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György Buzsáki

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György Buzsáki

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University of Pécs, Hungary, MD, Medicine (1974)
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University of Texas, San Antonio, Postdoc, Neuroscience (1980–1981)
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Assistant-Associate Professor, Institute of Physiology, University of Pécs, Hungary (1975–1988)
Associate Professor in Residence, Department of Neurosciences, University of California, San Diego (1989–1990)
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Krieg Cortical Discoverer Award, The Cajal Club (2001)
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The Hans-Lukas Teuber Lecture, MIT, Boston (2009)
The Talairach Lecture, Organization of Human Brain Mapping (2010)
Brain Prize, Lundbeck Foundation, Denmark (2011)
Elected Member, Academia Europaea (2012)
Most Cited Researcher, ISI (2006–present)
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Doctor Honoris Causa, University of Aix-Marseille, France; University of Kaposvár, Hungary; University of Pécs, Hungary (2015, 2017)
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Special Lecture, Society for Neuroscience (2019)
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György Buzsáki defined synaptic-cellular mechanisms of hippocampal sharp waves and theta and gamma oscillations. His theories and innovative methods have turned studies of brain rhythms into one of the most active research areas. Buzsáki's work changed how we think about information encoding ("neural syntax") in healthy and diseased brains. His most influential work is known as the two-stage model of memory trace consolidation. During learning, inputs transiently modify hippocampal network. In turn, the time-compressed tagged events are repeated hundreds of times during sleep to consolidate memories. Buzsáki has been a strong advocate for studying spontaneous brain activity in its natural state, such as sleep and for brain-body interactions as the evolutionary source of cognition. His demonstration that in the absence of changing environmental signals, cortical circuits continuously generate self-organized cell assembly sequences, specific to recall or the animal's route planning, is a breakthrough for the neuronal assembly basis of cognitive functions.

György Buzsáki

One's accomplishments are judged by others rather than by the first-person reminiscence of the storyteller because we all suffer from source-memory problems (i.e., whether an idea was genuinely ours or creep into our brains unnoticed). Thus, autobiographies should not be trusted as a reliable source of events because of the unreliability of memory "traces." If our memories were accurate representations of life events (analogous to movies), we could regard them as "things" that could be made, reliably recalled, replaced, repaired, exchanged, cloned, or even stored for eternity in some immortalized format and downloaded into someone else's brain. However, this movie metaphor is misleading. A movie, once created, exists in isolation and does not interact with anything or anywhere else anymore. It can be watched many times without altering its content. In contrast, when our life experiences are relived, they inevitably are mixed with experiences acquired between the last and the currently recalled events. Such perpetual modification of the trace may go on even without conscious recall in our sleep. This is the neuroscience of memory, the field I have been trying to contribute to and against which personal recollections should be evaluated. But perhaps instead of this long paragraph, I should have introduced my problem more poetically: "When we remember something, do we remember the thing itself or just the last time we remembered it?" (Jorge Luis Borges, *Funes the Memorious*).

Upbringing

I was 11 years old when I killed my best friend. The next day, I went to school as if nothing happened. Neither my classmates nor my teachers asked any questions, and our neighbors, who surely knew about the details of the previous day's events, did not accuse me of wrongdoing. I did not commit the crime alone but with my family's involvement. It was my father who stabbed my friend's heart with a sharp bayonet he kept in a special drawer since his days in the army. I only assisted. This was the norm of life in my part of the world back then. My best friend, whom we killed on that cold January day in 1960, was Rüszü, our pig. During his life, he and I were bonded friends. Rüszü was always eager to get out of his pen and follow me to our favorite destination, a small, shallow bay of Lake Balaton, Hungary, just across the street from our house (Figure 1). Although many readers may regard pig slaughtering as a barbaric practice, it played an essential role in the lives of Europeans for centuries. First, home breeding and slaughtering of pigs was an economic necessity. Second, a pig slaughter feast was a social event, often linked to a family wedding or christening. Third, and perhaps most important, the entire pig was processed from skin to intestines and nothing was discarded. Instead, the various parts were treated with time-tested methods, such as smoking, and there was a strict choreography for their consumption, starting with fresh meat in winter months, followed by smoked ham at Easter, and ending with pork rinds with onion or scallion in warmer months. When I disliked some parts, my father firmly reminded me that food I was trying to refuse was part of our beloved pig and no part should end up as trash under any circumstance. We must remember and respect the animals we killed.

