Remembering Saul R. Korey, M.D.:
50 Years, a Lasting Legacy

Tuesday, November 12, 2013
Very few people at Einstein recall who Saul R. Korey, M.D., was. His time at Einstein was cut short, so it can be difficult to fully understand what he—the founder and namesake of our academic department of neurology—means to our institution.

This booklet, which is a companion piece to a special exhibit commemorating the legacy he has left us 50 years since his death on September 27, 1963, makes tangible things we say about people after they’ve gone—about their character and the quality of them as individuals. The contents herein represent the thoughts and feelings of those who knew, worked with, learned from and venerated Dr. Korey, as well as those who have come after and treasure his influence, still prevalent at Einstein today.

The truth is, in spite of his passing at the young age of 45, just eight years into Einstein’s establishment, Dr. Korey remains an integral part of our College of Medicine, its culture and its reputation as a leading research, clinical and teaching institution.

Dr. Korey was one of the founding visionaries of Albert Einstein College of Medicine, who set “translational research” in motion here long before the practice was acknowledged, or the term coined. A pioneer in his time, he created the model of interdisciplinary scholarship that has been the cornerstone of our institution since its inception. With his spirit of creativity and humanism, he embodied our current motto, “Science at the heart of medicine.”

I invite you to get to know Dr. Korey through the words of his colleagues, students and friends, and today’s beneficiaries of what he envisioned more than 50 years ago.

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I came to know Saul in the late ‘40s at Columbia Presbyterian. I would meet Saul and Murray Gluksman in the cafeteria at dinner time. They were fascinated to learn that I, an M.D., internist and hematologist, was working in a biochemistry lab supported by a postdoctoral fellowship and thought that was a good idea.

The opening of a new medical school in 1955 was a big moment. I had been recruited as chair of medicine and was determined to make the school a research medical school. I realized that I would be responsible for neurology, so I called Saul, who was in Cleveland at the time. He accepted immediately.

Saul and I saw eye to eye about making a research school and I remember that he said to me, “Harvard, move over.” Neurology was to start as a division of medicine with the commitment that it would become an independent department, which it did. My relations with Saul were always harmonious and we worked closely together on school affairs.

In 1958, Stanford tried to recruit me. When I visited, I realized that the scientific basis of Einstein was stronger than that of Stanford, so I didn’t go. Saul and I went to Dean Kogel shortly thereafter and told him we needed a new building to house multidisciplinary (cell biology, biochemistry, etc.) basic research in clinical departments. Kogel was very receptive and started fundraising within two weeks. Ullmann opened in the 1962-63 academic year.

When I learned about Saul’s illness I was devastated. I was on sabbatical in Paris at the time. When I visited him at Montefiore he was jaundiced and realized his prognosis was poor but said he wanted 10 more years. Harold Rifkin was his personal physician. I believe Saul’s diagnosis was delayed somewhat because he had a negative diagnostic colonoscopy, although I think he said there was extrinsic deformity of the colon.

We were both very active and collaborated in recruiting first-class faculty for other departments as well as for our own. After obtaining a grant on aging research, we were able to recruit Harry Eagle and Paul Gallup. And I recall that Saul, incredibly, wrote the multidisciplinary grant on diseases of the brain in a weekend and sent it out in a week. That grant was the foundation for the department of neuroscience at Einstein.

June 2011

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It is a great privilege for me to join in this tribute to Saul Korey, 47 years after his death. It gives me an opportunity to describe how much he influenced my life and how much he influenced the history of neurology.

Obituaries published soon after he died paid appropriate homage in a more formal way.\textsuperscript{1, 2, 3, 4} Those early obituaries painted a picture of a highly productive clinical scientist and his research, a man in a hurry. He reportedly slept five hours a night. His daytime activities were so packed it seems that he knew presciently that death would come prematurely. He died of pancreatic carcinoma after four months of symptoms. He was 45 years old.

Saul grew up in New York City, went to Cornell University from 1933 to 1935 and seems to have left without an undergraduate degree. He chose the University of Western Ontario for medical school because the accelerated program there promised an M.D. degree after the sophomore year. After an internship for two years, he enrolled in the Coast Guard for military service from 1942 to 1946. Patrolling the Atlantic Ocean in those days was serious business because of the hazards posed by hostile German submarines. He started training in neurology at the New York Neurological Institute in 1946 and became chief resident from 1947 to 1948; he served under both Tracy Putnam and Houston Merritt. From 1948 to 1951, he was a research fellow in biochemistry with David Nachmansohn at Columbia. Then he spent a fourth biochemical year with Nobelist Severo Ochoa at New York University. Few other neuroscientists of his age combined such sophisticated training and clinical work as well.

Korey himself was concerned about the problem faced by clinical investigators then and now—how to concentrate primarily on the laboratory and yet maintain clinical skills. Saul Korey asked two residents, Robert Fishman and me, to help him initiate this dual goal. Every Saturday morning we prepared patients for him to examine and discuss. So began my friendship with him when I started my residency in 1950, a friendship that had lasting consequences.

First, he asked me to proofread a monograph he wrote on pituitary tumors.\textsuperscript{5} That was done in the dining area of his apartment in the Bronx and under the supervision of his wife, Doris. The monograph was published in 1953, a year after he had been appointed associate professor of neurology and chief of the division of neurology at the Case Western Reserve School of Medicine. That arrangement did not last long. By 1955, he and Irving London had created the first faculty for Albert Einstein College of Medicine, propelling that new school immediately into the top ranks of American medical education. London, like Korey, had been trained in biochemistry and was an expert on hemoglobin.

Saul invited me to join his new faculty, which was a lure to other friends who had trained at the Neurological Institute. He asked me to be the administrator for a new grant he was expecting, which proved to be another historic first for him. He had put together a team of electron microscopists (then a new technique), led by Robert Terry. Another key appointment was Kunihiko Suzuki, a lipid biochemist. This multidisciplinary approach was applied to Tay-Sachs disease, a terrible familial brain disease of children. This novel concept for research on a human disease proved attractive to committees funding research supported by the National Institutes of Health. The multidisciplinary approach was then called a Clinical Research Center, now a “Program Project.”

In 1954, when Saul began recruiting faculty for Einstein, I was working in a junior position at Montefiore Hospital, which, at that time, was affiliated with Columbia University. All faculty appointments at Montefiore also carried appointments at Columbia. I had no training in biochemistry and only had vague ideas about doing research on Duchenne muscular dystrophy, a
lethal disease of young boys. Saul invited me to be the administrator of his program project. He said he would keep administrative tasks to a minimum. In return, he would teach me biochemistry.

These events took place long before the tools of molecular genetics could be applied to human disease. At that time, research on hereditary diseases had to incorporate biochemistry.

One Friday evening, I called my chief at Columbia, Houston Merritt, to ask him what he thought of this idea. In his typical nondirective psychotherapeutic approach, he would grunt in reply to any question I posed. His answers were neither “yes” nor “no.” I told him I had a deadline and would have to reply to Dr. Korey by the following Monday.

Monday came and I was about to accept Korey’s offer. I called Merritt to tell him what I was doing. He said, “No, don’t do that! I will be certain to match any offer he is making.” Befuddled, I later asked Merritt what made him change his mind.

He said that, at the time of our first conversation, he could not compete with Montefiore Neurology, a hospital service in his own department. By the following Monday, he said, he was free of that constraint because I had already decided to resign my Columbia position. Well, it all worked out for me and I spent four years in the biochemistry department at Columbia, working with Professor David Shemin there, which was sufficient preparation for me to join Robert Fishman in 1961 as co-directors of a Clinical Research Center at the Columbia Neurological Institute. Saul Korey and I continued to be good friends.

He had been pivotal in creating a new area of biomedical research, making a major contribution to human genetics by conceiving and leading multidisciplinary research and creating a major new medical school. Quite some achievements for anyone, especially one who died so young.

August 2010

Lewis P. Rowland, M.D.
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Former Director, the Neurological Institute of New York
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References

5 Korey S, Nurnberger JI. Pituitary Chromophobe Adenomas, 1953, Springer.

Those of us who were fortunate enough to experience Saul’s arrival as chair of the newly established division of neurology of the department of internal medicine were introduced to one of the most dynamic figures in the rapidly expanding area of clinical neurology and to Saul’s unique role in the emerging area of multidisciplinary neurobiological research that by the late 1980s became the nascent field of neuroscience. Saul’s research interests included not only his own lipid neurochemistry research, but extended to the emerging area of cognitive neuroscience. Saul believed that neurobiological research should include Freudian psychoanalysis, of the biological basis of which Saul was a firm supporter. Along with psychiatry chair Milt Rosenbaum and the prototypic psychobiologist Mort Reiser, Saul initiated the first interdisciplinary program in neurology and behavior, the so-called “ID Program.” Many ID fellows, including me, contributed to the foundation of modern neuroscience in their later careers. Saul’s clinical prowess was legendary, combining hands-on patient evaluation with an unshakeable belief in the potential of neurological therapies to combat even the most profound neurological damage due to stroke. He was an early advocate of anticoagulant therapy for ischemic stroke, and the application of hypothermia to treat brain swelling.

Saul Korey’s most remarkable characteristic was his extraordinary presence. Only a brief contact with him was required to sense the enormous vitality of his thinking, backed up with a seemingly endless reservoir of energy that drove even his most sweeping visions to reality. Saul was the main initiator and motive power behind the development of Einstein’s first new facility mainly devoted to research laboratories—the Ullmann building, whose octagonal design provided innovative laboratory configurations and challenged the navigation of its inner maze-like corridors. Despite its unusual configuration, the Ullman building provided a much-needed expansion of laboratory and departmental space.

My favorite recollection of Saul’s personal decision process was his inability to say “no” to proposals that were repeatedly delivered to him if they had underlying merit. Persistence and logical argument rarely failed to yield ultimate approval of meritorious proposals.

One of Saul’s creative enterprises as chair was his support of six-month research sabbaticals for his newly recruited faculty. In my case, this enabled me to devote a solid six months to set up an independent laboratory and to initiate my basic research on human visual electrophysiology with the support Professor Hans Lucas Teuber, the newly appointed chair of the department of psychology at MIT.

The last months of Saul’s brief 45 years were principally marked by extraordinary sadness and grief over his premature terminal illness and passing. Those of us who had the privilege of knowing and working with Saul are forever indebted to him and will never forget him.

IN MEMORY OF SAUL KOREY

November 2008

Herbert G. Vaughan, Jr., M.D.
First resident in Neurology at Einstein
Professor Emeritus, Dominick P. Purpura Department of Neuroscience
Professor Emeritus, The Saul R. Korey Department of Neurology

Bob Katzman and I met Labe on July 1, 1954, as fledgling junior residents on Ward 4E at the Neurological Institute of Columbia Presbyterian (Neuro). He was our senior resident and hands-on teacher and mentor. Labe was a Southerner all the way, born and educated in Memphis; after a year in the Navy he graduated with honors in 1945 from the University of North Carolina and Alpha Omega Alpha (AOA) in 1948 from the University of Tennessee Medical School. After a rotation internship and a year of psychiatric residency in Illinois, he returned south as Air Force chief of neuropsychiatry at Barksdale Hospital, in Louisiana, before moving permanently to New York for his residency in neurology. Labe was Dr. Korey’s first recruit from Neuro as director of neurology at the Bronx Municipal (Jacobi) Hospital. He was responsible for recruiting three other attendings from Neuro to Einstein: Bob Katzman (in 1957); me (in 1958); and Elliott Weitzman (in 1960), who is a pioneer in sleep medicine.

We all knew Labe as a brilliant neurologist whose main clinical and research interests were neuroimmunology and multiple sclerosis (MS). After serving as co-chair of neurology, Labe was appointed Einstein dean during the turbulent days of the Young Lords’ takeover of Lincoln Hospital and their severing of ties with Einstein. As dean, he strongly supported the unpopular view of medical schools’ training of primary care physicians with support from multidisciplinary colleagues in a “home” center. This model is now touted as a promising salvation for the practice of medicine in the United States. He applied it to the Einstein Comprehensive MS Center, where patients could be seen at one visit by any of several specialists and therapists they might need. He said forcefully that you can always improve the quality of life, even of patients for whom there is no cure for their illness, a care model now widely emulated in MS centers throughout the nation. Very active in the MS Society, he served as its president, and his many contributions to research on the immunology of MS and the care of patients were recognized by several awards and prizes. He became professor emeritus in 1995, but remained active in MS circles until his death at age 78.

Compiled by Isabelle Rapin, September 2013

Labe C. Scheinberg, M.D.
Clinical neurologist
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In July 1957, Saul R. Korey recruited Bob Katzman, the second graduate of the residency in neurology from the Neurological Institute (Neuro) of Columbia Presbyterian Medical Center (I was the third, in 1958), to his clinical faculty. Born in Colorado, Bob served in the Navy in World War II before enrolling at the University of Chicago, where he met his wife Nancy and received bachelor’s and master’s degrees in biochemistry. He graduated from Harvard Medical School in 1953, in the same class as Dominick P. Purpura with whom he was to cross paths again at both Neuro and Einstein.

He started research on electrolyte transport into the brain as a medical student, winning the Borden student research award in 1953. He served his internship at the Boston City Hospital, where he came under the neurologic influence of Maurice Viktor and Derek Denny-Brown. He and I were partners during the first month of our joint residency at Neuro. He told me that his career plan was to be appointed chair of neurology by age 40. He realized his plan a year early when he was appointed Dr. Korey’s successor in 1964, following Saul’s death the previous September.

Bob continued his research on brain swelling and cerebrospinal fluid. He also noted ventricular dilatation on brain imaging in both young and elderly patients with dementia, which led him to team up with Robert D. Terry to carry on Saul’s pioneering work on Alzheimer’s disease (AD). Epidemiologic work in a quasi-illiterate population of women in China suggested the concept of cognitive reserve, in which education may aid in postponing the onset of clinically evident dementia.

In 1976 Bob wrote his concept-altering editorial showing that pre-senile and senile dementias were the same disease process and that dementia was a major public health problem worldwide. For 10 years, it was the most cited paper in neurology. Prior to its publication, there were a total of 150 articles published on AD. Following the publication of the editorial through 2006, there were 45,000 articles on AD published.

