EVE MARDER AUTOBIOGRAPHY

I was born on May 30, 1948 at the Columbia Presbyterian Hospital at 168th St in Manhattan, New York City. By today's standards, my parents were quite young; my father was just 23 and my mother was 21.

My earliest memories come from the days we lived on Manhattan's Upper West Side, on 84th St between Columbus and Amsterdam Avenue. It was walking on Central Park West that my mother taught me that North was uptown, South was downtown, West was the Hudson River, and East was Central Park. My parents were poor then and I remember my mother carrying groceries and my stroller up many flights of stairs when she was pregnant with my brother, who was born in October of 1951.

The first scientific experiment that I remember carrying out was in the 86th St playground. The Central Park playgrounds had vertical bar railings, and at the ripe age of 3 or so, I was curious if my head would fit through the railings. So I did the experiment and got stuck. This was a life-defining moment for me, because as I sat there with my head between the rails I felt unbelievably stupid, as I realized I could have used my hands to measure the relevant distance. As all the mothers at the playground started panicking, my mother calmly walked over, picked me up, twisted my body sidewise, and pushed me through the railing, saying that babies were born head first, and she figured that my head was more important than a few scrapes on my arms or legs. Both parts of this memory were formative, pride in my mother's commonsense problem-solving intelligence, and the reflection that comes with rational self-assessment.

When I was 4 ½, we moved to Ridgefield, New Jersey, just across the Hudson River, to a small Garden Apartment in a modest suburb. My parents made no particular attempt to teach me to read before I started school, and in first grade we were taught to memorize words with the now infamous "Dick, Jane, Spot" textbooks. After a month of first grade my father asked me if I could read, and I said I could read the words I had been taught. He said I either could read or I couldn't and handed me the New York Times and said "read". Thus I discovered that I knew how to read. My mother took me to the small library just next to the school, and I got a library card, and over the next few years I indiscriminately read every book in the children's section of the library. One day as I happened to be reading a science book, my aunt asked me what I wanted to be when I grew up, and I answered "a scientist". She nodded happily, and from then on whenever grownups asked me that question, I said "scientist" because it deflected all further conversation.

When I was 10 ½ we moved to Irvington, New York, a small town on the Hudson River, a 45 minute train ride mid-town Manhattan. My father took the train to work in the city, and I entered the 5th grade and I discovered that I loved stringing words together in essays. In 1960, when I was 12, my sister was born, and my father started a market research firm. I had a marvelous 6th grade teacher. She let us work on our own, and spent most of her time with the students who needed help. A few of us raced to finish the our 6th grade math workbooks, which we did in a few months. She then gave us 8th grade math books. In the spring, when I had finished 8th grade math, she came back with an Algebra book. A few days before the end of school I went up to my teacher and told her I wasn't going to do 8th math again, because I had done it. On the last day of class I was promoted into 8th grade, and placed in 9th grade Algebra, with students 2 years older.

I only realized much later how good Irvington High School was in the early 1960's. I wrote 100 page term papers (in the days of the typewriter), I learned algebra, geometry, calculus, chemistry and physics, and memorized endless kings and queens of European countries, the US presidents, and the amendments to the US constitution (quite relevant today). In advanced biology I studied respiration, photosynthesis, and first learned about action potentials and synaptic potentials from Scientific American articles.

At age 16 when I was applying to college, I loved learning, and I knew that there were many people smarter than I. I was rejected by Radcliffe College (the women's branch of Harvard) after an interviewer told me that they didn't know how to evaluate my grades because they had never seen an application from Irvington High School. When I told her that I thought the whole purpose of the SAT exams was to provide more objective criteria (my SATs were very good), she said, "Well, I guess you are academically strong enough, but we are looking for young women who belong here". I understood that was code for saying that I was a smart Jewish girl from Westchester County but not special enough for them to overlook that I was a smart Jewish girl from Westchester County. When I started college in the fall of 1965 at the then just established Brandeis University, I knew that I would get an excellent education and not be part of the "in-group" provided by an elite institution. The same was true 4 years later when I started graduate school at the even younger UCSD, after being denied admission by Harvard and Stanford. And just as Brandeis provided me an outstanding liberal arts education and allowed me to discover that I wanted to study neuroscience, UCSD gave me the freedom to develop as an independent scientist.