Only many years later, I began to confront myself with the events of that January day, especially because in our profession, we face the same moral dilemma with our laboratory animals. In my childhood, people had a close and caring relationship with their farm animals. I can say with confidence that today, this is true for our laboratory animals. Animals in the wild are eaten up alive by others, die of hunger, freeze to death, or succumb to disease. In contrast, our laboratory animals live in a comfortable environment with plenty of nourishing food, often surrounded by their littermates. At some point in their lives, we sacrifice them under humane conditions. We give them life and take it away. This is exactly what our imagined Gods do to us. God allows us to enjoy a few happy moments before "calling us back home" when, according to him (her, they?), our time is up. Of course, we are not Gods and thus do not have the same rights, but the analogy remains. If we put aside the analogy, the next moral issue is whether we have the right to exploit other animals for our benefit. Perhaps nobody has a good answer, but I feel this reasoning is a bit of hypocrisy. We do this to our young all the time. We rob them of their freedom and send them to war to sacrifice their lives for us. How is this practice different from sacrificing animals we care for and love while they live? I am writing these lines at a time when in Ukraine, Sudan, and other parts of the world, thousands of young lives are extinguished regularly under the banner of high moral values. It is in this larger cultural, historical context within which we have to make judgments. Such contexts shape our brains and morals, and unless we are acutely aware of this truth, we may rush to quick conclusions about our students and associates coming from different cultures. Emphasizing this cultural embeddedness of the brain in today's polarized world when we deny opportunities or break our colleagues' careers in the name of "here and now" morals is as important as ever.

My childhood included many animals: turtles, a family of hedgehogs, fish in a toilet tank, pigeons, barn owls, and rabbits, in addition to our family's cats and chickens. Lake Balaton was the source of many happy moments in my early life. It provided everything I needed: swimming during the summer, skating in the winter, and fishing most of the year around. I lived in complete freedom, growing up in the streets with other kids from the neighborhood. We made up rules, invented games, and built fortresses from

Fig. 1. The house of my childhood. In my memory store, it was a "huge palace" with spacious large rooms, and this is how I described its features to my daughters. Not surprisingly, they were disappointed when I showed it to them sixty years later. To clarify my betrayed memory, I had to explain to them that the brain always calculates ratios and that subjective spatial scale is proportional to the number of steps we take. This is why my palace shrank. (Picture taken in 2019.)

rocks and abandoned building materials to defend our territory against imagined invaders. We wandered around the reeds, losing ourselves only to find our way out, developing a sense of direction and self-reliance. My parents had only one rule: "Be home before it gets dark." The definition of darkness was, of course, negotiable. I grew up in a child's paradise, even if those years were the worst times of the communist dictatorship for my parents' generation.

Summers were special. My parents rented out our two bedrooms, bathroom, and kitchen to vacationers from Budapest, and we temporarily moved up to the attic and I was sent for a few weeks to stay with my maternal grandparents in Szekszárd. My grandpa was a farmer and a jack-of-all-trades. I undoubtedly inherited my manual dexterity from him. He was a happy, proud, and forward-looking individual. That is until the day when a group of people with red armbands showed up and "nationalized" all their land and property, forcing grandma and grandpa to join "collectivized farming" under the glorious Stalinist socialist plan. This was the first and last time I had seen my grandpa cry. My only benefit from the tragedy was that he started to talk about his life. As a young cavalryman, he fought in the infamous 12-battle war in the Isonzo River valley during World War I, where his beloved brother was killed. During World War II, he survived the battle of Stalingrad and became a prisoner-of-war (POW). He told me how they had to march day and night through Ukraine, sleeping while walking and being prevented from falling by fellow POWs. While serving in the army, my grandfather had a sidekick, György Buzsáki, who was 19 years younger than him. György, mainly out of respect for my grandfather, married his only daughter.

