Interview with William M. Landau, MD
American Academy of Neurology
Oral History Project

August 10, 2012

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Interview with William M. Landau, MD
Professor of Neurology, Washington University
Neurologist, Barnes-Jewish and St. Louis Children's Hospitals
St. Louis, Missouri

O'Leary Conference Room, McMillan Building
Washington University, St. Louis, Missouri
August 10, 2012

Victor W. Henderson, MD, and
Barbara W. Sommer, Interviewers

William M. Landau: WML
Victor W. Henderson: VWH
Barbara W. Sommer: BWS

Track 1036

BWS: Today is August 10, 2012. We are interviewing Dr. William M. Landau for the American Academy of Neurology Oral History Project. We are in the O'Leary Conference Room in the McMillan Hospital on the Washington University campus in St. Louis, Missouri. The interviewers are Dr. Victor Henderson from Stanford University -

WML: We have changed the name of the institution.

BWS: OK. What is it?

WML: That is an interesting story all by itself. It is called Washington University in St. Louis.

BWS: Washington University in St. Louis.

WML: For quite a while, the Chancellor – I'll tell you the story. It is an amazing story anyway.

BWS: I'll finish with his name, then we'll do that. The interviewers are Dr. Victor Henderson from Stanford University, a former resident of Dr. Landau's and Barbara W. Sommer.

Now what is the story?

WML: Well, our great Chancellor for many years is Bill [William Henry] Danforth who is a physician. He is as old as I am. His own career is fascinating. He is a scion of a wealthy St. Louis family and the Danforth Foundation. He tells part of the story himself on one of the University tours, my wife and I when we went to Sweden, and we were chatting. There are a lot of Washington colleges in this country and he wanted to do something to make this one – what should we do? One of the things they discovered is that the brass knobs in the Administration Building had an interesting design on them. They used that as part of a necktie, but they needed something special to identify it in St. Louis. Everywhere in the country is a Washington. So he got himself an artist to design and put it on a necktie – he told me this story himself – and had the Arch, and then inside the Arch – the one on the riverfront – was the Administration Building which is a handsome building out on the hill. The hill campus is the main campus of the University. He went to some national meeting of University leaders and one of his friends came up and looked at the necktie and said, "Oh, you work for McDonald's." [laughs] That was the end of that artist. So they thought about what would they do. If you look at this building, it says Washington University School of Medicine. But the new building – the Education and Teaching Building – says Washington University in St. Louis. That is the official name. It has to be called Washington University in St. Louis so you can distinguish it from all the ones in the state of Washington and elsewhere. [BWS: That's a great story.] I am now at Washington University in St. Louis but it wasn't called that when I came here first in 1942 to apply for medical school. You will get to that. [The main campus is now designated as the Danforth Campus.]

BWS: That's actually where we want to start – your decision to go to medical school. Your training and how you ended up taking the path to neurology.

WML: I was born at 4732 Westminster which is a street about a half a mile over here [north from my office].[1] I am fond of saying that I was born at home because I wanted to be near my mommy. It was commonplace for healthy mothers to be born at home. The obstetrician was on the faculty so it was all right. I went to the public school which was two blocks away – the Eugene Field School. It was a comfortable neighborhood. On the way I passed an apartment building where Tennessee Williams lived for a while with his sister who had a [glass menagerie] – his most important play.[2] We didn't know anything about him at the time, but he lived a half a block away. It was an interesting neighborhood because that apartment building was on the east side of Walton and Westminster. On the west side was a comfortable house. The guy who lived there [Sam Breadon] was the owner of the St. Louis Cardinals, which didn't do very well in that period. But anyway, that is another story.

(5:10)

Anyway, I finished grammar school in 1937. About that time, it was discovered – this is as current as a book I'm reading now – but I didn't know at the time. Eugene Field was a guy who had grown up in St. Louis. He was a reporter and columnist for a Chicago newspaper and wrote children's – he wrote "Wynken, Blynken and Nod*"* – he wrote children's stuff.[3] He was born in St. Louis in a house on Third Street. His father was a lawyer who was not yet famous but his father was the lawyer for [brief discussion] Dred Scott. He was Dred Scott's lawyer. He should be more famous because Dred Scott's lawyer volunteered to do the job and lost. If he had won, we wouldn't have had a Civil War. But in any case, I'm just reading his biography now. My daughter bought it for me.

In any case, at that time, it was discovered that the house had been left to the School Board and the School Board didn't know what to do with it. They decided they had to do something. So they had a formal opening of this little old row house on Third Street just across the street from the present baseball field. And the eighth grade students went down to open it up. When my daughter was visiting last year, we went down and inspected it – I had been there before – and

[1] Dr. Landau's parents were Milton Landau and Amelia (Rich) Landau.
[2] "The Glass Menagerie," published 1945.
[3] The children's poem, "Wynken, Blynken, and Nod," also is known as "The Dutch Lullaby," was published in 1889. Eugene Field was the son of Roswell M. Field, a defender of Dredd Scott.

there was a photograph from the newspaper in 1937 with me at the front door. A little [12-year-old] boy with big glasses. So I can document that I was there at the opening of the Eugene Field House. Nobody's interested in damned baby poems any more. "Wynken, Blynken, Nod." But the father volunteered to take on Dred Scott and worked very hard at the job.

Anyway, so then I went to the high school here – public high school. I rode my bicycle there. That was several more blocks away. Called Soldan High School. It was a very competitive high school at that time. It had a large population of European Jewish immigrants. It was the north St. Louis ghetto which ultimately became a black ghetto. Where I lived [middle class], which is on the south side of Delmar – Delmar is the street that was sort of a division between the intrinsic – blacks lived north of there and south of there they – Did you know the Loop, Vic, when you were here? Anyway, the Delmar Loop where the streetcar used to circle to go back downtown is a very popular shopping area and a wonderful neighborhood because it evolved commercially as a desegregated place where black young people and older people and white older people lived and congregated. Went to movies and so forth. It has very much developed.

BWS: Were you thinking of medical school at that time?

(9:58)

WML: No I didn't think of anything. Well, I was thinking of a scientific career. [BWS: Of some sort.] In any case, it was a very competitive school. The point I am getting to – I applied for and got a scholarship to the University of Chicago, which is interesting because my brother[4] – who is eight years older than I – had grown up in St. Louis, gone to the same schools, finished medical school here [1941, in St. Louis], and had not yet gone [to Chicago]– I've got to get the years right. He ultimately was an intern at the University of Chicago hospitals. That is going to be an important part of my story. Anyway, I got a full tuition scholarship to the University of Chicago which amounted to one hundred dollars a quarter. [laughs] I subsequently was admitted to Harvard but I didn't do anything with that because the University of Chicago was more than enough. So I went – graduated from high school in June of 1941 – a year that may be of some importance to the story here. I was in the dormitory on the south side of the Midway in Chicago

[4] Dr. Landau's brother is Richard L. Landau, MD.

– a nice dormitory – with kids from all over the country. An interesting event happened on December 7 [1941] that you may recall. We undergraduate freshmen got dumped into another dormitory and a bunch of persons in the Navy program moved into our dormitory. There were a lot of young graduate students that were in a program that was called – it was a word that implied that they were doing something about fixing the heads of bullets. There was a word that implied making metal tougher [metallurgy]. It turned out that these were all the graduate students that were interested in physics and commuted to New Mexico [Los Alamos] then, but we didn't know that. We thought that they were involved with some sort of Federal program. Our naïve idea was that they were developing better bullets or something like that. [No mention of radioactivity.]

At that time the University of Chicago had gotten out of the Big Ten a couple of years before. They did not have a University of Pennsylvania scandal.[5] The football field, at the [east] end, had some stands in the end zone. In front of the stands about where the safety zone of the football field was, across the field they had a place to practice your chipping [golf] game and a green. Along with others I sometimes used the chipping green; you chipped onto the green about a hundred yards. What I didn't know at the time was that inside these stands was a laboratory that was being run by an Italian fellow named [Enrico] Fermi. [VWH: That's right.] The first year the work was done there he didn't know if the whole city would blow up or not. [VWH: In the bleachers.] It was the end zone, not the ordinary bleachers. They made a laboratory for him inside the bleachers at the end zone.

BWS: You didn't know this at the time?

(15:35)
WML: I didn't know is at the time. There is an anecdote that goes with that too. It is from my brother. The University of Chicago Medical School had moved from downtown and was purposely integrated in the University. So my brother, who was in the medical campus, was very well acquainted with other people in the faculty. The University had obtained a fancy modern statue which they were going to put where I had practiced my golf. The President of the

[5] WML is referring to the Pennsylvania State University child abuse scandal, 2012.

University appointed a friend of my brother's who was a professor of English to a special committee. Their responsibility was to find a name for the statue. 5700 Ellis Avenue is where the statue was already scheduled to be put. The committee met several times over the years – over a couple of years – because there was no big hurry about it. We were fighting a war. Eventually they came up with a name. If you walk down past the bookstore now on Ellis Avenue in front of this statue, there is a metal sign that says "Atomic Energy." [laughs] That's what it says. That is where Atomic Energy stood.[6]

BWS: You were there during that time.

WML: I was in the University of Chicago program, which was a very important program in American college. The Rockefeller Foundation has over the years, supported three medical schools and one university very well. The University of Chicago was really established by the Rockefeller Foundation. There is a Rockefeller Chapel. The graduate program there did very well from the last part of the last century. We're going back another century. But the undergraduate program wasn't doing very well as far as colleges. In Chicago, Northwestern was the college. So the Board decided they wanted to do something to build up the undergraduate program. Anyway the guy they hired to run the college [Robert Maynard Hutchins] was the Dean of the Yale Law School. He was brought there to develop an undergraduate program. [The chosen medical schools were the only ones with full-time salaried faculty: Johns Hopkins, University of Chicago, Washington University at St. Louis.]

BWS: While you were there?

WML: Yes, it had just gotten started. This was the undergraduate program at the University of Chicago.

BWS: This was going on while you were –

WML: It started in the late [19]'30s.

[6] "Nuclear Energy" is a bronze statue by Henry Moore; it stands on Ellis Avenue in Chicago.

BWS: You were kind of in the middle of that.

WML: It had just gotten started. [Robert Maynard] Hutchins is the guy's name. He and a guy named [Mortimer] Adler, who was a philosopher, developed "A Hundred Books" which every civilized person should do.[7] The undergraduate program included four major areas that every undergraduate had to get in to – social science, biological science, physical science, and the humanities. Then you had a few elective courses. The first two years were pretty constrained to these general civilization culture courses. It got a lot of publicity. He was described in a big article in the *New Yorker* at the time. He was not even a lawyer but he had been Dean of the Law School at Yale. He became President of the University of Chicago at age thirty-four. He was famous for his motto – at least, his students knew about it. He said, "Every time I am tempted to exercise, I lie down till it passes." He only lived into his 70s. Anyway, it was, and still is, a thinking college and university. There was I in this - the physical sciences were stronger than I even knew at the time. But there was a great deal of good biological science which I didn't appreciate at the time. At that time, my brother – who is eight years older than I – he just had his ninety-sixth birthday two days ago – was an intern. In those days interns didn't get paid anything but they got four meals a month for their relatives. He didn't have a spouse yet. So I got to have lunch with him and the other interns on Sundays. By this time, in my sophomore year, I knew I was fascinated by physics and biology. I knew I wanted to do something in science. But sitting in the lunch room – you've been in – the interns would talk about their cases. Having observed that for several months, I knew I wanted to be – I wanted to get into that game because it was science and argumentative – which is part of my style – and fun. And it had the gratification of being what medicine is. So I was seeing physicianship at the basic level as a sophomore in college. So I decided to go to medical school. My brother went to college and medical school here [Washington University].

(20:57)

It would be 1942 now. Everything [elective] was aborted and put together as far as undergraduate work then. Instead of the usual four years of pre-med, the schools – there were no

[7] Great Books of the Western World.

electives. Everything was pushed together. So I applied – at this time, it is hard to believe – but neither this building nor that building [pointing] – none of these buildings existed. Oh, this building [the McMillan Building] did exist – I'm sorry. It was a hospital building. The library building didn't exist. That was an empty lot between the two buildings here.

In those days there was no national exam for medical school. You applied and I had to write an essay, I know, because [I retrieved it from] the Registrar subsequently – I had written a one-page legible paper saying why I wanted to be a doctor. The admitting officer [assistant dean] was a guy named [Carlyle] Jacobsen who was a neuropsychologist. I subsequently learned that he was a very important neuropsychologist. He had been brought along here by a guy who had been appointed to be head of Neurology and Psychiatry. The guy who hired him went back to Baltimore to become a psychoanalyst, but he was left behind. He did monkey psychology and had monkey facilities on the thirteenth floor of this building. I didn't know any of this then. He was a guy who had a Ph.D. in psychology so, willy-nilly, he was the Dean for Admissions. Now coming back to family –

VWH: Let me back up for a second. The CV which your office sent out said you were at Chicago for two years. [WML: Yeah] This is an accelerated program because of the war?

WML: Yeah, right.

VWH: Did you graduate to enter medical school?

WML: No, I didn't even graduate. That's an easy story. At the end of – early 1943 – by this time, I received a letter saying I would get a Ph.B. degree for a twenty-dollar diploma fee. The Ph.B. was one of the things that this new President of the University had pushed. His idea was that in two years, given two years of this kind of training and education after high school, you were then ready for a University. This was the concept of the University of Chicago's undergraduate program. It was patterned after the European program and to document it, he proposed that the graduate would get a Ph.B. – Bachelor of Philosophy. I told you the tuition was

a hundred dollars. I figured that for twenty dollars I could get the diploma, but I figured unless I talked fast, what good was a Ph.B.? [VWH: Yes] So I declined. So I never graduated.

(25:33)
Subsequently when I gave some money to the University of Chicago, in more recent years, I conditioned my gift on their discovering and sending me a degree. My Ph.B. But the Registrar insisted I was only eligible for an Associate, which I knew they would have never offered. It was an interesting bait that he [Hutchins] had developed – come to Chicago and you will get a Ph.B. for just going to college for two years.

VWH: And twenty dollars.

WML: And twenty dollars. Anyway, so I never got a diploma. But the war was going on and I was past eighteen. You see I graduated from high school when I was sixteen. But you were draftable at eighteen. There were a lot of people going to war in both directions. Now what was going on in St. Louis? Carlyle Jacobsen, this Ph.D. guy – this says something about the culture of St. Louis. My mother – her mother died essentially in childbirth when she was only a few weeks old, so she was the only child of a businessman and contractor who did a good deal of building of the World's Fair in 1904. That's another story. She got to ride in the steam shovel that dug the river that – there is a big river underground in the park. My grandfather had six weeks to put it under ground and he did it in 1901.

BWS: Did she point you to medical school, you and your brother?

WML: No, I decided because of this experience watching them [in Chicago].

BWS: She didn't - ?

