looked at his eyes and he [the patient] had unequal pupils. Then, he did a Babinski and he found that he had a positive Babinski and weakness on one side. He tapped over his skull and the guy winced. He said, "What are we waiting for?"

DL: [chuckles]

SH: He said, "We're taking him down to the radiologist." He said—I'm sure you know—"Did you notify the neurosurgeons? Most likely, there's a subdural hematoma. It could be bilateral, but, you know, start on the side of the pupil that's abnormal."

"Next patient." They took him to a patient who was in his early sixties. He had severe dysarthria and he wasn't able to swallow very well. That was the history. He said, "Open your mouth." It was like a bag of worms. Then, he tapped on the jaw for the jaw-jerk and it was exaggerated. He went down and did the Babinskis and both of them were going up. So he just made a motion with his hands and shrugged his shoulders, you know. What do you want me to say? [chuckles]

"Next patient, please," he says. Somebody comes from the back of the room. Somebody was visiting. It was a young assistant professor at that time from Houston. He said, "Dr. Merritt. Dr. Merritt." Houston Merritt said, "Yes?" without turning. He said, "We at the University of Texas in Houston have been giving intrathecal steroids for people with ALS [Amyotrophic Lateral Sclerosis] with some fantastic results." Houston Merritt moved his head like maybe five or ten degrees to the side and said, "Bullshit."

“Let’s go.”

DL: [hearty laughter]

SH: What do you do about cuss words, bullshit like this? You will put the appropriate things there, maybe a couple dash spaces.

DL: If you wish.

SH: Well, it’s up to you.

DL: Usually, I leave them in. That’s the standard. But if you wish to have it extricated, I’ll...

SH: No, it’s up to you. I’ll leave it to you.

DL: Fair enough.

[break in the interview]

DL: Bob Daroff mentioned part of that whole sequence. He must have heard it from you, I’m guessing.

SH: From Joe Foley. We both heard it from Joe Foley.

DL: Oh, Joe Foley. I’ve got it.

SH: I wasn’t there. That’s a story from Joe Foley.

DL: Good to know the source.

SH: Yes. Yes, the source is absolutely important.

DL: All righty. Anything else you want to tell me about Houston Merritt before we switch to Fred Plum?

SH: No. No.

DL: Fair enough.

Let’s talk about Fred Plum. Fred Plum earned his medical degree from Cornell University School of Medicine in 1947, after which he stayed at Cornell for neurology residency under Harold [G.] Wolff, MD [1898-1962]. In 1953, Plum went to the University of Washington in Seattle where he was named head of the Department of

Neurology. After the unexpected death of Harold Wolff in 1962, Plum returned to Cornell as Chair of Neurology at Cornell Weill Medical College.

Sami, what can you tell us about Fred Plum?

SH: He was my mentor, [a] very decisive person. When I went there, people were raving [about him]. When did you say he became head? Harold Wolff died in 1962?

DH: He died in 1962 [February 21, 1962]. Plum returned to Cornell then.

SH: In 1964 or something like that, there was search.

DL: There wasn't a long interval.

SH: Yes. It wasn't a department of neurology that Harold Wolff was in.

By the way, about Houston Merritt, maybe the only thing I want to say is that Houston—Dr. Merritt—also has contributed tremendously to the science of neurology. You know phenytoin?

DL: Yes.

SH: The first paper about the clinical use of phenytoin was by Houston Merritt, I believe, in 1938.<sup>12</sup>

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<sup>12</sup> Putnam and Merritt wrote six papers together between **1937** and **1941**

1. Putnam TJ, Merritt HH. Experimental determination of the anticonvulsant properties of some phenyl derivatives. *Science* 1937;85:525-6.
2. Merritt HH, Putnam TJ. A new series of anticonvulsant drugs tested by experiments in animals. *Arch Neurol Psychiatry* 1938;39:1003- 15.
3. Merritt HH, Putnam TJ. Sodium diphenyl hydantoinate in the treatment of convulsive disorders. *JAMA* 1938;111:1068-73.
4. Merritt HH, Putnam TJ. Sodium diphenyl hydantoinate in treatment of convulsive seizures. Toxic symptoms and their prevention. *Arch Neurol Psychiatry* 1939;42:1053-8.
- 5 Merritt HH, Putnam TJ. Further experiences with the use of sodium diphenyl hydantoinate in the treatment of convulsive disorders. *A m J Psychiatry* 1940;96:1023-7.
- 6 . Putnam TJ, Merritt HH. Chemistry o f anticonvulsant drugs. *Arch Neurol Psychiatry* 1941;45:505-16.

See also:

1. Rowland LP. Introduction. *Epilepsia*1982;23 (Suppl. 1): S1-S4.
2. Rowland LP. *The Legacy of Tracy J. Putnam and H. Houston Merritt: Modern Neurology in the United States*. New York: Oxford University Press, 2009.

DL: Yes, that was Merritt and Tracy [Jackson] Putnam [1894-1975].

SH: Right. It was in mice.

DL: Yes. It was the late 1930s anyway.

SH: Yes, 1938. They were putting electrodes into the ears of mice and producing seizures. That was a *very useful* model. They tried something like 5,000 chemicals. One of the best was phenytoin. Phenobarbital was also effective. These were the ones. Phenobarbital made people sleepy and phenytoin came and we're still using phenytoin up to the present time, so that was a laboratory discovery that had very important clinical use. This is the only addition. Sometimes, I'm a bit slow.

Fred was in Seattle. He had trained with Harold Wolff but then he was, I think, drafted into the Navy. In those days, things were different, you know. You were called to serve your country and people were not supposed to help you dodge the draft. So Harold Wolff did not help Fred, although it could have been better for neurology if Fred was doing work in lab and on the clinical side. Fred went to the Navy and then remained on the West Coast, I believe in Seattle, and became, in a very short period of time, the head of the Division of Neurology.

That's where he met Jerry Posner and many of his earlier people that he brought with him back to New York, Wayne [E.] Crill<sup>13</sup> [c1935-2012] and others.

He trained with Fred at the New York Hospital partly. I think he may have started his training... But he was an M.D. Ph.D. and he was a neurophysiologist and he was professor of both physiology and of neurology and went back to Seattle. He is originally from the northwest.

He [referring to Fred Plum] brought with him a lot of people from the northwest. Jerry Posner, for sure, was there with him. William Shapiro, I think was an intern or was a medical student in Seattle, and so on. He [referring to Fred Plum] would have stayed over there, but they did not want to make neurology a department. Actually, neurology

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3. Kaminski. Review of Rowland LP. The Legacy of Tracy J. Putnam and H. Houston Merritt: Modern Neurology in the United States, *N Engl J Med* 2009; 360:941-942.

4. Friedlander WJ. Putnam, Merritt, and the discovery of Dilantin. *Epilepsia*.1986;27 Suppl 3:S1-20.

<sup>13</sup> Wayne E. Crill M.D: Obituary.

<http://www.legacy.com/obituaries/seattletimes/obituary.aspx?pid=159615300> [Accessed 7-16-14]

did not become a department at the University of Washington in Seattle until maybe ten to twenty years, one of the last.

Is Duke a department in neurology now?

DL: I think so, yes.

SH: But Duke was later.

Anyway, Seattle had a very classical person as head of Internal Medicine and they didn't want to make an exception. They didn't want to fragment Medicine, they thought.

At New York Hospital, neurology was not a department, but they made it a department. Fred was of the opinion that neurology had to be a department, so that was one of the earlier departments that was made for a major medical school. I didn't do research on this, but this is my impression.

Let me inject this story here, because it goes to how I first heard about Fred Plum. Like you said, I graduated with MD after a rotating internship in 1965 at the American University of Beirut. That was the law of the land that you get the MD after internship there to satisfy the laws. Then, I did internal medicine. I was a bit young, you know. When I finished my internship, I wasn't yet 24 years of age. I was of the opinion that you needed to know all of medicine before you went into neurology. It was a waste of time.

DL: [laughter]

SH: But I think it gave you some maturity and I needed it, for sure. [chuckles]

When I was a resident in medicine, Doctor Ray Adams, who had an association with the American University of Beirut—I don't know exactly how it happened—actually had a position at the American University of Beirut, an honorary adjunct position. He also liked to spend time in Lausanne [Switzerland]. If you look at Harrison's [*Principles of Internal Medicine*] where he was the editor of the neurology section, you would see that he always mentioned that he was a professor at the American University of Beirut and at the University of Lausanne. In addition to being an exceptionally good neurologist, there are two things that he did pretty well and not very many people knew about that. One of them was playing golf and the other was playing tennis. He used to play tennis with the residents and was a much better player than I was. One time, I was available and I think Dr. Sabra mentioned could I play tennis with Dr. Adams. I played tennis and I got to know him more. He wasn't your friendliest person, but I think at a game of tennis, we became more friendly. He said, "I hear that you want to become a neurologist." I said, "Yes, once I finish my internal medicine, I want to do clinical neurology and then I want to go to neuro-pharmacology and neuro-chemistry, because I want to become a clinician scientist." At a very young age, I knew that much. He said, "Why don't you come and do your neurology residency training at the Massachusetts General Hospital where I'm

the head?" I said, "Gee, that would be perfect. I know it's very difficult for foreign medical graduates to go to such important institutions," and so on. He said, "Oh, come on. We can play tennis and all of that."

DL: [chuckles]

SH: He was in a very good mood. So I said, "Okay." He said, "When the time comes, you just write to me."

Then, I saw him again. I brought the same thing... I felt badly. He was not as friendly then. I said, "Look, I understand that you told me to write to you personally, apply when the time comes for the Massachusetts General Hospital residency. But, let's assume, for the sake of an argument that I won't be able to make it there. Do you have any other good neurologists that you could tell me about where I could train?" He said, "There are two rising stars on the horizon in America. One of them is George Milton Shy [1919-1967] and the other is Fred Plum." I said, "Where is Milton Shy?" He said, "Milton Shy is in Philadelphia." I said to him, "And where is Fred Plum?" He said, "Fred Plum, he's not a very personable guy. He is a tough character. He's not the friendliest of people. But I believe he is very good. He's now leaving an indelible impression on the scene. He is at the New York Hospital, which is Cornell Medical School." So I said, "Okay. Anybody else?" He said, "This is my recommendation at this time."

