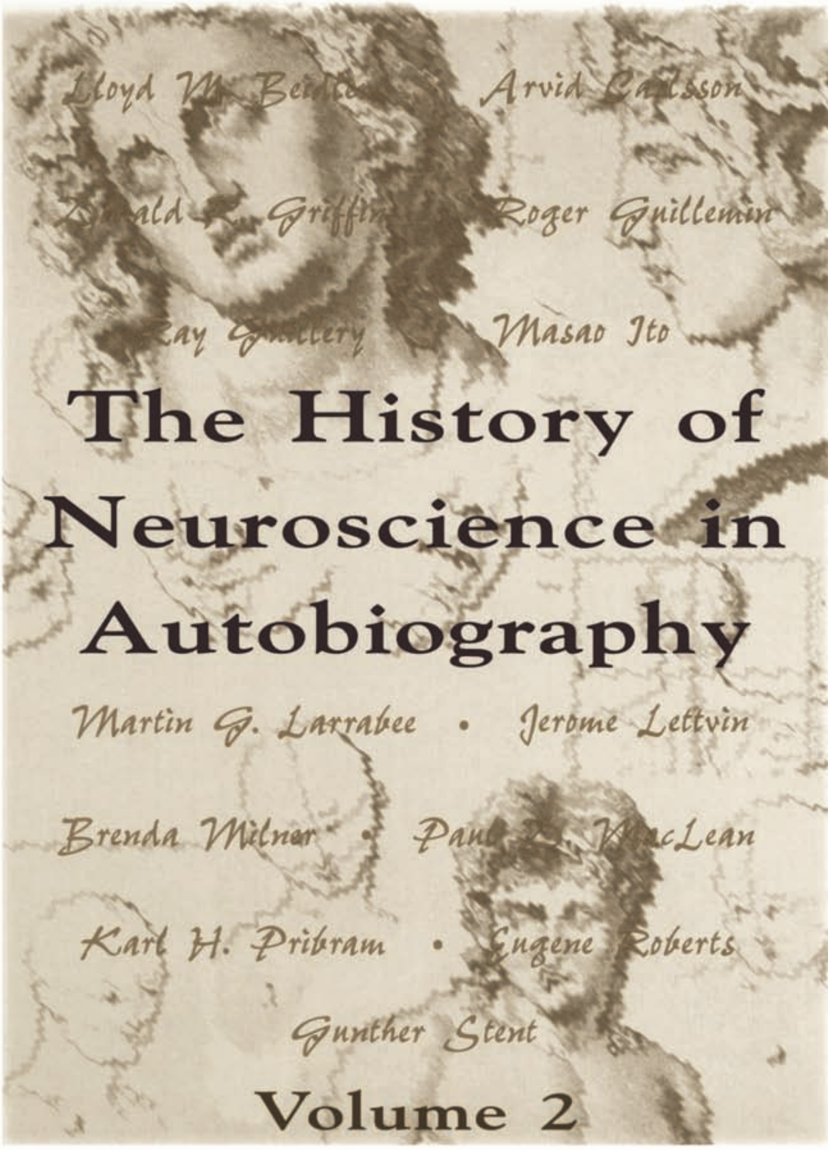

Edited by Larry R. Squire

The History of Neuroscience in Autobiography

VOLUME 2

Edited by Larry R. Squire

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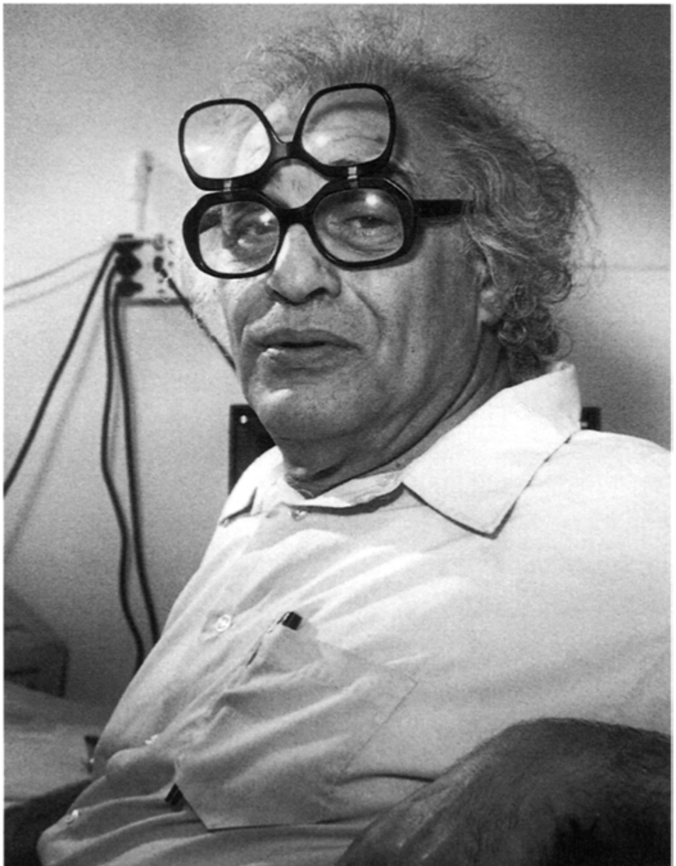

Jerome Lettvin

BORN:

Chicago, Illinois
February 23, 1920

EDUCATION:

University of Illinois, B.S. (1943)
University of Illinois, M.D. (1943)

APPOINTMENTS:

University of Rochester (1947)
Manteno State Hospital, Illinois (1948)
Massachusetts Institute of Technology (1951)
Rutgers University (1988)

Jerome Lettvin was trained in psychiatry and then carried out neurophysiological studies in the spinal cord, made the first demonstration of feature detectors in the visual system, and studied information processing in the terminal branches of single axons. He is remembered for his original and charismatic classroom teaching, especially on the subject of perception.