I have never met my paternal grandfather. He died at a young age as a war hero and received a "knight" award (Vitéz Buzsáki). The honor of the Vitéz prefix of my grandfather became a significant burden on my father in the Stalinist era. My grandmother raised her four children alone in a deserted old farmhouse. Their source of water was the rain and a shadoof next to the pigsty and a giant chestnut tree that provided shade (and leaves falling into the well). When I enquired why there were frogs in the well, I got the answer that if the frogs were alive, the water was drinkable. Satisfied with the answer I always enjoyed pulling up the bucket and drinking from this special water.

The rest of my summer days were spent in Siófok. Once my father told me that one of the vacationers was a "scientist-philosopher" who knew everything. I wondered how it would be possible to know everything. I took every opportunity to follow him around to figure out whether I could discover something special about his head or eyes. But he appeared to be a regular guy with a good sense of humor. I asked him what Rüszü thought about me and why he could not talk to me. He gave me a long answer with many words that I did not understand, and at the end, he victoriously announced, "Now you know." Yet I did not and kept wondering whether my pig friend's seemingly affectionate love was the same as my feelings for him. Perhaps my scientist knew the answer, but I did not understand the words he used. This was the beginning of my problem with words used in scientific explanations. Only much later I realized that too often, when we do not understand something, we often make up a word or two and pretend that those words solve the mystery. I guess I became a scientist as a consequence of striving to understand the true meaning behind explanations. Of course, there were other reasons as well.

A short walk from us lived an electrician who generously allowed me to hang around in his workshop. He was patient enough to explain to me why there is a coil in a bell. I assume it was his advice to my parents to present me my first scientific book *How Does the Transistor Work?* as a Christmas present. This book was a dialogue between the Curious Kid and the Big Smart Head with many cartoon illustrations and offered explanations of difficult concepts. I read it multiple times until I memorized every word. Not having access to such a piece of wonder as the transistor, I built a crystal detector radio, soldered wires to the earpiece of a broken telephone set, and stretched a wire antenna between the apricot tree and the pigsty. And *voila*, I could listen to the local radio through the sizzling membrane of the earpiece in our abandoned chicken coop (my "workshop"), which was much more fun than sitting next to my parents' radio in the house. Once my electrician adviser and I rode our bikes to a huge antenna tower outside Siófok. There, an engineer showed us what I thought was surely a magic trick. He connected strangled wires to a light bulb which lit up and dimmed depending on how he was folding the wires. If he told me he was God, I would have believed him. Unfortunately, my friendly adviser was electrocuted later that year by touching a bare wire and standing barefoot in his shack. After that sad event, my parents tried to talk me out of everything that had to do with electricity. But I had already tasted the apple, and my life was about to change in a major way.

My mother always wanted to return to the city, which meant Pécs for her, where she spent a few years of her youth and "which will provide the necessary education and culture" for me and my sister, Judit. Mom was the progressive member of our family and tried to fulfill her dreams through her children. Without her dedication and sacrifice, I wonder how my life would have been shaped. While my parents were packing our belongings, I joined my good friends from the neighborhood for a last soccer game. Trying to save the ball from entering the goal, I jumped on it and tried to turn it backward, at which moment the laws of physics decided what happened next. I flew forward, hitting the telephone pole, which served as part of the goal head first. My friends walked me home. I lay down on a bed that was still left in the house and the world disappeared for two days. When I vaguely regained consciousness, I overheard my worried parents' conversation about whether to unload the truck or take me to the hospital and leave my mother with me while my father carried out the move. Feeling guilty about the situation, I stood up and bravely stated that I was okay and ready to go. During the four-hour drive, I vomited profusely several times and arrived at Pécs in miserable condition. I had my first concussion with retrograde amnesia. The story I just told is but a reconstruction from the feedback I learned from my parents and the information they got from my buddies.

My Education in Pécs

Pécs greeted me with plusses and minuses. Our apartment was much smaller and without a garden, and there was not even a park nearby, not to mention the absence of my forest and Lake Balaton in Siófok. My classroom's floor was soaked with black wood tar and whenever I fell in a fight, the proof stayed on my clothes the whole day.