Well, of course, I applied elsewhere. One of the places I applied was at the University of Michigan. [chuckles] I got the shortest response, "I regret to inform you that we won't be able to consider your application for neurology training at Ann Arbor, Michigan, because you are a foreign medical graduate, and we don't take foreign medical graduates." Period. I still have that letter, I believe.

DL: [chuckles]

SH: Just to put it in the proper perspective, that was none other than the first Editor-in-chief of *Neurology*.

DL: Yes, [Russell Nelson] DeJong.

SH: Yes, Russell DeJong.

I wrote to many people like I said. There were some people who invited me but I wasn't satisfied. I got a very nice letter from George Milton Shy saying he would have loved to consider my application because I had good recommendations, the packet, and all of that, but he had just taken the job at Columbia University to replace Dr. Merritt. They had taken their slots of residents. But, he said, "Why don't you apply to Dr. Bud Rowland, who I understand has been chosen to replace me at the University of Pennsylvania?" I didn't know who Bud Rowland was at that time. It was a big mistake. He would have been a great chief and I would have been very fortunate to learn neurology under him. I also applied to many other places. Massachusetts General, of course, was one of the first.

I wrote a personal letter to Dr. Adams and nothing happened. Of course, I wrote to Cornell. One time, they called me from the mail room at the American University [of Beirut] and they said, “There is a telegram for you.” I said, “Who from?” You usually get a telegram when somebody dies.

DL: Yes.

SH: In those days, snail mail that is. I went down. It was very unusual. Western Union from New York City, all capital letters saying, “**BASED ON YOUR APPLICATION AND THE RECOMMENDATIONS OF YOUR MENTORS, YOU HAVE BEEN CHOSEN TO BE A RESIDENT IN NEUROLOGY AT CORNELL UNIVERSITY MEDICAL COLLEGE-THE NEW YORK HOSPITAL EFFECTIVE JULY 1, 1968. PLEASE RESPOND WITHIN TWENTY-FOUR HOURS BY PREPAID CABLE.**” I didn’t know what prepaid cable was.

DL: [chuckles]

SH: Twenty-four hours to think about it. He [Fred Plum] took me sight unseen. I think the one that made the difference was a neuro-physiologist who was my professor of neurophysiology in Beirut, who met Fred Plum. He got his Ph.D. in neuro-physiology in addition to his MD in Seattle. He would have arguments with Fred Plum. This professor of mine, Suhayl Jabbur, who is still alive and is a famous neuro-physiologist and is very well published, said Fred did understand neuro-physiology very well, as much as neuro-physiologists themselves. He understood it like a physiologist. He would go to their meetings and discuss them sometimes. He wasn’t very gentle in his critique of them if somebody would make a mistake. People were kind of afraid of this guy who was a clinician who was playing physiologist. So Suhayl said very good things. He said, “Harik did work in the neuro-physiology lab with me. He was the first student I ever got here when I came to Lebanon in 1960,” and so on and so forth. I did accept.

Fred was a very decisive person. He had a reputation as being a taskmaster, maybe a slave driver. He never gave anybody a full mark because the work always was deficient in one thing or another. He was a taskmaster. He earned the respect of everybody. He was intellectually honest, I believe. He wasn’t a [warm and] fuzzy guy, you know, like many other neurologists. He wasn’t the most friendly person. He was very nice to me, I believe. The first year I was in New York City, he invited me to Thanksgiving. When I mentioned that to the rest of the residents, everybody said, “He never invited any of us to Thanksgiving.” But I think I understood—I thought I understood—because I was a foreigner, and my family wasn’t there, and I had nobody [to be with for] Thanksgiving. Everybody was astonished. He would invite you to the Christmas party, for example. He would say, “It is from four to seven.” And at seven o’clock, he would shoo the people out. [laughter]

You understand what I am saying.

DL: Yes, yes.

SH: That was Fred, you know. He would make some outlandish remarks sometimes, particularly politically. He was, I think, very conservative. [pause]

I got married June 2 [1968], and we honeymooned in Spain, and I arrived for neurology training on July 1st. Thank God, I had a full month of vacation for a honeymoon. For example, my wife developed low back pain, which we thought was a herniated disk. Do you think I would ever have dared to consult Fred about it?

We always had the good cop and the bad cop. The good cop was Jerry Posner. He was the second in command—or so we thought. He was the head of Neurology. The day I started my first rotation at Memorial Hospital, Jerry Posner moved from the New York Hospital to Memorial Sloan Kettering on that same day. He was the only attending we had. The others were part-time people who were at Mount Sinai. At Mount Sinai, some of them remained. The neurology load, prior to July 1, 1968, was carried by residents at Mount Sinai and their attendings. They would come; although the New York Hospital was across the street. I think Fred told Jerry Posner, “You are the neuro-oncologist,” and he invented the field of neurooncology and became, I believe, the founder of neurooncology.

DL: Certainly the preeminent practitioner of it, yes.

SH: Certainly.

My wife had a disk problem and I went to Jerry Posner who said, “What are you doing? Just get her over and let me see her right away.” He always found time. He was a taskmaster himself. He didn’t put fear into you; he put shame into you.

DL: [chuckles]

SH: But there were equally hard. I remember the first day I was there, Jerry Posner said, “By the way, there are five EEGs that were done today. I want you to read them.” I said, “I’ve never read an EEG in my life, officially.” He said, “Here is [Leslie.G.] Kiloh and [John W.] Osselton, a monograph [*Clinical Electroencephalography*] on the EEG and how to read it, and the three volumes of Gibbs [*Atlas of Electroencephalography*]...”

DL: [laughter]

SH: ...which had all kinds of examples of what seizures look like, all kinds of seizures, and what triphasic waves would look like, and hepatic encephalopathy. That was baptism by fire, I’ll tell you. I did the best I could. It was a sleepless first night. I remember it wasn’t yet a month probably since I got married. [laughter] I don’t think I went outside of the hospital or our apartment, which was adjacent. That was a good thing about the New York Hospital is that we lived across the street from the hospital. Actually, I lived in Memorial, because my first rotation was in one of the Memorial buildings, which was even better.

Both of them were hard, you know. Fred Plum would tell you, “This patient needs a spinal tap. You are to do it immediately.” Jerry Posner would tell you the same thing. The difference between them is that Fred would come fifteen minutes later and say, “What about the results of the cerebrospinal fluid?” And if he finds out that I have not done it, he would belittle me in front of everybody, and shout at me, and so on. Jerry had a very different technique. If he came back fifteen minutes later, he would go to the patient and say, “Did Dr. Harik do the spinal tap?” and [if] he [the patient] said, “No,” he [Posner] would call the nurse and say, “Get me a set. I’ll do it myself.” Then, you come over there. That was even more effective—he was associate professor at that time—to see him do it and not you. At least to me, that was much more effective than somebody shouting at me. Yes.

DL: Yes, I get it. It would be that way with me, too, frankly.

SH: Fred was a hardball player, I think. He taught me how to think before I talk. Like I would see a patient, an alcoholic, with headache, altered mental state, unequal pupils, weakness on one side, exaggerated reflexes on one side, and I would say, “I suspect a subdural hematoma.” Fred would say, “That’s the Houston Merritt approach.” Fred told me *never* to use that. He said, “How do you know it is a subdural hematoma, not an abscess? How do you know it’s not glioblastoma multiforme? Wouldn’t it be better for you to say, ‘This is the result of a slowly expanding mass in the left cerebral hemisphere that is likely, given the patient’s alcoholism, a subdural hematoma?’” He taught us to think physiologically rather than clinically or anatomically alone.

DL: Or just jump into the final answer.

SH: Yes. I was just like this. I remember a patient that was presented who had a brainstem glioma that’s catching many of the cranial nerves. I was asked to the case and I said, “This is catching all the lower cranial nerves, has no sensory findings because it goes along with the nerve fibers but does not interrupt the central pathways.” And I said, “This is a brainstem glioma.” Fred said, “What’s your evidence?” I said basically the same thing because it’s so typical. He said, “You’re probably right, but that leaves you... Now you made the diagnosis of a brainstem glioma, so you are going to collect the evidence for or against it but in collecting the evidence, you are going to lean so that you will favor *your* diagnosis, because you don’t want to have egg on your face; whereas, if you said, ‘This is a mass either compressing or infiltrating the lower pons and amygdala,’ then that would involve many things, such as multiple sclerosis or an extra-axial tumor, such as a chordoma or so many other things.” Frankly, you can do that and that’s a very good thing if it works. But in the case of a brainstem glioma, I think [unclear] the central pathways affecting the peripheral nerves. There are many other clinical things that make it. I think that was a very, very important thing. Keep the door open. Never commit yourself to a histological diagnosis.

I think despite Fred's advice, as you remember, I would put my neck out sometimes. [chuckles] There's kind of the poker player in me and common things are common and rare things are rare.

DL: Yes. I do remember this. I remember many brain-cutting conferences. You would come up with some really stretched diagnosis that we, as residents, then pondered. How did he come up with that? [laughter] If I had known more about Houston Merritt, I would have had a sense that you were...

SH: I was a fan of his.

DL: ...certainly a kindred spirit, in any case. Most of the time, I recall you were spot on and you stunned everybody, but, once in a while...

SH: I'd fall flat on my face.

DL: Yes. It happened. From my perspective, I was absolutely floored that those wild diagnoses that you came up with proved to be absolutely correct. [laughter]

SH: It's a fun game.

DL: It is! It is and you were a master at it, I thought. That's just great.

SH: It's good for teaching. Right. It has its limitations, unfortunately. One should not take himself too seriously clinically. Nowadays, I don't think we need the Houston Merritts anymore, except that he is right in the fact that if you don't get the diagnosis from the history in around 75% or 80% of the cases, you'll never get the diagnosis no matter what you do.

DL: And you waste about ten million dollars chasing nonsense.

SH: Yes.

SH: Did I tell you enough about Fred?

DL: I want to touch base on a couple other little things.

SH: Okay. Ask.