The History of Neuroscience in Autobiography, Volume 2

Jerome Lettvin

Beginnings

My parents immigrated, each from the Ukraine, in the decade before World War I. They met in Chicago. Mother was a piano teacher, father was a Kropotkin-type anarchist turned lawyer. In 1915, father, who had come to America only fours years earlier, graduated Tuley High School in Chicago, winning the prize for oratory. He then embarked on social reform, speaking at street corners and in the hobo's park (next to the Newberry Library), supporting labor unionization and a variety of other causes. Mother, a handsome woman, was much sought after by dentists, doctors, and other such elevated types. But she saw in father the fire she wanted to impart to her children. So she arranged that he be given tickets to concerts, plays, and other events, and always by accident happened to be seated next to him. In a short while, of course, he fell in love, but when he proposed, she made it clear that she would accept him only if he went to law school. So he did, supporting himself by selling papers and doing odd jobs. Immediately on graduating he collapsed, but she nursed him back to health, married him, and I was born February 23, 1920. Thereafter there were two more sons and a daughter.

My brothers and sister agree with me that we never got to know father at all well. Father at home was gently taciturn and unassuming. Mother ruled the house and had a driving ambition for all of us. I played my first piano concert at the age of eight then quit. After all, my sister and youngest brothers were already prodigies at the piano. They appeared a few years later individually and together as soloists with the Chicago Symphony Orchestra. I stubbornly wanted to write poetry, so mother finally drove me that way.

We lived in an enchanted ghetto, Humboldt Park. Rosalind Tureck was next door. Saul Bellow, Isaac Rosenfeld, Sam Wanamaker, and Sidney Harris were all at Tuley High School with me. There are a great many less well-known literati, musicians, artists, and actors who attended that school from 1932 to 1936. The surroundings were materially poor but culturally rich; it was impossible not to resonate.

I went from high school to Lewis Institute, a working-man's college ($15 a course), now part of Illinois Institute of Technology. It closed because stones began falling from the parapets, endangering the already run-down Madison Avenue environment. I spent my third year of college (1938) at the University of Chicago because my mother had decided I was to be a doctor. Her decision was irrevocable.

Friends

My education began at the University of Chicago where I met my two lifelong friends: Walter Pitts (the mathematician), and Hyman Minsky (the economist). Both are now dead. Indeed, I owe whatever learning I have to Walter. I was 18 at the time, and he was 15. It was my last year as pre-med. My mother, who had a whim of iron, had laid out my career. Walter had run away from home in Detroit the previous year and become a nonstudent at the University of Chicago. The school was well aware of him and turned a blind eye to his presence in classes. Before we met, he had wandered into the office of Carnap, the logician, with a marked and annotated copy of Carnap's recently published book on logic. Without introducing himself he went into a careful analysis of weaknesses and even outright errors in the text, and after a long colloquy, he left without ever giving his name. Carnap spent half a year searching for "that newsboy who understood logic," and when he found Walter, arranged to get him a job (for Walter was penniless). In any case, nobody threw him out of the graduate school classes he attended.

He was an autodidact who had taught himself Latin, Greek, and Sanskrit along with German and French. He began his interest in logic and mathematics at the age of 12 when some bullies chased him into the Detroit public library. He hid out in a section where Russell and Whitehead's *Principia Mathematica* was shelved. He spent the next two weeks reading it, and sent a letter to Russell about some questions raised by the first volume. Russell replied affably in the fashion of addressing a fellow.

I really don't remember how we met, but only that, for some reason, we became fast friends. I knew no science and less mathematics but was much taken up with poetry; this was our initial common bond. Going to medical school dismayed me for I wanted to be a writer, but my mother's hold on me was unyielding. During the year at University of Chicago, Walter, knowing my innocence of formal reasoning and philosophy, proceeded gently, ever so gently, to give me some notion of the underlying ideas. It is very hard to explain what is meant by the delusion of a glimmering of what you don't understand, but somehow that is what he evoked in me, and very skillfully indeed. However, much of our time was spent in experimenting with literary forms. We collaborated in trying to write a play.

There was a wonderfully comely young woman there at the time, Josephine, now a psychologist, to whose room Walter and I generally gravitated. She was warm, witty, and knowledgeable, and visiting her was like attending a salon of the sort we had only read about in accounts of past centuries. It goes without saying that Walter and I fell in love with her, yet always from a respectful and admiring distance.

Hy Minsky was the leader of the young socialists on campus. He and Walter and I would get together every few weeks to discuss politics. I remember how horrified he was when he heard that I was ghost-writing pamphlets for a member of the YCL, the Young Communist League. I pleaded that it gave me a certain kind of practice in putting forth vehemently what I don't believe, but he and Walter persuaded me to stop.

The only department at University of Chicago that Walter called home was Rashevsky's group in mathematical biophysics. I had another friend there, Bob Williamson, and was always welcome for coffee from the large, ever-hot urn. Sitting through seminar after seminar, I understood little of any lecture until Walter explained it to me later. Rashevsky tolerated me as a well-meaning visitor.

Medical School

In the fall of 1939 I entered the University of Illinois School of Medicine. The requirements for entrance were nominal as compared to today. The elementary premed courses in physics, chemistry, and mathematics could be easily passed simply by knowing the rules but without much understanding. Everything to be learned in medical school then was entirely empirical; there was little explanation of why things were as they were and how they worked or functioned. The studies were arduous but not difficult—all that was required was a good memory.

As far back as I recall I was always an overweight slob, disheveled and careless of appearance. After I entered the school I was "rushed" for the Jewish medical fraternity, and a week later was the only student ever depledged, to my relief. My father was an anarchist and I inherited his fear of organized in-groups.

The one faculty member I followed was Gerhardt von Bonin, the neuroanatomist. He was the only one to whom I showed my poetry. Liking it, he tried to persuade me that poetry was more interesting and challenging than medicine, and that despite my parents I should drop out and follow the higher calling. At our final practical examination at the end of the course, he came up to our table, on which lay the cadaver that my partner and I had dissected. Looking fixedly at me he said, "I will pick up an organ. If you name it you will pass and go on to be a leech. If you fail to name it you will fail and become a poet." Without looking aside he reached into the lower abdomen with forceps and held up something. I was torn, but finally muttered "the cerebellum," leaving the choice to him. He dropped the tissue back without looking at it, and said with contempt. "You named it, you pass. And god have mercy on your soul." This anecdote may sound apocryphal, but Tom Kemper, the eminent neuropathologist, had much the same experience with Gerhardt.