WML: My mother would not have told my brother to decide – my father was in business in St. Louis. He grew up in St. Louis. He was first in a business with his siblings and father in wholesale groceries. Then he established his own business, which was linen rentals. That is

rentals to hospitals and restaurants. [BWS: You didn't have any –] He wanted his boys to go into his business. Both of them went into medicine and he tolerated it. He was hoping we would follow him into his business.

BWS: According to your CV, you went into the Army.

WML: I haven't gotten that far yet. [BWS: Oh, okay.] In order to escape the draft, one had to be admitted to medical school. [BWS: That was after your second year at –] Right. Now you have to understand the episode of admission is amusing because, as I told you, there was no national organizational structure. My mother was a liberal of the period who marched and got the vote for the Nineteenth Amendment in 1919. She was very active and was the chairman of the local American Civil Liberties Union [ACLU], which started in St. Louis [in 1920]. When I was growing up, the committee would meet in our living room – they didn't have any permanent staff. A member of Civil Liberties Committee in St. Louis was this Carlyle Jacobsen, who knew me as a child, you see.[8] So I walked into his office across the street here, which is still near where the Dean's office is, and we talked about my interest in medicine. The interview ended and he said, "I think you'll get admitted, Billy." [laughs] That was it. He had known me when I was ten or eleven. [to VWH] It was much more organized when you came along, as you know.

(30:40)

I was interested in science at the University of Chicago. I didn't know about atomic science but I was impressed enough with medicine that that is what I wanted. I never – my wife [Rebecca Hornbein] – that's still another story. She passed away three years ago. She went to the University of Chicago a year after me. She went to a St. Louis High School [University City High School]. The fact that she was going to the University of Chicago and got a degree in astrophysics – it put us together. She was very bright and interested in physics and ended up teaching physics. That's a separate story which is an important part of my life. But in any case, I was set for medicine before I was eighteen.

[8] Dr. Landau also worked with Leon Jacobson, endocrinologist at the University of Chicago. But references throughout this section of the transcript are to Carlyle Jacobsen.

BWS: How old were you when all this happened?

WML: I had to apply – I was born –

BWS: You would have been about eighteen years old when all this was going on.

WML: Right.

BWS: Just reaching draftable age. A lot was going on around you.

WML: My birthday was in October and I started college [medical school] just before my eighteenth birthday. [BWS: Sixteenth?] Wait a minute. I said, ’41. I was born in ’24.

VWH: Just before your seventeenth birthday.

BWS: Yes, just before your seventeenth birthday. You were a young one.

WML: Right, but everybody was young because the pattern was set. The war ended during medical school. So we got a little elective time, which is important in my scientific evolution, which we’ll come to next. But in any case, there I was.

I almost got drafted out of medical school. Anyway, there I was. I was admitted. We started – most of the students were in either the Navy program or the Army program. The Army program was called the Army Specialized Training Program [ASTP]. They had regular Army uniforms. A poor regular Army guy had us marching around out here every Saturday much to his distress and everybody else’s.

BWS: What was that program? Something that supported medical students?

WML Yes, there were two programs that were supported by, in effect, a Federal scholarship. The Navy program and they had Navy uniforms –and the Army program. Nobody wore them except when they had to march once a week.

BWS: Were you asked to go into that?

WML: No, one applied to it. This is part of a complicated story. We're going to get to Harry Truman here. [BWS: You were - ?] After being admitted officially to the medical school, I received an appointment as a second lieutenant in the Army, which was designed just to protect me from the draft. I didn't have any duty or salary. This [policy] protected me [and others in the same situation, I was told] from December of – when I started medical school in '43 – for about a year. This was why I couldn't be drafted.

BWS: The war was –

WML: The war is on. This was 1942.

BWS: 1943? You are in the middle of all of that.

WML: This was in 1942, so this protected me. But I wanted to get into the regular program where you would be a private and have your tuition paid like the two major groups here. Among our freshmen group there were three classes of students – those who were enlisted in the Army group, those who were enlisted in the Navy group – we had two students who were alumni of the eviction of the Japanese from California, one of whom is still in St. Louis, a pediatrician – and several who didn't get into either, who were just civilians. I was one of those. Two or three who could afford it just paid their tuition on purpose and were civilians, deferred because they were in medical school. I wanted to get into the paid program. At that time, in order to get in, you had to have good vision without glasses – and I was as nearsighted then as I am now. Anyway, I had to have a physical examination to do this. I was still living in Chicago – now this was just before I came down. At that time, contact lenses were very new; they were heavy pieces of glass that covered the whole eye. So I got myself fitted with those because I

wanted to have apparently normal vision. That worked fine but the anxiety was such that I had a rapid heartbeat and the examining doctor said I had a murmur. [laughs] So I couldn't get in the Army.

(36:02)
So I came to medical school and by December we were into gross anatomy and I was doing all right. But I was a civilian and I suddenly got a notice about the first of December that I was going to be drafted out of medical school. I was a bona fide student in medical school and I was a civilian. I no longer had this funny lieutenant's appointment. I went down to Jefferson Barracks [a military post in Lemay, Missouri] because that was where draftees went. My father, as I told you, was a businessman. He decided that [my situation] wasn't a good idea. So he got in his automobile and drove to Washington. The problem was to cut through the bureaucracy and get me into – at least to be deferred as a civilian. It was government policy – we needed more doctors. There was no problem with the policy. So he first went to our Senator [Harry Truman], who was a little retired haberdasher from Kansas City whom everybody knew was crooked because he was part of the Pendergast operation.[9] There was no question he was a half-assed, somewhat crooked politician before he became President. He rose to greatness. But my father was not naïve. He got the idea [very directly] that Harry [Truman] was not going to do anything for me without a [direct] contribution of cash. So he [my father] went across the street to the Representative from our district – a fellow named [John] Cochran who now is the name of the VA Hospital on Grand Avenue.[10] [VWH: Yes.] Mr. Cochran took care of me. I still have the telegram he sent my father saying your son is okay. He fixed it. So by January, I was okay. I got into the ASTP. By this time the restriction on vision for potential doctors had been resolved.

VWH: You didn't have to resort to the fake contact lenses?

WML: I'd failed at that anyway, you see. And the murmur – in any case, I had a functional murmur. I went to the cardiologist here and he couldn't hear the damn murmur. But I wasn't as

[9] Corrupt Kansas City political boss Thomas Pendergast.
[10] John Cochran Division, Veterans Administration, St. Louis Health Care System.

anxious as I had been naked – you see I was naked except for the contact lenses during this episode. So my dishonesty led to all of this trouble.

Anyway, so medical school went on. In 1944, I was in the sophomore year. That was the year we read in the paper that our Chief of Physiology, Dr. [Joseph] Erlanger, was going to get the Nobel Prize.[11] The [nominating] letter about it had been written to the Nobel Committee by our Head of Surgery who was really the most important figure in this medical school – Evarts [A.] Graham. By the way, Evarts Graham lived across the street from where I was born and his two sons were friends of my brother and me. The elder son [David] became a physician too. The younger son [Evarts], whom I became acquainted with, became editor of the [St. Louis] *Post-Dispatch*. The Head of Surgery lived across the street. His wife [Helen Tredway Graham] was assistant professor of Pharmacology. She was a nice lady who, I remember, took good care of her lawn, but I didn't know any of these were important people. I knew the guy who lived two doors from them who owned the Cardinals – I can't think of his name now, but everybody knew that he was there – but the Head of Surgery, who was a father figure of the development of this medical school between the two great wars, was Evarts Graham. He was a real hero figure. Anyway, getting back to where I was.

BWS: You were going to medical school.

(40:54)

WML I was in medical school and the point is that we were in the second year. Erlanger was the Head of the [Physiology?] Department – a very dull, pompous fellow. But I was doing very well in physiology and they were short-handed because of the war, so I became a teaching assistant for the succeeding class of students. When I was a junior I became a teaching assistant in physiology because I had become turned on by the nervous system. That was just when the giant axon and Hodgkin-Huxley – when Hodgkin-Huxley came along. That was when I was already interested in neurophysiology.

[11] Joseph Erlanger was awarded the Nobel Prize in Physiology or Medicine in 1944 for improving the understanding of nerve impulse transmission. He shared the prize with Herbert Gasser.

BWS: You were thinking in that direction or starting to move in that direction?

VWH: This was the squid giant axon.[12]

BWS: This was what?

VWH: The squid giant axon. It was a basic model for looking at the [nerve fiber function].

(40:54)
WML: Not that I had them. The point is that the science of neurophysiology made a definitive step which impressed everybody. We didn't have any squids here but we knew about the mathematics. Now meanwhile, in biochemistry, my partner and I – we worked in partners – he subsequently became a pathologist – we had a wonderful instructor named [Ethel] Ronzoni [Bishop] – a female. In those days there was great prejudice about females, on the faculty even. I mentioned that the assistant professor of Pharmacology [Helen Tredway Graham] was the Chief of Surgery's wife. Everybody who was a female was put down. There were only three females on the faculty in that period. The teacher of gross anatomy was the other [Mildred Trotter].

Anyway, Ethel Ronzoni was a biochemist and we had an exercise in biochemistry where we had a rat and we put a tube down the rat's stomach and put some sugar water in there and then we chopped off bits of its tail thereafter and measured the blood sugar as it rose because it had sugar in its stomach. Our task was to develop a glucose tolerance test on the rat, chopping little bits off the tail to get our specimens.

Dr. Ronzoni came around and looked at our graph, which is something like this [/\/\]. Fred and I – this was our honest measurement. She said this wouldn't do. I said, "We'll make a better one." She said, "That's not what I meant." [laughs] In any case, she was a wonderful teacher and I never knew about the second glucose tolerance curve. But I know we honestly tried to do it

[12] Alan Hodgkin and Andrew Huxley studied the squid giant axon, developing a mathematical model to describe how action potentials are initiated and propagated. In 1963, they were awarded the Nobel Prize in Physiology or Medicine for this work.

again. But anyway, I let her know that I was very much interested in neurophysiology and in the nervous system. She said, "Go see George." And that's where I got into physiology.

VWH: George is George [H.] Bishop.

(45:45)

WML: George Bishop. Now George Bishop is quite a separate story.[13] I just got a new book about the history of neuroscience which spends a couple of pages about his trouble with Erlanger. What happened was that he had a graduate student who discovered the C–wave, the electric potential of unmyelinated fibers. Erlanger thought that was too important for a graduate student to discover. The result is that Bishop got moved over to this building, which was still another story. By the end of the [19]'20s, this had been built. A sociopath head of ophthalmology – which is still another story – the scandal of the medical school – had stolen money. In any case, it was overbuilt. There was a lot of space here. Bishop became interested in the visual system. They had all this laboratory space so he was made Professor of Biophysics – a new word. And was [nominated for] promotion by the same professor [Graham]. He [Bishop] and Ronzoni were living together, unmarried. This was very unstylish in the '20s. They had come here in 1921.

BWS: You were beginning to think about the nervous system?

WML: By this time, I was sold.

BWS: You were sold.

WML: She said, "Go see George," who was here in this building on the eighth floor.

BWS: You really thought this is where you wanted to be?

[13] George H. Bishop held several appointments in the medical school including Professor of Neurophysiology in the Department of Neuropsychiatry. He was known for work in the development of electroencephalography as a diagnostic tool in the understanding of epilepsy. For more information on his career and his work with Drs. Joseph Erlanger and Herbert Gasser, see the George H. Bishop oral history interview, Washington University School of Medicine Oral History Project, OH004, http://beckerexhibits.wustl.edu/oral/interviews/bishop.html, accessed July 25, 2013.

WML: I knew I was interested in the nervous system.

BWS: Where were you – in your third year of medical school?

WML: Yes. Now, along this time, I mentioned – what's-his-name – who had been the admissions guy. [VWH: Jacobsen.] [Carlyle] Jacobsen. I had talked to him about my career a little bit. In my sophomore year, a very important instructor was George Saslow, who was a psychiatrist who had been a hematologist imported from Boston, whose interviewing technique was just wonderful. His way of dealing with patients is something that made my career of interest in clinical medicine. He could sit down with a patient he had never seen – I saw this happen more than once with the whole class sitting around – and develop historical data important in the patient's story that everybody else had missed. He just knew how to take a history. Several people got interested and became our leaders in psychiatry from the same influence. But I knew I loved the idea of talking to patients and being interested in patients. So I went to Jacobsen and said, "Here I'm interested in the action potential and how nerve axons work. And I'm interested in patient behavior as it relates." I did an elective with Saslow. That's how I got to graduate with cum laude. The elective was – I went through a whole bunch of clinic charts looking for the word "functional."[14] I found that the word functional correlated with the thickness of the chart. [laughs] These were patients that nobody was interested in and they kept dumping ["functional"] – there are still some. [VWH: Yes, yes.] So I said to Dr. Jacobsen, "Is it reasonable" – somehow I got this across – "to be interested in people and illness and also be interested in the action potential and neurophysiology?"" He said something like, "I think so." Now what I didn't know was his career – he [Jacobsen] was a guy that first worked in [John Farquhar] Fulton's lab at Yale before he came here. He had amputated a chimpanzee's frontal lobes and discovered what behavioral deficits resulted [brain and behavior].[15] So here was a guy – I was just talking about – I didn't know that. Subsequently I got to see his CV and learned that his career –he was one of the first psychologists interested in experimenting with primates and

[14] "Functional" refers to a symptom without an identifiable organic cause.

[15] Todman, D. (2009) History of Neuroscience: *John Farquhar Fulton, 1899-1960.* IRBO History of Neuroscience. http://ibro.info/wp-content/uploads/2012/12/Fulton-John-Faquhar.pdf, accessed August 9, 2012.

brain injury. Anyway, he encouraged me to go ahead. I didn't do anything more with him. He subsequently left here and became a medical school Dean in the Northeast somewhere. I forget [which]. Subsequently I talked to his [Jacobsen's] wife about his career [and that conversation].

(50:25)
Anyway, so there I was. An elective period because the war ended then. I had time to work in Bishop's lab on the eighth floor. In Bishop's lab, O'Leary – O'Leary was my other major mentor. O'Leary had been – Bishop's background was that he had been [an undergrad in electrical engineering, but his Ph.D. was in the physiology and chemistry of honey bee biology!]

[Break in interview]

Track 1037
[general comments]

BWS: Talk to us about O'Leary a little bit.

WML: O'Leary had come from the University of Chicago, [perhaps] our first [faculty double degree] MD/PhD.

VWH: This is James O'Leary, Jim O'Leary.[16]

WML: It is important that he didn't have an internship. That is part of my career. But he was brought here because they had a hole and he – this was during the war period – [Professor Ranson] who had taught neuroanatomy had gone back to Northwestern. He [O'Leary] was hired – he had finished his PhD training. He had his MD. [His clinical experience was limited.] He'd never had an internship. So he was brought here to [fill the hole], teach neuroanatomy, which he did very well. Then came the war and he had become associated with Bishop and electrophysiology [off to war and Army EEG]. He was also interested – down the street here is

[16] James L. O'Leary was the first head of the Department of Neurology at Washington University School of Medicine. The James L. O'Leary Prize was established and named in his honor.

the Central Institute for the Deaf. At the Central Institute for the Deaf was a fellow named [Rafael] Lorente de Nó, who was Ramón [y] Cajal's last student.[17]

VWH: I didn't know he was here.