DL: I got a different flavor from Bob Daroff. He said that you guys...

SH: Bob did not train with him.

DL: No, no. I mean about your reaction to Fred... He knew that you guys had some rough bumps at times.

SH: Yes, we did. Yes. I left the program not speaking to Fred.

DL: Is that right? Wow.

SH: Yes.

Fred in my last year as a resident took a year of sabbatical and he went for the first six months to Lund [Sweden] to work with Bo [K.] Seisjo—pronounced “Boo” in Swedish. The next six months, he was with somebody in London, an experimental epileptologist—he’s actually a good friend—Brian [S.] Meldrum. The acting chair for us residents here was Jerry Posner.

I told Jerry Posner that I was looking for a job in pharmacology. Fred never liked that, by the way, when I told him about that. I had been successful in getting Sol Snyder, who is one of the preeminent neuroscientists in this country, at this time, and founding chair of the Department of Neuroscience at Johns Hopkins University, a very, very bright individual. Sol had promised me a job as a fellow with him. He asked me what I was doing. In the third year of residency training at Cornell, we used to have an elective. You spent six months doing whatever you wanted to do. So the first six months I did the clinical work. I was at the Memorial Sloan Kettering and that was why I’m probably good at neuro-oncology. That really cemented my clinical knowledge and the knowledge imparted on me by Jerry Posner, who is probably the most astute neurologist I’ve ever met and a *very* good older-brother kind of person. I said, “There is the head of Pharmacology at Cornell. Why don’t I go and meet with him about a possible project that he may have in office.”

So I went and talked to the guy. He said, “Sure. I have several projects in mind. Why don’t we discuss them? What do you want to do?” I said, “I want to work on levodopa and carbidopa and how they produce what they produce.” He said, “I’ve always been interested in the synergism between carbidopa, levodopa, and atropine-like agents, like Artane and Cogentin and so on. We’ll design an experiment.”

We sat down and we designed an experiment. There was no carbidopa at that time. This was in 1970. It wasn’t clinically available. We used to accentuate the action of levodopa by pre-treating the animal with monoamine oxidase inhibitors. We wanted to see the hyperactivity that gets in normal cats when you give them levodopa after monomine oxidase and see if there’s any synergism. If we inject them with atropine, would that make the hyperactivity more? Then, we would say, “What is the physiological basis for the hyperactivity by doing lesions in the striatum, in the caudate nucleus, in the head of the caudate nucleus to be exact. So that’s what we did. He gave me money to make thermal lesions in the brain. It was about a \$5,000 investment. We did a lot of work and we passed the animal protocol. We did all the appropriate things.

Then, Fred left for New York like December 20, four or five days before Christmas. So he wanted to meet with all the residents. Each one was given half an hour or forty minutes to meet with him and tell him what he wanted to do, how things were going,

which was just a nice thing. I got into his office and he said, “Sami, I know that you’re thinking of going to Snyder, but this is something maybe a bit more important than what Snyder is doing.” I said, “Okay.” He wanted me to work on glucose metabolism in the brain, which eventually has been a subject that got my interest. I’ve always been interested in that and how the cerebral blood flow is adjusted and controlled to give the energy subserving the organ, the appropriate metabolism for the brain to do the functions it has to do. He went on and on. I tried to interrupt him several times, but he wouldn’t let me. He was like a locomotive. He wouldn’t let me. I said, “Dr. Plum, I have discussed this with Dr. Posner before and for the last three months I have talked with the head of Pharmacology here and he gave me a lab and an office. He invested money in instruments and we saw to the animal protocol. We have a whole thing that is…” He said, ‘Yes, yes, Dr. Posner told me about it. I don’t think that’s an important project at all. You’re missing the boat. I’m giving you a much, much better project,” at which time I said, “Can I ask one more question only?” He said, “Yes, go ahead.” I said, “Is this my elective or your elective?”

DL: Oh! [laughter]

SH: Fred didn’t like that. He said, “I think the meeting is over, sir.” And he didn’t talk to me. Then, when I finished my work, he didn’t call me to his office to say, “Bye, bye,” or anything. I received my certificate of the residency by mail. So it didn’t work.

DL: Yes. I’m sorry.

SH: That’s among other things. But I feel indebted to Fred. He taught me quite a lot. I wish he had a bit more of the milk of human kindness—or maybe he had it but he had a weird way of showing it, sometimes.

DL: You’re not the only one who had some issues [with him].

SH: Yes, I know that. I know. The Fred Plum stories when we meet abound. Yes.

DL: Bob Daroff told me that more of his residents committed suicide than for any other program director.

SH: Yes. He was a taskmaster. There’s very little question about it. But maybe he chose people who were very sensitive. I don’t know. I wouldn’t make a cause-and-effect relationship. [laughter]

DL: No. There are enough different stories. Bob Daroff was always grateful to Plum for the help that Plum gave him in his career, but he acknowledge that he was assertive, and forceful, and authoritarian, and difficult with a lot of people.

SH: Bob liked him. Joe Foley had other explanations.

DL: Yes, he did.

SH: He told you that?

DL: Yes. I've heard it from Joe and from Bob. I think the quote was something like—this is from Joe—“You can only understand Fred if you realize he had bad acne as a teenager and his father was an alcoholic.” That's what Joe said.

SH: Joe Foley said that?

DL: Yes.

SH: Yes, there are other things. We'll discuss it some day over there.

DL: We'll look forward to that.

Let me just ask you too, Bob Daroff recounted Fred's self-experimentation with various things including having himself curarized and put into an iron lung, and experimenting with catheterizations.

SH: He wrote seminal papers on the feelings of the bladder. He catheterized himself, the full bladder and recorded his findings and his sensations and so on.

DL: Bob said he often encouraged his residents to do those things also.

SH: I didn't work with him. He wasn't going to be there anyway. That's the other thing that I didn't go with him. Here he was giving me a very nice project, but he wasn't there to supervise me, so who was going to do supervision? It was a lot of intellectual issues here.

DL: Yes.

SH: He had a question on his mind and between you and me, I'm sure that his question... By the way, I had difficulty in publishing the results, but we did produce some papers out of it. We had a problem.

[break in the interview]

DL: Were you there when Fred did some of his self-experimentation?

SH: Yes. Yes.

One time, I went into the lab to discuss something with Fred and I was shooed out. Basically, there was a famous paper published in the *Archives of Neurology* on the effect

of hypercapnia [sic] on cerebral blood flow and on the pH of the cerebrospinal fluid.<sup>14</sup> I hope I'm not mistaken. There were four subjects [sic]. All of them were authors on that. One of them was the technician in the lab [sic]. I think they put him as a co-author—I'm not sure about that.<sup>15</sup> The other three were Fred Plum, and Jerry Posner, and Marcus Raichle. Here is what they did. They put a needle in the jugular bulb at the base of brain. Okay?

DL: Yes. So this is the Kety-Schmidt technique?

SH: Yes, the Kety-Schmidt. That is done on animals. Kety and Schmidt, well, eventually, they did it on humans afterwards. I don't know whether they catheterized the internal jugular vein, which is easier. But Fred was a purest. He didn't want any contamination, so he wanted to get it from the jugular bulb. That's putting it up there. You can have a hematoma there in the jugular foramen, at least have a problem with the vagus nerve and the glossopharyngeal and the spinal accessory. These are three nerves that pass through the jugular foramen at the base of the skull—and the jugular vein, of course. [D.] Gordon Potts<sup>16</sup> [born 1927] was a wizard. He was head of neuroradiology at the New York Hospital. He did it for them or helped them do it. They had the jugular bulb needle—not a catheter—that was dripping blood at intervals and they also had a needle in the brachial artery. Okay?

DL: Yes.

SH: And they had a needle in the intrathecal [space] to sample CSF.

DL: Wow!

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<sup>14</sup> Raichle ME, Posner JB, Plum F. Cerebral blood flow during and after hyperventilation. *Arch Neurol.* 1970;23:394-403.

The authors did not study the effect of hypercapnea in human subjects, but rather they studied the effect of hypocapnea induced by prolonged hyperventilation.

<sup>15</sup> According to the published paper, “Cerebral blood flow was measured before, during, and after voluntary HV [hyperventilation] in four experiments in three fully conscious, healthy men.” The paper does not indicate which 2 of the 4 “experiments” were on the same subject. The statistical analysis considered all 4 experiments as being on 4 different individuals. The technician did not apparently participate as a subject, and was not included as an author on the paper.

<sup>16</sup> Douglas Gordon Potts [born 1927] was appointed professor of radiology and chief of the section of neuroradiology at The New York Hospital-Cornell Medical Center in 1967. His five-volume text on neuroradiology, coedited by Hans Newton, was considered the definitive work at the time. Potts left in 1985 to accept the chairmanship of radiology at the University of Toronto.

Newton H, Potts DG, eds. *Radiology of the Skull and Brain*. St. Louis: Mosby, 1971.

SH: And they had also the gadgets on the chest to measure, because they were hyperventilating. So the brachial was for getting the periodic arterial samples. They hyperventilated for like four hours [sic, five hours].

DL: Gee. [chuckles] That's intense.

SH: But they produced a paper and the rest is history.

I was recently discussing this from the medical ethics point of view. I think it did have an effect on us. I had wanted to do a preliminary experiment based on some basic science things that I was doing on the effect of apomorphine on amino acids in the blood. The amino acids are co-substrates, like levodopa, for transport across the blood-brain barrier. See, this is why if you take a high protein meal and take levodopa, then levodopa is not going to be effective, because you raised the blood concentration of the amino acids that compete for the carrier of levodopa and then less levodopa will be able to enter from blood to brain. Okay?

DL: There's also the same thing affecting [transport from] gut to blood.

SH: True, gut to blood, but that is a different transporter.

DL: Okay.

SH: That is a concentrator. This is not a concentrator. It's not ATP [adenosine triphosphate] requiring. It facilitates diffusion here. It's not sodium dependent and it's not ATP requiring. So it's passive. So basically I thought that apomorphine may decrease the concentration of those amino acids, because the amino acids will go into the muscles and the liver cell to enhance metabolism and would leave the blood, and therefore there's nothing to compete with levodopa.