In my third year, Warren McCulloch came to the school as Professor of Psychiatry. His function was to set up a laboratory of neurophysiology in the basement of the Illinois Neuropsychiatric Institute. Gerhardt and Percival Bailey knew him well and were looking forward to collaboration. Gerhardt took me along to meet Warren. When I was in high school, I had read Buckle's history of the Scotch mind in the 17th century. It gave me nightmares for weeks afterward. Warren would be the first Scotchman I ever knew and, truly, I had a frisson going to see him. This was not lessened when I met him. He sported no beard then, but certainly had the most piercing eyes I ever encountered. However, in very little time, he, acting most friendly, dispelled my nervousness.

During the first two years of medical school, Walter and I met often and far into the night. Walter would visit a medical class, I would take time off to go to one of Rashevsky's seminars. I brought Walter to visit Warren. Whoever has read Warren's essays knows his abiding interest in philosophy. There was no question at all but what Walter instantly became part of Warren's coterie.

Adoption by the McCullochs

Walter was impoverished, as I said earlier. I was increasingly chafing at my mother. I loved her, but home had become intolerable. She had taken a strong dislike to Walter and to Bob Williamson because, as friends, they might divert me away from the goal she envisioned for me, a career as an eminent doctor, impeccably dressed, who in courtly manner would give wonderful speeches while accepting award after award for his skill. Warren and his admirable wife, Rook, adopted Walter and me as family and we went to live at their home.

It was there, late in that year (1942), that Warren and Walter conceived and wrote their famous paper, "On the Logical Calculus Immanent in Nervous Activity," and its sequel, "How We Perceive Universals." These papers are at the root of what later became AI, artificial intelligence. They were published in Rashevsky's *Bulletin of Mathematical Biophysics*. It is hard to describe the ferment in those long evenings, but for the first time I began to perceive what was at issue in the study of the brain.

Walter stayed on but I moved a few months later in 1942. Residents and nurses had been leaving for the army and the Illinois Neuropsychiatric Institute, a research hospital, became understaffed. I was in my last year of medical school and already strongly driven to neurology, having devoured all the books on the subject in the medical library. When I volunteered to Francis Gerty, the head of psychiatry, as a fill-in for nurse, intern, or resident as needed on a 24-hour call, he quickly accepted and gave me a resident's room so as to have me instantly accessible. I was already familiar with psychiatry from my reading and had impressed the rest of his staff when I attended ward rounds and conferences on patients.

Low Key Vengeance

One virtue of writing an autobiography is that you can do your own deconstruction like a Tinguely sculpture. So I will dispose of one canard promptly. I am not now nor have ever been a true scientist. My credentials for incompetence are unflawed, and testimony of colleagues will bear me out. Yet the first paper I ever signed as co-author was with Walter, "A Mathematical Theory of the Affective Psychoses, Part I." It was meant as a joke, but now, as I read it again, I find it even more well-done than I remember and would bet it could be passed by some referees today. The spirit behind the paper was simply revenge.

I was working on the wards fairly steadily and Walter would come over often enough to keep me company. He would stay over in my room because Warren's house was in a suburb and public transport was awful at night. Next door to my resident's room lived a Hungarian fellow aspiring to certification. This schmuck complained to Gerty that he suspected Walter and me of being homosexuals. Gerty, who well knew otherwise from the nursing staff, called me in to warn me of the fellow's charges. Walter was as incensed as I was at the imputation. We plotted revenge. It is hard to explain our processes of thought. Both of us felt somewhat like literary characters out of some imaginary novel that Edmund Wilson might admire. The graceless Hungarian was a poseur. Standing with one foot advanced, head bowed, hand on chin, he would nod understanding at well-timed intervals. This characteristic allowed him to pass muster wordlessly not only with patients but with colleagues. The greatest revenge would be to stimulate him to that pose by a lecture which he couldn't understand while at the same time he would know that we knew he didn't understand. The thinking was convoluted, but literary. It was more satisfying than outright battery to shame someone publicly while he alone would know he was being shamed. After all, we were young and loved complexity for its own sake.

So the next evening we sat over a bottle of potent wine and plotted. Our target's preferred interest was in depression. Well and good. But how to use the notion? At that time an odd discipline was emerging that even I could smell as contrived, topological psychology. Walter had a lifelong aversion to psychiatry. But I laid out what were the current doctrines and suggested we reduce them to diagrams so unfamiliar as to carry instant conviction. Walter was most interested in Leibniz, whose dictum on logical machines was the root of the paper with Warren. Half drunk, we cooked up a broth, he processing the formal expressions, I providing the ideas to be so expressed. We wrote the paper that night, using Leibniz's words—"affection" for being acted on, and "conation" for striving to act.

Next day we showed it to Warren, who was amused but interested. We did not tell him our motives. He advised that we show it to Gerty, which we did. Gerty, who wanted some rapport between psychiatry and science, glanced at it and proposed a public seminar. That is what we aimed at. Walter coached me on how to present the mathematical part which I didn't understand (but no one would ask about) and I coached him in the current catch-words and concepts in psychiatry with cases in point. When the day came we walked into a surprisingly crowded lecture room. Rashevsky was there; so also were Franz Alexander, the head of the Psychoanalytic Institute, and several other dignitaries. In the audience and standing in a side aisle as if to be ready for a medical emergency was our target. Undaunted we went through our routine, expecting at any moment an embarrassing question from Rashevsky's group or Alexander's circle. But nothing happened except general acclaim. And there was our victim in the exact pose we wanted. Our triumph was complete.

But it backfired. Rashevsky wanted to publish the paper directly in his journal. It is insulting to tell such a figure that he fell for a hoax. Later Alexander invited both of us to lunch and proposed (I swear this is true) that we be hired to put psychoanalysis on a firm theoretical basis. We demurred on obvious grounds. I was already in the Army and Walter had a difficult job to finish. A decade later we were further embarrassed when offered a grant to write Part II. This is the whole story and I trust to have saved my reputation as a nonscientist.

Internship

While working at the INI and finishing medical school I wrote an essay on the physiological explanation for the Argyll–Robertson pupil. It differed considerably from that advanced by Merritt and Moore, and was carefully argued. I submitted it along with my application to the Harvard Nerve Service at Boston City Hospital. It persuaded Merritt to accept me into the service as an intern. Our class graduated 3 months early because of the war.

Denny-Brown had just taken over Harvard Nerve Service when I arrived. Norm Geschwind and I have always claimed not only that Denny had a well-developed sense of humor, something our colleagues think preposterous, but also that he was the best teacher of neurology around. Going on rounds with him became a game. He would often make outrageous or wrong-headed diagnoses, forcing the residents and interns to argue with him on physiological and anatomical grounds. We learned much more from debating those diagnoses than from simply filing and classifying them.