Allen Selverston joined the faculty in biology at UCSD the same year I started in the PhD program. In retrospect, I could not have had a better thesis supervisor. Al knew how to do things, to build things, and how to make things work. He had, even at the beginning of his career, outstanding biological intuition, and the work in his

laboratory presaged much of what is happening today in circuit analysis and connectomics. Most importantly, he provided an environment and space that allowed me to make my own mistakes and my own discoveries. So when I completed my Ph.D., I knew that I had done the work independently, and I credit him, to his day, with understanding that independence was the best gift a thesis supervisor could have given to the student I was at that time.

I had become fascinated with transmitters and receptors as an undergraduate, and it was no accident that my Ph.D. thesis project was to determine the identity of the transmitters used in the STG. My goal was to determine the identity of all of the chemical signaling molecules in a complete circuit, rather than study individual transmitter molecules in isolation. Even then, I had the intuition that there were insights to be gained by trying to understand the choreography of transmitter (and now modulator) action in functionally active circuits. To this day, my laboratory continues to work on these questions.

The early 1970's were simpler days for a graduate student. I read a considerable fraction of what was published on transmitters/receptors/pharmacology in invertebrates. So I discovered Dick McCaman, Jimmy Schwartz, David Barker, JacSue Kehoe, Philippe Ascher, Ed Kravitz, and Hirsh Gershenfeld from the literature. Dick McCamon, Jimmy Schwartz, and Ed Kravitz had pioneered methods of single neuron transmitter identification. Most notably for me, Jimmy and Dick had worked on single neuron choline acetyltransferase assays. Dick was 120 miles up the road, in Los Angeles, so one day I got in my car and drove up to City of Hope to learn his methods. He became a great friend for years. I met Jimmy Schwartz when he came to UCSD for a seminar, and he also became a very special friend, who read and critiqued my first grant proposal several years later. David Barker was a postdoc in Ed Kravitz's lab when he identified octopamine and became interested in amine modulation. It was he who first understood the results I had obtained when I had applied every transmitter candidate known then to neuroscience on the STG. And it was the extraordinarily elegant papers of Kehoe (and subsequently Ascher and Gerschenfeld) that pointed the way to the kind of clean neuropharmacology that I aspired to do. So it was not an accident that on finishing my thesis I decided to spend a year in Oregon in David Barker's lab before I was able to move to Paris to work at the Ecole Normale Superieure, in the Kehoe/Ascher/Gerschenfeld group.

I had studied French in high school and college, but I had never spoken it, or really heard it. Nine years after my last French class I arrived in Paris and couldn't understand a word. Everyone in the lab spoke English, but all lab conversations were uncompromisingly in French, except those that were one-on-one with me. So I learned French for the second time; this time it wasn't the conjugation of the future subjunctive, but it was lab and Parisian slang. While in Paris I had the frustrating sensation that

there was a wall between the part of my brain that had studied French from books when I was in school, and the part of my brain that became reasonably fluent in conversational French. I never lost my American accent and was reminded of that whenever a Parisian cab driver willfully refused to understand the way I pronounced "Rue Descartes".

I came from a laboratory at UCSD part of the "simple systems, circuit cracking" field (doing then with electrophysiology and dye-fills what people today are doing today genetic lines, optogenetics, etc in flies, worms, and mice) and I had entered the world of channel biophysics. Consequently, I had to learn French and biophysics at the same time, just to follow the lunch time conversations at the lunch canteen. When I left for my foreign postdoc "they" told me I would never get a job from abroad, but I had two offers for good faculty positions, and I returned to the US in the fall of 1978 to start as an Assistant Professor at Brandeis.

Having lived in the 4th and 5th Arrondissments in Paris, I immediately looked for places to live in the most urban environment I could find (walking distance to films, coffee houses, and books stores). After a month of desperate looking for a rental, I ended up buying a tiny place in the heart of Harvard Square with wood floors, fire place, white walls, a minimal kitchen and a sketchy bathroom. But it had big bow windows and a southern exposure, and given that I had no furniture, the fact that it was really small was an asset, and after all, I was returning from tiny Parisian apartments. I had no money, and ended up with two mortgages. By today's standards the $27,500 price tag was nothing, but my salary was $16,000/year (by today's standards also nothing), and there were months that I would have to put together quarters and pennies to buy coffee or milk. But I loved that little apartment, and being three blocks from the middle of Harvard Square. I also explored the rest of Boston, and wistfully looked at the beautiful buildings in the Back Bay (where we later lived for 20 years) and used to go to the Italian section of town (the North End) when I missed France and Italy. We still go to the Café Vittoria, which I discovered in 1978 had the best cappuccino in Boston.