It was a shot in the dark and I requested and said, "We want to test it in five individuals." It was me and my nurse clinician and two neurology attendings and one, a friend of mine, who is an oncologist. That was in Little Rock [Arkansas]. I was a volunteer. It's an IRB [Institutional Review Board], you know. I said, "We are doing this ourselves." They said, "You have no right to do this. First of all, you're not a standard bearer. If you are crazy... You have no right. These guys work for you."

DL: [chuckles] Yes.

SH: I respected that opinion. Here, it was a simple thing. The history of medicine is full [of researchers who made progress by self-experimentation]. The first guy to do the right heart catheterization and diagnose whether the patient has patent foramen ovale or not or an atrial septal defect did this on his own.<sup>17</sup>

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<sup>17</sup> In 1929, German physician Werner Forssmann (1904-1979) inserted a catheter into the vein of his own antecubital vein, guided it fluoroscopically into his right atrium, and took

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an X-ray picture of it. Forssmann was twice removed from his position at the Charité Hospital in Berlin over issues related to his self-experimentation. From 1932 to 1945, he was a member of the Nazi Party. At the start of World War II, he became a medical officer. In the course of his military service, he rose to the rank of Major, until he was captured and put into a U.S. POW camp. In 1956, he shared the Nobel Prize in Physiology or Medicine for his achievement.

See also:

Berry D. Pioneers in cardiology. Werner Forssmann-sowing the seeds for selective cardiac catheterization procedures in the twentieth century. *Eur Heart J.* 2009;30:1296-7.

Berry D. History of cardiology: Werner Forssmann, MD. *Circulation.* 2006;113:f27-8.

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Bourassa MG. The history of cardiac catheterization. *Can J Cardiol.* 2005;21:1011-4.

Braun R. Voyaging in the vein: medical experimentation with heart catheters in the twentieth century. *Nuncius.* 2011;26:132-58.

Delfino AJ, Cornet PS. Recollection about Werner Forssmann as a surgeon. *Eur J Med Res.* 2006;11:413-4.

Forssmann WG, Hirsch JR. 50 years Nobel Prize: Werner Forssmann and the issue of commemorative stamps. *Eur J Med Res.* 2006;11:406-8.

Forssmann-Falck R. Werner Forssmann: a pioneer of cardiology. *Am J Cardiol.* 1997;79:651-60.

Hart FD. Werner Forssmann (1904-1979), auto-experimenter/medical martyr. The original cardiac catheterization. *J Med Biogr.* 1997;5:120-1.

Heiss HW. Werner Forssmann: a German problem with the Nobel Prize. *Clin Cardiol.* 1992;15:547-9.

Hollmann W. Werner Forssmann, Eberswalde, the 1956 Nobel Prize for medicine. *Eur J Med Res.* 2006;11:409-12.

Meyer JA. Werner Forssmann and catheterization of the heart, 1929. *Ann Thorac Surg.* 1990;49:497-9.

Raju TN. The Nobel chronicles. 1956: Werner Forssmann (1904-79); André Frédéric Cournand (1895-1988); and Dickinson Woodruff Richards, Jr (1895-1973).

DL: Yes.

SH: He was looking at the fluoroscope.

DL: Yes.

SH: I don't know what medical ethicists say about that. I always was brought up to believe that if you treat people like you treat yourself, you can't be...but that is not good enough these days.

DL: I think there's a difference between—I'm not saying it's right—doing it on yourself and doing it under people that might feel pressured to do it on the basis of your authority or influence. That's a little bit different.

SH: Yes, that's a bit different. You're right. But, here, I think Fred did it on himself, did it on Jerry Posner, did it on Marcus Raichle, all important neurologists, you know. Marcus Raichle is now a member of the National Academy of Science.

DL: Sure. But, I have been told, he [Plum] pressured his residents to do it, too. That's where the influence issue could come up, I'm sure.

SH: Yes.

There was a very important head of medicine at the University of Miami when Bob Daroff and I were there, and he gained his fame. He was one of the youngest department chairmen. He is the one who injected himself with the plasma of a patient with thrombocytopenic purpura. He got the platelets. So he took the plasma. In those days,

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Lancet. 1999;353:1891.

Sette P, Dorizzi RM, Azzini AM. Vascular access: an historical perspective from Sir William Harvey to the 1956 Nobel prize to André F. Cournand, Werner Forssmann, and Dickinson W. Richards. *J Vasc Access*. 2012;13:137-44.

Siegel D. Werner Forssmann and the Nazis. *Am J Cardiol*. 1997;80:1643-4.

Steckelberg JM, Vlietstra RE, Ludwig J, Mann RJ. Werner Forssmann (1904-1979) and his unusual success story. *Conn Med*. 1981;45:110-2.

Steckelberg JM, Vlietstra RE, Ludwig J, Mann RJ. Werner Forssmann (1904--1979) and his unusual success story. *Mayo Clin Proc*. 1979;54(7):746-8.

Truss MC, Stief CG, Jonas U. Werner Forssmann: surgeon, urologist, and Nobel Prize winner. *World J Urol*. 1999;17:184-6.

there were no HIV [Human Immunodeficiency Virus] and all of that. He got the plasma, washed it out of the buffy coat and white blood cells of a patient with thrombocytopenic purpura...allergic.

DL: That to me sounds bloody crazy, frankly. [chuckles]

SH: He did it and he published it on a single person. He measured his platelet count over time after he injected that. He got a nice drop [in platelet count] that lasted about five or six hours [sic, 4 days] and, then, it came back to normal [on the fifth day]. Fortunately, he didn't get a bleed in the end [though he did have a seizure].

DL: Yes.

SH: It's crazy. [William J.] Harrington [1923-1992] was his name.<sup>18</sup> He did that when he was, I think, a young assistant professor [sic, hematology fellow] at Wash U [Washington University in St. Louis, MO] and immediately got promoted. That is a single experiment that proved that thrombocytopenic purpura is an autoimmune disease.

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<sup>18</sup> Harrington WJ, Minnich V, Hollingsworth JW, Moore CV. Demonstration of a thrombocytopenic factor in the blood of patients with thrombocytopenic purpura. *J Lab Clin. Med.* 1951;38: 1-10.

The experiment was conducted at Barnes-Jewish Hospital in St. Louis, Missouri, in 1950. William J. Harrington and James W. Hollingsworth were hematology fellows there, and they decided to test their idea that the cause of idiopathic thrombocytopenic purpura was a circulating factor in the blood that caused the destruction of platelets. The Harrington-Hollingsworth experiment established the autoimmune nature of idiopathic thrombocytopenic purpura (which since has been referred to as immune thrombocytopenic purpura, preserving the acronym). Bone marrow biopsies from Harrington's sternum, both before and after the infusion, demonstrated normal megakaryocytes (necessary for platelet production), supporting destruction of platelets as the basis for the precipitous drop in platelet count experienced by Harrington. The circulating factor has subsequently been identified as IgG antibodies with specificity for platelet glycoproteins.

See also:

Obituary: William J. Harrington, 68, doctor and specialist in blood diseases. *New York Times*, September 6, 1992. <http://www.nytimes.com/1992/09/06/us/william-j-harrington-68-doctor-and-specialist-in-blood-diseases.html> [Accessed 7-17-14]

Schwartz RS. Immune thrombocytopenic purpura--from agony to agonist. *N Engl J Med* 2007;357: 2299-2301.

Stasi R, Newland AC. ITP: a historical perspective. *Br J Haematol.* 2011;153:437-450.

DL: William [B.] Castle did some of the parts of his work with intrinsic factor using himself as the prototypical normal stomach to provide the intrinsic factor to boluses of meat that he stuck down [into his stomach] on a string and, then, pulled them back to use in patients who had pernicious anemia.<sup>19</sup>

SH: I see.

DL: But he didn't put himself at terrible risk with that either. There are other people that put themselves at amazing risk.<sup>20</sup> Joseph Goldberger and his wife and colleagues used

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<sup>19</sup> Lanska DJ. Historical aspects of the major neurological vitamin deficiency disorders: the water-soluble B vitamins. *Handb Clin Neurol*. 2010;95:445-76.

Lanska DJ. William Castle. In: Aminoff MJ, Daroff RB (eds.) *Encyclopedia of the Neurological Sciences*, 2nd ed. Oxford: Academic Press/Elsevier. 2014;1:608-609.

<sup>20</sup> At least 8 deaths have resulted from medical self-experimentation since 1800, including the deaths of Peruvian medical student Daniel Alcides Carrión (1857-1885), U.S. Army physician Jesse Lazear (1866-1900), and British entomologist Arthur Bacot (1866-1922) (Weiss 2012). Carrión infected himself from the pus from a skin lesion of a patient with Oroyo fever (bartonellosis) in 1885, and died several weeks later from the disease. In Cuba, in 1900, U.S. Army doctors from Walter Reed's (1851-1902) research team infected themselves with yellow fever, and one of them, Jesse Lazear (1866-1900), died from yellow fever complications. Bacot was studying typhus and allowed the lice to feed on his arm, ultimately dying of the disease himself, which a colleague similarly infected nearly died.

See also:

Bryan CS. Gloria in absentia: Walter Reed, James Carroll, and the ethics of authorship. *Pharos Alpha Omega Alpha Honor Med Soc*. 2006;69(4):16-22.

Carmichael EB. Jesse William Lazear. *Ala J Med Sci*. 1972 Jan;9(1):102-14.

del Regato JA. Jesse William Lazear: the successful experimental transmission of yellow fever by the mosquito. *Med Herit*. 1986;2(6):443-52.

Greenwood M. The life and scientific work of Arthur William Bacot. *J Hygiene* 1924;22:265-304.

Osler W, Paton S, Thayer WS. Jesse William Lazear memorial. *Science*. 1901;14(345):225.

Pierce JR. "In the interest of humanity and the cause of science": the yellow fever volunteers. *Mil Med*. 2003;168(11):857-63.

the lesions of people with pellagra and scarified their own arms and tried to transmit what was then contested as an infectious or dietary disease to themselves.<sup>21</sup> They also swallowed the fecal matter pills[from pellagrous patients] and this and that. He put his wife at risk. He believed he was right, but nevertheless one could argue about [the ethics of] that as well.