Denny was a physiologist, trained by Sherrington. One of his masterpieces, the work on the Brown–Sequard syndrome, is a cautionary essay on spinal cord physiology even today, especially today.

For some reason he allowed me to clown. One night I taught a wall of patients in the women's ward to practice the positive Babinski reflex in the right foot only. The next day some visiting dignitaries in neurology were to be shown around. It was only at the third successive right positive Babinski during rounds that Denny turned and glared at me. The reflex did not figure in the rest of the rounds, but Denny never took me to task for it nor mentioned it thereafter.

During this period a cointern in medicine, Raisbeck, persuaded me to go with him on a visit to his uncle at MIT, Norbert Wiener. Wiener had just lost his valued postdoctoral fellow to a fatal ski accident. I described Walter to Wiener in glowing terms that he disbelieved. So Warren financed a ticket for Walter on the train to Boston. Walter and I walked in on Wiener who after a gruff "hello" said to Walter, "Let me show you my proof of the ergodic theorem." They went next door to the blackboards, and by the time the second board was covered, after frequent acute questions and comments by Walter, it was clear that he was in. Walter moved to Boston. Many years later I found a soothing letter from Denny to my father who worried about the dread influence of Walter's diverting me to science instead of medicine, and asked Denny to dissuade me from such fell interests.

I went off to the army in January 1944 after serving a 9-month internship which was truncated because of the war. But before going overseas I spent three months training at Bellevue neuropsychiatry in New York. Walter had been coopted by the Kellex Corporation, a branch of the A-bomb project in the Woolworth building. So we took an apartment together. Wiener visited us several times.

Kellex was losing young men to the draft, for there was no provision for sparing scientists in the law. I later accused Sam Wortis, the head of neuropsychiatry at Bellevue, of engineering the appointment of A.A. Brill as chief psychiatrist at the 42nd Street station. He never affirmed it but refused to deny it. Brill not only had issued a poor translation of Freud into English but held to an unshakable belief that dealing in symbols was the mark of schizophrenia. Hence all users of mathematics were, by definition, nuts. In full seriousness he labeled every drafted physicist and mathematician prepsychotic. Just such uncelebrated accidents save important effort in critical times. Walter was enraged by the diagnosis as were his colleagues. Some of them used to meet occasionally in our apartment because they were not permitted by General Groves to talk to each other at work. If the army had known this I would never have been allowed to go overseas.

After I returned from my work as chief of psychiatry in the 237th General Hospital at France, Denny was very cold to me. It was hard to persuade him until much later that I had not turned into a psychiatrist but had been dragooned into shrinkship by the army. Note that at the time one could move into psychiatry from any field of medicine, but not back. Nobody trusted a psychiatrist to do real medicine. Nevertheless, I had been offered good openings but I really wanted to do research on the nervous system.

Educational Interlude

Then a new option was offered. Walter and Wiener felt it was time I learned some science. So Wiener arranged for me to be admitted as a special student on the condition that I would take an overload of courses for a year. At the same time, the Veteran's Administration gave me a paying post as an outpatient psychiatrist in downtown Boston. I accepted and Hy Minsky (then at Harvard), Walter, Oliver Selfridge, and I took a single large room on Beacon Street. There wasn't much money between us; mostly it was my salary from the VA. We kept it in $20 bills between the leaves of Spengler's *Decline of the West* as the last place any thief would look.

That year gave me a freshman introduction to science. I remember most vividly Dirk Struik, who is still alive at almost 103, for his engaging course on differential geometry. But before the end of the academic year I fled to Chicago. Walter had lost a manuscript that Wiener was working on and I decided to take the blow. It was about a month before the end of the term. I was exhausted from the load of five courses along with doing my stint as clinician, and it was obvious to me that I would never be a true scientist. So no sacrifice was involved.

In Chicago, Warren gave me the run of the laboratory and I cadged from my parents for a few months by staying at home. Then Warren suggested that if I wanted to go into research I should apply for a post as physiologist and see what happened. After all, as an M.D. I had credentials. During this time I had been learning methods of physiology at Warren's lab so I could proclaim myself a physiologist. The department of psychology at the University of Rochester hired me to work at inducing motion sickness as an abortifacient for cats. This was because one of the popular methods for inducing abortion at the time was to take a sequence of high rides at amusement parks or a boat cruise in bad weather. It was a silly project, foolishly set up, but there were some good physiologists in other departments who were doing single unit recordings in the auditory nerve and studies on other parts of the nervous system. I learned electronics from John Kanwisher and something about axonology from Bob Taylor, and set up to study vestibular fibers in carp and cat.

Just before I left Chicago for Rochester, I met my wife, Maggie, and two days later proposed. She accepted, over the violent protests of our parents, and we married in Rochester. This was in 1947 and we have been happily in love ever since. She claims to have few if any regrets. I can only bless my luck, for she is certainly the most comely and sympathetic and wise woman imaginable.

I left Rochester after a year, having worked with Wendt's elevator that could bounce cats up and down by sine waves, trapezoid waves, triangular waves, and square waves. It was silly work but I had contracted to do it. In my spare time, however, I had made some then-novel discoveries on the vestibular fibers in cats. But Wendt forbade me to publish them from his department because he positively hated electrical records. So I left to go to Utah where I had been offered a position to do neuropharmacology. But the Utah funds fell through just as I started out by car. We stopped in Chicago where our first son, David Warren, was born within a few days, and now I had to make serious plans.

At Manteno State Hospital, 50 miles south of Chicago, there was a shortage of physicians but a most admirable administrator, Dr. Paul Bay. I made a deal with him that I would serve as neurologist and night physician if he would give me space for a laboratory. He agreed. Warren persuaded Von Neumann to persuade ONR to give me a start-up fund of $5000 to outfit the lab. I built my own amplifiers, stimulators, and animal holders. The only commercial device was an oscilloscope that I modified to give me a linear sweep, and an old Bell and Howell 35-mm movie camera that I fixed to give me single frame records.