It was eerie to return as a faculty member to the place I had left as an undergraduate 9 years previously. In some ways the Brandeis campus was much the same, in other ways it was totally changed, and not for the better. I returned to the consequences of 9 years of deferred maintenance. Buildings that had been new when I left in 1969 had leaking roofs and were unpainted in 1978. I had left a campus of 1800 undergraduates and came back to one whose enrollments had been expanded to cover budget deficits. There were new buildings, and new faces. Happily, the senior faculty members who remembered me were wise enough to leave me to find my own way.

When I arrived in my new lab it was filled with years of departmental detritus. Eventually, I was able to set up our first rig, purchased with grants from the McKnight

Foundation and the NSF. Chris Lingle (today an eminent channel biophysicist at Washington University), whom I had met when I was in Oregon, joined the lab as a postdoc. Shortly thereafter, I benefitted from a terrible tragedy: Fred Lang, a wonderful developmental neurobiologist working at Woods Hole, was killed by a drunk driver, leaving 8 graduate students, including Judith Eisen (now an eminent developmental neurobiologist at the University of Oregon) who joined my lab. Judith was already an accomplished scientist/student, and within a few months, we were working together as peers and colleagues. Soon afterwards, I hired Michael O'Neil as a technican (several years later he wrote our first dynamic clamp program) and Scott Hooper (Professor at Ohio University and Univ of Cologne) joined us as my second Ph.D. student. I remember walking around the lab one evening to discover 3 rigs were running simultaneously, wondering how it had all happened.

I received tenure in 1984 after our first papers on neuromodulation were published. Six years later, I was promoted to Professor, for work that continued our studies of neuromodulation. By this time, Arthur Wingfield (my husband whom I met in 1981 at Brandeis) and I had moved from the tiny apartment in Harvard Square to a dramatic 5th floor apartment in the heart of the Back Bay.

In the late 1980's I started working with theorists, Nancy Kopell, Irv Epstein, and Larry Abbott. The collaboration with Larry Abbott was quite special, and led to the first model of activity-dependent regulation of intrinsic excitability, or homeostatic regulation in 1993. Gina Turrigiano was then a postdoc, and when she left to start her own laboratory, Gina switched to rat cultured neurons, and she and Sacha Nelson soon built on the initial concept of homeostasis and extended it to synaptic strength, or synaptic scaling. In due time this work led us to both computational and experimental work on degenerate or multiple solutions to neuron and network behavior, and in turn to animal-to-animal variability, the problems we study today, with neuromodulation and homeostasis, using both computational and experimental methods.

Arthur and I now live on the Boston waterfront where we see glorious sunsets and watch the endless varieties of boats, large and small. This fall of 2016 I am once again teaching "Principles of Neuroscience" to 90 students. I love this course because it starts with the fundamental principles of cellular neuroscience and then moves to explore how cellular mechanisms contribute to higher level processes. I enjoy watching the light go on in students' eyes as they understand a new concept. There is joy in watching undergraduates and graduate students in the lab develop as mature and independent scientists. I am privileged to see ex-postdocs now successfully running their own laboratories, or else in other positions. As I think back on my career, I can point to numerous contributions of my lab trainees that took us in new directions and pointed us to fundamental insights that I would not have stumbled upon without their

creativity, technical know-how, and intelligence. Indeed, my role has been to hear the messages found in the work my lab has carried out. For them, I am eternally grateful.

I have stayed at Brandeis University for 38 years. I have stayed largely because of my colleagues, who are wicked smart, compassionate, have great senses of humor, and who share a belief that the creation of new knowledge is one of the most important of human endeavors. Sometimes when I walk back to the rig rooms and watch recordings of the rhythms of our preparations I find myself in the same state of wonder that I first felt as a beginning graduate student. Peeking into the mysteries of life never gets old.