SH: Yes.

DL: Going back to Fred Plum then, were you ever asked to participate in any of these types of things?

SH: No. No.

DL: Fair enough.

SH: Fred, I used to stand up to him. I was a bit immature. He is [was] very good at putting you down. I didn't like that very much. People there knew about Fred. He wanted economy of words so you don't need to say, "The airplane is completely full." It's either *full* or *not full*, but *full* meaning [that] every seat is taken. Now, I wasn't there. I remember Fred Plum the other time was there and said, "The airplane is extra full."

DL: [chuckles]

SH: "What do you mean *extra full*?" Or if you go to an Italian restaurant and they say, "The olive oil is extra virgin," as if *virgin* is not enough, you know.

DL: [chuckles]

SH: So one time Fred Plum was in a big round with a lot of students. He gets derailed very easily. He would quip that he reserves the right to do this all the time. One time he

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Smith AH. Bacot, a martyr to science. *Scientific Monthly* 1922;15:359-263.

Weisse AB. Self-experimentation and its role in medical research. *Texas Heart Institute Journal* 2012;39:51-54.

<sup>21</sup> Lanska DJ. Historical aspects of the major neurological vitamin deficiency disorders: the water-soluble B vitamins. *Handb Clin Neurol*. 2010;95:445-76.

Lanska DJ. The discovery of niacin, biotin, and pantothenic acid [Invited. review]. Discovery of the vitamins – 100th anniversary special issue. *World Review of Nutrition and Dietetics. Annals of Nutrition and Metabolism* 2012;61(3):246-53.

Lanska DJ. Joseph Goldberger. In: Aminoff MJ, Daroff RB (eds.) *Encyclopedia of the Neurological Sciences*, 2nd ed. Oxford: Academic Press/Elsevier. 2014;2:461-463.

was talking about the higher intellectual functions and, for some reason, I just said, “In contradistinction to the lower intellectual functions?”

DL: [laughter]

SH: Higher cortical functions [compared] to the lower cortical functions? [laughter] He glowered at me. I knew that I was in [trouble]. He didn't chastise me in front of everybody. I think, basically, Fred was a bully to some of the residents, but he usually did not take people that he thought were crazy or unpredictable. So it came to me afterwards that his idea of the Arab culture was entirely... There was some basic flaw to it. So he would have these simplistic ideas that Arabs and other people of the Orient have loss of face and if they have loss of face, there is no telling what they could do. I believe he never insulted me in public, because he thought I may have been unpredictable and I could have thrust a dagger at his heart or something.

DL: [laughter]

SH: I didn't do anything to discourage him from thinking that.

DL: Okay.

SH: When Fred Plum died, I cried. I think he had a tremendous influence on me. We can talk all we need about that, but it's neither here nor there. I hope you'll edit this here gently and appropriately. Maybe what I said was not too great to the memory of this great person. He was really a great person. He brought *Stupor and Coma*, a major contribution to our understanding of the cortical functions and of consciousness.

DL: Yes, it certainly is.

SH: Like he said, I think at maybe the twenty-fifth anniversary of his becoming chair at Cornell, he became chair in 1962. I joined him in 1968. He was a lion with a full set of teeth, all his canines sharpened and developed.

DL: [chuckles]

SH: I'll tell you he was a lion and he behaved like a lion and without much consideration, a very effective teacher. But, he had a way of really humiliating people, which, I must admit, never happened to me, fortunately.

DL: That's good.

SH: I would sometimes egg him on. Partly, it was my fault. I should have kept my mouth shut. On many occasions I could have become much more friendly with him.

DL: Bob Daroff said there was a difference between how Plum interacted with Posner and with you. He attributed it to Jerry being a little more gentle and easy going.

SH: Jerry was in awe of him. He was like a father to Jerry.

DL: Yes.

SH: Jerry understood him. He was always at his side. When Fred was on his sabbatical, I would be giving morning report to Jerry. The call from Fred would come during the morning report, sometimes when he called. You always knew that it was Fred on the other line because Jerry couldn't take the call sitting down.

DL: [laughter] Why?

SH: He loved him. He put him on a pedestal. Yes, yes. He thought he was a very, very bright guy.

DL: Yes, well, they clearly worked well together. That's certainly true.

SH: Oh, yes. I don't think Fred was always nice to Jerry.

DL: Oh, I don't know that he was. I don't know that. I know that every time I met or talked with Jerry, he's always been kind to anybody I've ever seen him interact with.

SH: He's the kindest person in the world.

DL: Yes, exactly. Bob said something similar and others have said the same thing about Jerry. But Fred got lots of different reactions from different people.

All right. Let's switch gears now to Peritz Scheinberg [1920-2005]. Peritz Scheinberg was the founding chair of Neurology at the University of Miami School of Medicine, a position he held for more than three decades, from 1955 until his retirement in 1989. At the University of Miami School of Medicine, Scheinberg established a research laboratory and studied human cerebral circulatory physiology using the Kety-Schmidt technique, the same technique Plum had self-experimented with.

What can you tell us about Peritz Scheinberg?

SH: Peritz was a very smart guy and he was effective. That is to his credit. He built a very good Department of Neurology at the University of Miami, which was a new medical school. Neurology and a few other departments, very few, maybe one or two, were, I believe, islands of excellence. Right. They were islands of excellence. That suffices.

DL: Okay.

SH: Scheinberg was a smooth guy. He was a debater. He actually used to win debating contests in college. He is logical. He is cool. People did not take him seriously, I think,

but he did persevere. He was born in Miami. When he graduated, there was no medical school in Miami. So he went to graduate from Emory or Duke [University]? From Emory, I believe.<sup>22</sup> He did his internship at Grady Memorial Hospital in Atlanta. Then, he did his training with Eugene [A.] Stead [Junior; 1908-2005] at Duke.<sup>23</sup> That's where he learned the Kety-Schmidt technique. He built the department mostly with people from Duke. That's where he got [Oscar M.] Reinmuth [1927-2011], and Nobel ["Nobby"] David [1927-2011] came to him from Duke, and [J.] Lawton Smith [1929-2011], who was the famous neuro-ophthalmologist and also a Duke graduate. All of these guys were classmates...Nobel David, Oscar Reinmuth, otherwise known as Mack Reinmuth. Peritz got the chair and he built a very good department. The University of Miami was kind of weak at the hospital. [pause] It wasn't Cornell or Hopkins, but it was very good.

My experience with him... He's another very decisive guy. After I finished my training in pharmacology, I went to Lebanon for three years. I decided I couldn't do research and I wanted to come back. There was no mail and there was no email. It was by telegram and telephone when the telephone works. I had friends here. One of them Jerry Posner who knew of my predicament and helped me with letters of recommendation. I was interviewed in many schools. I wanted to be an academic. I took the job with Peritz; although, there were much better...much better institutions that offered me positions. Peritz was very practical. He did his homework. It's very interesting. I went down. They took me to see patients, see how I react. I took the residents, and they presented cases to me, and I was making diagnoses. He was very happy. I think he liked my direct thinking. He liked the fact that I was not afraid to stick my neck out. I think he liked that. He didn't know too much science. We did talk, then went to his office, and he said, "Look, I like you. I've seen your CV [curriculum vita]. I've talked to your mentors. I talked to Jerry Posner. I talked to Fred Plum. I talked to Solomon Snyder. Based on my talks with them and my seeing you today here, I think we would like to have you join our department." I said, "Good. My most important thing is I want to do part clinical and part research." He said, "Okay." I said, "I'm thinking about 40% research, 60% clinical." He said, "That's wrong." I said, "Oh, here's problems again."

DL: [chuckles]

SH: I said, "How do you want it?" He said, "You should go 60% research and 40% clinical." He's a clinician. I don't think he knew too much science. He did learn the Kety-Schmidt technique and he milked it to the best, you know.

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<sup>22</sup> Scheinberg received his undergraduate degree from Emory University in Atlanta, and his medical degree from Duke University in Durham, North Carolina.

<sup>23</sup> Eugene Anson Stead Jr., MD (1908-2005) was a highly regarded medical educator and researcher, and was also the founder of the Physician Assistant profession. He served on the faculty at Harvard, Emory, and Duke universities. He was Chairman of Medicine (1942-1946) and Dean of the School of Medicine (1945-1946) at Emory University, and then Chairman of Medicine (1947-1967) at Duke University.

He said, "Look, tell me one thing, which I still can't make [understand]. What's between you and Fred?" [chuckles] I said, "How did you know about that?" He said, "I talked to Fred." I said, "What did Fred say about me?" He said, "Fred said you're a troublemaker. So I rushed to the phone and spoke to Jerry Posner and Jerry Posner said you're a good guy. Solomon Snyder said the same thing. So I am betting based on your CV and your publications, the one that you did at Hopkins, that you are going to be a good scientist and I'm ready to bet on you." He was a good horse buyer, you know? He looked at the horse and he said, "Is this horse going to do well or not?" I think that was good.

So I said, "When can I start?" I didn't have a license to practice medicine in the State of Florida. He said, "Are you set for the FLEX [Federal Licensure Examination]?" I said, "Yes, but I haven't gotten the result yet." He said, "You'll pass." Just like that. He said, "I can hire you. You can apply for the Florida licensure. Then, you can start seeing patients." So I started in July and didn't get my license until February. So I spent the first seven months in the lab, really building the lab. That was a good thing.

I said, "When can I start?" He said, "You can start any time you like." I said, "I just came back from Lebanon." During my interviews, I got typhoid fever. I had it probably on the plane with me because I had a fever. I was in the hospital for a week, so I was a bit pale and emaciated from being on IV [intravenous] fluids for a long period of time.