Within the year I was joined occasionally by Pat Wall, then an assistant professor of anatomy at the University of Chicago, Paul Dell from the University of Marseilles, and Tony Remond from the Saltpetriere in Paris. Dr. Bay was kind enough to allow them visiting quarters when there were some; otherwise they stayed overnight in our cottage. We worked on spinal cord physiology with particular interest in the effects of the bulbo-reticular inhibitory system. On looking back I regret that we did not publish the work. Incomplete as it was, there were some solid, interesting findings that have not yet been reported by others.

After a while graduate medical students began showing up in search of postdoctorate projects and there were times when the lab was crowded. Walter visited often from Boston and so did Warren from Chicago. Pat Wall and I wanted to go on to study spinal cord physiology with microelectrodes, but such a project would cut severely into my clinical time. I had gone for close to a year on four hours sleep a night. Furthermore, I had neglected Maggie dreadfully.

Incidentally, over the few years I was at Manteno I became a shrink that some members of the Mafia trusted. In return, they did favors for me. Chief among them was an old Cadillac limousine which I could use to round up feral cats that the farmers wanted to see taken away. So I never ordered experimental animals. I had no budget for acquiring or housing them. All experiments were acute.

Clinically I made two innovations. In 1950–1951 I was first to use apomorphine in treating Parkinsonism. In 1950 I was first to use oral myanesin as a human tranquilizer after Elwood Henneman showed its calming effect in oral form on Warren's dog, Puck, who was frightened by thunderstorms. I tried it first on myself in moderately high dose and sat in high good humor for about two hours, giggling. In lesser doses it worked wonderfully on patients. But adaptation to it was rapid and I recommended it be used only once every other day. Becker, meanwhile, had made a version of much lower solubility so that it could not be used in overdose. This issued as Miltown, and, if you remember, was overused for a time as a popular fad. Then it was dropped from the NNR precisely because of the marked adaptation at only two doses per day.

In the meanwhile Maggie languished as a housewife, bore our daughter, Ruth Anna Livia, on April Fool's Day, 1950, and then went into the unenviable suspended animation of a doctor's wife in a madhouse, having nobody to talk with but the ambulatory patients. The other doctors' wives were unapproachable; they didn't even commingle among themselves. By this time she had become seriously deaf from otosclerosis. In retrospect I can only regret how she must have suffered, but there was no treatment for her condition at that time and she retreated into plain servitude. She wanted more children, but every pregnancy worsens the affliction. Meanwhile she took an interest in patients and, with very good judgment, she liberated more women through her back door than would be freed by the clinical board. The hospital had 8000 patients and only eight doctors, two of whom were fairly incapacitated. Of the remaining six, three were refugee German Jewish psychiatrists who had not the simplest knowledge of ordinary people. I mentioned they were Jewish because I remember being taken aback when they refused to release a Polish inmate who gave his profession as "Shabbas goy." They would not believe me that there was such a calling, and I had to bring a sociologist from the University of Chicago to verify it.

Although one member of the board was a woman, they were chary of releasing women inmates who had been immured on almost no grounds except the malice of a relative or some other triviality. That is where Maggie rightly stepped in. Dr. Bay knew what she was doing and tacitly approved.

Translation to MIT

Late in 1950, Wiener had persuaded the Research Lab of Electronics (RLE) at MIT to import nervous physiology. Warren, Pat, and I were invited to come as research personnel, not faculty. Warren and Pat resigned their academic posts and the three of us metastasized to MIT in 1951 to be joined there by Walter. A separate group was set up by Rosenblith, who came from Harvard. Jerry Wiesner, above all, was our protector and guide.

By 1955 we produced our first major work in the physiology of the spinal cord, a set of momentary maps of sources and sinks of current at various times after a dorsal root volley. The task was arduous experimentally but yet more arduous computationally. It took several people close to a year to process the data. Today we could have had the results in less than an hour. The study showed that a volley in one root had a profound and protracted effect on the results of a volley delivered in an adjacent root some 10 ms later. The effect of the first volley on one root was to block for many milliseconds the invasion of a second volley onto collaterals of an adjacent root. This interaction occurred far presynaptically. Later Eccles discovered the same effect by other methods.

Bob Gesteland had come over from General Radio as our first doctorate student at Walter's suggestion and addressed the problem of recording from single units in the frog's olfactory epithelium. It took some time to work out a clean air delivery system that carried sharply limited olfactant pulses, but his admirable work gave the first records of single unit activity in this system. He has since become a notable authority on the sensory processes in smell.

At about this time Brad Howland appeared. I have thought of putting him into the Guinness Book of Records as the only graduate student who stayed as such for 31 years. He was a brilliant inventor and engineer whose specialty was optics. Self-supporting, he saw no reason to engage with time-wasting society and he proposed to remain in the comfortable Teflon tower of MIT as long as he could. He had his own laboratory down the hall. There is no space here to describe his clever work.

Familial Interlude

I break off to tell of my family. In 1952 Maggie had a fenestration on her left ear that made her hearing better than mine. She flowered marvelously thereafter. In 1954 our third child was born, Jonathan Democritus. I was spending so much time in the lab that Maggie had the full burden of raising our kids. Then something occurred that foreshadowed her future role.

One of my best friends at MIT was Giorgio de Santillana, the historian of ideas. He was a most learned and kindly man with a mordant wit. Walter, Wiener, and I often hung out at his office. Giorgio was a past-master at fortune-telling with the Tarot. Wiener loved having his fortune told. Giorgio vainly tried to persuade him that the Tarot should be a rare and sometime thing to be used only in crisis, but Wiener would have none of such excuses. For example, Walter and I used it when we started a new experimental venture. There's nothing mystical about it—it brings up, by chance, associations that you do not ordinarily consider and in that way serves to break the constraints that hemmed your thinking. It is a charming way of introducing overlooked contingencies. So every month or so, Giorgio would give in to Wiener and come up with a fortune together with a complex character analysis. At the end of the reading, Wiener would exclaim "But that's not me, that's X (or Y or Z)" where X, Y, or Z were fellow mathematicians. Giorgio would shrug and say "You may have been thinking of them when you picked the card."

Anyway, one evening in the mid-1950s, when Maggie and I visited him, he offered to tell her fortune for he liked her. He read it to show that by age 40 she would become famous, author several books, and be popularly admired for her wisdom. I think he was as startled by this as was she. Giorgio never flattered anyone—but there it was in the cards. She never forgot that evening after the predictions all came true. But by that time Giorgio was dying and she could not tell him how right he was.