Bob Katzman, Bob Terry and Leon Thal were recruited from Einstein to the University of California, San Diego, where they established one of the major centers of research on Alzheimer’s disease, with Bob Katzman as chair both of neuroscience and of one of the premier departments of neurology in the United States. Bob and his group received numerous prizes and awards for their work on aging. He was a member of the Council of the NIH Institute on Aging from 1982 to 1985, and was elected president of the American Neurologic Association in 1985.

Bob Katzman was the epitome of Saul’s fostering of clinician-scientists among neurologic faculty who also are teachers and leaders in their field.

Compiled by Isabelle Rapin, September 2013

In the spring of 1957, I was working in the organic chemicals department of the DuPont Co., where I had been since receiving my Ph.D. from Princeton in the late autumn of 1953. It was not where I had hoped to be. We were at war and I was 1A, but deferred until I graduated. I could have followed my thesis advisor to NIH, but I faced being drafted and sent to Korea, which seemed like a stupid waste of education. Fortunately, I had offers from companies who needed scientists and DuPont said it was working on crucial war-related projects, and promised I would get a deferment. I was switched from one project to another in my three-plus years. I had some competent group leaders and one dishonest one. I was not happy with them and they were not happy with me. I laced my weekly progress reports with harsh criticisms and snide comments. My last group leader was a chemical engineer who had never done laboratory research. On my last day, we had an argument and more or less simultaneously he said “You’re fired” and I said, “I quit.”

I had been reading extensively in the fairly new field of neurochemistry and had decided that’s what I should work in, but had no entrée. My wife-to-be, Lila Mazur, was from New York and knew Harold Strecker and his first wife. Strecker was a well-known biochemist who had worked at Columbia and recently moved to Einstein College of Medicine. He had also published several significant papers on neurochemical topics. I met Harold and he told me that Saul Korey, the head of neurology, was building a department and was looking for a basic scientist. I had an interview with Saul in June 1957 and was amazed that after we talked for an hour he said that I was hired and could start on July 1.

I was indeed grateful to Saul and was very happy to be back in academia. I could do what I wanted to do but first I had to learn something about the nervous system. Saul told me about his research and what he was interested in, but never implied that I should work directly for him; I should establish my own projects. When I arrived on July 1, 1957 only Saul and Labe Scheinberg and several technicians were working in the neurology wing of the first floor of Forchheimer. We had two big labs and one small one, a commercial walk-in refrigerator used as a cold room, a small office, where Labe and I had cubicles, Saul’s office and the secretaries’ office.

Saul and I shared one of the big labs. Saul transferred one of the technicians, Miriam Brotz, to my project. Miriam was a very able assistant. She could kill rats, fix tissue, cut frozen sections, etc. She was not trained in chemistry but once she learned a procedure she could reproduce it exactly every time. Saul was involved in the biochemistry of neurologic diseases. Many of these diseases, such as the gangliosidoses and multiple sclerosis, involved alterations in lipid chemistry. I also met Maurice Rapport at this time and heard about the unusual phospholipids called plasmalogens, which were in high concentration in brain. I decided to first focus on histochemistry of brain lipids, since that would complement Saul’s interests and I could also get advice from Maurice.

I was very pleasantly surprised that Saul, who had many clinical duties, also worked nearly every day in the lab performing experiments with his two technicians, Mildred Orchen and Arlene Stein. Saul’s demeanor encouraged a collegial atmosphere. We were comfortable and good humored but diligent and focused. Saul, however, did not suffer fools gladly. He was in the middle of an experiment when the rabbi at the cafeteria called (not Lubin, but the first cafeteria on Morris Park Ave.). Apparently someone in neurology had ordered a sandwich that contained both meat and cheese and a non-Jewish man at the counter had actually made it. The rabbi wanted Saul to find this customer and determine whether the sandwich was cut; if so all the knives would have to be purified. Saul couldn’t contain his anger and chewed this man out and, in unprintable words, said he had better things to do with his time.
I also heard Saul berating one of the junior neurosurgeons for screwing up an operation.

Among the most innovative and useful of Saul’s ideas was to establish the Interdisciplinary (ID) Program, which was in operation when I arrived. Many of the junior members of the various departments who were doing research on the nervous system were members of this program. We met frequently and got to know one another and their research projects. The ID program also brought in outside speakers. Within a year or two under Saul’s tutelage, I realized I had become a neurochemist as well as an organic chemist.

A couple of anecdotes stick in my mind… One day Saul and I were looking at a slide of a brain that I had stained. There was a large neuron with many processes in the center of the field. Saul said something about the axon, and I said, “That’s not the axon, that’s the apical dendrite.” As soon as I said it I thought what am I doing? I’m contradicting the chair of neurology and I never even had a course in biology, let alone neurology or histology. Saul took another look and with a laugh said, “You’re right.” I was embarrassed but he wasn’t. Maybe he was testing me?

In the early summer of 1958, Saul told me that we had to write a grant. The deadline was about two to three weeks away. We divided up the chore; I wrote part and he wrote part. As soon as we had a few pages we would give them to Nancy Weiss to type. In a week we had a finished application and mailed it. In August, Saul went to Woods Hole and I went to Maine. When I returned after a few weeks Carol Buschke, one of our secretaries, told me Saul and I had missed a site visit. While we were away the executive secretary of the study section sent us a letter saying they were coming on a certain day and if that was not convenient we should let them know. They got no answer so they arrived. Surprised that we weren’t there, they went to see Abe White, chair of biochemistry, because I had a joint appointment in that department. Abe White was not very helpful because he didn’t know I had written a grant. So they left for their next appointment. We got the grant and I had it for 37 years even though in the first three years I worked on something completely different from what I had proposed in the application.

One of my last memories is of May 1963. Saul and I were going to march at graduation. We were putting on our regalia and Saul was complaining about pain. I asked him what was wrong and he said it was either nothing important or it was pancreatic cancer. I was shocked but didn’t question him, but I have often wondered whether he had already received the diagnosis.

I have had 55 years to ponder what my life would have been like if I had gone to NIH in 1953 and not to DuPont, or if I had stayed at DuPont. But I have had 51 years to realize how much I owe Saul and that, because of him, I have had a great career and one that I have enjoyed immensely.

November 2008

William T. Norton, PhD
Professor Emeritus, the Saul R. Korey Department of Neurology
Professor Emeritus, Dominick P. Purpura Department of Neuroscience

I obtained my Ph.D. in clinical psychology from Teachers College, Columbia University, in 1957. I was not enthusiastic about a career in psychotherapy or diagnostics with psychiatric patients, but had enjoyed my rotation through neurology during my internship at the Bronx VA Medical Center. On the day I deposited copies of my doctoral dissertation with my mentor, Dr. Joel Davitz, he told me his cousin, Dr. Robert Katzman, had indicated that there was a fellowship open in the NIMH Interdisciplinary Program in the Nervous System in the department of neurology at Albert Einstein College of Medicine.

I first met Saul Korey when I interviewed for the post. He impressed me with his intelligence, energy, enthusiasm and charisma. I eagerly accepted the fellowship when it was offered.

I was not disappointed. Part of Saul’s genius was identifying and attracting people committed to a career in multidisciplinary research. I soon met Herbert Vaughan, Isabelle Rapin and Herman Buschke, all of whom were vitally interested in the interface between the nervous system and behavior. Vaughan and I established a laboratory on ward 4 West at Jacobi Hospital where we embarked on research in what was just coming to be known as clinical neuropsychology. We studied the organization of sensory, motor and cognitive functions in patients with diffuse and localized neurological disorders. The interdisciplinary program brought in a series of guest lecturers with whom we could share our findings.

Vaughan started an evoked potential laboratory where we explored the relationship between behavior and electrophysiological events in normal subjects. The advice and stimulation provided by Josiah Macy, Jr., and Herbert Shimmel were invaluable in this work. I joined Isabelle Rapin in her research program on children with language disorders, wedding neurological and experimental psychological approaches to the understanding of their behavior. Neuropsychology is a multidisciplinary endeavor. I joined with others in the establishment of the International Neuropsychological Society, open to members of all disciplines, which now has almost 5,000 members.

Saul died in 1963, and his loss was a blow to all of us, but he had been with us long enough to strongly influence our professional development. His desire to see the whole picture became fused in my mind with the realization that neurological disorders have profound social consequences.

Consequently, I was pleased to foster the development of the University of Victoria’s Centre on Aging, which brings together social scientists, lifespan developmental psychologists, neuropsychologists and physicians. The centre is now directed by my former student Professor Holly Tuokko.

For me, a further confirmation of the validity of Saul Korey’s approach to research involved my participation in the Canadian Study of Health and Aging. It is a nationwide study involving 18 university centres and 52 senior investigators, including neurologists, neuropsychologists, gerontologists, geneticists, epidemiologists and sociologists investigating the incidence, prevalence, risk factors, care-giver burdens and course of dementia in an aged population of over 5,000 Canadians. Clearly Saul Korey’s conception of scientists of many disciplines working together to increase our understanding of the nature and consequences of neurological disorders has come to fruition.

July 2010

Louis Costa, Ph.D.
Professor Emeritus, University of Victoria
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The first time I met Saul Korey and went to Einstein was on a miserably cold, wet day in February 1958. Little did I know what a momentous day that was to turn out to be...Two of my former co-residents had moved to Einstein straight out of residency at the Neurological Institute of Columbia Presbyterian (Neuro); Labe Scheinberg in 1956 to run the Jacobi Neurology Service for Dr. Korey; Bob Katzman, my partner in the first months of our residency in 1957. I had decided on neurology for children as a medical student, had completed my neurology residency and in 1958 was spending an extra year at Neuro, which included child neurology under Dr. Sidney Carter and EEG under Dr. Eli Goldensohn. I was supported by what was probably the first NINDB fellowship training program in child neurology in the country. Saul was to write a later one that supported child neurology at Einstein from 1960 to 1972.

Labe had casually invited me to visit Einstein, and I was so naive that I had no inkling I would be interviewed for a job as child neurologist at Einstein. As a result I was almost tongue-tied when Dr. Korey sat me down and asked me what I would like to do. He didn’t ask me whether I planned to do research but what type of research I intended to do. Fortunately, he invited me for a second visit, when he asked me whether I would like to work in neuropathology with Bob Terry, which I didn’t realize at the time would have been a tremendous opportunity that I regret missing to this day. I explained I was already doing some research on delayed auditory feedback and its effects on programming of speech and rhythmic key tapping with a gifted P&S medical student, Richard Alan Chase who, believe it or not, had his own NIH research grant in the ENT department at Columbia P&S! Ah, those days of easy research support...I accepted Dr. Korey’s offer and came to Einstein after Labor Day in September 1958. I didn’t enquire how much I would be paid (it was $7,000.00/year, which seemed a lot to me). I don’t remember signing a contract or undergoing a physical. Dr. Korey must have made some phone calls to Neuro about me, but that was it. The blessed simplicity of those days...

Saul admired Labe’s clinical talent tremendously, but he kept a close view himself of what was going on in Jacobi Hospital. He impressed on all of us green attendings that patient care came first with him and that we should never hesitate to come over to his lab when we needed his advice with a problem case. All the neurologists, residents and faculty alike, trouped to the Jacobi morgue every Wednesday morning for brain cutting, run by Dr. Robert Terry, and invariably attended by Saul; often Dr. Davidoff, chair of neurosurgery; Dr. Alfred Angrist, chair of pathology; and Dr. Milton Elkin, chair of radiology. It was a critically useful teaching session, in those days the final venue to inform clinicians about the accuracy of their skills.

Saul was to be found every afternoon in his lab, having met with the residents and tended to departmental and school affairs earlier in the day. The door to Saul’s lab was invariably open. The neuroscience enclave at the east end of the first floor of the Forchheimer Building included Saul’s neurochemistry lab with his technicians and other assistants, including Jackie Gonatas, Bill Norton’s myelin lab, and Labe Scheinberg’s neuroimmunology lab. Bob Katzman’s lab, where he studied brain electrolytes in astrocytes, was originally next to the Jacobi EEG lab, but later moved to a ground floor lab in Forchheimer. Dr. Leo Davidoff, an authority on brain tumors, was next door to Saul. Neuropathology (Drs. Nick Gonatas, who actually wooed Jackie in the enclave, and Robert Terry) had research labs across the hall. Terry and Gonatas collaborated with Dr. Korey on his studies of Tay-Sachs disease and, soon, Alzheimer’s disease. Led by Dr. Korey, who believed that micro neurochemical methodology had progressed
to the point where it would be informed by electronmicroscopy and histochemistry, the entire group made common frontal attacks on human disease.

Saul and Labe Scheinberg had already, in 1956, attracted a talented resident in neurology, Dr. Herbert Vaughan, who, as I recall, had served his medical internship at Cornell and had spent undergraduate time at McGill, where he had come under the influence of the neuropsychologist/neuroscientist Donald Hebb and probably also Wilder Penfield, the pioneer in epilepsy surgery. I had met Herb when he rotated through Babies Hospital of Columbia Presbyterian for the requisite training in child neurology under my mentor, Dr. Sidney Carter.

Einstein urgently needed a card-carrying child neurologist to train the residents who followed Herb. I was lucky to be the only one in NY just graduating from training.

The first Einstein fellow in child neurology, Dr. Larry Schneck, a former Downstate student and pediatric resident, had bravely started in 1957, before Einstein had a formal child neurology accredited training program. Larry commuted from Brooklyn every day, plowing through the multivolume Handbook of Neurophysiology on his hour-long subway ride. He had served his year of adult neurology by the time I came on the scene. He alone took care of the pediatric in-patients, emergency room and clinic outpatients.