Anyway, he was very helpful. I said, "I need a technician." He said, "You can hire a technician any time. If you start work tomorrow, you can have a technician the day after tomorrow, if a technician is available." We just started writing the ads, you know. Of course, he made it dramatic. But that was very important. There was a guy that was a neuro-chemist. He said, "I don't have money for high-ticket items to get heavy equipment. Go see what's in the lab." I went to the lab. They had a scintillation counter which is very expensive [and] which is desperately needed, and all the appropriate centrifuges, tissue homogenizers, things like that, things that I needed. I needed very few things. So I said, "The equipment is good." He said, "How much in supplies do you need?" I tried to inflate it at that time, you know. I said, "Maybe \$20,000." He said, "We can manage that. The money for the technician, we can manage that." He didn't say, "You've got to write grants, get your money. Once you get your money..." Then, I would not have accepted Miami. So I accepted Miami. He was a visionary in a way. What he didn't tell me at that time was he was taking the money from some *other* faculty. I don't know what he promised to some of them. They said that they were promised this, and, being the new kid in the department, I was getting the new toys. There were other things with him. I said, "How much are you going to pay me?" People think I'm from the Middle East, so I'm good at bazaaring, like rugs.

DL: [laughter]

SH: In this aspect, I'm a real Anglo-Saxon. I said, "Look, Dr. Scheinberg, I'm not here to bazaar about my salary. I want a salary that's commensurate with my contributions to the department and with my rank. So here I was. I'm trained in internal medicine. I'm trained in neurology. I'm trained in neuro-chemistry and neuro-pharmacology, and I've

been assistant professor at the American University of Beirut for three years, during which I published some six papers, or they are in press or in preparation. I want credit for that. So I want to be considered for associate professorship within two years and I want to be paid at the higher end as assistant professor.” He said, “How much is that translate to?” I said, “I don’t know, whatever you pay.” He said, “Would \$35,000 per year be good with you?” This was in 1976. I said, “If that’s what you pay the other people, I’m not arguing about it.” That was wrong, because he was the horse trader. I found out before [later that] his conscience was bothering. He would give everybody [annual raises] like that 2%, 3%; he’d give me 12%. I knew why he was giving me that, because even with all these percentages, I was still the lowest paid academic in the department. I made him pay for that, though. That was one of the reasons I left Miami.

Basically, he was a very nice leader, I think. He was decisive. He had his heart in the right place. He bet on me and I’m forever thankful for him. I started my scientific career on my own. All the publications I had before, unless those in Beirut and they were mostly clinical. People thought I was there because I was with Sol Snyder and he had a big operation. Of course, people do a good deal. I started on my own, stood on my own two legs. It was good for him. I was pretty well granted by the time I left. I didn’t cost him anything. I took the residency training position for him. I taught the residents, gave them courses in neuro-pharmacology and neuro-chemistry, which were [previously] their lowest points in the in-service exam. During the six months when I didn’t have a license, I did teach the residents a lot. He put me in charge of the residency training, like I said. I equipped the lab and started going and writing grants. So it was good for him. It was a win-win situation. I didn’t like the shenanigans about the... I learned a lot from the people that didn’t treat me right sometimes, because when I became a chair of a department, I was forever attentive. I never would be embarrassed if a [news]paper [published the salaries in the department]...which actually happened, because in Arkansas it is public knowledge. It is a state university. Sometime in late January, I believe, the Little Rock paper publishes how much everybody in the medical school makes. I never had any problems. I maintained the internal equilibrium [of the department] and I was always ready to defend why I’m paid so much if somebody...

DL: Yes, it’s important.

SH: Very important. There’s nothing that destroys [support and cooperation like inequity]. I spent a sleepless night when I found out that I was lower paid as associate professor than many of the assistant professors.

DL: Yes, that just breeds terrible discontent.

SH: Absolutely.

We became good friends. I knew his problems. He begged me not to come to Cleveland when Bob Daroff offered me the job. You know we’re speaking about a lot of dead people and I’m feeling very guilty about the issues... [chuckles]

DL: No, I think...

SH: ...that nobody does tell you, probably.

DL: No, you would be surprised. I've heard many things.

SH: Good.

DL: I can tell you I've been told that he was always very tough with students and that they were generally scared to death of him. It sounds a lot like Fred Plum in that regard.

SH: Mmmmm... No, it's a different thing.

DL: Is it?

SH: I caught him after he lost his teeth. I was there in 1976 and was a bit older than Fred. I don't know what year he was born. Maybe 1920 [Fred Plum born January 10, 1924]. So 1976... He was fifty-six. He was getting older. When I was a resident with Fred, he was like forty-something, maybe forty-three.

DL: In terms of his dealing with students and residents, how was Peritz?

SH: I don't know with the students. But the residents, they all liked him.

DL: Is that right? Good.

SH: He was not a bullshitter. He was suave. He'd have these glossy suits. He was a dresser. He married a younger woman later on. There's nothing wrong with that, but in those days it was considered to be not too fashionable for an academician.

DL: What did you think of him clinically?

SH: He was very smart. He saw patients at a very fast rate. He is not as sharp as Houston Merritt but, given the time he spends per patient, per new patient, I think he was very fast. Let's not describe numbers here or be responsible... One time he became sick and I took over some of his patients. I thought I'm going to find a lot of mismanagement. How could he do all of this or that diagnosis? I'll tell you, I didn't see a thing [wrong with his diagnoses]. He had a good knack.

DL: Maybe I'm wrong, but what I understood from Bob Daroff was if he [Peritz Scheinberg] had to spend more than twenty minutes with a patient, he either admitted them for a workup or...

SH: That's right, that's right. He did that. But you can't find a misdiagnosis, almost.

DL: Yes, sure.

SH: He either knew it or didn't know it and referred them to somebody who could diagnose. He didn't spend too much time, didn't like shaggy-dog stories. Neither did Fred.

DL: [laughter]

SH: That's when I learned about shaggy-dog stories. He didn't want... History has to be precise, has to be rehearsed, has to be delivered within two minutes.

DL: [laughter] Shaggy-dog stories.

SH: Good guys, you know. I've met with good guys all my life.

DL: Yes, exactly.

SH: Bob Daroff was the real person superior to me that I became very friendly with. I could tell him to his face that this is a mistake. I wouldn't do that if I were you. I was in that kind of a relationship with Bob.

DL: Yes. He respected you for all of that, too. I can tell you that.

SH: I would hope so. I did many of the things he didn't want to do.

DL: Yes, he trusted you fully. That was my impression.

SH: Maybe I burned my bridges at Western Reserve because of being very loyal. I went there to fight the department once I was convinced. Yes, that was my job. I was the vice chair.

DL: Yes. That's like being a vice principal at a high school. It comes with a lot of garbage sometimes.

SH: Yes, it comes with a lot of things.

DL: Fair enough. Anything else you want to tell me about Peritz or shall we move on to Bob Daroff?

SH: Peritz, I know, is maligned by a lot of people. I didn't agree with him about politics of the Middle East. I did not agree with him on a lot of issues, but he was always honest. He had the humility to apologize once he was wrong.

DL: Which is very credible, indeed.

SH: Yes. I always remember him fondly.

DL: As did Bob. Bob did as well; although, we was always puzzled by Peritz's approach to headaches and things. He [Peritz] apparently thought all headaches were psychogenic, that sort of thing.

SH: He's probably right. Anyway...

DL: Yes, at any rate. Are we ready to go on to Bob?

SH: Yes.

DL: Bob Daroff started his post-fellowship career in Miami in 1968, so he was there about eight years before you came. What can you tell us about your interactions with Bob while you were in Miami?

SH: Well, Bob had a meteoric rise, you know, in Miami. He rose very quickly through the ranks. He was the academic in the department, although he did some research with Lou [Louis F.] Dell'Osso. He had a lab. Lou Dell'Osso gave Bob Daroff the scientific legitimacy of becoming the preeminent neuro-ophthalmologist, because of the recording and quantitation of the eye movements. Otherwise, it would have been like Cogan, you know. There's nothing wrong with that. Cogan was very shrewd and he came at a time...and you can only have so many Cogans. Bob Daroff and Lou Dell'Osso started tracking the eye movements by putting coils on the eyeball and asking people to look here and there and recording eye movements, scientifically measuring the speed and the velocity using vectors and all kinds of things.

So he came in 1968. I think he was forced on Peritz by Ophthalmology. Ophthalmology was the other great island of excellence. They were interested... The neuro ophthalmologists, Nobel David and Lawton Smith, I think wanted Bob, and Peritz took him. He was in the V.A. where they were good, so that allowed him to run his lab and all of that, but he also saw private patients and maintained... He quickly rose through the ranks.

He and I clicked together about a lot of things. He would help me. He took me under his wing when I came. Like writing my CV, he showed me a good way of writing my CV and all of that. He and I talked about things and we rapidly became friends.

When I found out that Peritz was not paying me as much as he should have, I wanted to make a big scene, but Daroff intervened and asked me to cool down my temper and to go about it in a more civilized way, less volcanic. So I learned a lot from Daroff, how to achieve results without too much steam and heat, more light.

When he came up here, he was building a department.

DL: By here, you mean Cleveland.

SH: Cleveland...yes, Western Reserve.

Bob believed he was given certain [faculty] lines and he thought that he should fill the lines as quickly as possible even in the absence of very good applicants, because if you don't fill them, they'd be lost to you, something that I didn't agree with, so I helped him. I was his scientific advisor because of my connection with my friends who were in Sol Snyder's lab. Many of them became leaders. Basically, that lab was spinning [out] a lot of people. I made a lot of friends and I had lots of friends with Fred, that program. It was full of all good people. So the important thing is that you develop friendships, so if you have a problem in science or clinical, you can always ask. Daroff would ask some questions at times and I would find the answers for him because of my wide connections at Hopkins and Cornell. He was thankful.

He asked me to consider coming to Case Western Reserve and I thought that Case Western Reserve had a better reputation academically than the University of Miami. I thought it was free of politics—a big mistake. [chuckles]

DL: Yes.

SH: There's nothing free of politics. Don't ever kid yourself about that.

DL: I have not, trust me.

SH: There's no place that is free of politics.

I thought that Case was going to be much superior, but as I became older and wiser, I somehow feel that it's [reputation is] overinflated. The Ivy Leagues, the old universities... The University of Miami and that's about 25 years [ago] when I joined it. The lifetime of academic institutions compared to Bologna, and Oxford, and Cambridge, and Sorbonne, and Montpellier was...

DL: Trivial, yes.