Walter's Tragedy (Expurgated)

Returning to the conditions at MIT Walter, as I said, fled his home in his early times because of his father. Warren and I were his close friends but Wiener became the father he never had. One day, in 1952, Wiener sent from Mexico City a registered letter to Jerry Wiesner severing forever all relations with Warren, Walter, and me. Years later, Arturo Rosenblith, at whose house the Wieners had stayed at the time, gave an account of what happened. It is a shameful story; Wiener himself was as much a victim as we. The tale is not worth telling here. But the anathema destroyed Walter. He gradually and politely bowed out of being, lost interest in pretty much everything, and became inaccessible. After the few years that Warren and Rook, then Maggie and I, took him in, he did what he could to disappear in a small rented room where he died after years of unrelieved despair. He had burnt everything he ever wrote.

I have often wondered whether I could have done anything. After all, as psychiatrist I could have a found a colleague to take him on. But the problem was that Walter would not go, and in any case was dead of despair long before he died. Wiener was unapproachable on the matter by any of our many mutual friends.

The Frog's Eye

After 1955, I was drawn to study frog vision by a young visitor from Scotland, Alex Andrew, who thought physiological work on cats barbaric. Part way into the study he extended his proscription to frogs, mainly because his future wife was a firm anti-vivisectionist. However, we had begun to have some interesting results and I was reluctant to abandon the work. By that time I had met Humberto Maturana, whose doctorate thesis at Harvard on the electron microscopy of the frog optic nerve showed that only 3% of the half-million fibers were myelinated. I invited him to join the laboratory and within a year we had worked out the method of recording from single unmyelinated optic nerve fibers of diameter 0.1–0.5μ mi. Umberto had a delicacy in surgery that I could not achieve, a broad learning, and a keen mind. I thought we worked well together but afterward his wife told Maggie that he would arrive home in a fury, unable to sleep for cursing at me. In retrospect, I don't blame him. Nevertheless, the work got done by 1957. Warren and Walter pointed out questions we had to answer by designing new tests and classifying our results. Oliver Selfridge helped write the paper.

One aspect of our now well-known paper, "What the Frog's Eye Tells the Frog's Brain," (for which title I am much indebted to John Moore) is that nobody to my knowledge has ever repeated the recording of the unmyelinated fibers in optic nerve. As Arthur Grant and I showed in 1991, the easily recorded signals in tectal neuropil are the responses from active glomeruli, the dendritic appendages of tectal cells. Their activity represents the correlated activity of many neighboring retinal ganglion cells of the same type. The pulse trains of glomeruli are readily distinguished from those of single optic nerve fibers in pattern and duration.

Some Details of Work on the Frog's Eye

One of the deficits in previous papers on the frog optic nerve was that the accounts of Hartline and Barlow were obviously on the easily recorded myelinated fibers. The receptive fields were large, and though Barlow made a good attempt at dredging visual resolution from possible higher order processing of the combinations of field operations, it was not persuasive. Until we could record from the unmyelinated fibers which were 30-fold greater in number, there was no way of qualifying the retinal output. We did not want to poke electrodes through the vitreous or through the retina to record from the small ganglion cells (whose axons are unmyelinated) because the presence of such an electrode itself distorts the images in the neighborhood.

A good deal of research consists of endless floundering until a lucky accident happens. It was well over a half year before we recorded our first unmyelinated fiber by accident. At that time the conventional way of telling whether you were recording an optic fiber was switching light on and off and seeing if a signal occurred. After all, the retina is photosensitive. If there was no signal the electrode was thrust further. As Umberto moved his hand to advance the micromanipulator there was a mutter of activity on the loudspeaker. He stopped. Again there was no response to turning the light on and off, but again, moving his hand as if to touch the manipulator evoked the mutter. It did not take long, with minute excursions of the tip, to maximize transients well above the noise level. With various small targets, moved about in jerks, we could estimate the size of the center receptive field at 3–5°, much smaller than the fields previously recorded.

Pastiche of the Decades

Most of my own work has not been submitted for publication except as students or colleagues were involved. Since this is an autobiography rather than an official history, I can dally on some of those peripheral efforts and tell of two projects. The first arose from Steve Raymond's doctorate thesis in which he showed by an elegant new "threshold hunting" method the course of threshold change in a single axon after an impulse. The aftereffects of an impulse can be tracked for a long time, but, for practical purposes, Steve limited his observation to a few seconds. The aftereffects of a short train can endure for half an hour.

These aftereffects of a pulse on threshold of a nerve fiber are of two sorts after the refractory period: a short saturating period of hyperexcitability, and a long, nonsaturating period of depression. The first does not build up with repeated stimuli, the second does; the decay time of the latter increases markedly with the magnitude attained.

The existence of these two phases suggested a new approach to the decoding of information encoded in pulse-interval patterns. It had long been known that the impulse down an axon usually invades only a fraction of terminal synapses. This can be shown by collision experiments where an orthodromic pulse invading the tree allows an antidromic impulse to pass back into the axon from stimulation at the synaptic region. In axons that have been post-tetanically potentiated, at the peak of the potentiation the whole tree must be invaded since none of the anti-dromic impulses get through. The normal partial invasion of the whole tree under ordinary conditions sets the problem: what determines if a branch is invaded? At a bifurcation, there is a general principle that seems to hold for all branching systems, that the two twigs into which a branch divides are asymmetric in diameter. At the same time, the safety factor of impulse transmission drops markedly at bifurcation.

Since the time courses of the same membrane processes change with the surface/volume relations of a tube, it occurred to us that bifurcation of an axon along with the low safety factor makes a two-bit switch which depends on the time since the last invasion of the tree. In that case, the axonal tree becomes a decoder of pulse-interval codes, giving a history-dependent sequence of the partial invasions of the terminal synaptic field. Steve Raymond and Paul Pangaro did a nice movie of the threshold aftereffects and their role in temporal coding.

The first time I proposed this, a talented young mathematician, Rusty Bobrow, chose to do a doctorate thesis on the model. He was already getting some interesting results with a simple tree of eight terminals when the math department disallowed the project for not being certain enough of a result. It was not a well-defined problem. So Rusty quit and went on to make a name for himself at Bolt, Beranek, & Newman, a prestigious company in systems analysis and design.