In Saul’s eyes, neurology, not the patient’s age, was the issue in training child and adult neurologists. His view has prevailed so that Einstein-trained child neurologists view themselves as neurologists for children with a strong grounding in the neurosciences, not as pediatricians trained in neurology. In the early years some child neurology graduates carried out the Korey model and populated the faculties of other schools as distinguished clinician/investigators, among them Lenny Graziani at Jefferson, Jerry Ehrenberg in Cleveland, Gerry Golden in Galveston, Ben Shaywitz at Yale, Sakku Naidu at Loyola, then Hopkins, David Holtzman at Stanford, then Harvard, Roberto Tuchman at Miami...Most have gone into practice, often after an additional year of subspecialty fellowship, in EEG in particular. Almost all are competent practitioners and educators, as are many more adult neurology residents. A number have stayed to serve on the Einstein faculty. A few have gone to the pharmaceutical industry or become biomedical entrepreneurs.

When I came to Einstein in 1958 I shared responsibility for inpatient child neurology with Dr. Lawrence T. Taft, a child neurologist who had spent a year in child neurology at Boston Children’s Hospital under Drs. Bronson Crothers and Randolph K. Byers, whose main research interests were mental retardation and cerebral palsy. Larry Taft and I each took the Jacobi child neurology service for six months and ran the child neurology clinic together year-round. I took service for six months in adult neurology as well, often making the two services run concurrently so I would have some unencumbered time for research. The child neurology service was often light, whereas adult neurology occupied two wards in Jacobi, 4 West for women, 3 West for men. Larry Taft’s chief responsibility, for which he was uniquely qualified, was the Children’s Evaluation and Rehabilitation Clinic (now Center), which still provides multidisciplinary care to children with static encephalopathies and handicaps of all kinds and which enriches the training of developmental pediatricians and child neurology fellows.

Saul Korey’s neurology department was an incredibly exciting place to be. Virtually all the original faculty and residents he attracted went on to distinguished careers in academia or, if they had no interest in research, were helped to get a good clinical job. Labe Scheinberg’s clinical and research interest in neuroimmunology resulted in his organizing a multidisciplinary multiple sclerosis

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1 See Dr. Terry’s account on how they chose these two diseases.
research and treatment center. The Einstein MS Center provided basic scientists like Cedric Raine and his group with blood, CSF and even brain tissue for study. Stuart Cook, Peter Dowling and John Prineas (the latter from Australia) all joined this effort and brought it to other medical schools. Labe’s attitude was that you could always find something to help patients with chronic diseases, an attitude that spilled over to other chronic diseases. Several former Einstein neurology residents started neurology departments in other medical schools.

Saul and Bob Terry’s fundamental studies on Alzheimer’s disease in the late ‘50s-early ‘60s led to the inescapable conclusion that dementia in the aged is a disease, not the result of inevitable wear and tear, which is now the prevailing view worldwide. Bob Katzman’s influential editorial of 1976 emphasizing the high prevalence and malignancy of dementia, and his studies in China in the mid-‘80s that showed that literacy is a protective factor because it contributes to cognitive reserve in the elderly, are the direct consequences of the Korey/Terry collaboration.

Saul was not locked into a purely biologic model of disease. Before I came he had already hired a young Ph.D. neuropsychologist, Louis Costa, to study empirically the behavioral consequences of focal and diffuse brain pathology. Lou and I went on to collaborate on a number of studies of developmental disabilities of children. Herb Vaughan spent six months of fellowship with Dr. Hans-Lukas Teuber at MIT, whose rigorous cognitive studies of veterans with brain wounds informed modern views on the neurologic basis of behavior. Herb went on to apply the tools of neurophysiology to this endeavor and trained generations of neurologists and basic scientists in the use of computers to study electrophysiologic correlates of sensory perception and language. He was appointed director of our Kennedy Center after its founder, the pediatrician Dr. Harry Gordon, retired. Herman Buschke, while still a resident in neurology in the Korey days, developed the Selective Reminding Test as a tool for probing the stages of memorization. He, Lou Costa and later Steve Mattis and a series of neuropsychologists were to jumpstart research on the disorders of memory in the aged and in children with developmental disabilities.

In the 1950s, together with Dr. Milton Rosenbaum in psychiatry, Dr. Leo Davidoff in neurosurgery, Dr. Harry Zimmerman in neuropathology and basic scientists in neuropharmacology, genetics, cell biology and other disciplines, Saul had written an interdisciplinary (ID) grant that supported the training of generations of fledgling neurologists, psychiatrists and neuroscientists. I was supported by the ID grant when I first came to Einstein, as were Lou Costa, a psychologist, and Kuni Suzuki, who worked simultaneously as neurology resident and neurochemist before moving full time into the Korey lab. Kuni became a world leader in molecular neurochemistry, focusing on the glycolipids and their diseases. Stan Samuels, Bob Ledeen and members of other departments involved in neuroscience research all started as ID fellows. Doris Allen, Ed.D., a developmental psycholinguist, came as an ID fellow in the ‘70s, took over the therapeutic nursery from psychoanalysts and, in collaboration with me, turned it into a preschool for children with communication and autistic spectrum disorders and into a pioneering hotbed of autism research.

The ID grant supported a series of exciting seminars and lectures, which we clinical neurologists were privileged to attend together with the neuroscientists. Prominent investigators from around the world accepted invitations to Einstein to present their newest research. I remember vividly a lecture by a Swedish psychiatrist/epidemiologist who showed around 1960 that schizophrenia was strongly genetically influenced, at a time when psychoanalysis still prevailed in psychiatry. Dr. Christian de Duve from Belgium, future Nobelist, discussed lysosomes and peroxisomes heatedly with Einstein’s Dr. Alex Novikoff. There were other heated arguments, for example on the size of the extracellular space in the brain; details of
the molecular ratios of the various myelin lipids in the membrano-cytoplasmic bodies of Tay-Sachs disease; and on and on. This grant survived Saul’s death by several decades and contributed to the launching of the careers of dozens of talented young scientists and clinician/scientists, many of whom laid the ground for the birth of neuroscience as a discipline.

The most exciting multidisciplinary seminar I called, which no doubt would have excited Saul as well, was the one in about 1982 in which we witnessed the birth of the peroxisomal neurologic diseases. It grew out of cumulative insights provided by the successive reports of clinicians and clinical geneticists, liver pathologists, neuropathologists, mitochondrial specialists, neurochemists, immunocytochemists and others. It was there Dr. Sidney Goldfisher related his inability to detect a single peroxisome in the liver of a baby with Zellweger disease I had cared for. I remember his comments on unique features of the botfly in the anoxic horse’s stomach, but not their relevance to the peroxisome. Others described glycolytic and mitochondrial abnormalities in the baby’s cerebral cortex. To this day, following the Korey tenet that investigating unexplained disease advances biology, I (“the gadfly”) have continued to challenge basic scientists at Einstein and elsewhere to investigate rare genetic diseases I was privileged to see clinically. The payoff has been the discovery of several new diseases or genetic variants thanks to these rare hitherto undiagnosed patients, among them lysosomal diseases attributable to the lack of activator proteins, the sialidoses, long chain fatty acids in peroxisomal diseases, and the giant mitochondria diagnostic of Canavan disease.

A vivid example of Saul’s unwavering scientific war on human disease was his decision to engage the collaboration in the research of the parents of the many Jewish infants in whom we diagnosed Tay-Sachs clinically during the ‘50s. We had nothing to offer the babies but care while they slowly died over two to three years, often in the Tay-Sachs ward of the Jewish Chronic Disease Hospital (now Brookdale) in Brooklyn. Our former fellow Larry Schnneck, then at Downstate Medical School, took care of these doomed babies, and the neuropathologist Dr. Bruno Volk studied their brains.
post-mortem. Saul explained to the parents that fresh tissue was required if he and his team were to discover the chemical basis of the disease that was killing their children. Several parents consented and the children were transferred to Jacobi, where I examined them. Brain biopsy was a big deal in those days when it was the only way to arrive at a diagnosis in some exceptionally baffling cases. And here we were proposing to take out a gram of non-eloquent brain tissue not for diagnosis but purely for research. I still salute these brave and desperate parents who generously volunteered their children as gifts to others. Dr. Davidoff himself did the surgery with no complication. Dr. Terry and Dr. Korey and their technicians were ready to receive the precious tissue in the operating room to make sure that it was processed correctly, without wasting so much as a millgram. From these studies came the revolutionary insight that the inclusions in cortical pyramidal neurons were multilamellar, biochemically complex organelles, not simple accumulations of undegraded ganglioside. Thus the lysosomal enzymatic deficiency did not act as a simple dam but subverted the cell’s genetically controlled developmental program. Dr. Terry’s electronmicroscopic photographs of these membrano-cytoplasmic bodies (MCBs) were so beautiful that they hung in the Museum of Modern Art.

The revolutionary insights MCBs provided jump-started microchemical studies that would no doubt have led the Korey group to the discovery of the missing enzyme had he not died in 1963. The neuroscientist Steven Walkley, D.V.M., Ph.D., and his collaborators are still working on Tay-Sachs and related diseases at Einstein, including on new potential treatment options for lysosomal diseases, spurred in part by immunocytochemical findings and by Dom Purpura’s camera lucida India ink drawings of distorted neurons stuffed with MCBs that hang in the Kennedy Center conference room. I would need a book to list the details of what I know about Saul’s sustained influence on our neurology department, on Einstein, and on the neuroscience of disease. And of course what I know is but a small portion of the whole...

After Saul died not one of us resigned from the department. (There was one neurologist Saul’s had just recruited who got cold feet and reneged on the appointment, which shows that he had not had the unique experience of working in Saul’s department.) The original nucleus of Saul’s faculty stayed together and continued to work at Einstein for close to 20 years after his death. We were as universally loyal to him as he was to us. We knew that, without a doubt, he would stand up for us and protect us if we needed it, and that if he had a criticism to make, it would be delivered in private. He was uncompromisingly honest, without quibble or shading in negotiations or in science. Many in the school found him intimidating; the members of his department revered him.

Saul was extraordinarily generous with help to his team. He insisted on reading himself every paper that emanated from junior members of the department before it was sent out. I remember his shaking his head at my fledgling prose and telling me that no matter how technical the data, scientific papers should be written so any intelligent person could understand them. I even remember his saying to me that the purple prose and many adjectives loved by his teenage daughter do not belong in scientific writing. He cared so much about the quality of grant and paper writing that he hired a departmental editor, Ms. Bette Pesetsky, who later became our administrator and...
is now a well-published writer of fiction. He took the time to discuss our work with us. He advised Dr. Elliot Weitzman, another Neuro resident he recruited to Einstein two years after me, that the pain research he had in mind was unduly cruel to monkeys. Elliott listened, decided to study sleep instead and became a pioneer in organizing a sleep lab, in which he discovered one of the most common sleep disorders, obstructive sleep apnea.

When I visited Saul in the hospital in August 1963 and he found out I was pregnant for the fourth time, with Peter Roy—named in memory of Saul Roy (Bob Terry’s son of about the same age is Nicholas Saul)—he shook his head and wondered how I would manage. The credit goes to my supportive husband, Harold Oaklander, who made it all happen. Saul supported women’s rights to be themselves. He would have been proud of his widowed wife Doris supporting their family by earning a master’s degree in social work and continuing into her 70s as a guidance counselor in the New York City public schools—she even helped my son Stephen’s dyslexic stepson get through middle school. Saul would have supported his daughter Cathie working as a labor organizer and later going to Einstein and becoming a first-class internist—first at an Indian reservation in Arizona, now in Massachusetts—to support her two children; and his other daughter Suzie, an artist, also working as an educator and administrator, both strengths of Saul’s. And of course Barry, who lamentably died of melanoma in his 50s, also an Einstein graduate, a superlative psychiatrist firmly based in the nervous system but also espousing the behavioral side of Saul’s interests and humanity.

Saul, you were and still are our hero and our leader. You showed us the way, and those of us whom you chose as members of your department are grateful to you to this day. You had no inkling of the breadth of your influence on us, on Einstein, on neuroscience and on science. All we can do is say thank you...

July 2009

Isabelle Rapin, M.D.
Professor, the Saul R. Korey Department of Neurology
Professor, Department of Pediatrics

Having been in academic medicine for more than 40 years, I can unequivocally say that Saul was the best chair I’ve ever worked with. Robert Katzman, one of Saul’s trainees, would be next. Saul was deeply involved with the clinical service and its problems. Unlike those chairmen who are trying to make it as “investigators,” Saul’s mind was on clinical problems and how they could more ideally be dealt with. He was concerned with his faculty and how he could best help them advance and work contentedly. He loved to teach, and a listener could almost hear his mind shifting gears as novel ideas flowed through it. He’d often stop, as if suddenly something new had struck him. Clearly, research was his greatest interest. But it was primarily clinical problems—neurologic diseases—that he wanted to solve. Finally, Saul was an excellent judge of people. He had, after all, chosen Herb Vaughan, Labe Scheinberg, Bob Katzman, Bill Norton, Isabelle Rapin, Bob Ledeen and me for his faculty.

Saul recruited me to the pathology department at Einstein in 1959. I guess that he might well have felt that he needed an electron microscopist to flesh out an almost entirely chemically oriented research group. Within weeks of my arrival from Montefiore, Saul and I began to think of diseases to research as a team effort. I’m not sure that there were such teams in academic neurology at that time. We sought our possible targets not in the research literature, but rather in the index of Houston Merritt’s neurology textbook! We were looking for one or two disorders that offered combined opportunities (i.e., questions) in chemistry and morphology. We came to Alzheimer’s disease and Tay-Sachs disease (TSD) in the alphabetic search. In neither area was there evidence of significant planned work elsewhere. There seemed to be no real competition.

I believe that I had brought with me from Montefiore a small N.I.L.I. grant, which mentioned Alzheimer’s disease as a sort of minor fishing expedition using electron microscopy. It turned out that it was the first grant application on Alzheimer’s disease that the N.I.H. had ever received! Saul and I set about enlarging the program, planning brain biopsies (of less than one gram) to be used for chemistry, electron microscopy and pathology. He, of course, had been well trained in neurochemistry, as it was known at that time. He did amino acid and lipid analyses, as well as Warburg respirometry; I did the EM and the neuropathology. We started with Tay-Sachs patients because there seemed to be so many of them in the New York area, and many of the parents were eager to support any research on their personal catastrophe.