WML: He was here - not in your career, but just before mine. Lorente was very jealous because he was delighted to have O'Leary working with him to learn neuro-staining techniques of Cajal but at the same time he [Jim] was working with Bishop on the eighth floor here, back and forth. So O'Leary became an expert and a national figure in neuroscience. This is important in the [American] Academy [of Neurology] history. I think he was the only PhD professor of neurology anywhere. So he was the head of every committee that had science. But he was a great teacher and investigator and his work with cerebral cortex and Bishop's work – we were all working together on the eighth floor. But he learned – he taught himself – he was one of the fathers of electro-encephalography but he was never comfortable with sick patients because he had never had an internship. The equivalent of Morning Report was reading the EEGs and talking about what had happened with those patients. He saw out-patients but he was never comfortable with in-patient neurology. This relates to my career, as you will see. In any case, during my medical student period then, Bishop started me on a project working with acute cat experiments. I was supposed to put strychnine in the brainstem and see what the electrical response was. This was just when [H.W.] Magoun and [G.] Moruzzi were talking about that system [the reticular activating system]. My experiments were a complete failure but I was turned on.

So then I graduated [and married]. I got an internship at the University of Chicago in general medicine. That is where I went in 1947. Then I came back for residency which was just being organized. I became a resident at the old [St. Louis] City Hospital.

BWS: In what?

[17] Santiago Ramón y Cajal shared the 1906 Nobel Prize in Physiology or Medicine for work on the structure of the nervous system.

WML: In neurology.

BWS: You were in neurology.

WML: Sam [Samuel A.] Trufant became the first resident in neurology based here [at Barnes Hospital].[18] I had the first one downtown.

BWS: It was a new – [WML: A new program.] A new program?

WML: I was the second resident there. The one before me was a nice fellow [Warren Mills] who turned out to have Huntington's chorea [VWH: Oh, no.] and did himself in at a later stage. But at the time I talked to him about finding an apartment here because I had gotten married just before I graduated in '47. He said, "An apartment is about as common as Wilson's disease." He'll [VWH] will tell you that is a very rare disease. But we did find an apartment on the South Side and the only patient I ever saw first with Wilson's disease came into City Hospital the next month. In any case, both things happened and I had a wonderful experience as the Chief Resident there. The interns rotated with me. I developed my own parallel bars to help my stroke patients learn to walk or to get by. We had twenty-bed wards with two separate beds for the sicker patients. This was where I worked as the Chief Resident in Neurology for two years. [My service was popular because interns vied for most autopsy permits.] Then I worked back here too. Then I came back here and they got me a fellowship, interestingly through the National –

(5:19)
BWS: National Institutes of Health?

WML: No, there wasn't any – they weren't going yet.

VWH: Paraplegia Foundation.

[18] Samuel A. Trufant was president of the American Neurological Association and chair of the department of neurology at the University of Cincinnati College of Medicine.

WML: Paraplegia - my interest in the pyramidal tract goes back to the very beginning. [BWS: Okay.] So I worked with the acute cat experiments mostly. Cats were supplied in a gunny sack to the department secretary for twenty-five cents apiece. You would see them in the elevator – the wiggling – wiggly – gunny sack. It was something of a sport to catch them on the thirteenth floor with a net and then anesthetize them. I did an experiment and sacrificed the cat that day. One day a cat jumped off the roof. That is still another story I won't bother you with. We chased down the steps after it.

In any case, I had a fellowship then for two years. Then came another war over in Korea. I had to meet that commitment. Meanwhile, with Bishop's help, I had a fellowship in Cambridge [United Kingdom] with Adrian – Edgar [Douglas] Adrian.[19] But then I was going to be drafted, so instead Bishop wrote to a friend of his who was at NIH [National Institutes of Health]. NIH was just getting going then. His friend was Wade [H.] Marshall. Wade Marshall was one of the earliest electro-physiologists of the forebrain. He was a colleague of – [to VWH] you know Tom Woolsey here?[20] [VWH: Yes] Tom Woolsey's father, Clinton Woolsey, and Marshall did the first evoked mapping of the cortex in a monkey. He also was schizophrenic and had been ill. But in any case, he had recovered pretty well from the schizophrenia with the help of a wonderful wife and had a laboratory at NIH across the street in Bethesda from the Navy Hospital, which is still there. What is now NIH in Bethesda was then a golf course. The laboratory that he had was in a temporary building left over from World War I.

In any case, the letter from Bishop – which unfortunately I don't have any more – asserted that I had been reasonably well trained in neurophysiology but I was not yet housebroken. This was the letter from Bishop to Marshall. So he hired me. I spent two years with him. We mostly worked with [cortical] spreading depression [of Leão], which was then a crazy field – it became, as you know, very important. Spreading depression was an epiphenomenon. We now know it is important in stroke, in migraine, and in a lot of other things. But it was a basic science phenomenon. It was a very dull kind of experiment. We took the skull off the cat. Then we scraped the surface of the cortex with a probe and watched a slow wave – an electrical wave –

[19] Edgar Adrian shared the 1932 Nobel Prize for Physiology or Medicine with Charles Sherrington.
[20] Thomas A. Woolsey is Professor of Anatomy and Neurobiology, Washington University.

move over the cortex. Anyway, we sat and just watched the electrical waves. It was a very dull kind of experiment. But it turned out very interesting and we established some important principles about what happened as the cortex actually swelled physically with this phenomenon. [BWS: Why were you doing that?] Because that was what he [Wade Marshall] was interested in. [BWS: And why was he interested in it?]

(10:06)
WML: Because he was crazy. [laughs] At the time I didn't know why. It was a phenomenon of the cortex. It had no practical value at all. There was a guy in California that was also working in the field. In any case, this was basic science at its worst. We had this strange phenomenon called spreading depression which had been discovered by a graduate student at Harvard named [Aristides-] Leão.[21] He had been – this brings us back to St. Louis. Leão was a graduate student working for Hallowell Davis. Hallowell Davis had come to the Central Institute for the Deaf to succeed – [VWH: Lorente de Nó] Lorente de Nó. He and O'Leary were national colleagues in the evolution of clinical EEG. So he was right down here.

VWH: And later evoked potentials as well.

WML: Yes. He is the one that developed the [VWH: Brainstem evoked response.] brainstem-evoked response. Where did I leave you here?

BWS: Was this - ?

WML: I'm still in Bethesda, Maryland.

VWH: You worked on the spreading depression of [Leão]?

WML: That is what we were working at as our primary entity. And a colleague [Walter Freygang – a great innovative neurophysiologist who died 3 or 4 years later] who is now deceased and whose name is less important to my story here. Leonard Berg joined me there as

[21] Leão AAP. Spreading depression of activity in the cerebral cortex. *J Neurophysiol* 1944;7:359-390.

did [Lewis P.] Bud Rowland. These became dear friends of mine. Leonard Berg [later] established the program here [at Washington University] in Alzheimer's disease, which is a memorial to him. We are going to have the Leonard Berg Annual Symposium two weeks from now. Leonard Berg had also been – had followed me in the same high school and was two years behind me here in medical school. He was brilliant. He was trained by [H. Houston] Merritt.[22] Merritt got him a job at NIH at the same time in the brand new clinical center which was just built.

VWH: I didn't know this part of Leonard's history.

WML: That is where I got acquainted with him. I mean I knew him from high school. And that is where I became very closely associated with [Lewis P.] Bud Rowland who got fired from NIH during the McCarthy period because he was a liberal from Yale.[23] That's still another story about Rowland's getting fired. It was quite a catastrophe for him at the time. Anyway, our wives and our children are very close because we were all at NIH at the same time.

So – here comes a guy named – the Institutes were just developing and a fellow named Seymour [S.] Kety from the University of Pennsylvania had been hired to be the head of both institutes – the Institute of Psychiatry and the burgeoning Institute of Neurology at the same time.[24] He had no laboratory and he had no help. So he was interested in the circulation of the brain. He had a colleague named Lou [Louis] Sokoloff whom he subsequently brought along. Lou Sokoloff still lives in Bethesda and did some very important work with [cerebral] glucose metabolism subsequently. But in any case, we were studying – I got involved with a program where we were interested in the local circulation of the cat brain. This involved radioactive tracers and then the animal had to be sacrificed by a French technique [guillotine] where we amputated the head. We finally discovered the best technique for this was an instrument that I discovered and we purchased from Sears Roebuck which is called a cow de-horner. It was a device with big clippers

[22] H. Houston Merritt was Chair of the Neurological Institute of New York and Dean of the Columbia University College of Physicians and Surgeons.
[23] Reference to Senator Joseph McCarthy and his anti-Communist campaign which became known as McCarthyism. Senator McCarthy eventually was censured by the United States Senate for his activities.
[24] Seymour Kety was a neuroscientist who applied basic science to the study of human behavior. He was the first scientific director of the National Institute of Mental Health (NIMH).

that cut off a cow's horn. I was the only member of the team that didn't like cats so I had the – the cat was somewhat anesthetized and then we chopped the head off and then froze the brain and then sliced it with a band saw and then put it on a piece of film. These were the first local circulation measurements of an animal brain.

(15:19)
What we saw was doing two things. I was working in Seymour's lab. Seymour [Kety] was a wonder. He was a role model for administration for me. While we were frustratingly working with the radioactivity in the cat, I remember he was trying to develop neurochemistry in regard to psychiatry. Subsequently, he hired two fellows who subsequently won the Nobel Prize, so he had some judgment. But he confessed to us that he had a couple of people he wanted to bring to NIH who were great chemists but they had never studied anything except the liver. [laughs] I can remember his saying, "They promised to study the brain, but if they don't, I'm not going to fire them." So I learned both about academic freedom and judgment from Seymour Kety. I never got involved with the chemists that he hired, but their names would be familiar if we mentioned them. But in any case, he was in the hiring mode because the whole institution was growing. He managed to do that at the same time we were doing the local circulation of the brain work.

After I had been at NIH for two years, the Korean debacle [Korean War] was over and I was free to return. Seymour [Kety] and [Wade] Marshall offered me an opportunity to stay there fulltime but I had already decided I wanted to do clinical neurology. This related to the opening I saw here. Not an opening – I wasn't thinking of become a department head, but [referring to portrait hanging in the room] that's O'Leary behind you there. He was a wonderful role model but what the institution obviously needed was somebody who would focus on clinical neurology more than he was able to do. [BWS: You saw that?] Yes. I knew that I wanted to do clinical neurology. [BWS: You knew that could be done here?] It was needed here. I'd been taught here. The primary teacher of clinical neurology was a guy named [Irwin "Bud"] Levy, who I think was here when you were here [to VWH].

VWH: I think he died during the time I was here.

WML: He had been well-trained at Columbia in New York and was our role model clinician in private practice. You were just talking to [faculty member] Stuart Weiss [VWH: Yes] who was his partner. So a few of us are still living. Anyway, at the time when I was [at NIH], I decided I would take the Board exam. The Boards still existed then. The Board had an eight-hour oral exam. There was no written exam. This was the American Board of Psychiatry and Neurology. It had two sections – one called clinical psychiatry and one called basic psychiatry. In any case, I flunked the exam. I flunked the neurologic exam. I had a wonderful case of sub-acute combined system disease. I did the examination perfectly but I couldn't think of the name of the condition. But the psychiatry business was all psychoanalysis at that time. Anyway I failed two or three –

BWS: Had you been trained in psychoanalysis?

WML: No. No.

BWS: You had nothing in that background?

WML: At that time, Eli Robins, who subsequently came here and became my dear friend, and the father of DSM4 – which is the modern system of classifying – at that time he was boarded in both neurology and psychiatry.[25] Ray [Raymond] Adams was his father figure in neurology.[26] But everybody in the program at MGH [Massachusetts General Hospital] had to be psychoanalyzed. This was everybody. He tolerated that and then knew what he was criticizing from the beginning.

(20:06)

In any case, I also failed the second time I took the exam. I wrote a letter to the then-secretary of the American Board of Psychiatry and Neurology who was a nice alcoholic psychiatrist at the Mayo Clinic. His name isn't important but he was secretary and a wine lover. The banquets were always loaded with good wine. I said, "I've just had trouble with the examination. Could you

[25] Diagnostic and Statistical Manual of Mental Disorders, 4th edition. The first edition was published in 1952.
[26] Adams was Professor of Neuropathology at Harvard Medical School and Chief of Neurology at Massachusetts General Hospital

really tell me, please, about basic psychiatry? What do you do with it now?" I lost this letter too. But he said, "Dear Dr. Landau, Basic psychiatry is that body of knowledge which is basic to psychiatry." [laughter]

Subsequently I passed the Board exam in New Orleans and I had a wonderful patient who had myasthenia gravis. I remember this vividly. I had learned how to talk about Freud's theories of anxiety but they were cutting back on that anyway. As I examined her, I discovered she had Argyll-Robertson pupils too. So I told the examiner that she had myasthenia gravis but that she also had syphilis. And it wasn't in her chart because it was so common in New Orleans. In any case, I passed with flying colors. The amusing thing is having failed the Board exam twice, I subsequently became a Director and President of the Board, you see. That is part of the story.

In any case, coming back here, we're now going to come to Landau-Kleffner [the Landau-Kleffner Syndrome]. But the geography of where we're sitting is perfect because the Central Institute for the Deaf is the building two doors down here.

VWH: Do you mind if I take things –

WML: OK. Whatever you want. [general discussion] What I've told you about this discipline, having flunked the Board exam, having decided that I wanted to be in academic neurology, O'Leary welcomed me.

BWS: Was that unusual – there was neurology and psychiatry – you were focusing – your research led you – ?

WML: It's complicated.

VWH: I was going to jump ahead to the time when you were President of the Board. The reason is [to hear] a little more about that experience. But part of it was – at the time I was in training – there was a reputation that you had been quite tough in your time on the Board. That all of your residents had better be well prepared because people might retaliate against us.

WML: I don't remember that. I didn't even know if it were so.

VWH: I don't know if it was so either, but we were told that you had been tough as a Board examiner.

WML: I thought that I was a fair leader. I'm disappointed the way the Board has evolved now because the national people never know what happens at the local level anymore. If they could just come and visit a program and do an exam on the scene now, I think it would serve the purpose that the national exam used to have.

BWS: Did it prove helpful to you – to go through that experience the way you did?

WML: Yes. Well, I was impressed that we – we wanted people in both specialties to be safe practitioners. We strived to see how they could take a history. I remember the psychiatrist who turned an ophthalmoscope into his own eyes. We thought that was –. Sometimes there were catastrophic things that happened. But I thought we conducted a fair kind of examination. We never disapproved without having a senior person come in and witness what was going on. There were always at least two examiners. One way or the other, they – this, what you are telling me about the social reputation that I had evolved on my candidates for the Board – nobody told me, even my own residents.

(25:16)
VWH: Well, I'm not saying it was true. It was just what they told us to scare us, perhaps.