The second time, two decades later, the same problem in another form won a doctorate for Gill Pratt from the department of electrical engineering and computer science where he is now assistant professor. But then it was too late for me to set up a physiological study to show its application, even though Gill and Arthur Grant and I showed the systematic spectral coding of light by a single fiber from the frog's eye, the type that goes to the thalamic nucleus.

The second project that I look back on joyfully and regretfully has to do with stentors and involved Eric Newman, now full professor at the University of Minnesota. Using Vance Tartar's method we fused pairs of stentors every which way. Stentors are single-celled trumpet-shaped protozoa which are wonderful to watch. They do many distinct acts: elongate or contract, make a hold-fast at the narrow end or retract it, move their membranelles clockwise or anticlockwise in changing the vortex that brings food or rejects unpalatable particles, change the program of beating in the cilia along their bodies, ingest or "spit away" the particles brought to the gullet by the membranelles' vortex, reverse swimming direction when bright light plays on the "eye-spot" in the field (the head end), etc. Their behavior has several such distinct patterns. What was astonishing to observe was that in a fused pair of stentors a master-slave relation developed. Whatever the master stentor did, the slave stentor did, although it had received no stimulus. If the master changed membranelle motion, so did the slave; if it twisted itself in one direction so did the slave; if it swallowed a bit of food, so did the slave swallow; if it put out or retracted a hold-fast, so did the slave; and so on. The correlation of behavior, while never complete, was so well above chance as to be unmistakable.

There is no nervous system in stentor, and the pair was connected at only one point anywhere along the bodies of the two. How are the different kinds of act communicated until the stentors finally separate themselves as they do (when not paired side-by-side to make a single stable doublet that propagates itself by fission)? Eric wisely saw this as an endless venture and switched to the analysis of current generation of the frog retinogram for his doctorate. But it was a wonderful problem, this communication of many distinct patterns of behavior between conjoined stentors.

I have remained haunted by this memory, delighted to have made the observations, regretful at not having pursued the matter further.

There are about a dozen such strange projects that occupied some of the time, were clearly worth taking to some presentable form, but could not be carried out to publishable state in a guaranteed way. At its peak the laboratory had about six or seven graduate students and postdocs working on clearly defined problems and there was room for much diversion (although I regarded it all as serious play).

Around the mid-1960s we lacked money for new apparatus. Brad Howland (my permanent graduate student) and I decided to remedy the deficit by making some ready cash. Miller and Licklider had published two papers. In one they showed that the highest intelligibility of chopped speech (sound alternating with silence) lay around a chopping rate of 100 Hz. In the other paper, they showed that speech versus noise gave highest unintelligibility at that same chopping rate.

We instantly cooked up an anti-wire-tapping device for the telephones of bookies. Their problem was to receive track information in a timely fashion, but since that was illegal, to avoid identification by a wire-tap. Since the speaker at the track does not have to listen to what he says, the receiver can chop with silence what he hears, and in the silent phases insert noise on the line.

The most excellent confusion was had by using as noise yesterday's race results recorded by the same speaker and an irregular chopping rate driven by thermal noise in the 90–110 Hz band. We tried it out and it worked well. For evidentiary purposes in court, the wiretap was useless (These were the days before computers were widely available).

I called a Mafia friend and asked him to find out if the bookies in New York would pay $10,000 for the gadget. After he confirmed this, Brad and I went to Henry Zimmerman, then head of RLE, to get his permission. This was Tuesday and the delivery of the device had been set for Saturday. Henry was amused and consented. But on Friday we were recalled to his office. There were two colonels. They asked to see the document that went with the gadget. We showed it to them. Then one pulled a stamp from his pocket, marked the paper TOP SECRET, and forbade us from the sale. Brad and I had to call my contact to tell him that the device didn't work. Louie knew me well, so he called that evening and asked if Maggie and I would go to the movies, leaving the device and paper in my desk at home. There would be a burglary and a nice surprise left behind. I called Brad to see how he felt about it. He advised that we chicken out. So we did.

Several historians of cryptology have tried to get access to our paper, but Henry told us that somehow it has disappeared. Oh well.

Later on the Chicago Mafia offered to give the lab $30,000, no strings attached. Jerry Wiesner was president then, and I told him of the offer. He said "absolutely not" in no uncertain a tone. But then, as I was leaving, he said thoughtfully, "Now, if it were 300k. . ." We both laughed.

I cannot forbear mentioning one success that got quashed. I had gone to lecture in Hawaii and while I was snorkeling one day with my son Jonathan, he called my attention to a flounder that was slithering along the bottom. I didn't know the species. When the flounder crossed a rock larger than it, it seemed transparent, as if the rock were somehow visible through it. After I reported this to the students back at the lab they accused me of finding a new way of smoking pot under water.

Bill Saidel, however, decided to investigate the coloring of northern flounders. In 1914 someone had photographed a flounder against four checkered bottoms of markedly different periodicities. It seemed evident from looking at the flounder against the bottom that it carried the periodicity of the bottom across its back. Now the eyes of the flounder are atop the flat head. It is hard to imagine how the flounder, resting on the bottom, can match the bottom periodicity by its vision, just as the flounder I saw crossing the rock seemed to complete the rock across its back.

Saidel took as his doctorate topic the physiology of the pigment movement in the pigmented cells of the flounder skin. This satisfied his thesis committee in the biology department. He bent to it with a will. But meanwhile, he made a startling discovery. When the old pictures were reproduced carefully (they weren't that good to begin with) and the flounder in each picture was nicely cut out from the background, all of the four flounder images were practically identical. There was no sign of any checkering period. They differed slightly in contrast as if the photos were differently developed, but that was all.

What seemed the case is that the apparent periodicity across the back of the flounder is filled in by the observer. The transparent long fins on either side of the flatfish are flecked by the same spots of varied size as over the body so that the background checkering is blended with the overlaid spots. In this smooth transition to the opaque body of the flounder, the surrounding periodicity is subjectively imposed by the observer on the spotted texture of the body.

There are several well-known optical "illusions" that have this character. For example, if you set a TV set to an empty channel, the screen is cluttered with random visual noise. When you intrude your finger over the screen from one edge and slowly wiggle it up and down, it is as if a ghost of the finger protracts itself ray-like well across the screen, a form construed from the noise.