Saul persuaded Leo Davidoff to do the biopsies. Davidoff was, without question, one of the world’s great neurosurgeons. He’d been trained by Harvey Cushing and had a huge clinical practice, but was extraordinarily patient with a young neuropathologist. In the operating room I stood behind Dr. Davidoff with a small, chilled plastic tray holding a drop of the fixative: buffered osmic acid or glutaraldehyde. The surgeon cut a large bone flap in the cranium over the right frontal lobe. He then chose a gyrus relatively free of large vessels and stabbed through the leptomeninges a square a bit smaller than a centimeter. This he lifted with a flattened scoop through the superficial white matter, turned and dropped the tissue on my tray.

I moved to a table in the OR set for the purpose and began to divide the tissue into three parts: the first slice into formalin for pathology, the next into the “glut” or osmium for EM and the residue into dry ice for chemistry. Note that the second or central piece was the least traumatized by the operative procedure. I then went on to cut the EM piece into minute bits, each less than a cubic millimeter, so that fixation was very prompt and thorough. It should also be pointed out that Dr. Davidoff’s patients who went through this biopsy...
procedure never had any post-op complications. This whole program had been planned in great detail with Saul.

The pathology always displayed the typical ballooned neurons of TSD. The chemistry showed large amounts of gangliosides in molecular proportions to triglycerides and cholesterol. The electron micrographs were spectacular, and I showed them to Saul with great pleasure and no little pride. One was, some years later, chosen to be exhibited in a show titled “Once Invisible,” in the Museum of Modern Art, in NYC.

We discussed their meaning and the molecular proportions of the isolated membranous cytoplasmic bodies or MCB as we came to call them. Finally I wrote a paper and showed it to Saul, who rejected it—“fill it out,” he shouted. Well, we went on to write five papers—Methods, EM, Biochemistry, MCB, and the Membrane of the MCB. They filled one issue of the Journal of Neuropathology and Experimental Neurology in January 1963, and established many of us.

The Alzheimer’s project started concurrently with the TSD, but proved much more difficult. We began with familial cases, following the textbooks, all of which described Alzheimer’s disease as rare, involving only pre-senile patients, and caused by a dominant gene. The chemical work was not very helpful in that they were not able to extract the amyloid, and the respirometry was unremarkable, as were the lipids. The electron micrographs of tangles were easy, but I mistakenly and stubbornly called them “twisted tubules” rather than paired helical filaments. By the time that I began to understand the ultrastructure of the plaque, Saul was already ill. I remember very clearly being with him in Atlantic City at the neuropathology and neurology meetings. We were standing in the shade of a hotel wall as he complained of back pain. I tried to brush it off, but he insisted that he had cancer. Again Saul was right.

July 2010

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How I got to New York

I graduated from the Faculty of Medicine, Tokyo University, in the spring of 1959. I had never intended to be a clinician but had decided on a career in medical research even before I entered the medical school. During my early undergraduate period, I was vaguely attracted to “some aspects” of brain research. My desire to study brain biochemically became clearer while I was working in Prof. Yukichi Kimura’s laboratory during my years as an undergraduate student in the section of history and philosophy of science of the College of General Education, Tokyo University. I worked on the then hot topic of the potassium effect using the Warburg manometer and rat brain slices. The potassium effect is a phenomenon discovered by Henry McIlwain that the oxygen consumption of brain tissue increases to 150% of the basal level when a brain slice is placed in a potassium-enriched medium. Outside the lab, I was having the best two years of my life. I was free to follow my purely personal interests into all kinds of intellectual endeavors without any regard to potential usefulness in my future. In retrospect, I was experiencing the best of the true higher education without which my present is unthinkable. Sadly, this type of higher education is an endangered, if not already extinct, species these days when universities everywhere, not only in Japan but in nearly all countries, have forgotten the essential mission of universities and are striving to excel as trade schools.

I was not a good medical student. First of all, after the two years studying philosophy of science, medical school lectures just did not interest me. I felt that I was forced to digress to the previous century. Besides, I had too many distractions—mountain climbing and skiing, bird watching, photography, the Noh play, the piano, editing of the medical school alumni newspaper. My mother had long thought that the medical school of Tokyo University had no lectures in the morning, since I rarely attended morning classes. During the last three months of medical school, I passed all final exams of clinical subjects by cramming one subject into my brain with amazing mental concentration, then throwing out as much as I could of what I had just learnt since the drawer of my brain had limited capacity and then cramming everything of the next subject. Still I managed to pass the national exam for the medical licensure in Japan and also passed the ECFMG (Educational Council for Foreign Medical Graduates) exam required for engaging in patient-care activities in the United States. During 1959, I was a rotating intern at the U.S. Air Force Hospital in Tachikawa outside Tokyo. This was because I wanted to acquire sufficient clinical experience even though my ultimate aim was medical research and not patient care. I did not have a clear idea or wish to go abroad for further studies at that time. However, there is no denying that the idea of going to the United States after the internship was acquiring some sense of reality as I was working in the environment of an American hospital with physicians who were also directly imported from the United States.

One afternoon, I took off from my duty as an intern and visited Prof. Hirotsugu Shiraki in his office at the Brain Research Institute of Tokyo University. He was a neuropathologist who first described post-anti-rabies vaccine encephalomyelitis and pointed out its similarity to multiple sclerosis. His report initiated a rush of subsequent EAE studies in the world. I had known Prof. Shiraki well because he was fond of mountain climbing and I, as a member of the student Alpine Club of the medical school, had often gone to beg him for money, for example, “to buy a new tent.” I did not realize then that the day would become pivotal in my life. I asked him for advice about the possibility of going to the United States immediately after my internship.
His advice was clear: “Don’t go if you just want to go to the United States. Go if and only if you get accepted by a place really worth going for your future. I give you an example of just such a place. There is a new medical school in New York, called Albert Einstein College of Medicine. A guy with the name of Saul Korey, whom I have never met, is the first neurology professor there. He is trying to approach diseases of the nervous system from new angles by integrating basic science, clinical neurology and neuropathology, utilizing new basic physical and chemical technologies. If you manage to go to such a place, by all means you should go.” He showed me a just-published paper on a neutron-activation analysis of patient brains as an example. On my way back, I stopped by at a medical book store, Nankodo, on the street just outside the medical school. There, by pure chance, I found two books edited by Saul Korey. One of them, *Neurochemistry*, had been published in 1956. The other, *Biology of Myelin*, had just been published.\(^1\)\(^2\) I perused through them but the subjects were mostly of basic science way beyond the poor intern who had just graduated from medical school thanks to his skill to cram in a lot of information in a short time. I knew well the words of Ogai Mori, one of the major literary figures in the Meiji-Taisho era, “Blind respect is not a respect even if it is directed to an object worthy of respect.” That I had conveniently forgotten those words at that particular moment helped me into my subsequent career. My blind respect told me, “If I can go to his department at Albert Einstein, maybe I can kill two birds with one stone and learn both clinical neurology and biochemical research of the brain.”

I immediately wrote to Saul. After introducing myself, I said “My ultimate aim is neurochemistry but I would like to learn clinical neurology as an essential background for my research career. Would you consider me as a candidate for your residency program?” This was my first contact with Saul. This was obviously reckless. Considering the mass of job inquiries I used to receive from all over the world, particularly from developing countries, no answer or, at best, a flat rejection would have been expected. It took a long time for letters to cross the Pacific those days. Nearly two months later, Saul did answer my letter without rejecting me outright. He said, “We’ll gladly consider you for the position of assistant resident. Please come to the United States for an interview.” My immediate reaction was, “Don’t be ridiculous.” It was half a century ago. Japan was still recovering from the war. The U.S. dollar was at 360 yen (now 90 yen in October 2009), and $500 was the maximum one was allowed to take out of Japan. Personal travels outside Japan were inconceivable. I wrote back, “It is unrealistic for me to make a trip to the United States without any assurance of the position.” The next answer from Saul said, “OK, collect copies of your medical school records, the ECFMG certificate and any letters of reference from whomever you can get and send them over.” Now this was realistic. I collected recommendation letters from Dr. Shiraki, Dr. Tomizo Yoshida (of the Yoshida sarcoma fame) who was then the dean of the faculty of medicine, Tokyo University, and also letters from physicians at the USAF hospital where I was an intern to assure that I had sufficient English to function as a doctor in the United States. Then, Saul’s next letter said he would take me as an assistant resident from July 1, 1960. To this day I think that both sides were awfully brave, if not reckless. I knew nothing about Saul as a person, nothing about the environment of the school, and very little about his residency program. Saul decided to take me only on the basis of written records and letters without ever seeing me in person or speaking to me directly even over the phone. I realized that he could simply fire me after one year if I turned out to be useless as a resident. Nevertheless, it must have been a gamble in Saul’s mind that he accepted me as the first-year resident without seeing my face. At that point, I told my mother, “I have been accepted by a hospital in New York. I will go to New York in the summer.” My mother being my mother, she hit me with a question: “Who is going to pay for the trip?” I was shocked into action and promptly applied for the Fulbright scholarship. Luckily, I was granted the travel scholarship of the Fulbright Exchange Program, which enabled me to make the trip to New York.

When I finished my internship, I became a staff member of the department of neuropsychiatry of the faculty of medicine, Tokyo University, following Dr. Shiraki’s advice that I should join a department even though I would stay for a very short period of time. After only two and a half months in the
department, I set sail on the President Cleveland from the rain-soaked Yokohama Harbor in mid-June, 1960. Mr. Godo Nakanishi, the founder of the Japan Wild Bird Association (Yacho-no-kai, an equivalent of the Audubon Society in the U.S.), came to see me off because I was on the Executive Committee of the association during my undergraduate and medical school years (Fig. 1). Japan was then in the middle of a social upheaval. The new U.S.-Japan Mutual Defense Treaty was the target of vehement protests not only from the far left, but also from the moderate left including socialists. Huge demonstrations were staged nearly every day. I still clearly remember the radio news I heard next morning on the ship that Michiko Kamba, a Tokyo University co-ed, had been crushed to death during the demonstration in the previous evening. The next two weeks were a dead bore. While the upper deck had a pool and other entertainment facilities, we, the poor students on scholarship, were housed in the lowest level of the ship with stacked beds. During the day, the scenery was the most monotonous ocean. We killed time playing ping-pong on the deck, cursing every time when a mishit ball sailed over the edge of the deck into the ocean. Then, we had to buy another ball from the shop. I also spent time photographing albatrosses and red-tailed tropical birds that followed the ship. I still have slides of those birds. I was never really seasick but was chronically uncomfortable with a mild sense of motion sickness throughout the voyage. The trip was interrupted by a half-day stopover at Honolulu before it arrived at San Francisco. I watched with awe as our ship passed under the Oakland Bay Bridge. Luckily, the first jetliner services between the West and East Coasts of the United States had started a year earlier and I was flown from San Francisco to the then Idlewild (now J.F. Kennedy) Airport in New York overnight. Many of my contemporaries who went to the U.S. those days crossed the U.S. continent to the East Coast by train, taking several more days. Thus, I arrived at the Einstein/Bronx Municipal Hospital Center one week before I was to start my duties as a first-year resident. A shared room was assigned to me at the Staff House on the hospital grounds.

Life as a Neurology Resident
My first encounter with Saul was during the introduction of new residents in the conference room of the department of medicine in the Jacobi Hospital, one of the two major hospital buildings on the campus of the Bronx Municipal Hospital Center. Neurology was a section of the department of medicine at that time. Dr. Irving London, the first chair of the department of medicine, first gave an introductory remark and then turned around and said, “Saul, anything you want to say?” A big guy sitting close to him just shook his head with his mouth tightly shut and did not utter a single word. My first impression of Saul was not a very favorable one. He appeared to me arrogant and brusque (Fig. 2). However, Saul was in a totally different mood the next day at a meeting of only neurology residents. He was congenial and explained everything in detail to us, the newly arrived residents. I learnt later that the section of neurology was to become an independent department of neurology soon. Saul
may well have felt at the meeting of the department of medicine, “I will soon be running my own show.” I had known that Einstein was a newly established medical school but did not realize until new residents got together that at least three of my fellow neurology residents in the same year were the first Einstein graduates—Joan Rumberg, Lenny Green and Cal Ackerman (my memory is not clear; was Carl Rosengart also in the first class of Einstein?). This meant that Einstein admitted its first students four years earlier.

Albert Einstein College of Medicine was founded from the beginning with a contract with New York City that the city build a new Bronx Municipal Hospital Center on the adjacent ground as its teaching hospital. Thus, residents were actually New York City employees and the annual salary was $2,200. At less than $200 per month, residents’ life was not luxurious even in 1960 to say the least. Nevertheless, nobody complained, at least openly, because it was the time when interns in some of the most prestigious hospitals, such as the Massachusetts General Hospital, worked without pay. We residents were provided with a room shared with another resident in the Staff House on the hospital grounds, and meals at the hospital cafeteria were all free, including night meals while on night duty. Even family members of residents could eat at the cafeteria free when accompanied by the residents. Those nights when the hospital cafeteria served steaks were particularly popular among residents’ families. Those days, first-year residents took night calls every other day and every other weekend on busy services, such as neurosurgery. You were rarely able to sneak some sleep during night duties. Particularly when I was on duty for a weekend, my duty started when I reported to the ward Saturday morning and continued for two nights and three days straight until I went off duty in the late afternoon on Monday. Under these working conditions, my resident life was 100% devoted to patient care during the first two years even though I had come all the way to New York looking forward to being trained in medical research.

For two years I had opportunities to observe Saul as a clinical neurologist before I entered the research laboratory. Several fond memories come to my mind from that period. I arrived in New York in the summer of 1960, which was the year of the Kennedy-Nixon presidential election. I was deeply impressed by the American political environment by watching the first series of TV debates between the candidates. Saul was then driving a bright red Saab equipped with a three-cylinder engine, which required not just gasoline but a mixture of gasoline and oil as its fuel. It was a rara avis and students nicknamed it the “Koreymobile.” On its bumper was a big banner, “Kennedy for President.” The American political far left was and still is not far from the moderate center for the rest of the world’s politics. Within this context, the Jewish population with its relatively high concentration of intellectuals, artists and scientists tended to be on the left side of the American political spectrum and the Einstein faculty was no exception.