WML: If it was true, in all honesty, we never even ran a – I never got to the point where we ran a pseudo-exam here. Did we? I don't remember that.

VWH: We did.

WML: You would be interested – [former Washington University faculty member and neurology residency director] Sven [G.] Eliasson is back in St. Louis. He is going to be visiting here on Monday. He's just visiting friends here. He has been living in the North [West: viz. California]. He lost his wife, too. But anyway, what we tried to do was to just see if you could take a good history – which is the main part of the game – and – I think people would have had a hard time who didn't take a history.

VWH: Yes. At the time I was here, you had – the residency program had mock Boards, one Saturday each [year], that everyone was required to attend, faculty and residents. And the underlying rationale, we were told, was because your residents better be [WML: well prepared] well-prepared.

WML: Anyway, my conscience never bothered by because I failed. I thought the emphasis on psychoanalysis was unfair but I had survived that. And I had failed an exam on a genuine neurologic patient. And I knew I had failed that.

BWS: Before we go back, did you have specific goals in mind or specific ideas when you were leading it that reflected your experience? Did you think about that at all?

WML: That was our whole responsibility. We were the national standard center. [BWS: Yes.] It just went with the territory. I wasn't doing anything –

BWS: You weren't doing anything –

WML: To be head of the Board and not to be interested in the quality of passing would have been silly because that was what that was there for.

BWS: You were keeping the standards - ?

WML: That is years afterwards. You are back into the last century. [BWS: Yes.] I was fond of always referring to Dr. [James] Parkinson who described parkinsonism – it is named after him in

1817 – and I was fond of saying that this history of parkinsonism goes back to the last century.[27] It is the century before the last century now. Anyway, when I was Head of the [American] Board [of Psychiatry and Neurology], it was the last century. I never thought I would live this long. Being an octogenarian is a painful experience. That is why I walk with a cane. I have a little neuropathy that Stuart Weiss hasn't cured. It's not diabetic. We have good neurologists. Stuart is still the father figure of clinical neurology here [Washington University] and he is my doctor.

Anyway, having decided – [James] O'Leary had indicated he wanted Leonard Berg back here and he wanted me back. Leonard – he wanted to become fulltime, but Leonard wanted to go into practice. This is important because he is a historical leader in Alzheimer's research who appropriately made the New York *Times* when he passed away because he really did establish [VWH: Yes] the first program of looking at normal people who volunteered to be studied every year until they got symptoms of brain rot.

VWH: This was before there were Alzheimer's disease centers which were subsequently –.

BWS: Before there were any centers?

WML: He established – we have, I think, the best center in the world right here with [David M.] Holtzman and [John C.] Morris. Morris is the clinician leader and Holtzman is head of the department. They are both terrific heirs. They have done wonderful work. There is a paper you are going to see shortly that is coming out of here. It is already in electronic copy. It's headed by their fellow, Bateman [Randall Bateman]. It is an international study in which everything came together of the natural history of this hereditary-dominant –

(30:23)

VWH: The DIAN [Dominantly Inherited Alzheimer's Network] project?[28]

[27] English physician James Parkinson published *An Essay on the Shaking Palsy* in 1817.
[28] For more information, see /hopecenter.wustl.edu/?p=9056, accessed February 4, 2013.

WML: Yes. You may have seen it, but in any case, most of the authors are on the faculty here. It hasn't come out in the printed edition of the *New England Journal* [*of Medicine*], but it is a long paper.[29] It's a monument to both the clinical and basic science because Holtzman's basic science lab and Morris' heirdom to the clinical program of Leonard has just been beautiful to sit by and watch. I have had no active engagement except wonder at it over the years.

VWH: That seems to me to be, not a metaphor, but sort of a reflection on a long history of basic neuroscience and clinical neurosciences across departments at Washington University.

WML: That's right. The tradition at this school – which goes way back – is the interdepartmental collaboration. It's amusing – Marc [Marcus] Raichle, I think, has four departmental appointments. This is our leader of a lot of things related to the nervous system. Interdepartmental appointments have been traditional now for a hundred years. That really is part of the story. Now what I didn't get into was O'Leary's clinical troubles because when I was here, he was fighting the alcoholic head of neuropsychiatry named [Edwin F.] Gildea – whose wife was a psychoanalyst – in order to develop an autonomous Department of Neurology. So this was evolving during my evolution here at the same time. When I came back, the Head of Medicine – [W.] Barry Wood – had asked him if he would head neurology even though he had never had an internship. But when Barry Wood came here as Head of Medicine, he had only published three papers himself.

VWH: So this was neurology as a division within medicine?

WML: No, it was within neuropsychiatry. But the head of medicine thought we ought to have it. Bud Levy operationally was Mr. Neurology by everybody's blessing. But he [Wood] wanted an academic leader who would take the chair for a salary. [Bud] Levy was never paid by the Department. Anyway, when I came back, my query to myself was, "What should I do?" I knew Lorente [de Nó] had left. I never met Lorente, in fact. But I knew Dr. [Hallowell] Davis. I

[29] *New Engl. J Medicine* 2012;367:795-804.

suggested to O'Leary it might be interesting to see what was going on neurologically with the kids who were in residence [at the Central Institute for the Deaf] down the street.

BWS: Why did you ask that?

WML: Because they were there.

BWS: They were there.

WML: This was just, "What can I do that was different?" I had never seen those [deaf] children except *en passage*, but I knew that here was a neuroscientist in charge there. The head of the institution was a guy named [Max A.] Goldstein who was a very – he was a scientist in audiology. I'm blocking for his name now too. I'm having a little trouble with this [left] hemisphere. In any case, I proposed this to O'Leary and to Davis. In those days we didn't need a grant. I was going to do a neurologic exam. O'Leary would do an EEG for free. And we got the radiologists to be in for a plain skull X-ray because that was all there was to do. The [research] program went to several papers just for the general findings among these kids.

(35:54)
The syndrome that got me into the dictionary is funny. We've written about it and published it.[30] [Frank R.] Kleffner is still alive. He now lives in Kansas. But he was then – there was a lady named [Mildred Agatha] McGinnis who was a teacher of the students here who never received the credit she should. She wrote a little book about aphasia in children.[31] She had become impressed about the teaching of the hearing impaired – that's a nice general term. There was a group of children whose trouble was with language, not with hearing. She developed a division of the school – now it was a residential school, the kids had to be mature enough to handle themselves in a dormitory setting. But she thought - these had a special program. Frank Kleffner

[30] Landau, W.M., Kleffner, F.R. Landmark Article. "Syndrome of acquired aphasia with convulsive disorder in children," *Neurology* 7:523-530, 1957. *Neurology* 51:1241 (reprinted 1998).
[31] McGinnis, M. *Aphasic Children: Identification and Education by the Association Method* (Washington, D.C.: Alexander Graham Bell Association for the Deaf, 1939). The second edition was published in 1963.

was her second banana or her first banana. She was still – she retired a year or two after I got interested here.

VWH: Frank Kleffner was a psychologist?

WML: He was a teacher of the deaf. He has a Ph.D. I guess his Ph.D. was in education or something. [VWH: Oh.] There were a couple of other psychologists. There was a psychologist named [Bob] Goldstein. There was a whole crew of people who were then interested in the evolution of the computer, one of whom became quite famous and is still alive. I take care of his wife. He was also in the Department of Physics. Anyway there was a lot of evolution of instrumentation that was going on over there. I had nothing to do with that. I decided I'll learn something by just seeing the patients. I guess hadn't been there a couple of months when Frank let me know that "What did I know about children that had seizures and lost their speech?" He had three or four of them in residence. So my genius was to say, "Oh?" [laughs] He indicated – he is a very sweet fellow, but he didn't tell me that I was an incompetent asshole. That being a neurologist, I didn't know about these cases because he had more than one of them. And that's the story. [VWH: Yes.] And the only thing that evolved later was the nocturnal business that these kids have dreadfully active epileptic-like EEGs in their sleep. But the seizures are seldom the clinical problem. They go away by themselves and they are easy to control with medicine.

In any case, that's the syndrome [Landau-Kleffner Syndrome]. We're still both supposed to be experts. There are very few cases of it. [John] Zempel who's the pediatric epileptologist looks for these cases and they are very rare. He gets one every year or two. They are not common. In any case, so we got six cases and later we had a few more that I wrote up with [John] Mantovani.[32] [Comment directed to VWH] You may have known Mantovani – he is a pediatric neurologist who is in private practice in St. Louis. A very good guy. That's the story. I'm famous for something that I didn't do a goddamn thing about except to say, "OK, let's look at them." [laughs]

[32] Mantovani JF and Landau WM. Acquired aphasia with convulsive disorder: Course and prognosis. *Neurol* 30:524-529, 1980.

VWH: It always struck me as curious because your research had been done in the motor system, the pyramidal system, neuromuscular junction, and sort of going back to my era – you didn't have a great affinity for higher cortical functions.

WML: Not at all. I have just taught you all the genius that went into this. We wrote about this – later I wrote an editorial about it. More recently, there was a paper that [Blaise F.D.] Bourgeois and I wrote an editorial about.[33] A group at MGH [Massachusetts General Hospital] described shrinkage – or at least smallness – of the first temporal gyrus [on the left and right sides] in these kids. They had four or six kids. We've not been able to confirm – well, we have one kid that we followed. She is now age fifty-three. She recovered completely. We have a wonderful movie of her. In fact, I'm going to show it – I've been invited to go to the Mexican neurology club next month. I'll show this movie that we took of her in 1961 when she was a beautiful little five-year or six-year-old. She recovered completely and now lives in Montana and has three grown-up children of her own. She had become head of the School Board in her town. We have a movie of her all grown up. In any case, it is a very rare condition. And there it is in the goddamn dictionary for no good reason. We still don't know anything more about it. I'm pretty sure it's related to – [former Washington University faculty member] Arthur [L.] Prensky described some kids that have a similar nocturnal EEG and get trouble in the motor cortex region. It has a name too. It isn't a human name.[34]

(41:36)
Anyway, subsequently – I'll finally give you one more anecdote related to pediatric neurology. After I came back and had just gotten started in interest in the Central Institute [of the Deaf], but this didn't take much time. I worked in the clinic. I worked everything – I was doing my laboratory work was with the motor system and spinal reflexes and spasticity. This was my primary research interest. I guess I had been here only a year or two – maybe three, I'm not sure how long – but what had happened was that O'Leary and [Alexis] Hartmann – who was the head of – by this time that I'm now talking about a Department of Neurology existed, so this must have been several years after I got back, not many – but Hartmann had acquired an endowment

[33] Bourgeois BFD and Landau WM. Landau-Kleffner syndrome and temporal cortical volume reduction: cause or effect? *Neurology* 63; 1152-1153, 2004.
[34] Acquired Epileptiform Opercular Syndrome [Rolandic].

for a Chair of Child Neurology. He was head of Pediatrics so he and O'Leary had to find such a person. It happened that Dr. Hartmann decided to die [in 1964] just before he was supposed to retire. So they had this on their hands but no Chief of Pediatrics to help select him. O'Leary asked me if I would be willing to take care of Child Neurology until they could get a permanent person. I thought about it a while and I said – I knew it would be interesting – I said, "If you could get David Goldring – if I could get somebody to cover me for babies, I will." I knew it from nothing. So I got David Goldring, who is Sidney's [Goldring] elder brother.[35] [VWH: Oh.] He was a pediatrician who took a subspecialty of cardiology. He became, from a general pediatrician professor, he became Cardiologist-in-Chief for [the St. Louis] Children's Hospital.

(44:57)
VWH: This was before Phil [Philip] Dodge?

WML: That is part of the story. I'm now getting to the next chapter. So I said, "I'll do this but I don't feel comfortable about taking care of sick babies unless somebody will hold my hand." David agreed. Every time I had a sick baby – all we had was Dilantin and phenobarbital anyway – but I didn't want to kill one. So he held my hand when we had sick babies. We were just learning a great deal about very sick babies and their maintenance in the first year of life.

After a couple of years of this, it was almost two years – nothing was happening administratively. There was a Search Committee [for the endowed chair]. So I went to the boss, O'Leary, and said, "I enjoy this but I would like to spend my clinical time with adults as I used to. Can't you get the committee to get off their ass and move on this?" I remember he said, "Bill, you're right. I'll talk to the Dean." He called [M. Kenton] King and literally the next day Dr. King calls me and asks if I would join the Committee. The first meeting – the second meeting I went to – the Head of Pathology [unrelated comment] [Paul Lacy] returned from a trip to Boston and described a person [Philip Dodge] who was seeing children at MGH [Massachusetts General Hospital]. "What did I think about him?" I'm very proud of this anecdote. As you've heard, I'm talkative. I put him down. I said, "He's not going to do any chemistry. He's not going to do like Eli Robins [chair, Department of Psychiatry] did. He's not

[35] Sidney Goldring was a former Chair of Neurosurgery at Washington University.

going to be like our head of Pharmacology [Oliver H. Lowry] who was the father of neurochemistry here." I said, "I don't know him very well but he has a reputation as a great clinician. If that is what you want, that's what we will have. He will not have a wet lab." I remember saying that. "He will not have it." The reason Leonard Berg went into private practice was because he didn't want a wet lab. O'Leary had told him that he could have all the basic science training that he wanted because he wanted him to be a Professor of Neurology with a lab. But nobody could do that. It wasn't appropriate at that time. So, it didn't happen. We then got Phil Dodge and that solved the problem. We had both the new Head of the Department and the Head of Child Neurology and all evolved very well. Subsequent to that he became Head of Pediatrics before I became Head of Neurology.

VWH: Arthur Prensky [later, chief of the Division of Pediatric Neurology] wasn't there at the time?

WML: No, that is still another anecdote I will tell you.

VWH: Stuart Weiss said that he [himself] was originally a pediatric neurologist.

WML: He trained with the first Columbia [University] pediatric neurologist.

VWH: Sidney Carter?[36]

WML: Sid Carter trained Stuart. So here was another school – the Boston school. Stuart has told me that more recently Phil was kind of tough with him. He had to step away. He didn't want to be fulltime and didn't want to confine himself to child neurology. So he just became a full partner of [Bud] Levy's and he really stopped seeing children as in-patients.

BWS: Because Phil Dodge was –

[36] Sidney Carter was one of the founders of pediatric neurology. He was with Department of Neurology at Columbia University and the Neurological Institute of New York, and from 1969-1971 was president of the American Academy of Neurology.

WML: Was the Chief of both. [BWS: Was the Chief of both? Child Neurology and the Department of Pediatrics.] He was ready to take him on if he wanted to be fulltime, but he didn't want to.

BWS: Did Phil Dodge establish - ?

WML: He was a father of Child Neurology. Subsequently Phil and I became very close friends. He told me much about his own decision to come here. Interestingly, before he came, the Chair had been offered to – oh, God, here's another name – the guy at [Johns] Hopkins who most recently was interested in the Chinese variety of acute neuropathy. You know – the name begins with an M.

(49:44)
VWH: Guy McKhann was a pediatric neurologist.

WML: McKhann. McKhann.

VWH: He worked with Krabbé's disease early in his career.