SH: ...was trivial. I think if you get good people and they start working, you don't need... Tradition, I think it's important. I don't want to bear down on tradition...very important, but, also, you have to know what you're doing and get the young people, although they could be a bit of Neanderthals in the beginning or could be bulls in the China shop.

Bob taught me a lot of things. I helped him shape the Department of Neurology, I believe, and I helped him run it. Unfortunately, his reign did not last for as long as I wanted it. He took the chair in 1980 and he left in 1994.

DL: To become Chief of Staff, you mean.

SH: Yes, to become Chief of Staff, which is, you know...

DL: Well, you worked with him for...

SH: A great guy, solid, honest, big brother to me. Yes. I helped him get all these people like [R. John] Leigh, [Robert L.] Ruff. Ruff, I got him because of my connection with Jerry Posner. I said, "Who could we recruit?" He said, "The best guy you could ever recruit, probably the best resident I ever had, was Bob Ruff, for a variety of reasons."

Were you there at Ruff's festschrift?

DL: Yes, sure, I gave a talk.<sup>24</sup>

SH: I gave a talk, too.<sup>25</sup>

I remember all these trips when I got him [Robert Ruff] and Louise [Ruff's first wife] there and we discussed. He was hesitant at the beginning and [I] kind of got him involved and he's still there. These were all good people that I helped. He got some people before I came that were not all that great and eventually left. I think the department was quite solid.

DL: Oh! It was fantastic, I thought.

SH: Yes.

DL: It was a great place to do residency for me. It was really fantastic. You worked with him for, roughly, two decades, right?

SH: Yes, between 1976 and 1994, eighteen years.

DL: You're in as good position as anyone to provide kind of an assessment of him as a person, as a teacher, and as a leader, don't you think?

SH: Yes. Yes. He is fantastic at everything. He is a good teacher. There's no question about it. He's a very good teacher. He is a good clinician. True, he's very good at some of the softer things like headache. I'm like Peritz that I'm not so sure I know why, for example, Bob would give them monoamine oxidase inhibitor sometimes or something else at the other times. He had his way of dealing with this. I think he took headaches seriously. I didn't think there was a scientific basis to underline it, you know. So

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<sup>24</sup> Frontiers in Neuromuscular Disease and Neuro-Rehabilitation: A Conference to Honor Robert L. Ruff, MD, PhD, October 18-19, 2012. University Hospitals of Cleveland and Louis Stokes Cleveland VAMC, Cleveland, Ohio.

Lanska DJ. An overview of VA contributions to neuro-rehabilitation. *Neurological Institute Journal* 2013;5(2):16-17.

<sup>25</sup> Harik S. Neurological complications of bariatric surgery. *Neurological Institute Journal* 2013;5(2):16-17.

effective leader? He is an effective leader. Bob Daroff taught me a lot of things. I'm forever in his debt. He made me Associate Editor of *Neurology* and that got my interest going in medical journalism. I've always been interested in it, but he offered. It turned out that he has interest in journalism ever since he used to write the college paper at the University of Pennsylvania in Philadelphia.

DL: Right.

SH: I have learned a lot from him. I have learned how to tone down, relax. He is very kind to teachers. He is very kind to residents. Medical students love him. Residents love him. [

His problem, I think, is [that] I don't know that he could stand up to bullies, sometimes. I think he was bullied out of his job as the chair, personally. He's very straight. I think Case Western Reserve and Neurology would have benefitted from him for an additional ten years as chair of Neurology.

DL: Yes, I'm sure that's correct, actually. He expressed frustration to me...

SH: Enough said. Enough said about this. A lot of people, many of them are dead and gone.

DL: Sure.

SH: The statue of limitations has passed.

DL: Yes. I get it.

SH: We're talking about a lot of people who aren't alive.

DL: No, I understand. He did, though, say that he was worried that the finances were difficult and that he wouldn't be able to keep all the balls in place, so to speak, and that it was getting harder and harder...

SH: Better with the finances. How do you do better? He had the support of [Robert A.] Ratcheson [Chairman of the Department of Neurosurgery at University Hospitals of Cleveland] and ...

DL: Tell me that again.

SH: [chuckles] Uhhh... There was a group of people at Case that didn't want Bob to stay on, I believe. That's my understanding now. I don't know the exact details, but that's my hunch.

DL: Okay. All right.

SH: Bob is still there and he doesn't want this to be out, I don't think. So you better edit it.

DL: Well, he told me some of this, so some of this he actually...

SH: I don't want it to come to me if you can. If you can, just make a note of it.

DL: I will see what I can do. I will leave it there.

Any other stories you wish to share about Bob or your interactions with him over the years?

SH: There were a lot of stories. This is a long interview here. I guess I'm running out of steam.

DL: Okay.

SH: We have a lot of stories about Bob. [laughter] I don't see Bob very frequently [any more]. [pause] Not any dramatic stories like the Peritz Scheinberg and Fred Plum. Bob is a mild person. He doesn't do anything outrageous that leaves an imprint for eternity. He was a kind person. He understood eye movements, I think, in science, but I think he depended on me to give him the microbiology and chemistry and pharmacology part of science. He is an ethical person. He attained very good positions in American neurology: president of the American Neurological Association and Editor-in-Chief of *Neurology*. He did a very good job with the journal. All of these are feathers in his hat. He left a good legacy. His residents love him. He has good followers among the people he trained and among his friends and colleagues. He is not a controversial fellow at all.

DL: Yes, I agree.

Shall we switch to Joe Foley then to finish up?

SH: Yes. Let's do that.

DL: You also interacted with Joe Foley during your time in Cleveland until you left for Arkansas in 1994.

SH: And after [unclear].

DL: Yes, he told me. [laughter]

SH: Joe Foley was my Father confessor.

DL: Yes.

SH: That year—going back to Fred [Plum]—when Fred went on vacation, it was a fantastic year for us. He, I think, donated part of his salary or something. But, he arranged for important American and British neurologists—we got Henry Miller [1913-1976] from England and some people from Germany and some people from Sweden, so not only American, but mostly American and mostly New Yorkers, and from the Midwest and Northeast—to come to Cornell and give grand rounds. I think in that third year, Houston Merritt was the dean but anytime you consulted him to see a kid who was brain dead, he would leave his deanship. Everybody I knew that knew Houston Merritt... He must have hated the deanship.

DL: [chuckles]

SH: Every time somebody came into his office and said, “Dr. Merritt, I have a case we can’t figure...” he said, “Let’s go.” He’d put on his coat and go. Houston Merritt came two or three times. But, then, we had everybody come. The residents, we used to have dinner with them and lunches and speak with them one-on-one. It was very well done. I met Bob Fishman [1924-2012] and I met Bud Rowland. We met all kinds of good chairmen and Joe Foley was one of them.

Joe Foley had a very distinguished career. How long did Joe Foley stay as chief? He was chief of the Division of Neurology.<sup>26</sup>

DL: Yes. Quite a while. I have it written down in the other room. It was quite a while.

SH: He went there in the 1960s, late 1960s probably, to Case Western Reserve, and stayed only till the 1980s.

DL: Yes.

SH: So he didn’t stay too much. He had twelve years, maybe. He kind of lost interest at the end and the department disintegrated. When Bob went, I don’t know how many neurologists were there.

DL: Not many.

SH: Very, very few.

DL: Yes.

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<sup>26</sup> Foley was Professor of Neurology at (Case) Western Reserve University from 1961 to 1986, after which he became Professor Emeritus. He was Director of the Division of Neurology in the Department of Medicine from 1961 to 1980, after which he became a neurologist in the Geriatric Ambulatory Assessment Unit, which was renamed the Joseph M. Foley Elderhealth Center in 1987. In 1992 he was named to the Honorary Staff of University Hospitals of Cleveland.

SH: It was a great opportunity, because you can build a department in your own image basically. So that was good.

DL: Bob Daroff, by the way, told me that Foley also was horribly underpaid at the time that *he* came, that he was paid lower than most assistant professors, and he had never really contested that fact with the Chairman of Medicine.<sup>27</sup> So Bob took it upon himself to make sure he was paid appropriately at the time when Bob came as chair.

SH: Joe Foley is Joe Foley. Joe Foley is the son of very poor Irish immigrants.

DL: True.

SH: But he rubbed shoulders and he befriended people who were Brahmin Bostonians, like E.P. [Edward Peirson] Richardson [1918-1998], who was a pathologist, whose salary from Harvard Medical School with one dollar per year. I think if you are from the Richardson family or from the Lodges or from other prominent families from Boston, then you can afford to do that and maybe Joe liked that and he thought it was a feather in his cap.

But I'll tell you that's when I went to Peritz Scheinberg. That was a very rude interruption because I took it as a personal insult that he took me for a ride, because he took my naïveté and I said, "I'll take anything that you think is fair." And he didn't do that. So I told him to be fair and he wasn't fair.

DL: Yes.

SH: That was a blow below the belt. He said, "Money..." I said, "I don't have expensive habits. I go skiing once a year here and there. I don't gamble. I don't drink too much... I'm not a high roller." But that's my business, you know. All the others, he paid them more. That is...

DL: That's not fair.

SH: That takes you for granted. I had to catch Peritz red handed or with his pants down. Peritz is a very strong guy. You can't bully Peritz, but he knew that I was leaving. He knew I was mad. I said, "Don't ask me how much. Besides you're making four times as much as I do and I find *that* nauseating."

DL: [laughter]

SH: I said, "I want that remedied retroactively," to the beginning of that year. That was another mistake. I should have made it to the time I'd been in Miami, all this time. "I want something that is *truly* commensurate. Now look at the salary list here and see what where do I fit in that group here and that will be my contributions to the department."

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<sup>27</sup> Charles C.J. Carpenter, MD

Between you and me, at that time, I was like more than 70 or 80 percent paid by NIH [National Institutes of Health] funds. Nothing was coming out of his own pockets.

DL: Yes. He had a free ride with you.

SH: He was okay. I got a very fat check that November, you know.

DL: That's good.

SH: It was retroactive to July of that year and modified the contract and all of that.

Joe is Joe. Joe is a breath of fresh of air. He was my Father confessor. I could discuss [Fyodor] Dostoevsky with him.