One day during my first-year residency, a young woman was admitted to the ward from the emergency room with a textbook history of tetanus.
She had stepped on a rusty nail a while ago and developed troubles with her jaw movements that day. As soon as Saul heard about the patient, he gathered all residents on the ward and declared, "Tetanus can be cured completely. Given proper care, there is no reason for any tetanus patients to die. I won’t tolerate it if you let any tetanus patients die on my service." From that day, residents on night duties slept on the bed across the room where the patient’s bed was. One night when I was on call, a nurse ran to me; "That patient pulled out the tracheal tube herself!!" When I rushed to her bed, she was already a little cyanotic. I tried but, in my panic, the tube would not go back in smoothly. It must have been a matter of seconds, but I felt it like eternity until the tube was finally inserted back and the patient started breathing normally again. What a relief!! Thus, I avoided Saul’s wrath, and the patient recovered completely and was discharged. Throughout this episode, Saul stopped by at the ward every day, examining the patient and checking her chart. I learnt an important lesson from Saul, the clinician. There is a follow-up story about this episode. The patient’s husband was apparently a cab driver. A few months after the patient was discharged, my wife had an occasion to take a taxi home. As it was passing by the Jacobi Hospital, the driver told my wife, “Recently, my wife contracted tetanus and was taken care of in this hospital by the neurology people. Everybody was wonderful and she received the best care. She is completely recovered and now back home. This is the best hospital in the world.” When my wife told me this story, I couldn’t help but wonder, “What if it had taken a little longer to insert the tracheal tube back in?”

During the first three months of my neurology residency, I rotated to neurosurgery as a part of the training program. I had thought I knew the American hospital system from my experience at the USAF Hospital Tachikawa during my internship. However, my daily life in the neurosurgery service was a revelation. The Bronx Municipal Hospital Center is in the Bronx, the northern part of New York City. It is the only part of the New York City that is on the North American continent. All other parts are on islands. The Bronx, particularly its southern part, became notorious a decade later as a very dangerous place with poverty and crimes, but it was still a quiet residential area around Einstein in the early 1960s. But, on the neurosurgery service, I saw patients of all types—a young girl who was shot in the head at night, a young guy who jumped into the pool from a high tree branch trying to show off to his girlfriend and ended up breaking his neck—ad infinitum. First-year residents had no time whatsoever even to sit except while eating meals or, when you were not on call, between 6:00 p.m. and 7:00 a.m. next morning. When I visited fellow resident friends in pathology, they were sitting at their own desks!! Pathology was located in the basement of the hospital, but I thought up and down was mixed up and they were in the heaven and I was in the hell.

I still remember a fellow first-year resident, Zuckerman (I do not remember his first name), who was also a “rotater” to neurosurgery from general surgery. He claimed, “I am a devout Jew. According to the Jewish rules, I cannot work from the sunset on Friday to the sunset on Saturday. I have to ask you to take calls during those periods. I will take calls every Sunday.” I didn’t think it was a good deal for me, but I accepted for a while since it is always difficult, if not impossible, to deal with anyone who likes to prop up god and hide behind it. But soon it was discovered that, even according to the Jewish rules, one was allowed to work during the Sabbath if it was a matter of life and death. Zuckerman had to give up nice Friday evenings and take calls. But the story was not that simple. He reluctantly stayed in his room in the Staff House on Friday evenings. Since he was in the neurosurgery service, he often got calls after midnight. The Jewish rules dictate that one cannot use any contraption that is a product of human civilization. (If I am not mistaken, there were no flights in and out of Tel Aviv airport from the sunset Friday to the sunset Saturday for many years.) He therefore would not pick up the phone in his room. Someone had to go from the hospital to his room to wake him up and summon him to the operating room. As often is the case in many hospitals, the OR of Jacobi Hospital was on the top floor. Zuckerman, not being allowed to use that epitome of civilization, the elevator, ran up the stairs to the operating room. Needless to say,
he was totally useless for some time, panting and exhausted. I firmly believe that religion is at the bottom of most, if not all, human ills and that true peace will never come on the earth as long as plural monotheistic religions exist. This time in 1960, I could not avoid being directly and personally victimized by a religion, or more accurately, by people who proclaim to be religious.

There was another hilarious incident concerning the Jewish rules. One day, an elderly patient was admitted with a mild stroke. In a few days he was doing quite well, so I told him that I would put him on a regular diet. Unexpectedly, I encountered a strong resistance. He kept insisting that he must have **** diet, which I had never heard of. After several exchanges of futile arguments, I went back to the nurses’ station. “That patient is well enough to be on a regular diet but he is insisting that he must have a strange diet that sounds like ‘kosher’ diet. What is it?” The room was immediately filled with loud laughter. I for one was totally at a loss, having no idea why I was being laughed at. According to the concept common in Japan, I had thought Jewish was defined as a race and did not know that it was defined by a religion. Naturally, I had no idea whatsoever that it imposes so many strict rules on people’s lives. Zuckerman’s case above was one example, and this story about the kosher diet was another. This alone would keep me away from the Jewish religion, because I love shrimp, oysters, pork, particularly bacon and eggs with my toast in the morning. These incidents did not diminish my lifelong antipathy against all organized religions.

Albert Einstein College of Medicine, itself being a unit of Yeshiva University, the bastion of the Jewish religion, was and I assume still is supposed to follow the Jewish rules. For example, having a class that required attendance by students was prohibited from Friday evenings to Saturdays. The meat and dairy days were strictly separated at the college cafeteria. Not only were the days separate, the trays, dishes and utensils were also separate. To push it to an absurdity in my mind, separate openings existed where finished trays, etc., were placed for washing. If you bought a chocolate bar from a vending machine during Jewish holy days, it came out with a sticker proclaiming that it was blessed by a rabbi for consumption during the Sabbath. My wife joined me from Japan in December 1960 and we rented a basement apartment of a private house just adjacent to the hospital grounds. The landlord, who was not Jewish, told us the story. Before us, two Einstein students lived there. They were devoutly Jewish. They turned on all electric switches before sunset Fridays and kept them on until the sun went down on Saturdays. I guess it is true that electric switches are products of human civilization. Under these circumstances, it was no surprise that the Yeshiva University side complained often that Einstein faculty regularly worked Friday evenings and on Saturdays. Luckily to me, these complaints were completely ignored by the Einstein faculty members.

It was during my residency period when I realized that the American education system was quite different from that of Japan. One day we residents were discussing something, I no longer remember the subject. The discussion came to the point that something was “close to infinity but not quite.” One of my fellow residents said, “Then it is infinity minus one.” I, as someone who always loved “useless” mathematics since my college student days, was appalled and protested, “There is no such mathematics.” It was obvious, however, that it would have made no difference in the discussion if I had argued in Japanese. If I had said, “There are many sizes of infinities and in fact, there are an infinite number of infinities of different sizes. Whether sizes exist in between different sizes of infinities was one of the biggest problems in mathematics until the establishment of Kurt Gödel’s logical undecidability principle,” I am sure I would have been suspected of having come not from Mars but from Pluto. Another similar but much later example: one of my fellow faculty members came to borrow an electronic calculator from me, saying that he had to do some logarithmic calculations. I handed him my calculator and told him that he had to use the log key if he needed to do base 10 log calculations and the “ln” key for natural log calculations. I couldn’t believe my ears when he replied that he had never heard of logarithm other than base 10. I told him that one could not get in to college...
and major in any branch of science in Japan, if one did not know the natural log. His reply was, “Why should I know such a totally useless thing?” I then realized anew that the United States is by far the most pragmatic country in the history of mankind. There is an indomitable undercurrent among the general population, “What is useless cannot be valuable.” Because of this, even scientists who are not interested in practical applications of their work are forced to come up with some justification that their work, although its usefulness may not be obvious now, would one day be proven useful for mankind. Otherwise they may lose their research funding. To my mind, they are selling their souls to Mephistopheles in order to catch Gretchen. This extreme pragmatism of the U.S. intellectual climate is entirely contrary to my ingrained conviction through my upbringing and education. To me the scale of practical usefulness and the scale of value are two separate and mutually independent parameters. This, together with the unreasonably strong religion, was the major factor that made me conclude that the U.S., despite so many advantages while I was actively working, is not the country I wanted to live in after my active professional life was over. It was this psychological factor that prevented me from acquiring U.S. citizenship while living in the U.S. for 47 years altogether. I am now back in Japan and have given up my U.S. residency status. I have crossed the Rubicon at this old age and I have no regrets.

Saul and the Einstein Environment
Before I present my fond memories of my life in the laboratory with Saul, I would like to give an objective description of the professional environment that surrounded Saul. When the Einstein College of Medicine was founded in the middle of the 1950s, discrimination against Jews had not yet been completely erased. Saul himself graduated from the Medical School of the University of Western Ontario located in London, Ontario, Canada. During one of the long evenings in the lab with me, he told me that no medical schools in the United States would accept him as a student. I understood that Einstein had been created as a college of Yeshiva University, the bastion of Jewish education, to fight against this anti-Semitic atmosphere. Albert Einstein, who was still alive, blessed its inauguration by allowing use of his name. The first students were admitted in 1956. The initial faculty members were all Jewish, recruited from among the most promising young Jews not only from the U.S. but also from many corners of the world. Among them were the husband and wife team of Ernst and Berta Scharrer of neurosecretion fame (anatomy), Alex Novikoff, who later provided the morphological base for the biochemical concept of the lysosome proposed by Christian DeDuve (pathology), Alfred Gilman, who was known for his pharmacology textbook with Goodman and is also the father of Alfred G. Gilman, who later received the Nobel Prize for his discovery of G protein together with Martin Rodbell (pharmacology), Leo Davidoff, who was one of the top disciples of Harvey Cushing (neurosurgery), Herbert Scheinberg, who was known for his work on Wilson disease (medicine), Abraham White, who was also famous with his biochemistry textbook (biochemistry). The college was developing with an amazing speed and vitality for a new medical school. We should remember that the political climate in the United States was in the dark ages of its short history during the 1950s. It was the time when Joe McCarthy campaigned in the U.S. Congress against any hint of communism—in fact, also socialism, since he could not distinguish communism from socialism, as the majority of the U.S. population still cannot. His hysterical “red purge” was rampant and many talents in all fields of intellectual endeavors and the art world were banished with the label “RED,” a “scarlet letter.” They suffered from irreparable damage not only to their reputations but also to their daily lives, since many lost their jobs and many potential employers would not hire them. This was precisely a mirror image of the Zhdanov doctrine under Stalin in the Soviet Union, and the negative consequences were the same. I do believe that Einstein should be given great credit because it courageously hired many such young talented people when many cowardly universities shied away from them in order to “protect” themselves. Alex Novikoff was one of those people rescued by Einstein. I am not without complaints, in fact I do have a lot of complaints against the Einstein administration in later years, but I do respect
the courage it demonstrated in its early days to
fight against the prevailing evil wind. Saul Korey
was invited to such an environment as the first
chief of neurology and soon the first chair of the
department of neurology. He did his neurology
residency at Columbia University. He had also
spent time in the laboratories of some of the
most distinguished basic scientists, such as David
Nachmansohn, one of the earliest researchers
who studied acetylcholine, Harry Grundfest, a
physiologist, and Severo Ochoa, a Nobel laure-
ate. Saul had had publications from his work in
these laboratories. Thanks to his background in
basic science laboratories, Saul was able to un-
derstand science for its own sake independently
from any possible applications to clinical med-
icine or to human disease. As a person, he was
gregarious; and aggressive and argued vocifer-
ously, pushing his own ideas like a bulldozer. This
was not necessarily a trait that would endear him
to everyone. He had a lot of supporters but also a
lot of detractors, with relatively few in the middle
neutral position. Gil Glaser, who was chair of neu-
rology at Yale in later years, once told me long
after Saul’s death, “I was the chief resident in
neurology at Columbia when Saul was a resident.
I believe I was the last person on the earth who
could give orders to Saul.” I don’t doubt that Gil
must have been right.

Initiation to the Korey Lab
After two years of residency, I was summoned
by Saul: “You came with the goal of gaining
experience in the research lab. From the third
year, you should limit your clinical duties to night
and weekend calls and start working in the lab.”
The main subject of the Korey lab then was
Tay-Sachs disease. This is a rapidly fatal genetic
neurological disorder of infants. Patients are born
apparently normal, but first symptoms and signs
appear around six months. Clinical manifesta-
tions are largely limited to the nervous system.
After a relentlessly progressive clinical course,
most patients die by 3-4 years of age. It is highly
prevalent in the Ashkenazi Jewish population, so
much so that for many years, it was thought to
occur exclusively among Jews. From the mid-
1930s to the early 1940s, Ernst Klenk identified a
new group of lipids that accumulated abnormally
in patients’ brains and named it “ganglioside.”

When I arrived in the Bronx, Terry and Korey had
just discovered that those distended neurons in
the brain of Tay-Sachs patients contained abnor-
mal organelles with an onion-like membranous
structure. They called them “membranous
cytoplasmic bodies (MCB).” Some French au-
thors still call them “Terry bodies.” Soon after I
entered Saul’s laboratory, Makita and Yamakawa
determined the chemical structure of the gan-
glioside in the Tay-Sachs brain. It is now called
GM2-ganglioside according to the most widely
used nomenclature by Svennerholm. However,
its biosynthesis and degradation, and naturally
the mechanism of its abnormal accumulation in
Tay-Sachs disease, were a complete mystery in
the early 1960s. Saul was trying to approach the
problem by integrating clinical medicine, pa-
thology, electron microscopy and analytical and
metabolic biochemistry utilizing less than 1 g. of
brain tissue obtained by brain biopsy. This was
epoch-making. The first set of the results of this
team effort was published in 1963 as a massive
104-page article. When its reprints arrived, Saul
gave me a copy on which he inscribed, “To Kuni
whose success is certain” (Fig. 3). He must have
thought, “This young Japanese came all the way
from Japan and is struggling in the lab. He needs
a bit of encouragement.” This trick worked and
I was happily working day and night in the lab.
I was first given the task of measuring oxygen
consumption of biopsied brain slices under vari-
ous metabolic conditions. By pure coincidence, I
used the Warburg manometer identical with the
type I used in Prof. Kimura’s laboratory several
years earlier as an undergraduate student. But
the genetic diseases, such as Tay-Sachs disease,
are rare, and brain biopsies from patients were
on the average no more frequent than once every
few months. I was therefore also given another
project to do during intervening periods. This
was to examine protein synthesis in vitro using
isolated microsomes. Although Saul’s long-range
plan was to apply the system to biopsied brain
tissue from patients, I first tried to establish the
experimental system using rat brains. Protein
synthesis was measured by incubating the brain
microsomal and cytosolic fractions prepared by
ultracentrifugation, together with radiolabelled
lysine. The system was then scaled down so that
it could be applied to a small portion of already tiny biopsied brain tissues. This series of studies resulted in an article which, for the first time in my life, I could really call “my own work.”