WML: Yes. McKhann was offered the Chair before – during the interim.

VWH: I didn't know that part of his history. He [McKhann] came from Stanford to Hopkins.

WML: McKhann was offered the Chair [of pediatrics] before Phil. [VWH: Okay.] He had been Phil's trainee. He had worked under Phil – I don't know where. It must have been in Boston because Phil never left Boston. But he had had the experience of being Phil's protégé. In those days, people were hired with two or three papers. [Bernard] Becker had written four papers when he was made our Head of Ophthalmology.

In any case, Phil then came here with the idea that our problem was more than just Child Neurology. What he recognized was that pediatrics, academic pediatrics nationally, was sick. It was just a bunch of primary care child doctors like David Goldring had been. Like [pediatric neurologist and emerita faculty member] Jean Holowach [Thurston] is – or was. Who just assumed that they were going to do what they did, but there was no specialty in neurology. But there weren't any other specialties. What Phil explained to me was that he recognized that his job was only to use Child Neurology as a role model. But he wanted to develop all the subspecialties that had become the pattern in internal medicine. So then they began to develop Pediatric Oncology, Infectious Disease – the whole smear. That is what he saw as his accomplishment was to organize a subspecialty Department of Pediatrics that covered the scientific waterfront. [VWH: He certainly covered Pediatric Neurology very well.] And he did it.

You mentioned Arthur Prensky. I had never met Arthur but Phil had offered him a job. He came to me to ask what my opinion was in taking the job with Phil. I'm quite sure [he recalls] what I told him. I said, "Arthur, if you don't, you'll be the most stupid asshole I've ever met." [laughs] He remembers this and he is very pleased that he took the job because he is, as you know, a superb clinician and has done a wonderful job. His second wife has just become seriously ill. He lost his first wife, who was an ardent smoker, of cancer. [aside comment] It was tragic. The second wedding was lovely, two years ago.

VWH: Why don't we take a break so I can stand up and stretch?

[break in interview]

Track 1038

BWS: We're going. Dr. Landau, Track Three (of three recorded tracks)

WML: The book arose as a product of my enjoyment of writing letters to the editor and Bob [Robert] Daroff's welcoming of that activity.[37] The big fuss I got into about carotid sinus syncope because my wording was too strong, even by hindsight, in condemnation of people who were involved in the game.[38] In any case, I'd written more than one letter to the editor and I got the idea – I suggested to him that if he would, I would write critical letters – not critical letters but sort of editorials about subjects that interested me – subject to his editorial review. He said, "Fine," and so they started. When there were enough of them, I got the idea of putting them into a book. Subsequent editors weren't so happy with the prefixed editorial decision although I expected the editor to do more than copyediting if he wanted to. But successors didn't welcome the idea and so my subsequent critical writings are not collected. Some of them are in the book and some of them have been printed elsewhere since the book was published.

VWH: To back up, the neuromythology series came out while Bob Daroff was editor of *Neurology?*[39]

WML: Right. That is where I first stole a word from my hero – [British neurologist] Sir Frances [M.R.] Walshe. He invented the word "neuromythology."[40] I just borrowed it. He was an Irishman and Irish people always write better English. He invented the word and I borrowed it with his blessing. Now there is another enterprise here. [John W.] McDonald – this I will tell you quickly about it. He was interested in spinal cord injuries – still is. He was taken away to [Johns} Hopkins [School of Medicine]. [aside comment] He [McDonald] was in charge of – he was interested in spinal cord injury. [aside comment]

BWS: We are running here.

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[37] William M. Landau, *Clinical Neuromythology and Other Arguments and Essays, Pertinent and Impertinent* (Armonk, NY: Futura Publishing Company, 1998). Updated edition published in 2001. Robert Daroff was the 1990-1991 president of the American Neurological Association and editor-in-chief of *Neurology* from 1987 to 1996.
[38] Landau, WM, "Clinical neuromythology XIII: Neuroskepticism: Sovereign remedy for the carotid sinus syndrome," *Neurology* 44:1570-1576, 1994.
[39] VWH is referring to a series of letters to the editor about "neuromythology" topics in *Neurology,* 1988-1997.
[40] Walshe, *Critical Studies in Neurology*, 1948.

WML: Okay, so be quick. He invited me into – new *Handbook of Clinical Neurology* of spinal cord injuries was in press from Elsivier.[41] In 2006 he invited me to write a critique of all the thirty-eight chapters online, and I did. I submitted them last year. He and the editor decided they didn't want to print them. [aside comment and comment to VWH] You'll enjoy reading it. But in any case, they decided not to publish it, so I hired a lawyer and I won. I'll get enough back to pay my lawyer and they are going to apologize. [aside comment] In any case, they are not going to publish it in the [*Hand]book* but as a product of my litigation, I'm getting paid $5,000, which almost pays my lawyer, and an apology that they reneged on their commitment to print it.[42]

BWS: One of the things that is written about your neuromythology book is that you really encourage critical thinking.

WML: Yes, that is the whole point.

BWS: And that you write it well.

(5:09)
VWH: You know, in terms of neuromythology I wonder if you might talk a little bit about DATATOP because it is where - I'm not sure where this started, but most of the heat was – or a lot of the heat was – and also a little bit about spasticity.

WML: What is DATATOP now?

VWH: The Parkinsons – the Deprenyl Trial. [Deprenyl and Tocopherol Antioxidative Therapy for Parkinson's Disease]

WML: Oh, the Deprenyl Trial. That's where I really got into it.

VWH: Yes, exactly.

[41] Joost Verhagen, John W. McDonald, *Spinal Cord Injury* (Elsivier, 2012).
[42] Landau, WM. "Handbook of Clinical Neurology, Spinal Cord Trauma, Third Series," 2012.

WML: It speaks for itself. I did regret hurting peoples' feelings and I apologized. I can only say that the critique piece spoke for itself.[43] My use of language, which referred to swindlers at the time of the first depression, who were very similar to those of our most recent depression, did not mean to imply that the DATATOP people were themselves of questionable virtue. But I confessed in later publications, the reason I got that reaction is because several of them were. What's-his-name, the guy who wrote a novel – a neurologist in Chicago who wrote a novel and died of a coronary [VWH: (Harold) Klawans] – told me privately that the reason I got all this reaction – this was a period in the '80s of the last century when all of our sensitivities about conflict of interest had not yet been established – these guys – many of them thought this [Deprenyl] was a great thing to invest in before everybody else got aboard. So what I had intended was to tackle them in regard to professional ethics, not in regard to economics at all because it was beyond my thinking at the time. Also it never would have occurred – in that period nobody worried about "you're prostituting yourself to a drug company." It was a new idea and I hadn't thought of it, but it was accepted that that is what I was accusing them of. So – and I wrote this – in my apology I indicated I did not intend to accuse anybody of unethical economic behavior. It was that I was concerned that they were bending scientific standards because they already had shown by their own records that, with time, the disease progressed again anyway. That was the whole issue. [VWH: Right, right]. The *New England Journal* [*of Medicine*] was stuck, along with the story.[44]

Now, spasticity – .

VWH: Let me back up there. [WML: Okay] Even before there was formally a neuromythology, there was "the emperor's new clothes" – the editorial you wrote about spasticity. [WML: Yes.] One of the reasons why I bring it up – it surprised me somewhat –there is something called the

[43] Landau, WM. "Clinical neuromythology IX: Pyramid sale in a bucket shop: DATATOP bottoms out," *Neurology* 40:1337-1339, 1990. Landau, WM. "Special correspondence: DATATOP and clinical neuromythology IX," *Neurology* 41:771-777, 1991. Daroff, RB. "Correspondence to the editor: Neuromythology IX and DATATOP," *Neurology* 41:1703-1704, 1991.

[44] See: Effect of deprenyl on the progression of disability in early Parkinson's disease. The Parkinson Study Group. *N Engl J Med.* 1989 Nov 16;321(20):1364-71; Effects of tocopherol and deprenyl on the progression of disability in early Parkinson's disease. The Parkinson Study Group. *N Engl J Med.* 1993 Jan 21;328(3):176-83.

"Web of Science" where you can look at citations and this is your number five most cited publication – that editorial – "the emperor's new clothes."[45]

WML: That's good. I recently found I was cited in *Cecil's Textbook [of Medicine]* in regard to an editorial I wrote about how to do an examination.[46] I was very pleased with this. It was by Fred Plum and [Jerome] Posner, in their section of Cecil. I can show it to you. I pulled it out. This was an editorial I wrote. It was an editorial that the editor asked me to write about a paper concerning stroke prognosis. The authors were in New York – they were in the North Manhattan Group. What I described in it was only my own concept which is that "history is everything" and this jibed exactly – to have this referred to by Posner and Plum, which I just discovered several weeks ago, was just a delight because I knew Fred – better than I know Posner, but they are both wonderful. To be cited by Fred Plum as a clinician was something from the grave – I was in the book, in another edition of Cecil.

(10:42)

My concept was simple-minded. It wasn't any different from Adie [Adolph L.] Sahs.[47] He wrote about the same thing. He said if you don't have enough time, you spend a half a minute on the EEG and the rest you take a case history because history is everything in neurology. I think it is going to become even more so with Obamacare [Patient Protection and Affordable Care Act – PPACA], because neurology is a chronic-disease specialty. I'm glad to have this on the tape. We have to help the primary care doctors by being in more – in direct charge without having to charge for additional MRIs-of-the-month for chronic care. We should be – as a matter of fact, I thought the sub-specialization in stroke was silly. I still think so. I think being interested in neurologic intensive care as a career is wonderful, but I think strokology is – it doesn't turn me on scientifically or otherwise. But I think it is important that physicians in neurology be comfortable in taking care of somatoform conditions and migraine and epilepsy and gliomas.

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[45] Landau, WM. "Spasticity: The fable of a neurological demon and the emperor's new therapy," *Arch Neurol* 31:217-219, 1974.

[46] Posner, J.B. "Disorders of Sensation" in Bennett, J.C., Plum, F. *Cecil Textbook of Medicine*, 20th ed. (Philadelphia, PA: W.B. Saunder Co, 1996): 2031-2036.

[47] Adolph L. Sahs was chair of the Department of Neurology at the University of Iowa, a founder of the American Academy of Neurology, and an early president of the Academy. Sahs' comment on the importance of the neurologic history is recounted in Robert Joynt's obituary of Sahs (*Arch. Neurol.* 1987;44:562).

These are all our bag. They all are chronic. I don't think a single visit solves many of these problems. I've been particularly interested in complex regional pain syndrome [CRPS]. My hero in this field is José [L.] Ochoa who has written a lot about it. It is a somatoform condition and that usually includes malingering. It's been so poorly dealt with by doctors that don't take histories. I just had a phone call from Oklahoma yesterday or the day before by a doctor – by a lawyer whose patient – whose potential client – has had the diagnosis of complex regional pain syndrome. She is now eighteen as he described her. The problem was that she had an operation done on her knee when she was thirteen and they discovered post-op that they had operated on the wrong leg. He was to be employed as her lawyer to see whether that wrong-limb operation on the circulation near the knee had contributed to her illness. So I just emailed him Ochoa's bibliography yesterday and explained that she had a somatoform condition and this didn't make it any worse or better. I didn't think it made it any better. It wasn't a good idea to operate on the wrong leg. But anyway this comes back to my interest in history. Ochoa is a superb – he has done some of the great original work on axonology of pain. He actually was the first person to record from single [human] peripheral C fibers when he was in Britain. He is a Peruvian originally. He lives in Portland. He has done some excellent work in the clinical description of these patients that have fascinated me because of my primary interest in what you know about a patient if you just take a careful history and see what happens.

(15:04)
BWS: Did that answer the question for you?

VWH: Yes. Maybe we could talk a bit about your time as chair and how the transition from O'Leary came. [WML: This came -] The relationship of psychiatry to neurosurgery that combined –

WML: That's very interesting. The primary problem for O'Leary was that he needed a separate budget and some help to become autonomous. [Edwin] Gildea wasn't mean; he was just uninterested in helping this division of the department evolve. Henry [G.] Schwartz who was Head of Neurosurgery was friendly politically, but he wasn't a department head either. So it was

a quite a push. And at that time, the Executive Faculty, which was department heads whose structure and power went back to the report in 1910 –

VWH: The Flexner Report.[48]

WML: The Flexner Report. It was clearly understood that there was some recognition that the more departments, the less power – the bigger it is, the less power they had for the other department heads. In other words, there was a reluctance to set up another department which would be another vote. It was this general resistance to expansion of the Executive Faculty that was his problem politically. So eventually this got resolved and it became a department. When he [James O'Leary] retired in 1970, I was offered the job. I got some other offers – I remember Jersey City [and Ohio] which didn't turn me on. Anyway, so it was then a department.

Now subsequently, my dear friend from high school and personal friend, Sid [Sidney] Goldring, had become Professor of Neurosurgery. He had spent a period in Pittsburgh which he didn't like. In the end he came back here. He'd spent some time in Vietnam – not Vietnam – in the Far East. I guess he was in – anyway he was in the Far East. It wasn't Vietnam. Maybe it was Siam [Thailand]. In any case, it came to pass about 1975 or something like that that Henry was retiring – Henry Schwartz – and Sidney was offered the chair. Sidney did not want it. He said he didn't want it because he didn't want to work under [Walter F. Ballinger], who was the Head of Surgery.[49] He said he appreciated the honor but he didn't want to work as a derivative of the Department of Surgery under [Ballinger]. [aside comments] This was Sidney's firm position which he communicated to O'Leary. O'Leary came to me and said, "You trust each other. Why don't you – would you be willing to accept being a partner?" I said, "If he handles his budget and I handle mine, that's what we were doing already. Why not?" So I did. What we agreed was – the Executive Faculty met once a month and we would take turns unless one of us had a problem and was re-appointing or promoting somebody or had something to do – otherwise we would just take turns because neither of us wanted to go to the meetings. We did that for several

[48] The Flexner Report on Medical Education in the United States and Canada (1910).
[49] According to the Washington University School of Medicine in St. Louis Department of Surgery Timeline, the Head of Surgery from 1967-1978 was Walter F. Ballinger II. Source: http://www.surgery.wustl.edu/About-Us/Timeline, accessed February 4, 2013.

years. Then he retired first. But in any case, when [Ralph G.] Dacey [in Neurosurgery] came along, we continued this for just a year or so. Then when I was succeeded by [Dennis] Choi, [aside comment] they became separate departments again. [aside comment]

BWS: You actually – how did it work with the two of you [you and Goldring] then when you were both doing it?

WML: Each of us had an autonomous budget.

(20:48)
BWS: You kept – ?

WML: He didn't know about my money and I didn't know about his.

BWS: You kept that separate?

WML: They'd always been separate and they stayed separate. But for administrative affairs of the whole school, we were jointly responsible. [BWS: Okay] This was negotiated via O'Leary and the then-Dean [of the School of Medicine] who was Ken [M. Kenton] King.

VWH: This was – part of the negotiations enabled both neurology and neurosurgery to part from separate departments and come together?

WML: Well, neurology was already independent.