DL: [laughter] Of course.

SH: We could discuss not throwing any bricks or stone at people, because he who is without sin... His Catholicism and my Eastern Orthodoxy got together and it was like the meeting of the Pope or the patriarch of Constantinople. [laughter] He knows a lot of Greek and I knew some Greek because of my going with my grandmother to church. It's still used. Arabic and Greek is the liturgy in the Greek Orthodox Church, in the Antiochian Orthodox. He is a man of great intellect. I was impressed with his knowledge of the classics. He went to Boston's Latin School for Boys, which has produced more Nobel Prize winners than Harvard University [sic].<sup>28</sup>

DL: Wow.

SH: He had full knowledge of the classics. He is a classicist, so we did talk about life and sipped some whiskey when we met at his home or at a bar. He was a free spirit. Joe was like a father to me. Every time I went to Cleveland, even at the end when he was demented, I would go visit him no matter what. The guy had a lot of issues. He didn't have an easy life.

DL: No, he did not.

SH: He did not have an easy life. It was difficult, but he bore it all with magnanimity and with a smile on his face and was very philosophical. He was a total mensch.

DL: [chuckles]

SH: He was a man. He had a quip for every occasion.

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<sup>28</sup> This statement is apparently not correct. The only Nobel laureate that I found as an alumnus of Boston Latin School was Merton H. Miller, which was a Nobel laureate for Economic Sciences (1990).

DL: Yes, he did.

SH: Whenever I had a problem with other people, I'd go discuss it with Foley and Foley would make me forget the issue very quickly and get me to see it in a different light. He was a peacemaker.

DL: Yes, he was and he always did it with a certain kind of gentle humor.

SH: Yes. Yes. I asked him about many of his people visiting him and not many people at the end—not that it really mattered to him, but I think it did.

DL: Well he remembered Joe Martin visiting him and he also remarked very positively about you visiting him. He said that to me. He was so thrilled that you came.

SH: Maybe it was a few days after I visited him, it was still fresh in his mind, or he had some things written to him.

DL: No, he was very clear about it. He spoke then in 2011 and before. I talked to him about you several times. He always just spoke so highly of you. He admired you. He respected you and he found you immensely entertaining. I think you found him the same way, actually. It was kind of a paired appreciation.

SH: Martin was his golden-haired boy. He was so proud of training him.

DL: Yes. Well, Joe Martin was proud to have been trained by him too, I can tell you that. I communicated with him recently by email. Yes, he was very proud to have been trained by him. In Joe Martin's autobiography, *Alfalfa to Ivy: [Memoir of a Harvard Medical School Dean]*, he recounts some of his fond memories of Joe Foley. Joe [Foley] actually gave me a copy of that book when I came to visit him. When I interviewed him, he said this: "I knew Sami very well. We used to have some great times together, Sami and I." I asked about what the great times were and he replied with a laugh, "Going out drinking." [laughter]

SH: I don't know where that came from. We didn't really go out drinking too much at all. In fact, I'd go to his house or he'd come to my house. I can't hold liquor. I'm not a great drinker, particularly Scotch.

DL: [chuckles] Yes, I don't know that he was either. He liked talking about it, though.

SH: Yes, he did...he did. He'd be nice and tipsy sometimes, but that's when he was at his best.

DL: I never saw him when he didn't have a quip, I'll tell you that. He had a clever little story or an icebreaker. You could come into a room full of people who were all upset with each other and he'd tell a story and, all of a sudden, they were all laughing and forgot the whole darn thing.

SH: Yes. He had jokes. The thing I missed about Joe is he was my source. When I went to Arkansas, he was my pipeline. He would tell me what Oscar [D.] Ratnoff [1916-2008] told him that morning or something like that.

DL: Yes, all the funny stuff.

SH: All the funny stuff, yes. Good. Joe Foley is a great guy. Let me tell you this: I do not know of anybody who speaks ill of Joe Foley.

DL: Nor do I.

SH: He did have some enemies at some times or so-called that.

DL: He had his little issues with people along the way that he thought were a little too pompous for their own good, especially earlier in his career. He mellowed, I think, some of that down over time. [chuckles]

SH: It should be emphasized that Joe Foley is the first person to hold the presidencies—now, it's a common thing because of the paucity of good candidates—of the AAN and the ANA.

DL: Actually, I think he was actually the fourth.<sup>29</sup>

SH: Really?

DL: Yes.

SH: Who held both before him?

DL: I've got a list. I went through that once before. But, yes, I think he was the fourth.

SH: It can't be.

DL: Yes. I wrote an obituary on him.<sup>30</sup> I think I put it in there. I'll send it to you.

SH: How many people were chairs before? Joe Foley was chair of the American Academy of Neurology before 1975.

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<sup>29</sup> Foley served as president of the American Academy of Neurology from 1963 to 1965 and as president of the American Neurological Association in 1974, becoming only the 4th of 9 individuals to hold both offices (after Adolph Sahs [1906-1986], Augustus [Buck] Rose [1907-1989], and Abe Baker [1908-1988]).

<sup>30</sup> Lanska DJ. In memoriam: Joseph Michael Foley, MD (1916-2012). *Arch Neurol.* 2012;69:1656-9.

DL: I'll send you the list. I promise.

SH: It's between 1948 and it was two years... Okay, do that. I don't think there was anybody before him. I may be wrong...I may be wrong.

DL: This one I've already researched.

SH: Okay. I stand corrected then.

DL: It's no big deal. He was certainly one of the few at that time and he earned it by his...

SH: I'd like to know who was it, you know. You see he was the bridge. He was with the ANA. He was with Denny-Brown.

People don't know this but when Fred heard that I was elected Secretary of the American Academy of Neurology, he wrote me a letter. We made up. After I came back and went to Peritz and he [Plum] gave me the bad recommendation, he told Peritz not to take me. Peritz said—he was on the Council—“He's very powerful on the Council. I've seen him root for his boys. I don't care what the Study Section [says], I want this guy elevated and given the money.” I submitted a grant at that time and didn't make it. Fred said, “I didn't lift a finger to help you.” [He said that] to me. That's what's partially broke the ice. I said, “Why?” He said, “You have no business working on the polyamines.” I said, “Dr. Plum, I'm now at this age and you're telling me what is my business? I can do research about anything I like if I have a good reason to do it, sir. You don't know anything about the polyamines.” So after that by about two or three years, somebody from his lab called me and he said, “Dr. Plum suggested [I call] because he found something about the polyamines and how they regulate the glutamate receptor in the brain. The guy who knows the polyamines—this is an archaic field—is one of my residents Sami Harik.” [chuckles] So then I went ahead. Peritz Scheinberg suggested—who didn't like Fred also—that I don't use Fred at all as any reference for as long as I lived, which I didn't. It would always be odd. One of them was when I was elected to the ANA. That was in 1980. So I came to Miami in 1976. I became a member in 1980 or 1981.<sup>31</sup> You can check it out in the ANA. I was associate professor at that time. Bob Daroff helped me. We decided to go without Fred. It was one of the few, because one of your mentors in residency should be [a reference for this]. It so happened that year that Fred was the president of the ANA. So he first heard about it in the Council meeting when I was being elected. He wrote me a letter, a nice letter, which I think I can dig out. It says, “I'm glad to hear that you have been elected to the ranks of the American Neurological Association. This is, indeed, a great honor and it augurs well for the—this is the kind of vocabulary he uses, *augurs*, you know—for the Association to have you.” So then I started talking to him after that. But I never sought his help at all. I think he had his chance with me. [chuckles]

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<sup>31</sup> Harik was elected to membership in the American Neurological Association in 1980.

DL: This, I understand.

SH: Yes. Then, I was elected Secretary of the American Academy of Neurology and he wrote me a letter unsolicited. He said, “I read in *Neurology* that you’ve been elected secretary of the Academy. What are you doing with that bunch of fools,” or something like that. [laughter]

DL: Wow.

SH: No, he didn’t think too much... The Academy was started by people who couldn’t make it into the American Neurological Association.

DL: That was the East Coast way of thinking, yes.

SH: Well, East and West Coast.

DL: Okay. Yes, Adams felt that way, too. He didn’t want to be part of it initially either.

SH: Augustus [S.] Rose [1907-1989] was head of<sup>32</sup>—he was pompous—Neurology at...

DL: [chuckles]

SH: Take that one out; strike it out. He was pompous. He was head of Neurology at UCLA [University of California-Los Angeles]. I met him once when I looked at a position there...that famous thing when I looked at Pertiz Scheinberg and Wash U [Washington University in St. Louis, MO] and other places. Anyway, I think Miami was a good place for me. I really was able to get back into science very fast, thanks to Peritz and his finding for me the funds that belonged somewhere in the department.

DL: Perhaps in your salary. [chuckles]

SH: Right, perhaps that, too. [laughter] I wish I knew. I would have, of course, used it as a tax deduction.

DL: Yes, exactly.

While we were chatting, I did look up my obituary of Joe Foley and it was Adolph [L.] Sahs [1906-1986], Augustus Rose whom you mentioned, [and] Abe Baker [1908-1988] who had been president of both organizations before Foley. So he was, indeed, the fourth.

SH: Abe Baker was?

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<sup>32</sup> Augustus S. Rose was the founding chairman of the Department of Neurology at UCLA Medical Center.

DL: Yes.

SH: Of the [American] Neurological Association?

DL: Yes, of both.

SH: Wow.

DL: I had looked at this...

SH: I believe Augustus, yes, and who else?

DL: Adolph Sahs.

SH: Yes, Adolph Sahs. Everybody loved Adolph Sahs, too. He built a very nice... I met him through Foley. We would get these people. Every time Adolph Sahs passed through Cleveland, he would look up Foley. Foley, I think, on a couple of occasions took me out to have dinner with Adolph Sahs. Yes, a good guy.

DL: I've probably pushed you much longer than you wanted. I do appreciate your time and your thoughts. It was truly a pleasure and an honor to talk with you. Thank you.

SH: Thank you. Bye.

DL: Bye, Sami.

SH: Have a good night.

DL: You, too. Bye.

[End of the Interview]

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