Once in the laboratory, it was immediately obvious that this was the life I wanted. Either taking care of patients or in the lab, I was working until late into the evening every day and I did not mind that at all. However, in clinical medicine, a clinician’s time is not his but his patients’. It was impossible to plan anything ahead of time. As a third-year resident, I had no patients on the ward and was taking only night calls. I had to go to the hospital only when I was needed. Nevertheless, it was totally unpredictable as to when I might get a call. Since I could not interrupt an experiment in the middle and go to see patients, I had to stop working in the lab at 5:00 p.m. on the days when I was on duty. On some nights I might be busy getting multiple calls, but on some other nights, I might not get any calls at all. Then, I felt, “I could have done this and that in the lab, if not on call.” Even during daytime when I was supposed to be free from any clinical responsibility, I had to talk to patients’ families when they telephoned for any reason. It was not the long hours but the mental difficulty of frequently switching back and forth between patient care and lab work that eventually took me out of clinical neurology altogether. I do have a medical license in Japan and also in New York State. Legally, I can practice medicine or surgery tomorrow. But, to do so now would endanger not only patients and society but also myself.

During later years in New York, the only tangible benefits of my M.D. license were limited to the occasional privileges accorded to licensed physicians such as escaping from minor traffic infractions or declining jury duties. Some of my friends who remember my old days say, “You used to be a neurologist.” Many other colleagues who got to know me more recently do not even know I am a licensed physician and in fact was once engaged in patient-care activities. Frequently I receive correspondence addressed to me as a Ph.D.

Looking back from far away, both in time and distance, I learnt a lot from Saul. Like any academic clinicians in Japan who divide their time between patient care and laboratory activities, he was shaking test tubes and running chromatographic columns himself in the evenings and during weekends (Fig. 4). I did not think anything of it after coming from Japan but I realized later how extraordinary it was for an American clinician. He carried back and forth between home and the laboratory his huge square leather bag, the kind salesmen carried, packed with documents and literature.

One Monday morning, I was busily preparing for the experiments I had planned for the week. I was not entirely happy when Saul caught me and explained to me in detail about the new pathway for bacterial cell wall lipopolisaccharide synthesis, which he had read about during the weekend. I now understand well that he wanted and even needed someone to talk with in the lab while working during the evenings and weekends. He had two technicians working for him but they would not work in the evening or on weekends. While many researchers worked in the lab in the evening, I was the closest to Saul physically since my bench was opposite Saul’s in the same room. We kept discussing all kinds of subjects into the late evenings, from purely technical laboratory problems to fundamentals of science, arts, politics and social issues. He was the type who loved the challenge when people disagreed and argued against him. He became unhappy on rare occasions when I kept agreeing with him. He wanted to be challenged by counter arguments. And I must say that suited me fine too. I do remember some of the things he said on such occasions.

Figure 3. Saul’s psychological trick worked.
For example, when Nick Gonatas was to go to Penn, he said to me, “If you have someone good, you must find a good opportunity somewhere else for his future, even though losing the person may mean a big loss to yourself.” On another occasion, “The only experiment worth doing is the one done right. If you have to make excuses for no money, no time, no manpower or no equipment, then you might as well not do the experiment. You are wasting your time.”

New York Musical Scenes

Lest the above may give a wrong impression, I should say I did not work every night in the lab. When I arrived in New York in 1960, it was the last season of Charles Munch at the helm of the Boston Symphony. In addition to its regular season series in Boston, the Boston Symphony had a series of six or eight concerts in New York during a season. I bought the season ticket for the Boston series. It was also the last season for the Boston Symphony in Carnegie Hall, which had just been rescued from the wrecking ball by concerted efforts of musicians and citizens, such as Isaac Stern. Also, since I did and still do have a strong affinity for contemporary music, I was going to the series of contemporary music concerts held monthly in the Carnegie Recital Hall. The series was basically for performance of new compositions by young, active American composers, such as Ralph Shapey, Charles Wuorinen, Milton Babbitt, Mario Davidovsky, Harvey and Sophie Sollsberger, et al. It was well before the time when Wuorinen had his bitter tenure fight against the music department of Columbia University.

The reason I bring up this series is that I experienced an unforgettable evening at one concert. Each of the concerts included one “contemporary” piece that had already established itself as a classic, in addition to new compositions. In one evening, they played Edgar Varèse’s “Ionisation.” And Varèse was in attendance!! At the end of the performance, he stood up from just a few rows in front of me, turned around and acknowledged the applause. The Carnegie Recital Hall is tiny. He was literally face to face with me. He was a big man with a rugged face. His face and his demeanor are etched into my memory forever. Varèse died within a year or two afterwards. In that series, I often saw Dr. Ryuta Ito, who was a few years senior to me as an alumnus of the Tokyo University faculty of medicine. He was also known in Japan as an active composer in addition to being a physician. I believe he was later professor of pharmacology at the Toho University School of Medicine in Tokyo. I remember he once made a point to me during the intermission in one of the concerts that the main issue of contemporary music was how to “clash” different sounds. His use of the word “clash” somehow made a long-lasting impression in my mind.

Another talented young Japanese composer, Toshi Ichiyanagi, was studying then at Juilliard. He lived near Amsterdam Avenue close to Columbia University. We, several of the Japanese residents at the Bronx Municipal Hospital Center, often played mahjong with him. If my memory is correct, he had just come to New York after a triumphant season a few years earlier when he swept the NHK competition, winning the first prizes in all three categories of compositions; solo, chamber and orchestral. He also received the Suntory Music...
Prize in 2002. Seiji Ozawa also had made his dramatic debut around the same time, replacing Leonard Bernstein at the last moment at a Tanglewood concert. But I did not have a chance to listen to him until much later.

When I told Saul that I was not working in the evening because I had a concert to go to, Saul made a long face. It meant to him the absence of an opponent in the evening arguments. In Japan, I often hear strange stories that departmental staff cannot go home until the professor goes home. Luckily to me, it was New York. No matter how long a face Saul made, I just ignored it and enjoyed the musical life in New York.

Saul’s Research Philosophy and His Staff
Saul’s laboratory at that time was unique. In the research group led by Saul, he had been the only clinician until I joined the group. He often told me during our late evening discussions, “This lab does not need any more clinicians beyond me and you. We need rigorously trained basic scientists in several key areas.” The corner of the first floor of the Forchheimer Building was teeming with top-class basic scientists, who later established themselves in their own fields. Among them were Bill Norton, who was recognized for his work on neural cell isolation and on myelin and who later was president of the American Society for Neurochemistry and chief editor of the Journal of Neurochemistry, and Bob Ledeen, who joined the Korey laboratory to participate in the Tay-Sachs disease project and devoted his entire research career to studies of gangliosides. Both Bill and Bob were organic chemists by training. Next door to the Korey lab was the neuropathology laboratory, headed by Bob Terry, who was an indispensable collaborator of Saul’s from the beginning, and Nick Gonatas, a young upcoming neuropathologist, who later moved to the University of Pennsylvania. Both Bob Terry and Nick had been trained by the godfather of neuropathology, Harry Zimmerman, at Montefiore Hospital in the Bronx and were both expert electron microscopists. Nick’s wife, Jackie Gonatas, had been Saul’s technician as Jackie Orloff. My wife, Kinuko, was being trained by Zimmerman when I entered the Korey lab. It was after Saul’s death when she joined Bob Terry’s group. In the biochemistry department a few floors above was Maurice Rapport, who is one of the pioneers in the field of glycolipids. He had discovered lactosylceramide. (He named it cytolipin H.) Maurice often came down to our floor and socialized with us. I never forget what he told me one night when I was alone in the lab with him: “Your work is not finished until you publish it.” Bill Norton and Bob Ledeen have remained good friends of mine ever since. Both are three years older than I. Given my feeble knowledge of organic chemistry, Bill has been of great help to me. He has an unusually lucid, logical mind. There have been even a few Nobel laureates who did not give me an impression of being particularly intelligent when we discussed scientific matters personally. But I count Bill as one of the most intelligent people I have met. Bob Ledeen is a solid organic chemist who accomplishes a lot by quietly working in the lab. Bill retired a few years ahead of me, but Bob is still active. Looking back, those were happy years. We just worked together in the lab until late at night, only pursuing our research interests without any administrative burdens.

We All Lost Saul
We all happily drove ourselves busy in the Korey lab. However, this happy state was not to last long. One day in May or June of 1963, Saul held a house party for residents and young attendings. We played softball and barbecued on his lawn (Fig. 5). Saul was complaining about his back pain while playing softball. In retrospect, that must have been the telltale symptom of his cancer. Apparently, he was deceived because he had long suffered from back pain and thus did not pay the attention he should have paid. Soon afterwards during the summer, he suddenly developed an intestinal obstruction and was diagnosed as having pancreatic cancer. Laparotomy indicated extensive metastases to the liver and no further surgery was possible. Saul died only three months later. There are conflicting reports about the nature of his tumor, whether it was pancreatic or intestinal. I do not know its true nature. However, the neurology department put up the last fight against the challenge. When Saul’s illness was diagnosed as an inoperable cancer, an article published in Science just earlier came to our attention. It was authored by Albert Szent-Györgyi, who had won

KUNIHIKO SUZUKI, M.D. (continued)
the Nobel Prize for his discovery of vitamin C. It described a potent anticancer agent extractable from human urine. I do not remember the name Szent-Györgyi gave to the agent. We promptly placed a large glass jar in every men’s room throughout the hospital and the medical college and collected them every day. Bill Norton and Stan Samuels drove to Woods Hole to learn directly from Szent-Györgyi how we should go about preparing his anticancer substance from collected urine. None of us was optimistic that it would cure a cancer declared by surgeons inoperable. We all knew too well that we were trying to hold on to a piece of straw as we were drowning. Still we felt that we could not accept defeat without putting up a fight. I remember well Maurice Rapport’s words about Szent-Györgyi’s anticancer substance, “We don’t want to see Saul die as the last person who had to die of cancer.” None of us did anything else for two months other than collecting and extracting urine for the elusive anticancer substance. In the eyes of bureaucrats, this would have been viewed as an inexcusable dereliction of our duties for which we had been hired. But, I would say without hesitation, “The real life cannot and should not be measured by ready-made rulers only.”

Saul left us seemingly in an instant. Dying in the middle of his 40s, he did not have enough time to make lasting contributions of his own. Few even remember his name now. Some even say he accomplished nothing. However, my interaction with Saul, short as it was, gave me the base on which I could shape my career in later years. More than anything else, it was his vision as to which direction academic neurology should be heading that guided me. Bill Norton agrees with my assessment of Saul’s influence. At the time of his death, Saul was working on his concept of the “gangliosidase system,” meaning that there should be a metabolic system that degrades gangliosides systematically and that something must be wrong somewhere in the system in Tay-Sachs disease. It foresaw the identification of the enzymatic defects in lysosomal diseases, cloning of the responsible genes, identification of disease-causing mutation, and the currently active search for effective treatments, all of which occurred after Saul’s death.

If Saul had not died when he died, I probably would have returned to Japan within a few years. After Saul’s death, I was asked by the senior members of the department just to continue what I had been doing with Saul. I groped around and slowly learnt how to swim myself. Then it was inertia that defined my subsequent path. I ended up living and working in the United States altogether for 47 years without ever making a conscious decision to remain in the U.S. I already mentioned above the reason why I decided to return to Japan permanently after my retirement from active academic life. That I could not stand watching the smirky expression of George W. Bush on the television screen gave me the last and definitive push on my decision to come back to Japan. Looking back, I feel that, after all, I just

Figure 5: At the softball-barbecue party in Saul’s house in late spring of 1963. The ladies are Mrs. Korey (Doris, left), and one of his daughters (right). Saul was complaining bitterly about his low back pain while playing softball. In retrospect, it must have been a symptom of his cancer that killed him four months later.
followed the path Saul had shown me. I was given an opportunity to give a Saul R. Korey Memorial Lecture at the annual meeting of the American Association of Neuropathologists in 1993. I would like to end this reminiscence by quoting my concluding paragraph of the published lecture since it still expresses well how I feel about Saul.

“I am writing this exactly 30 years to the month after Saul’s death. Few remember Saul’s work. He simply did not have time to make long-lasting contributions of his own. Nevertheless, those of us who had the privilege to know Saul and work with him realize too well that he anticipated exactly where the field should be moving. He set his own laboratory in motion in that direction. He knew that the ‘useless’ science driven by inner curiosity of basic scientists is the most useful in the long run for solving more pragmatic problems of the clinically oriented. I still vividly remember one Monday morning when Saul caught me—he needed someone, anyone, who might listen—and discussed with great excitement a paper on the bacterial cell wall lipopolysaccharide synthesis which he had read during the weekend. If not his own work, it was his vision that set him apart from other ‘competent’ scientists. Most of the conceptually modern developments of Tay-Sachs disease occurred after his death. Yet, I cannot help but to feel that Saul would smile and say, ‘I knew it’, if he were to see what has happened in the past 30 years.”