VWH: That had already broken off from neuropsychiatry?

WML: Neurology already was autonomous. [VWH: But not neurosurgery?] Since I was stepping down from being an independent head of Neurology, I didn't become head of Neurology independently ever again. Because when I stepped down administratively, Choi succeeded me in the same role. But operationally, it didn't make any difference, you see.

BWS: Your role was the still same?

WML: It was just the same. It didn't do anything except that I was co-head of Neurology and Neurosurgery. Operationally this helped us a great deal. In certain enterprises – the space in this building, the structure and negotiation with the hospital about the intensive care unit – which we both wanted.

BWS: You could join forces?

WML: We knew we wanted an up-to-date and ahead-of-the-date intensive care unit. But we agreed that it would be best to have our successors run it and plan it and not put it together and then let them be stuck with it. So we talked to Barnes Hospital [the primary teaching hospital of Washington University] about deferring the structure of our ICU until our successors were in place.

BWS: But you laid the groundwork for it?

WML: Everybody knew we wanted it and it was a collaborative enterprise conceptually, then and now. [BWS: You put it together.] But they became separate departments, I guess, a year after Choi took over.

BWS: You stayed together during most of the time you – ?

WML: Most of the time between 1970 and 1991 – and 1993. I don't know which year it became double, but – '74 or '75.

BWS: A couple of years on your part.

WML: Yes.

VWH: You were chair of Neurology, either separate or combined, for about two decades.

WML: Yes.

VWH: Within your part of the combined department –

WML: Neurology – functionally, it was a department.

VWH: I'm interested also in how you grew faculty and what you did with the program. Where you took it. How you built it.

WM: Well, I never sat down and just decided about what we were going to do. Sven G. Eliasson came along. He's going to visit me next week. Let me tell you how we hired him. O'Leary was head of the department and he got a letter from this guy at [the University of Texas] Southwestern who was an Associate Professor of Neurology – a professor trained in Sweden who wanted to get a residency. O'Leary asked me what he should do. I said, "Get him as an Associate Professor." I remember this episode. And he did. Now things were kind of loose with the Boards then. As I mentioned, O'Leary was everybody's friend. This was A.B. Baker and [Adie] Sahs and the genesis of the Academy [AAN].[50] This is for the Academy. O'Leary was "Mr. Science." If there was anything associated with anything that had to do with sophisticated neuroscience, he knew it. So he was always involved. And he had a great friendship and respect with Adie Sahs and A.B. Baker and the whole bunch that established the Academy. So in this context, he hired Sven. Then called Adie [Sahs] and said, "Couldn't you sort of wink at his residency requirement and let him take the exam?" And so he did and Sven got his Boards. He was very interested, as you know, in teaching. I just gradually let him take complete charge of direct teaching organizational structure. His name is on the plaque on the wall for the teacher. [VWH: Right] When he called me last week to tell me he was visiting, I reminded him that we can still find his name on the wall on the brass plaque. He was good at it. He was doing it. The

[50] The neurologists most closely associated with the founding of the AAN in 1948 were A.B. Baker, Russell N. DeJong, Francis M. Forster, and Adolph L. Sahs, sometimes referred to as "the four horsemen of neurology."

residents liked him. I just asked him to do what he was doing. I never sat back and had any strategic thought about it.

(26:05)
BWS: But you knew he fit a certain need?

WML: He was already doing it.

BWS: And the department needed it?

VWH: Part of the other reputation of the department, this is a department that has always done a lot of neuroscience – basic science for a lot of the faculty. I would imagine that was part of how you continued to build the department.

WML: We had one of the things that was invented by Bob [Robert B.] King, who was a neurosurgeon who went from here to become Head of Neurosurgery in Rochester, New York. We would have a monthly meeting called The Needlework Society. It was fun. Everybody would talk about what they wanted to talk about – often their own research or somebody else's. The basic science people – Max [William Maxwell] Cowan was involved and the people that Max Cowan hired – [Washington University neurologist] Tom Thach was hired by Max Cowan. Tom is just retiring. I talk with him often. [aside comment] He was Mr. Cerebellum and still is. But anyway he was hired by Max Cowan.

A good example, I can remember this. When Max Cowan was hired to be the first real neuroscience head of Anatomy - he was a South African – a sophisticated neuroscientist, O'Leary managed somehow to slip him an electron microscope. I've never tried to check into it but I know perfectly well that he had a big grant – which was very big by the standards of the period. [VWH: Those were expensive.] And he helped Max get moving with it. We got one in our department that O'Leary and Goldring both worked with – that O'Leary and Bishop both used. But it was very important. Meanwhile [Bernard] Becker bought one for ophthalmology. This was a status symbol at the time – but they were used. The point is, the interdepartmental

collaboration was such that if you could help your collaborator get moving by somehow moving a piece of equipment out of your lab into his before anybody discovered it – somehow they did it. I know that he – I forget how I discovered it, I think just in conversation. O'Leary mentioned that he was helping Max get going. This was before Max became politically powerful in his own shop. But this degree of collaboration was just what was expected of people. My favorite example – [remark directed to VWH] do you know about excitotoxicity? Do you know that story?

(29:54)
VWH: I don't know the story. But I know the –.

WML: It is my favorite illustration of Washington University School of Medicine in St. Louis. John Olney was a resident in psychiatry. [discussion replaced with "The Olney Story" by William M. Landau at Dr. Landau's request]

"This story, in the style of Sherlock Holmes, is the Curious Case of the Fat Mice and the Compulsively Ultramicroscopic Psychiatrist. The fable justifies our pride in the inevitable fact that it happened here at Washington University School of Medicine in St. Louis.

Iowa stubborn born and bred, John Olney was University of Iowa educated. His decision for medical school was deferred but was ultimately motivated by the tragically severe fatal multiple sclerosis illness of his younger sister. He took catchup science courses. 'The first thing on his naïve agenda was to discover the cause of multiple sclerosis.' Early on in medical school he was fascinated by the 'magnificent cytoarchitectonics' of brain cells. He won a Borden Award for undergraduate research that included differential staining of brain cells. I cannot debate his identification of psychiatry as the most challenging field of clinical medicine related to the nervous system. He was attracted to Eli Robins' Department of Psychiatry because of its 'strong tradition of neurobiological and clinical research.' That's what John wrote. Still fascinated by the anatomy of nerve structure near the end of his residency in 1967, John asked Robins if he might seek training in the then new technology of electron microscopy. Immediately Eli picked up the telephone and called Adolph Cohen, Professor of Anatomy. Cohen was interested in the retina, and Bernie Becker, the Head of Ophthalmology, had established a laboratory for him in the eye

department. Cohen welcomed John with the singular constraint that they were committed to studying the eye. None of the parties considered it to be unusual for a psychiatrist to obtain graduate basic science training from a Professor of Anatomy in the Department of Ophthalmology.

This tradition of interdepartmental collaboration is the central feature of our heritage since 1910. Robins himself, having completed clinical training and Board certification in Psychiatry and Neurology, came to Washington University in 1951 to learn new modes of brain chemistry from Ollie Lowry, our then new Head of the Department of Pharmacology. Both of Ollie's predecessors won the Nobel Prize. Herbert Gasser, with Joseph Erlanger, was nominated for the Prize by Evarts Graham, our Head of Surgery. I was a medical student in physiology in 1944 on the day that it was awarded. The next winning Head of Pharmacology, in 1947, also a professor of biological chemistry, was Carl Cori. His co-honoree was a special take-home partner named Gerty.

The Cohen-Olney project was to check out a decade-old report from British investigators that the injection of the amino acid sodium glutamate into baby mice caused degeneration of the inner layers of their retinae. The results were confirmed. By chance, a few of the leftover baby mice grew up. The caretaker and John observed that some of them were fat. Neither Cohen nor other informal consultants seemed interested. Casual suggestions were that they were overfed or just lazy. John assumed the idea of surveying the tiny mouse brain and discovered severe damage in the even tinier hypothalamus, the phylogenetically oldest part of the brain that controls pituitary, endocrine, and central visceral functions. Further studies showed that the damage occurred within hours and could be produced by oral feeding as well as by injection. Infant animals were more susceptible than mature ones. This susceptibility was attributed to the immature diffusion barrier between blood and brain in the mouselets. Cohen decline coauthorship because it was entirely John's initiative.

A long sidebar story was the unexpected public health complication of Olney's virginal scientific effort. He had not known that glutamate was a popular nutrient supplement for the tasteless pap of baby food. The babies didn't care, but it provided an attractive beef broth sort of flavor for mothers who chose to taste it. Defending himself against attack by the prostitute scientists of the industrial baby food establishment, he survived a hot time testifying before

Senate committees. His partial victory was that glutamate was withdrawn from infant baby food mixtures, but still goes into foods that are fed to babies.

Beyond when and where, Olney came to the how of cell death by showing that it was postsynaptic cells, those that are naturally activated by transmitter chemical from other nerve cells, [that] were acutely destroyed. Glutamate was not just a nutrient, but rather a role model excitatory transmitter agent. Excessive excitation caused the driven cells to exhaust their nutritional and metabolic endurance. He coined the concept and the word excitotoxicity, essentially burning out from excessive activity. The idea obviously fit the well-known phenomena of brain damage related to uncontrolled persistent nerve cell activity in epilepsy. Later studies indicated that excitotoxicity could be a complicating manifestation of diseases as common as stroke, where the tissue was suddenly deprived of oxygen and nutrients. Evidence from many sources now supports the hypothesis that excitotoxicity may be a complicating mechanism of many infamous diseases – common, uncommon, now incurable, and often with human names like Parkinson, Huntington, Alzheimer, and Lou Gehrig. Such a mechanism, of course, invites specific therapeutic effort to suppress excitation. Furthermore, chemical excitation is not confined to nerve cells or heart and muscle tissue, but to most living cells whose rates or degrees of performance are externally regulated.

Last week, Dean Shapiro announced the establishment at this medical school of the Center for the Investigation of Membrane Excitability Diseases. Two score years of scientific evolution from the fat mice.

John is not resting on his laurels. Most recently he provided evidence of the risk of general anesthetic to infants' brains and at the other extreme, the potential collateral brain damage of the popular therapeutic drugs for Alzheimer's disease.

Tonight we honor our institutional capacity for sustaining a unique conscientiously purposeful curiosity. That's our John Olney."[51]

VWH: Okay. In your own faculty, I gather, most of the faculty that you recruited or developed, were people who wanted to have basic laboratory interest?

[51] "The Olney Story" by William M. Landau, Second Century Introduction, 09/28/07. The Olney paper referred to by Dr. Landau is: Olney JW. Brain lesions, obesity and other disturbances in mice treated with monosodium glutamate. *Science* 164, 719-721, 1969.

WML: It happened that way. [Joel] Perlmutter, I think, is the most prominent one. He told me he was interested in movement disorder and he told me wanted to go to London – what's the name of the guy that died – the father of Queen Square movement disorder – who died in his youth. [(Charles) David Marsden] Anyway that is where he got his start out of here. [aside comment] Perlmutter. Oh, the guy in England. A lot of the early papers of the '80s and '90s were written by [Marsden].

(34:52)
WML: In any case, the – [Alan] Pestronk – we had several people in neuromuscular disease. The last of them that was locally generated was [George H.] Klinkerfuss, who then moved to Springfield. [VWH: Yes.] When we needed somebody for that, I asked Pearlman to run a committee to find somebody for me. He found Pestronk at Hopkins. He had some other advisors. He [Pearlman] already was – he was our secret weapon in undergraduate teaching. He had the program which [Tom] Thach subsequently took over – teaching neuroscience to the undergraduates. That was Pearlman. I didn't solicit that – it just happened.

We got a Chair from a patient – I had a patient with parkinsonism. His wife gave me a Chair which I negotiated with [Philip] Needleman [chair, Department of Pharmacology] who was just getting going with his program for pain remedies. We developed a Chair in Pharmacology and Neurology – I forget what words we used but the concept was there. Jim [James A.] Ferrendelli got the Chair first and enjoyed the relationship, as did Needleman. Then when he left [for the University of Texas Health Science Center at Houston] – now it belongs appropriately to the guy who ran the department [as interim chair after Choi]. Our guy who does AIDS.

VWH: David Clifford.

WML: I'm very proud of Dave. I'd like to put this on somebody's record. When AIDS became a disease, JAMA – there were papers about this disease. It affected the nervous system. It was hopeless. It affected the brain. There was nobody in neurology who wanted to see these patients – it was such a discouraging situation. I don't recall I've ever turned down such a patient. They didn't turn up often, but nobody knew the disease. It occurred in gay airplane pilots, in any

case.[52] But as they started to come in to our Center, he [David] decided to be a doctor and he just had the physicianship to do what he could with these patients. He's become known internationally – he now commutes to Africa. [VWH: Yes.] But the wonderful thing about it is because he got into this field not because he was particularly interested in pain, but because it was a challenging issue clinically and nobody else picked up the ball. Willy-nilly he became an expert at it. Anyway, I think he's a role model of physicianship. He still takes students to Africa with him as he can. I mean the whole thing.

So I guess in answer to your question, well, I have a motto for my own children that we discovered many years ago. It is a quotation from a play which we have hanging on the wall in our dining room. I guess it's my philosophy for faculty as well. It says, "Have them, love them, and then leave them be."[53] [laughs]

VWH: I like that.

BWS: Very nice.

(40:02)
[general discussion]

VWH: Speaking of quotations, one of the things that has impressed me – particularly in the neuromythology series but in other things that you write, you always have, or often have, a number of quotations that always seem very apropos – taken from medical sources and from classical sources. Where do you come up with those? How do you know that much?

WML: They usually are after-the-fact. I don't start with them. I try to find something like that.

VWH: Do you have a collection?

[52] Patient 0 in *And the Band Played On: Politics, People, and the AIDS Epidemic* by Randy Shilts (1987) was a flight attendant.
[53] Sidney Howard, "The Silver Cord," 1926. In 1933, the play was made into a movie starring Irene Dunne and Joel McCrea.

WML: No, no. [VWH: It catches - .] Well, there's one here that I've been working on combined – well, I've been interested in carotid sinus syncope. I wrote a critical review out of a lectureship – in '82 or '83 – it was an Academy lectureship.[54] While I'm now discovering – this is interesting because I'm going to publish what I'm going to tell you about yet. I've presented it as part of a big paper which *Neurology* didn't accept and the *New England Journal* didn't accept. But the concept is that this condition [carotid sinus syncope] was originally described seventy-five years ago and it was presumed that if you could make somebody faint by punching in here [the neck – carotid sinus], that this condition occurred spontaneously. Well, I have discovered that nobody has ever described it happening spontaneously. Doing this maneuver puts people at risk of their heart stopping and strokes, so it is not a trivial part of the physical examination and yet, nobody has ever [seen it occur spontaneously]. Sir Thomas Lewis described the common faint we call vasovagal syncope.[55] He got these young guys to faint. If they had to have venipuncture, some of them would faint at the sight of their own blood. So anyway, my title is easy. It says – it refers to the King of Denmark and then I have a quotation – subsequently I have two literary citations. One, the title has to do with the King of Denmark, which I think is one of these here. And the other is – I found a quotation in the Sherlock Holmes story of the dog that didn't bark [VWH: Yes.].[56] The point is that four generations of neurologists have been concerned with this condition, presuming that it occurs spontaneously. None of them discovered that it doesn't bark – it [carotid sinus syncope] never has occurred spontaneously, which would be just a trivial bit of neglect except that the test is harmful. A certain number of patients got strokes; it stopped their hearts. It stopped the patients' hearts by doing that. Anyway, I knew what I was after and I had to explore to find the story – the Sherlock Holmes story.