The art of speckling over the northern flounder's back is not that it portrays periodicities that the flounder can ill see, but rather that it leads you to protract into it periodicities that you can well see. That is great art indeed, and we were all enchanted by Saidel's wit and demonstration.

He completed his thesis on the mechanism of pigment migration in the skin cells of the flounder to the satisfaction of the biology department. But they struck from the thesis the whole appendix that carried the story of the induction of form into texture by the observer. To the department it was a wild speculation in psychology which had no place in biology. That deletion from his thesis was a bad blow to Bill. He published it two decades later in an obscure German zoological journal, but so tersely and cautiously written that one could barely grasp the point. He never fully recovered from the departmental snub.

Enough of these stories. Over three decades (1956–1986) of flourishing and florid ventures, more flowers for the imagination than food for thought, the more serious funded research work of the laboratory went on. We were kept going by funds from NIH, the Air Force, and, blessedly, Bell Laboratories which seemed to get more of a kick from our strange work than from our official missions. The bibliography of the laboratory is as variegated as it could be, and a source of the great pride I have in my students and postdocs. I say bluntly and sincerely that I served more as a catalyst to their ideas than as a source. They were a highly talented lot. The Research Laboratory of Electronics at MIT was a garden for ideas, with the richest intellectual soil that could be imagined, and a great many of the developments that MIT boasts were first nurtured there. But that would take a separate set of volumes.

Maggie

True to Giorgio's tarot reading, Maggie did become a famous figure by accident. Unable to move her arms for several months after a severe whiplash when she was back-ended at a traffic stop, she refused surgery. Instead, she studied my anatomy books and worked out her own therapy on mechanical grounds. The word spread and in a short time she helped so many students and staff at MIT with their back problems that they persuaded her in the 1960s to start a self-fitness class at MIT. By the end of the year there were 200 people a day taking her classes. Then Channel 4 picked her up, then Channel 2 (PBS), and her set of daily shows attracted a huge following. They were repeated for 17 years on TV. She wrote four books, of which one, *Maggie's Back Book,* is still in print a quarter-century later. When it first appeared, the great neurologist, Denny-Brown, ordinarily undemonstrative, embraced her at a party saying she had finally rid Boston of needless back surgery. Everyone was astonished and she loved it.

She is now finishing an expert system for the web, devoted to showing how to rid yourself of back pain.

Our three children have successful careers in fields they chose carefully to be those I know nothing about. Of our grandchildren, one is at Microsoft, one is an animator, one is writing for TV and films, one is a boxer (she also studies philosophy), and two have yet to graduate high school.

Bexley

In the late 1960s and early 1970s, Maggie and I spent six years as "houseparents" at the most intractable dormitory at MIT, Bexley Hall. One of these days Maggie will write her memoirs of that time under the title "Any Sport in a Dorm."

I can talk about two odd episodes here. Bexley had wire taps on the Cambridge Police lines, on an FBI office, and on a CIA observing office at 545 Main Street. Everyone assures us this must be false—such offices are not permitted on U.S. soil. But 545 Main Street was the home of advanced work in cryptography, artificial intelligence, and all those strange computer arts that spawned the modern age. The CIA would have been remiss if it had no presence in the shadows.

At any rate, a premed student on the 4th floor of Bexley was secretly financing his future career by manufacturing LSD which he incorporated into sugar cubes or thin gelatin sheets. One of his shipments to New York broke open at the post office. The damn fool put his return address on it, but not his name. Our students got wind of a federal raid on Bexley from their tap on the police line. In the morning, when the forces showed up, a large hand had been mounted in the courtyard pointing the feds to the proper entrance, and all along the staircase there were careful directions to the room they were to raid. Of course, everything had been cleaned up. But the next week was out of a Keystone comedy. Agents with binoculars lurked behind the windows in the ship museum across the street. Students, mounted on our roof, used ridiculously large binoculars to watch the agents. Various jokers, clad suspiciously, slouched in guilty wariness along the street. In the end, everyone gave up the nonsense. Meanwhile the premed student stopped cold when threatened by us with putting the episode on record.

As I say, this is one of the less involved incidents that over the next six years both enlivened and exhausted us. The images of our stay there are among the most vivid in our memories.

I had known Tim Leary (the LSD guru) for awhile. He once visited the lab to describe his allowed pot sessions at Concord Reformatory (officials deny it now). He had accumulated evidence that those who smoked pot in the jail had the lowest recidivism, and argued "post hoc, propter hoc." I don't know the details because he never published them. Before we went to Bexley, Tim was brought at some expense by the student lecture committee to give his argument for "Turn off, tune in, drop out," the incentive to drug use. There were no funds left over to bring in a contrarily minded notable so at the last minute, I was asked to fill in.

It was a memorable evening. At the end Tim muttered to me "that's the last time I debate a Jew." We both laughed. But the debate got instant attention by the press. The *Boston Globe* extolled me in an editorial, and *Variety* reported it as the first time "bull shit" was ever uttered unbleeped on TV. It was nice to be a hero for a few days. High schools throughout the country used the replay in the war against drugs.

Current Work

Gadi Geiger joined the laboratory 15 years ago. He and I began a study of "lateral masking" in human vision. A remarkable set of findings gave us a new non-reading visual test for dyslexia. The test, in turn, suggested a treatment which was eminently successful in all of the few adult dyslexics on whom we tried it. The treatment cost practically nothing and involved no supervision.

We saw dyslexia as a learned perceptual strategy and showed how it could be unlearned. This is heresy in an era of neurocalvinism, the determination of a disability by a defect in the brain. But then we made controlled experiments on grammar school children and got admirable and convincing results. Nevertheless, although our results have appeared in highly esteemed refereed journals, we are still regarded with suspicion.

The essence of our thesis is that there is a strategy in perception determined by the task to be performed under perceptual guidance. That is to say, the processing of visual information is determined by what use is to be made of it, and changes with that use. This is not a foreign concept in psychology. The works of Richard Held, Ivo Kohler, and many others show different applications of this idea. That there are physiological correlates, and physiological mechanisms involved, goes without saying. But the application of physiological psychology to what are classified as neurological disorders is now beginning to take hold. A fair amount of evidence on the "plasticity" of the continually reconnecting brain has finally become convincing and is changing the face of neurology.

I am glad to have been involved all my life in what interested me because I was given the freedom to pursue what I chose. What luck!