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SAUL KOREY WAS MY MENTOR

I met Professor Korey when I was a first-year medical student at Western Reserve School of Medicine. He was running an elaborate biochemical laboratory teaching exercise that began at 11:00 a.m. and did not conclude until 8:00 p.m. I was among the lucky few who stayed to the end, impressed by his charisma, his passion for science and his passion for teaching. He became my mentor, giving me reading lists in neurology and neuroscience, and offering much helpful guidance, even after he left to found what is now the Saul R. Korey Department of Neurology at Albert Einstein College of Medicine. His love of neurology was so contagious that I would go to the VA hospital to work up neurology patients on my free days (in those halcyon days at Western Reserve, Thursdays in the first two years were free to do anything we wanted). While still at Reserve, he found a place for me to learn about laboratory research (then students at Reserve all were required to carry out a research project culminating in a thesis) on an immunology project.

When I reached the clinical years, I asked him if he could find me a place as an orderly or nurse’s aide at Einstein; he responded by finding me a place as a summer student at Einstein.

I became the first medical student on the wards at Einstein, even before the first crop of Einstein medical students, mentored by the brilliant Milford Fulop. I was the only medical student present when the formidable Professor Irving London made chief of service medical rounds, for which the superb resident staff carefully rehearsed me.

After what was then called an internship in medicine, I became a resident in neurology at Einstein. Dr. Korey created a remarkably splendid department of neurology, with superb clinicians and scientists. When I was a second-year resident, he had me give a course on language and its disorders, requiring attendance by everyone, faculty and residents; amazing.

In my third year he gave me fellowship in the Interdisciplinary Program, which he had created to foster neuroscience at Einstein, and he spared me the pains of chief resident, allowing and encouraging me to do research. And he obtained a place for me in the laboratory of the great pharmacologist Murray Jarvik, who gave me space, supplies and laboratory equipment, and taught me the rudiments of cognitive psychology.

After my residency in neurology, I moved to Stanford University School of Medicine, attracted by its chair, Professor Frank Morrell, who was one of the then too few neurologists concerned with cognition. Six months later Dr. Korey again offered me a position at Einstein; before it could happen, he was gone. Gone but never forgotten.

Professor Saul R. Korey was unique; we could use a few more like him, but his talents, aspirations, vision and contributions are all too rare. Those of us who were fortunate enough to come under his umbrella received priceless gifts that transformed our lives as neurologists and as individuals.

July 2010

Herman Buschke, M.D.

Professor, the Saul R. Korey Department of Neurology
Professor, Dominick P. Purpura Department of Neuroscience
Lena and Joseph Gluck Distinguished Scholar in Neurology

SAUL KOREY’S LASTING INFLUENCE

My 1978 *Science* paper with Peter Spencer and my other neurotoxicology experimental studies were, to a large part, inspired by my association with Saul Korey. While I was a resident on rounds with Saul he emphasized that every patient’s condition, no matter how mundane, could and should serve as a focus for research. He felt strongly that it was not enough to simply make a diagnosis and attempt to relieve the problem. He tried to instill us with the notion that if we could discover some more about the mechanism of an illness we might advance its eventual cure.

As a clinical toxicologist I was called to Italy in 1977 to examine 20 young female shoemakers who had experienced heavy inhalation exposure to n-hexane and developed visual and memory impairment. This chemical was only believed to cause mild peripheral nerve disease, but having examined these girls (all of whom were told they had multiple sclerosis) I was convinced the chemical had damaged their brains as well. Upon my return to Einstein, Peter Spencer and I exposed cats to this chemical and its toxic metabolite. We produced central nervous system degeneration in the visual pathways and in areas essential to memory function.

The girls were able to get compensation (previously denied) from industry, and we were able to establish safe levels of exposure to the chemical. We and others explored further the basic mechanism using axonal transport studies, and were able to determine that the axonal degeneration was due to accumulation of neurofilaments at the paranodal regions, most likely from formation of Schiff bases.

Herbert H. Schaumburg, M.D.
Professor, the Saul R. Korey Department of Neurology
Professor, Department of Pathology

In 1960-61, Bob Terry joined Saul Korey, chair of neurology at Albert Einstein College of Medicine in the Bronx. In January 1962, I followed Terry to Einstein, to further my training in clinical and experimental neuropathology.

The Terry laboratory was close to Korey’s, which included four neurochemists, Bill Norton, Bob Ledeen, Stanley Samuels and Kunihiko Suzuki, as well as a talented cell biologist, the late Elliott Robbins.

The interactions among the members of this multidisciplinary group were spontaneous. The best way to describe the style of “governance” conducted by Korey and Terry was “benign neglect,” a term, I believe, used by Terry. The existence of such a group was unusual for most academic departments of neurology of that era. Indeed, the combination of clinical neurology with neuropathology, electron microscopy, cell biology and neurochemistry was unique.

Saul Korey, who brought this group together, interacted with us frequently, but informally, with brief conversations in the corridors, mixed with his “penetrating and dissecting” questions. We were encouraged to undertake collaborative projects, without much “clearance” from above. I felt as if I was thrown into a large “sandbox” together with eager playmates!

The intellectual vigor introduced by Korey and Terry exuded innovation and enthusiasm for new approaches and concepts. From the Einstein years I would like to cite the much-quoted paper on Alzheimer’s disease with Terry, which introduced me to neurodegenerative diseases; a study of a storage disease which started my life-long collaboration with my wife Jackie; and our paper with Elliott Robbins, which became a Citation Classic.

August 2009

Nicholas Gonatas, M.D.
Professor of Pathology and Laboratory Medicine
Perelman School of Medicine, University of Pennsylvania

References


REMEMBERING SAUL KOREY

Saul Korey was a remarkable person. As a physician, he was an excellent clinician who diagnosed his own terminal illness and strenuously disagreed with his colleagues who opted for a more benign explanation for his severe back pain.

As a researcher he was at the bench, putting in hours in the lab after his long day of clinical care, teaching, advising and administration. His view of neurological research was expansive. At a time when the understanding of lipid storage diseases was almost nonexistent, he put together a team of biochemists, electron microscopists and clinicians and it was his open vision and administrative skill that guided us all. The breakthrough findings on Tay-Sachs disease and the careers that began then were because of Saul. I was privileged to have been part of his team and instrumental in the isolation of the membranous cytoplasmic bodies that are pathognomonic of Tay-Sachs disease.

It was the beginning of a new widespread research effort. Saul was my patient advisor through my postdoctoral fellowship and made possible my subsequent appointment to the faculty at the NYU School of Medicine. I am forever grateful for all he did for me.

He directed and participated in the research effort, while he also chaired the department of neurology and Einstein’s college-wide, interdisciplinary neuroscience program. At seminars, he always sat in front, hunched over as if sound asleep. But as soon as the talk was over, he was on his feet with a penetrating question or comment.

It was fitting that after his terribly untimely death the neurology department should bear his name. I am very pleased that I suggested it and that it was adopted despite the usual custom of always naming for a benefactor. Saul Korey should be remembered for who he was and for what he was by the generations of faculty and residents that pass through the department that carries his name.

July 2009

Stanley Samuels, Ph.D.

Former Associate Professor, Department of Neurology, New York University School of Medicine

Meeting Saul Korey proved a life-changing experience. I was in the biochemistry department at Einstein at the time and knew that Saul had a reputation as “a pretty tough hombre”—a force of nature to be reckoned with. So it was a pleasant surprise to this young postdoc on first encounter to find myself chatting with a rather amiable person, a confident man of impressive demeanor who spoke seriously about his research program with a twinkling of humor. In that first meeting he outlined his philosophy of research, specifically the need for multidisciplinary attack on neurological diseases such as Tay-Sachs, which was his primary interest at the time.

He further explained why it made good sense for me, an organic chemist by training, to consider joining the effort. This was in response to my concern about having little knowledge and no experience in the neurological sciences. I was having enough difficulty at that time attempting a transition from organic chemistry to biochemistry. But Saul was reassuring and pointed out the learning opportunities his department and research program could offer.

Putting on his visionary hat, he foresaw the field of neurobiology experiencing exponential growth in the coming years and explained why that made it a good time to get in on the ground floor. Time has proved his forecast correct and his department did indeed offer rare learning opportunities to the young scientists he was recruiting. I’ve never regretted accepting his offer to join the neurology department back in 1962.

Somewhat later in explaining more about his group’s Tay-Sachs research, Saul went into some detail about the obscure group of brain molecules called gangliosides that accumulate and eventually contribute to the destruction of brain cells. An important goal, he felt, was to learn more about these substances, what they consist of and why they accumulate in the disease. Since the chemical structures were largely unknown, he suggested this would be a suitable starting point for an organic chemist. I was fortunate to publish my first paper in this field on the structure of the Tay-Sachs ganglioside (later called GM2) in 1965. As fate would have it, Saul was no longer with us when that happened, so he wasn’t able to witness that additional contribution by his group.

Saul Korey was a mentor for all seasons, a dedicated teacher with a wealth of knowledge and experience in the neurological sciences in addition to being on a first-name basis with many of the leading scientists of that time. It was awe-inspiring to witness the parade of Nobel laureates and other distinguished investigators he invited as seminar and symposium speakers to Einstein. This provided us all the best kind of exposure to cutting-edge science and the opportunity to meet some of its leading practitioners. Perhaps because of such activities and his own reputation he was able to attract several talented young neurologists to Einstein, a prime example being Bob Katzman, who admirably followed in
Saul’s footsteps of inspiring leadership and became scientifically distinguished in his own right. I shall always be grateful to Saul Korey for bringing me into neuroscience and introducing me to gangliosides, a field of enduring interest to my group. Saul the visionary understood well that future research would focus on the biochemical and neurobiological functions of gangliosides in normal brains, but he likely could not have foreseen the many ways this would develop, including their use as therapeutic agents. The latter efforts have shown only limited success so far for such disorders as Parkinson’s disease and spinal cord injury, but the future may hold more promise in light of major advances in our understanding of their biological mechanisms. I believe Saul would be astounded and delighted to see how far this field has progressed, and would perhaps feel some justifiable pride in having been one of the pioneers at its inception.

December 2008

Robert W. Ledeen, Ph.D.
Professor, Department of Neurology & Neurosciences
New Jersey Medical School
University of Medicine & Dentistry of New Jersey

A paper of mine, published in *Endocrinology*, illustrates Saul Korey’s pioneering view and contributions toward the understanding of hormonal receptors on cell membranes, which he shared with Dr. Irving London and other Einstein faculty, and to which I was exposed as a student at Einstein. His philosophy has been a guiding influence on my entire career at NIH in the Institute of Diabetes, Digestive and Kidney Diseases, at Johns Hopkins School of Medicine and here at Einstein.

Jesse Roth, M.D., Class of 1959

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In the very beginning, there was no department of neurology. Saul was in medicine, which consisted of about seven faculty members of which I was the most junior. Having lunch every day with him, Irving London and most of the others was a joy...Our discussions ranged from baseball to science and chemistry, and it seems as if to everything but...sex.

Saul was stimulating, energetic, creative, diligent, brilliant...He played tennis like a bear...but was a wonderful listener and let me ramble on about my research, which interested him very much. He was totally supportive and became a friend, mentor and role model. He consulted me frequently about his various symptoms and was a bit of a hypochondriac...except for the final illness, when his back pain was not a matter of imagination. His loss was terrible. I still miss our long conversations late in the afternoon, evening and sometimes at 7 a.m.

I did not work with or publish with Saul. I was an admiring colleague who was stimulated by our friendship.

I urge you to read his 1963 AOA lecture, which was written during his terminal illness. It tells much about the man...in his own words.

July 2009

Irwin Arias, M.D.
Former Professor of Medicine and Founding Director of the Liver Research Center
Albert Einstein College of Medicine
Emeritus Professor and Chair, Department of Physiology, Tufts School of Medicine
Senior Scientist, Head of Unit on Cellular Polarity, Cell Biology and Metabolism Program, NICHD
Assistant to Director, Intramural Program, NIH

(Please see page 46 of this booklet for a reprint of the paper.)
Thank you for the opportunity to add a few thoughts to a remembrance of Saul Korey. Contemporary writers remind us that exceptional success in life is contingent upon being the right person at the right time and in the right place. To this I would add; and doing the right thing. With this addition Bill Gates qualifies as a success, Saul Korey the more so. Superbly trained as a basic scientist and clinical neurologist, Korey accepted the challenge to build a new department of neurology in a new medical school at a time of increasing intellectual ferment in the neurological sciences. Saul productively catalyzed this development in the fertile environment he created here.

I first met Saul Korey through his relationship with my colleague Harry Grundfest at Columbia. Harry was generally regarded in the mid-20th century as “the dean of electrophysiologists” and was passionate about good neuroscience years before the word was invented by F. O. Schmitt. Grundfest was never shy in expressing his opinions about the many distinguished scientists who visited him e.g., Eccles, Hodgkin and Fatt. For Saul, Harry had the highest regard, which strongly influenced my judgment about the kind of department of neurology being built at Einstein.

Saul Korey’s gift to medicine was his demonstration that disorders of the human condition could best be solved by the integration of outstanding basic science and exceptional clinical acumen. The men and women he recruited into his department were imbued with the spirit of “translation- al” medicine decades before the concept gained traction. His success in this was evident to me well before I arrived at Einstein in 1967, several years after Saul’s untimely demise.

There is no question but that the model for a transdisciplinary research and training program that I pursued in establishing the department of neuroscience here in 1974 was fashioned after that initiated by Saul Korey in the department of neurology. We are all beneficiaries of Saul’s magnificent obsession with excellence.

August 2009

Dominick P. Purpura, M.D.
Dean Emeritus, Albert Einstein College of Medicine
Distinguished Professor Emeritus, Dominick P. Purpura Department of Neuroscience

Saul was born in New York City on April 12, 1918, the son of a physician, and the older of two children. His younger brother had Down syndrome and died at the age of 3, I believe of scarlet fever. Saul attended public school for the first several years, and then was accepted at a high school for very bright students; I can’t recall the name. What I do remember is his telling me that he was driven there but asked the chauffeur to drop him off a few blocks before the school; he didn’t want the other students to see him in a chauffeur-driven car. He also came with a lot of sandwiches for lunch, which he shared with the students who had little. I mention this only because these decent, caring qualities were part of his character all his life.