(45:12)

VWH: I think they are great. I think it is in your book somewhere – there is a letter to someone about a Mark Twain quote – or a Lincoln that was misattributed. A Lincoln quote. You point out that –

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[54] William Landau delivered the 1993 H. Houston Merritt lecture at the 65th annual meeting of the American Academy of Neurology on the topic of, "The Practice of Clinical Neuroskepticism."

[55] Lewis T. *The Soldier's Heart and the Effort Syndrome.* 1918.

[56] A clue in the Sherlock Holmes mystery, "Silver Blaze," involved a dog that didn't bark in a situation when it would have been expected to do so.

WML: This just annoyed me. It had nothing to do with my specialty. You're right.

VWH: Barb, I interrupted you a minute ago.

BWS: Where were we with this? I was going back to talk about your work with Department, but I think we've covered that. Is there anything else you would like to say about your work with the Department?

WML: I think what you've brought out of me – if I had any philosophy as a Department head, it was similar to that with my children. They've [his children] all done well. Only one became a physician but he went the other way as a gastroenterologist. [laughs]

BWS: It sounds as if you provided an atmosphere in which people could develop.

WML: If you don't let them go their own way, you won't succeed anyway. [BWS: Right] But my recollection about Erwin – now there were several people that got hired away against my will like Bill [William] Powers – he was a great guy to lose. I didn't want to lose him.

VWH: Now he is Chair at the University of North Carolina.

BWS: Were there Chairs that came out of your Department – Chairs at other places?

VWH: Bill Powers is one that I can think of. I don't know who else.

BWS: You did start to see this - ?

WML I'm blocking for the name of a guy – he was interested in epilepsy and when I told him I wanted him to stay, he went into tears. I can remember this. And he died of a coronary within a year of where he went.

VWH: That was Eric [W.] Lothman.[57] He was a fantastic -

WML: He was a beautiful person.

VWH: He was a beautiful person.

WML: I remember telling him how much I wanted him to stay and I literally drove him to tears. Where did he go?

VWH: He went to the University of Virginia with Fred [G. Frederick] Wooten.

WML: That's right – Wooten took him on. I don't see Fred. Fred is still around. Fred was here, of course. Fred was already a pretty senior person when he came here.

VWH: I think he came here as an Assistant Professor from the Cornell program. And Bob [Robert C.] Collins must have come about the same time.

WML: Collins is a peculiar – you know, after he became Department Head in LA [UCLA], he never published anything again.

VWH I heard – I think it was he who said, when he became Chair, someone asked him if he missed doing research and he said no, he never wanted to see another rat again.

BWS: We're getting at time. Is there anything else you want to cover?

WML: You can stay until one.

VWH: We should talk about the ANA and the AAN.

[57] See: Wooten, GF. Eric W. Lothman (1947-1955). *Neurology* 1995:45:1625. doi: 10.1212/WNL.45.8.1625

BWS: That is where we need to go.

WML: The AAN – now I was never really actively engaged with the AAN.

VWH: I think you were on the Executive –

WML: I was –

VWH: You were on their Board for a while.

WML: I was on their Board but I don't recall any activity except going to meetings. I submitted one paper to [Russell N.] DeJong for the first [issue or volume of *Neurology*]– – and he lost it. [laughter] So you'll find it in Volume Two.[58] He was just getting organized. I remember his apology because he lost a manuscript somewhere. But I've – I think *Neurology* has been our best journal. To be honest, I think the *Annals* [*of Neurology*] is a lousy journal. It is too technical and too useless. You are close to [Stephen L.] Hauser [chair, department of neurology, UCSF and editor, *Annals of Neurology*]. I have nothing against him but I just don't think the journal – it deals with obscurities. He is in favor of obscurities and against the neurologic examination. But he's a nice fellow.

VWH: The citation indices for both journals are good.

WML: I understand, but I think there is little in the *Annals* that interests me. There are obscure chemical things about obscure diseases. In any case, I'm not turned on by it as much as I am – *Neurology* is expanding – it is now three journals. [VWH: Yes.] They are not so good either but they publish everything. I think [Robert A.] Gross is doing – I like what Gross is doing [as editor of *Neurology*]. And he didn't take my paper either, but I didn't mind his not taking it. The one I was talking about with carotid sinus syncope. I tried to put that together with another one I am working one – which relates to – I think it's one I've got there which I'm working on – which is

[58] Landau WM. The essential mechanism in myotonia: an electromyographic study. *Neurology* 1952;2:369-388.

how to deal with a patient who falls. It is a generic paper I'm writing. It's in the third draft now. The question is how do you deal with syncope when you are the consulting doctor?

(50:56)

WML: That's in the works. You are seeing my literary product online here. I had got together about the fact that carotid sinus syncope is over-diagnosed and shouldn't be – and the concept of how you approach a new patient with syncope. And I ended up deciding – having corresponded with – I don't know whether he'll accept it, but I hope this may be accepted as how to deal with a patient who presents and says, " I fainted." How do you work it up? The best I could do was to end up quoting Sir Thomas Lewis because nobody else has ever really described exactly how these patients behave. What I'm ending up with is pages of quotation. Now this paper about – you asked about quotations. Take a look at this one about spinal cord injury.

BWS: Spinal cord trauma. [WML: Yes.] [brief discussion about articles]

WML: The point about the paper [aside comments] that's already in press about dorsal rhizotomy.[59] That's almost entirely quotations from a guy that gave this talk in 1904 because I couldn't – it's a history of this procedure of cutting the dorsal roots to cure spasticity.[60] It ended up bringing this up-to-date from 1905. There had been a lot of studies of it – all negative. It is such a popular procedure. [aside comment] Every research study has shown that it doesn't help. It will relieve the spasticity but it doesn't make them walk any better, you see. So that whole paper, I had a lot of quotations.[61] You see the small print. I know the editor [Roger Brumback]; he lives in Omaha.[62] He trained with Phil Dodge too. The small print paragraphs are all quotes. But I couldn't [just] tell the story. I decided you couldn't be critical about somebody else unless you quote them exactly. To just say this is straight critical writing, but the whole field – but the

[59] Landau, WM. "Rootless Century: Posterior Rhizotomy for Spastic Cerebral Palsy" in *J Child Neurol*, 28(1):7-12, 2013.
[60] Presumably Otfrid Foerster in 1913: Foerster O, *Surg Gyn Obst* 1913;5:463-474, reference number 2 in Landau, 2013, ibid.
[61] Landau WM. Rootless century: posterior rhizotomy for spastic cerebral palsy. *J Child Neurol* 2013:28:7-12. doi: 10.1177/0883073812442588.
[62] Roger A. Brumback trained in pediatric neurology at Washington University followed by a residency in neuropathology. He founded and edited the *Journal of Child Neurology* and served as chair of the department of pathology at Creighton University. Dr. Brumback passed away in 2013.

quotations have to be there. Without quoting the people that have dealt with this operation without controls, I couldn't just say they didn't do the job right. I've got to say what they said.

VWH: So jumping ahead to the ANA - ?

WML: The ANA – I was the first person that had a period of service [as president] of more than a year because we moved from the spring – from the fall to the – which way did we move?

VWH: You went from the summer to the fall.

WML: Right, because we were conflicting with the Academy. We met in Washington and our major political problem was with Fred Plum who wanted to get away from JAMA.[63] [VWH: Yes.] To our dismay, it [*AMA Archives of General Neurology*]didn't – the journal didn't die. It never got good, but it didn't die. He was ready for the Board to let it die. That was one political issue. The other was the evolution that I had to deal with – was the certification of Child Neurology. Sam Trufant was my representative. I called him [our Henry Kissinger], our own Secretary of State.

(56:24)
WML: Sam was a dear friend, but I told him then, and since then, that he had been my Kissinger because the Pediatric Board didn't want this to happen. It was like bringing Nixon into China. We are still stuck with – I think we have too much training, but at least we have – it has become a very popular specialty and I am delighted with that. But the problem is these guys have grandchildren by the time they finish residency now [in child neurology]. In any case, that was our major political issue at the time [special qualification in child neurology] and Sam worked very hard in bringing it off for me. I mean I talked to him about it but he went to their meetings [of the American Board of Psychiatry and Neurology] and talked to their Board. So those are the two things I was involved with. During that period, the presidency changed every year, not every two years. I really had nothing to do with the ANA subsequently.

[63] Fred Plum was Chair of Neurology, Cornell Medical College and, at the time, editor-in-chief for the *AMA Archives of General Neurology*.

BWS: After that?

WML: Yes.

VWH: The ANA historically is the older organization, the academic organization –

WML: I didn't tell you how I became first a director and then the president of the Board. [VWH: Yes.] The way I became a director – I was at a meeting of the ANA and the lights were out. A good-looking man with a mustache put his hand on my thigh. I remember this vividly – it was the President of the organization at the time [A.] Earl Walker.[64] [aside comment] He asked me if I would be willing to represent the ANA on the American Board. I was nothing, I was just a neurologist.

VWH: This is A. Earl Walker, the neurosurgeon.

WML: The neurosurgeon. He also did some great work with the anatomy of the thalamus.[65] He was a classical good scientist – an American neurological neurosurgeon.

VWH: He later worked on brain death.

WML: Yes.

BWS: There was a lot going on with brain death?

WML: No, Walker didn't do that.

BWS: He didn't do brain death?

[64] A. Earl Walker was ANA president in 1966.
[65] Walter AE. *The Primate Thalamus.* University of Chicago Press, 1938.

VWH: I thought he had a book on it later in his career.[66]

WML: I didn't know about that because it was a British guy that did[67] – in any case, it was his [Walker's] hand on my thigh that got me to the eight years on the American Board of Psychiatry and Neurology. Would I serve? I said, "Yes, of course. Thank you for the honor." [VWH: okay.] That's how I got on. I didn't apply for that at all.

(59:30)
BWS: You joined the AAN in 1956, so you joined early, early on.

WML: Well, Dr. O'Leary was the father figure – one of the father figures. Everybody had to join. We had to do. It was the thing to do.

BWS: It was the thing to do. How did you hear about it?

WML: Everybody knew about it. We got it in the mail. When A.B. Baker started advertising, it wasn't subtle. [laughs]

VWH: What do you think about the role of the two organizations now?

WML: I'm concerned with [Eva Feldman] – the new president of the ANA who wants to organize the ANA as an official training entity for academic neurologists rather than an honorific agency. Now the problems are peculiar, I think. I mean, one was an academic – it was originally an academic organization which included basic scientists like my mentors. It included Harry Grundfest [at Columbia University] as well as George Bishop.

In any case, what she wants us to do is to meet with young academicians and counsel them and mentor them so that they'll succeed in academic neurology. This is her goal. I'm not opposed to

[66] Walker AE, *Cerebral Death*, Professional Information Library, Dallas, 1977.
[67] WML is referring to neurosurgeon Bryan Jennett who helped establish criteria for brain death determination.

this; I just don't think it will work. I think mentorship is something you don't do at a national meeting for an hour. [VWH: Right].

BWS: What about the educational role of the AAN?

WML: I think it's been overdone, but it is highly subscribed. You can't argue with what succeeds. The Academy meetings are so sub-specialized [BWS: Yes.] I'll turn this around. I thought that the old ANA meetings were organized so that you became educated across the board. The traditional mode of doing this was that all the papers – there were no posters. Posters were an innovation for the literate. It was presumed that you couldn't read or write. So if you were accepted on the program, you gave a paper. But before you gave the paper, you had to present a long abstract to a discussant who was pre – who was loaded in other words. If I were to give a talk about cerebral circulation, I would have identified somebody from the University of Pennsylvania whom I knew who was working in cerebral circulation to be the primary discussant. He would be prepared to know what I had done and to both praise it or condemn it because he knew it well. He knew it because he had read it before the exposure - which is different from the present system. In the first place [at Academy meetings], there is not enough time to discuss it. In the second place, nobody has advance knowledge of it ordinarily. In any case, I thought this used to work very well [at meetings of the American Neurological Association] because there would be one main primary discussant. Others could still – [VWH: Right] meet it from the floor. But it became a discussion that was productive rather than primarily destructive. Now the oral presentations are presented as a whole order and somebody would want to say, "You're great," or "You're not quite so great." We never get a negative discussion anymore.

VWH: Occasionally.

WML: Occasionally but the concept of peers contributing together [VWH: Yes.] and being prepared for it, we don't do anymore. The best discussions nowadays often happen next to a poster but nobody else hears it. The posters – they met a sociological need but scientifically I don't think they're worth a shit.

VWH: The old *Transactions* [*of the American Neurological* Association] would capture the discussion.

WML: Yes.

BWS: They don't publish anymore.

WML: That's right.

BWS: They are gone.

(1:05:00)
WML: In my own reprints, I kept one where I gave [Derek] Denny-Brown a hard time.[68] He forgave me because I had such fun doing it. I thought he had an artifact – not having your brain torn up. In any case, the Academy politically has been very good. If we are going to survive Obamacare, it will be because of the Academy's role. If it didn't do anything else, I'm in favor of that. I contribute to that – whatever our political branch is. I think we have to have somebody that speaks for us because, while – I think one of our worst political problems is the pain specialists. These alumni of Caribbean [international medical] schools that don't even know where the brain is. I think the pain specialists are a bane. What bane is to – .

BWS: What about the whole role – the Academy is huge –

WML: I understand. It has become very big.

BWS: Fifteen thousand [members].

[68] Derek Denny-Brown was at the Harvard Medical School and Boston City Hospital. Landau WM. Discussion of Denny-Brown DE, Gilman S, Van Der Meulen JV. Patterns of cortical ablations leading to dystonic postures. Trans Amer Neurol Assoc 89:117-121, 1964.

WML: I think we have to define an operational role of chronic care specialization rather than procedurism. The neurologist that makes his living from the MRI on the local mall has to be shot – or emasculated anyway. People [neurologists] all went into EMG because that's where the money was. I invented EMG here along after Ed [Edward H.] Lambert.

VWH: I have a question about that. I know your early basic research – some of which was EMG related – and your clinical work was in that. When I was in training you had written the chapter in Baker and Baker on electromyography.[69] Yours was one of the few training programs that did not have a mandatory rotation through an EMG lab. Maggie [Margaret] Clare, whom I meant to ask you about, was a non-physician [WML: She was a physical therapist.] who was running a lab on a day-to-day basis.