After graduation he was accepted at Cornell. He told me that he played cards at Cornell, and joked around. “I didn’t work hard at all.” He attended only for two years, because at the age of 16 he learned he could be accepted at medical school in Canada, and went to the University of Western Ontario for six years.

He occasionally experienced anti-Semitism from other students there, and, oh yes, occasionally beat them up. At the end of six years of medical school Saul was graduated number one in the class. Saul had just turned 22.

The year was 1940/41 and Saul was shortly afterwards drafted into the Coast Guard. He served aboard ship till he fell one day and dislocated his shoulder. In a way that was lucky for me, because when he came out of the hospital I met him. We were married in 1944, and Saul was assigned to the Marine Hospital in New Orleans.

In 1945, when he was discharged from the service at the end of the war, Saul was accepted at the Neurological Institute (NI) of Columbia Presbyterian, where he became chief resident. When he left NI he spent a couple of years studying, I believe with Dr. Ochoa at NYU and Dr. David Nachmansohn back at Columbia. Then on to Western Reserve in Cleveland in 1955, until Dr. Irving London called Saul to come to New York to help set up the Einstein College of Medicine, particularly the division of neurology, which in three years became a department in its own right.

Please understand in every way that Saul was a great man. I remember he said to me when he was in the hospital dying of pancreatic cancer, that he hoped his work would be remembered. A little later, lying in bed in the hospital, he said “I’m wrong, it’s my family that is most important to me.” Yes indeed, Saul was a great husband, a wonderful father, a precious soul.

He died at the age of 45, on September 27, 1963.
I was deeply honored to receive the first Korey award named after our founder and first chair of the Einstein department of neurology. Ahead of his time, Saul Korey, a consummate physician, had a vision and the will to implement it—translational research as the core of an academic neurology department. And this, 55 years before the term became fashionable.

In many ways, it is like finally meeting him to receive the passing of the baton and keep the vision alive. And I thank him for showing us the way to work towards changing for the better the lives of people with neurological diseases through research, teaching and compassion.

July 2013

Solomon L. Moshé, M.D.
Charles Frost Chair in Neurosurgery and Neurology
Professor of Neurology, Neuroscience & Pediatrics
Vice Chair, the Saul R. Korey Department of Neurology
Director, Pediatric Neurology
Director, Clinical Neurophysiology

I arrived at Einstein in September 1978 to work with Dominick Purpura on his recent discovery of ectopic dendritogenesis in Tay-Sachs disease, an inherited disorder characterized by storage of gangliosides in brain. At the time I was a graduate student with Henry Baker in the department of comparative medicine at the University of Alabama in Birmingham (UAB). Several years earlier, Henry had discovered and characterized a family of Siamese cats that were a genetic replica of ganglioside storage very similar to Tay-Sachs. Subsequently, Dom showed that pyramidal neurons in the affected cats also grew ectopic dendrites similar to human cases, leading to collaboration between the two labs and to a remarkable opportunity for me to go to New York and assist in a series of experiments.

I had just returned to UAB from the University of Edinburgh where I worked with Alan Brown and others who were pioneering the use of single neuron electrophysiological studies coupled with horseradish peroxidase injections to reveal the morphology of recorded neurons in exquisite Golgi-like detail. This turned out to be a perfect preparation for Dom’s plans for analysis of cortical neurons in the cats—as he and other neurophysiologists in the Kennedy Center were keenly focused on similar “morphophysiology” studies (as Dom referred to them).

At this time Dom was not only chair of neuroscience but was also director of the Rose F. Kennedy Center for Research on Mental Retardation and Human Development. I arrived to find that the department and center were a remarkable mix of talented individuals, including both bench scientists and clinicians.

There were neurophysiologists studying gap junctions and related neuronal-glial interactions, vestibulomotor and basal ganglia/substantia nigra function and neural networks in the cerebral cortex. There were also developmental neurobiologists doing research on brain plasticity mechanisms, including those linked to early cognitive development. There were neurologists doing cutting-edge translational studies (long before the term “translational” was used) focused for example on therapy for multiple sclerosis. There were pediatricians working on a variety of developmental disabilities in children, mostly through the auspices of CERC (the Children’s Evaluation and Rehabilitation Center). There were also neuropathologists working on a variety of human neurological disorders, from Tay-Sachs and other so-called storage disorders to conditions like Canavan and Menkes diseases.

Many cutting-edge methodologies also were in use—from newly developed current source density analysis in cerebral cortex, to organotypic neuronal culture, to sophisticated single-unit studies focused on understanding neuron function. The environment was so rich in talented people, in new and innovative methodologies and in ideas that it took me, as a budding neuroscientist, several years to begin to put it all together and into context. Needless to say, my sojourn to New York, anticipated for one year, turned into what is now a 35-year saga.

During that first exciting year at Einstein, one name that emerged time and time again was Saul Korey. This started shortly after I arrived, when I began to explore the published literature on Tay-Sachs disease. A series of papers in the Journal of Neuropathology and Experimental Neurology in 1965 (indeed, the entire January volume) was composed of papers from Korey and his colleagues at Einstein. What I quickly learned was that Korey and his group (including Bob Terry, Kunihiko and Kinuko Suzuki, and others), along with another Einstein giant, Alex Novikoff, were on the verge at that time of solving the riddle of what caused Tay-Sachs disease. Yet they were beaten to the idea by H. G. Hers in Belgium, a
colleague of Christian deDuve, who had discovered the lysosome a decade before. This concept—that the “storage” in so-called storage diseases like Tay-Sachs was lysosomal and caused by genetic loss of a particular digestive lysosomal enzyme—was one of the great cell biology discoveries of those times, as it revealed a mechanism accurately explaining literally dozens of rare brain disorders that had been known clinically since the turn of the 20th century (including Gaucher, Pompe and Niemann-Pick). As someone who had developed a passion for lysosomal disease, discovering how close Korey and his colleagues were to this pivotal discovery further convinced me that I was indeed in the right place to be doing my work.

During this time I also noted that the department of neurology was named after Korey, and shortly thereafter learned that he had died prematurely after only a few years on the faculty. I also learned that I was surrounded by colleagues he had recruited to Einstein years earlier, and that much of the strength of the neuropathology group, including Terry, the Suzukis and Bill Norton were individuals that Korey had hand picked to join Einstein. This also included the pioneering pediatric neurologist Isabelle Rapin, whose personal support for me and my research during those early years in Kennedy was such an inspiration.

Everyone spoke of Korey with deep affection and respect and they seemed to carry with them a particular approach to their work that came directly from him. This involved focused study of human disease in an effort to understand disease pathophysiology, with the idea ultimately of using the information learned to improve the lives of individuals with that disease.

There was also a clear emphasis on the importance of mixing basic scientists and clinicians to work on the same problem—a type of collaborative science not widely practiced at the time, yet which seemed to be the norm at Einstein. Coming to understand this made me rethink my own image of what I should be doing as a scientist and instilled in me a great admiration for Korey.

Years later, when I had my own independent laboratory in the Kennedy Center, I gave photographs of “patron saints” to each of my lab members. These were neuroscientists and others who had gone before us and who through their careers could speak to us from afar...like Ramón y Cajal, del Río Hortega, Thudichum, Szentágothai...and for me, well, I gave myself a photo of Saul Korey. It hangs in my office still, now many years on.

Recently, after becoming the director of the Intellectual and Developmental Disabilities program in the Kennedy Center, I began focusing on the need for a training program for graduate students and postdoctoral fellows. What I had in mind was to design something akin to a program that I had seen in action as part of the remarkable atmosphere in the Kennedy Center in the 1980s, as mentioned above. This program consisted of “ID fellows” who were in a number of the research labs and clinics in Kennedy and who seemed to me to be the glue that held together a good deal of the collaborative science that was underway.

The “ID” I learned stood for interdisciplinary—and the goal was to link labs across disciplines as well as basic scientists and clinicians. When I inquired about how this program came to be, I was surprised to learn that it was started in 1957 by Saul Korey, following his successful appeal to NIMH to fund such a program. On his death its leadership passed to others over the years, including to Dom Purpura at about the same time the Kennedy Center opened in 1970.

Throughout the 1970s and 1980s the ID program was the backbone of postdoctoral training in Kennedy, carrying with it Korey’s original dream—one of collaborative research spanning multiple disciplines, directed at basic science discoveries translated into understanding and treating human disease. Remarkable really, the traction and durability of that simple formula. Indeed, one does not need to look far to find the origin of Einstein’s current mantra “Science at the Heart of Medicine”; it is indeed Saul Korey’s legacy to us all.

July 2013

Steven U. Walkley, D.V.M., Ph.D.
Professor of Neuroscience, Pathology & Neurology
Director, Rose F. Kennedy Intellectual and Developmental Disabilities Research Center

Indeed it is flattering to have your students who suffered your presence so recently ask you to speak to them once more before they move to another stage of their lives. There are, as Jacobovsky says to the Colonel, two explanations: either they may have seen so little of you during their medical school career that they just wanted to be sure that you (as a species) were for real, or, more likely, they felt you had reached an overly ripe state and wouldn’t be able to make it much longer—this invitation being their final act of charity.

As a matter of fact, my first response to the request was one of resignation—a realization that I was a member of an Establishment, having passed so to speak from presenility to senility, from rebellion to passive resistance, unknownst to myself, but not to others.

Nevertheless, I felt it would be quite a challenge to select a topic for which I didn’t have any slides, to allow myself certain freedoms of expression, and to be able to say it all standing up and without paying—a sort of fantasy that so many advertisements have made familiar to us.

Finally, after much deliberation, I have decided that I prefer to address you as an amateur philosopher and to sharpen your sense of reality.

The story is told of Peter fleeing from Rome to save his life. As he approached the gates of Rome, a step ahead of his pursuers, the majestic figure of Christ loomed before him. Peter stopped and asked, “Domine, quo vadis?”—My Lord, where are you going?—and Christ is reported to have replied, “I go to Rome to be crucified again.” The story ends with Peter retracing his steps to Rome to reach his martyrdom. I do not imply that you should start medical school over again to attain your martyrdom.

Nor do I expect you to respond to the query, “Quo vadis?” Rather I suggest that in order to know the direction in which you are going, you must first take your present bearings. And in this regard the allegory retold by Camus as the subject of his essay, “The Myth of Sisyphus,” suits our purpose. It was the fate of this human who earned the opprobrium of the gods to push a huge rock up a hill, and, as he reached the summit, to have it escape his grasp and roll down again. Camus found the dignity of Sisyphus in the moment at the summit when Sisyphus turns, surveys the world about him, and tirelessly returns to the foot of the hill to resume his trial. In this flash of realization, man finds man.

I have modified the story to be a bit less poetic and more stark in its implication. To me Sisyphus is joined by a large group of men, some pushing with him, others ambling along. The hill is never-ending, and the task grows more difficult. The rock slips from their hands, but it never rolls down the hill very far, because the men behind, either working or ambling, form a sort of human wedge—advertently or inadvertently. The rock’s downward progress is stopped by the entrapped humanity that piles up behind it—those who have worked and those who have ambled. Bitterly enough, the penultimate absurdity is that either one does and dies or one does not and is killed.

It is important that the hill is never-ending, for if it were to end, and the rock were finally to reach the top, both the rock and those who pushed it would go clattering into a wall-less, timeless abyss, the ultimate absurdity.

This, then, is the human condition shorn of its trivial fantasies, its delusions, its everyday sensations. And our myth is the belief that I have just told a myth.

If for the moment you accept the picture as sketched—a universe that is because it is, and man who lives and dies rhythmically—we at least know where we are: In a state somewhat different from that of Job, who in the face of his disasters was found to say, “I abhor myself and repent.”
Thus, in this neutral atmosphere we strive for some orientation, some order, so as to gain a footing for the climb. As individuals scientifically trained we have become accustomed to recognize that a hierarchy or order is believed to exist within the sciences: that which is most universal in its application or most general in its import, is the most significant. Discussions and data about elementary particles, about atomic structure, etc. hold a most estimable position. Without retreating to idealism, I might suggest that the scientific laws that govern the human organism, his behavior in the broadest sense, have equivalent importance. I recognize that the physical forces applicable to atomic arrangements and molecular functions, may, in certain combinations, be sufficient to explain all of man’s behavior—the so-called reductionist attitude (which I tentatively share). But the central issue is somewhat removed from this, namely, that man is the thing: his life and death, his biologic cycle, his works and thoughts. They are not to be depreciated by comparison with the life history of a star. A qualitative judgment between the works of Galileo and those of Freud cannot be made with substantial force.

Of course, we have become sympathetic to the idea that man occupies a central position in the universe: we are after all physicians and medical biologists, and man’s vital processes are our concern. Thus, we have advanced two stages:

(a) We operate under the rules of a myth.
(b) Our order, our security, stems from interest in and scientific investigation of ourselves as matters of great importance.

A chill must pervade most of us if the myth is taken to be plausible or even possible. There seems to be little dignity in sweating and shoving against immutable gravitational forces, only to be killed unfeelingly. Perhaps the alternative role, that of the spectator, is to be preferred. After all, he ambles along smoking, drinking, jesting, loving, to meet his end in similar fashion, at about the same time, and without the seemingly futile effort. Is there justice in this and instruction for a way of life? Believe it or not, there is no answer to this question. Are the Marines who lost their lives or their youth at Guam for a great cause to be envied more than the spectators who viewed their destruction, either with sympathy or indifference, but who survived?

You have journeyed too far to ask this question. As physicians you are committed, committed to the passion of life—and committed with a passion.

No one who has not been at the bedside of a patient can realize the extent of your commitment, your engagement. It is the strands of this commitment that bind you to life, now seen slowly slipping from some, now being reawakened in others. Such a compelling force, such an intimacy with life, such as we subsume under the word passion, exists for few men besides physicians in this world.

And it is this passion, attenuated though it may become, that moves with us from the patient to the laboratory—from the concrete to the abstract.

As with all wondrous possessions one ought not dissipate them. Each relation with a patient, each encounter with the investigation of his physiology or disease in the laboratory, reinforces the commitment, enhances the passion.

Without these one loses his position behind the rock—a position perhaps not enviable, but at least an identifiable one.