WML: I kept her doing it because she did it well and she knew how to do it. She wasn't a physician, which was an advantage because her reports were objective. I hadn't – the [EMG] lab hadn't been running for more than two years when – I remember the first time I was seeing a patient myself – a private patient – and I was thinking about getting a nerve conduction velocity. I knew perfectly well what was wrong with the patient – out of the right hemisphere. That [electrophysiology] is a profit center and I was a department head. So I was – as the administrator of the department, I recognized – I didn't keep our charges up to the community standard. They started to charge by how many limbs you stuck needles in. I recognized this as ardent quackery. There are very few patients even now where recording with needles is appropriate, where it helps you with the diagnosis or management.

VWH: Maggie Clare is someone who was a collaborator, I guess, of George Bishop for a long time.

WML: Yes, she was a physical therapist who joined him. She just died a few years ago. But she was a physical therapist and he first recruited her to work with him. I guess he wanted to see

[69] J.L. O'Leary, and W.M. Landau, , "Electroencephalography and electromyography" in A.B. Baker, ed., *Clinical Neurology*, 2nd ed. (San Francisco: Harper & Row, 1962):212-274.. The textbook was later released as A.B. Baker and L.H. Baker, eds, *Clinical Neurology.*

some Parkinson's patients and somehow – I forget how he got her associated with him. It is a good question. I don't know. But she was his fulltime research associate.

VWH: Including operative studies of the brain [unintelligible].

(1:09:19)
WML: Yes. Jackie [M.] Smith, whom I'm doing to dine with next week, was a similar associate of O'Leary's. She was and is a lovely lady now – in her late seventies – who was born in Algeria and spoke French. Grew up – anyway she ran the EEG lab for many years. [to VWH] Do you remember Jackie Smith? [VWH: I don't.] She ran the EEG lab and she then took over – when got our own electron microscopy, she ran it. There is a classic paper that I've been recently referring to where she and George Bishop measured axon diameter in a whole range of species from turtles to man.[70] What is interesting is that the most common diameter of axons from a marsupial to the frontal lobe of man is one micron. We don't – the conclusion really is a very important thing that Bishop suggested, which I just re-read and have now been arguing with – is that the reason axons are big is not because they have to conduct in a hurry – because the latency of going from here to your toe or vice versa, or from here to here in your brain – is measured in milliseconds. What is important is the diameter of the axon because the big axons have to carry a lot of garbage both ways. You know – particulate stuff and chemical stuff that goes up and down. It's a freight train. Bishop suggested this – that the reason that some axons are big is because if you have to conduct from here to there, you have to carry some nutrition and some chemical stimuli that go both ways. They don't get action potential; they flow in the water. So the speed is the difference between a one micron velocity from here to here and a [giant] Betz cell velocity is another millisecond. But when I wiggle my finger, it doesn't matter, you see. Anyway, this is an interesting thing because Bishop – you know he was originally involved with the fiber diameters in peripheral nerve. So we had an electron microscope so he spent two years looking at brain axons. Nobody else thought of it or wanted to do it. I didn't pay attention to what he wrote in the discussion myself until recently. I'm quoting him.

[70] G.H. Bishop, J.M. Smith. "The sizes of nerve fibers supplying cerebral cortex," *Exp. Neurol.* 9:483-501, 1964.

VWH: You also did some work where you are looking at optic nerve axon diameter.[71]

WML: Well, we use those now. Let me tell you something else he discovered that I'm trying to get published now. Somebody else did it. In this paper he points out that in all the areas of the brain he looked at, there was about the same distribution of axon diameter except in the auditory nerve. The auditory nerve – the axons were all within [2-3] microns – all about the same. Now if you also know – which is a fact – there is a noise level in the output from the cochlea in the normal anatomy. There is a little noise just recording from the axons. Well, if you appreciate that there is a noise level in this very sensitive organ called the cochlea and that all of the afferent axons have the same diameter, then you can visualize that their active foci where the action potentials are, are likely to bump into each other [this is ephaptic cross talk]. So you have a natural liability for tinnitus. See all of the axons have about the same statistical chance that a node will be next to another node – it's unique. He spent a paragraph in this 1969 paper saying the auditory nerve is peculiar. He didn't think of tinnitus or anything [clinical]. But this is a fascinating thing. I have a good friend [aside comment] who is a cochlear specialist. A Ph.D. here in the Central Institute [for the Deaf], by the way. I'm trying to get her to write a paper – a little paper with me for an ENT journal – just to point out that we're all stuck with this liability. The fact that our auditory nerve axons are the same diameter means that all of us have a risk factor for tinnitus that's built in. We don't care if the cats and guinea pigs have it, but we are stuck with it because they all have a uniformity which no other part of the nervous system - . You can see – in order to get accurate frequency determination, you can see why this might have evolved. Because if you want to detect frequency discrimination from the little part of the cochlea to the big, you don't want to get confused with conduction distance. Anyway –

(1:15:20)
BWS: We are getting close on time.

WML: What else do you want to ask me?

[71] For example: Bishop GH, Clare MH, Landau WM. Further analysis of fiber groups in the optic tract of the cat. *Exp Neurol* 1969;24:386-399.

BWS: Anything else about the AAN or the Academy?

VWH: I don't think we need to do that.

WML: I would say in regard to Academy meetings, I think some of the section meetings do have the kind of productive critical discussion that I would like, but the big sections at the Academy and the ANA, there is no interaction.

BWS: They've both gone that way?

WML: Yes, they are sort of honorary –

BWS: What you liked was the –

WML: I liked the criticism.

VWH: Thoughtful criticism.

BWS: Critical thinking, questioning.

WML: Gentle criticism. The older I've gotten, the more I've appreciated it being gentle. [laughs]

BWS: That's a good point. Anything else on that. On the AAN.

VWH: Maybe not.

BWS: We can do some wrap-up questions.

WML: In any case, I think the Academy serves different purposes from the ANA. [aside comments] There aren't that many females that are presidents in the ANA. Anyway, it is the academic, scientific apogee. That is what it is supposed to be. You are supposed to get it about

the time you get tenure. I think what she recognizes is that the attendance at the ANA has fallen off, and so what she is trying to do is to develop it as a training center for academics at the pre-tenure level. Maybe it will work. I don't know. I am just skeptical about it. It is a new idea and old people don't like new ideas.

BWS: Does the academic – does the demand for academic neurology continue or is it more difficult for people to [do the work]?

WML: There is a lot more support for fellowships and things than there used to be. [BWS: Okay.] There are many more avenues for research. [BWS: Or is the clinical –] What I don't like – let me express an important idea. Herbert Gasser, who shared the Nobel Prize [in Physiology or Medicine with Joseph Erlanger in 1944] – I never knew him because he had just left when I was a student here. He then became head of the Rockefeller Institute, which was a basic science research enterprise – many areas of research. But the Rockefeller Institute, up until World War II, was the NIH of all science – all biological science. They – if you look – when we needed a new professor of neuropsychiatry – the minutes of our Executive Faculty would say, "Dr. Graham talked to – got some money. The Rockefellers will give us the money." What he resented, he – Gasser, was the way research grants are now – and this relates to young people – nowadays, in order to get a research grant, you have to do the work first – which is dishonest. You are not even supposed to do the work on the grants time. You are supposed to think of the research while you are sitting on the private toilet and then you write it up and then you apply for the grant. In other words, if you haven't done the preliminary work, you don't get the money. What Gasser recognized was that good scientists just need to be let alone. Now this comes back to my philosophy. But his view was that in science administration, you should hire a good person and get out of his way. That he didn't have to write fourteen experiments ahead of time to get the money to do the next one. This is a fact now because, in tight money times, people find that they don't have – and department heads don't have the flexible money. Now when I was growing up, I never had my own research grant. This is important to document. O'Leary had some big grants and my monkeys were bought off of O'Leary's big grants. So the institutional grant is a much more important issue. The four-year fellowship is important because good science – you can't predict what is going to happen. I'm concerned in regard to administration. The most productive

training programs are those where the – nowadays the division head, [Joel] Perlmutter, has such a program where he has got some – he can have people apply for a fellowship to work in the movement disorder field but they don't have to do – they don't have to write it up ahead of time. He can negotiate with them about what they want to get into and then, if he wants, and I would hope, he lets them alone. He lets them – see, that's the issue. If there is one idea that I would lay out – I think we – a major problem for the progress of neuroscience in relation to clinical neurology is the rigidity of the support for people in the development phase. Partly it is due to the equipment too, but the salary is important – the guy's got children or she has – nowadays we have more women in medicine and in neurology and we're better for it. But how can we get support for her to develop new ideas by herself and still raise a family?

(1:22:19)
Now I'm going to tell you this. Perlmutter has a wonderful lady, a black lady in fact, who was with him and then she moved to Cleveland because her husband, who is also a physician, got a residency there. When he is working Grand Rounds, he puts her on and we can talk to her – back and forth. She has done some very interesting work in regard to the epidemiology of Parkinsonism. But partly because she is married and has a baby, but partly because he has got the support – he has got to try to keep her aboard so that when the husband has finished his fellowship, he is going to bring her back here because he wants her – she knows how to do epidemiology and she is a good doctor. He knows that he wants to let her develop. It is fun to see something like this, working uphill. But setting aside her marital combination, it's very difficult for a department head or a division head to provide enough freedom to younger people without telling them what to do. I think we still have too much rigidity in the grant application system.

VWH: I know they've made changes and continue to try to continue to make changes to try to compensate or overcome that because there is a recognition that not enough young scientists are getting – sustainable –.

WML: The big issue is if you want –

[General discussion and background noise.]

BWS: Is there anything else you want to mention?

WML: I have to sign the book here too.

BWS: Any advice you want to give anybody as you move along? Think about the future.

WML: No. Vote Democratic. [laughs]

VWH: You are in the wrong state for that, aren't you?

WML: We're going to win.

VWH: You're in a red state, aren't you?

WML What our Senator [from Missouri, Claire McCaskill], to whom I have contributed, has done – she advertised in favor of the guy who won the Republican primary for the Senate [Todd Akin]. The guy who is running for the Republican primary for the Senate had three opponents. She advertised in favor of him because he is an honest guy who is very reactionary and conservative, but he says what he thinks. [BWS: That's what counts.] She can run against him more comfortably than against the millionaire that he licked. It is a unique situation that is described in the New York *Times* today. The point is she put an ad in favor of her obvious opponent. She has no opposition on the Democratic ticket.

BWS: Anything else you want to say to young doctors.

WML: Stay in neurology.

VWH: Amen.

Dr. William Landau in the interview setting, August 10, 2012.

(L-R) Dr. Victor Henderson and Dr. William Landau, August 10, 2012.

(L-R) Dr. Victor Henderson and Dr. William Landau, August 10, 2012.

William M. Landau, M.D.

Date of Birth: October 10, 1924

Place of Birth: St. Louis, Missouri

Personal: Married to Roberta Hornbein - 4 children

Education: University of Chicago, 1941-43
Washington University, M.D., cum laude, 1947

Military Service:

Army Specialized Training Program 1944-46
Senior Assistant Surgeon, U.S. Public Health Service, Neurophysiologist, Research Branch, National Institutes of Mental Health and of Neurological Diseases and Blindness, 1952-54
Director, USPHS Reserve Corps, 1966

Graduate Training:

Rotating Internship, University of Chicago Clinics, 1947-48
Resident in Neurology, St. Louis City Hospital, 1948-49
National Paraplegia Foundation Fellow in Neurology, Washington University, 1949-51
Commonwealth Foundation Fellow in Neurology and Psychosomatic Medicine, Washington University, 1951-52
Commonwealth Foundation Traveling Senior Fellow (Resigned for Military Service), 1952-53
Diplomate in Neurology, American Board of Psychiatry and Neurology, 1957

Hospital Positions:

Neurologist, Barnes-Jewish and St. Louis Children's Hospitals, 1954-present
Neurologist-in-Chief, Barnes-Jewish and St. Louis Children's Hospitals, 1970-1991
President of Medical Staff, St. Louis Regional Hospital, 1987-1991

Scientific and Professional Societies:

Alpha Omega Alpha (Alumnus member)
American Association for the Advancement of Science
American Academy of Neurology (Fellow)
American Association of Neurological Surgeons (Associate member)
American Electroencephalographic Society
American Neurological Association
American Physiological Society
American Society for Clinical Investigation
Association for Research in Nervous and Mental Disease
Association of University Professors of Neurology
Central Electroencephalographic Society
Central Society for Neurological Research
National Committee for Research in Neurological and

Communicative Disorders (NCR)
St. Louis Medical Society
St. Louis Society for the Neurological Sciences
Society for Neuroscience
Sigma Xi

Honors:

Town and Country: Directory of Outstanding Medical Specialists in the United States, 1989
Woodward/White Inc: Best Doctors in America, 1992
Distinguished Faculty Award, Washington University School of Medicine, 1989
Honorary Membership, American Neurological Association, 1989
Honor Membership, St. Louis Metropolitan Medical Society, 1992
Alumni Faculty Award, Washington University School of Medicine, 1992
Alpha Omega Alpha Lecturer, Washington University, 1993
Netter Lecturer, American Academy of Neurology, 1993
Second Century Award, Washington University School of Medicine, 2001
Honorary Membership, American Academy of Neurology, 2003

Teaching and Research Positions:

Assistant in Physiology, Washington University, 1946-47
Senior Assistant Surgeon, U.S. Public Health Service, Neurophysiologist, Research Branch, National Institutes of Mental Health and of Neurological Diseases and Blindness, 1952-54
Assistant Professor of Neurology, Washington University, 1954-58
Research Associate, Central Institute for the Deaf, 1958
Associate Professor of Neurology, Washington University, 1958-63
Professor of Neurology, Washington University, 1963-present
Gast Professor fur Neurophysiologie und Neurologie, Ludwigs Maximilien Universitat Munchen, 1963-64
Head, Department of Neurology, Washington University School of Medicine, 1970-74
Co-Head, Department of Neurology and Neurological Surgery, Washington University School of Medicine, 1974-1991
Professor of Neurology, Washington University, 1991-present

Editorial, Governmental, and Advisory Positions:

Editorial Board, Neurology, 1963-73
Editorial Board, Archives of Neurology, 1965-76
Editorial Board, Annals of Neurology, 1977-79
Neurological Science Research Training Committee, NINDB, 1963-67
United Cerebral Palsy Research Advisory Committee, 1964-69
Executive Committee, Conference on Education in the Neurological Sciences, 1964-66
Board of Directors, American Board of Psychiatry and Neurology, Inc., 1967-75
President, American Board of Psychiatry and Neurology Inc., 1975
Council Member, American Neurological Association, 1975-76
Chairman, Membership Committee, American Neurological Association, 1974-76
Council Member, American Association of University Professors of Neurology, 1976-78
President, American Neurological Association, 1977
President, Association of University Professors of Neurology, 1978-80

Vice Chairman, National Committee for Research in Neurological and Communicative Disorders, 1978-80
Chairman, National Committee for Research in Neurological and Communicative Disorders, 1980-82
Honorary Member, American Neurological Association, 1989
Honorary Member, St. Louis Metropolitan Medical Society, 1993

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1975

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1978

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