But there were plusses as well. Pécs, which lies on the sunny slopes of the Mecsek mountains in Hungary's southwest, has one of the oldest universities in Europe (founded by Pope Urban V in 1367 as *Universitas Quinque Ecclesiensis*) and has been a major cultural center. It has a rich architectural heritage from the 150-year Ottoman occupation, including my favorite domed Mosque of Pasha Gazi Kasim in the city center. I soon discovered a youth club and joined the Radio Fox Hunting Club (aka Amateur Radio Direction Finding, ARDF). The hunters needed to have knowledge about triangulation by a directional antenna and good orientation and running skills because the cunning "foxes" (i.e., the enemy radio stations) broadcast only intermittently from various hidden locations in diverse, wooded terrain. When rotating the directional antenna, the radio signal changed in strength so that by running fast to another location while the transmission was still on, a skilled hunter could determine the likely direction and location of the fox. You get the picture. The critical ingredients for success are good running skills and of course a sophisticated receiver, which each competitor had to build. The club became my second home, where I acquired the skills to build a functioning device. One day, beeps of di-di-di-da-da came from one of the rooms. This was the first time I heard the Morse code. I was mesmerized when the operator told me he had a conversation with another ham in New Zealand. Conversation by beeps? I assumed that our operator did not speak English or Maori, and presumably, the operator in New Zealand did not understand Hungarian. Instead, I was informed that they "spoke" to each other with dots (short pulses) and dashes (long pulses) through a universal language in which most words consist of three letters called Q-code. I was instantly hooked. To be part of such a wireless conversation, all I had to do was to memorize the Morse code and the Q language, learn more about electronics, feedback, and feedforward circuits to stabilize oscillator frequencies, pass exams, build a transmitter and receiver, and set up a wire antenna between the chimneys of our house and the neighbors. Then, I could communicate with any short-wave (10 to 80 m bands) ham radio around the globe. And that is precisely what I did during my high school years. I had radio contacts with a few famous people, including Yuri Gagarin (UA1LO), U.S. Senator Barry Goldwater (K7UGA), King Hussein of Jordan (JY1), and Queen Noor (JY1H). The aviator Howard Hughes was also a ham radio operator (W5CY). Hams are not exceptional among neuroscientists—for example, the late W. Ross Adey (the University of California, Los Angeles, UCLA), John Hopfield (Princeton University), Terry Sejnowski (Salk Institute), and Fritz Sommer (UC Berkeley).

In learning Morse code, the biggest hurdle is recognizing the silent gaps in the sea of dots and dashes separated by short spaces. Packing and separating messages is the most crucial thing in any coding system, be it speech, written language, computer language, or spike transmission in the brain. In Morse code, letters are separated by silent periods of the duration of a dot, while words have boundaries of at least two dots' length of silence. Without silent gaps, the di's and da's are just nonsense noise. Reading neuronal messages is even more complicated because action potentials are all just dots, and they come from many sources simultaneously. I had so much fun in the radio club, and the problems of oscillations, coding, and communication have bugged me ever since.

The good news in the summer of 1964 was my successful entrance test and acceptance to a newly created special math-physics curriculum in the Széchenyi Gymnasium. It was a competitive class with a bizarre mixture of outstanding and less knowledgeable communist teachers. Our math teacher was an energetic young woman; she hosted "problem-solving" evenings and weekends in her tiny sublet, posted math problems on our bulletin board for extra credit, and prepared us for math competitions. She gave us P. A. Larichev's *Collection of Problems in Algebra*. By the end of our sophomore year, my good friend László and I had solved all 1,000 problems. Our teacher declared that we were math gurus and sent us to a mathematical Olympiad contest. This was a pivotal moment in my life. I could solve only one of the five problems. Then, during lunch after the test, I met the true geniuses. They enthusiastically discussed those "easy problems," clearly having fun, and I felt I was among Martians. But I did not feel defeated; on the train ride back home, I felt elevated and relieved by the realization that I just eliminated math as a career option. Instead, I could concentrate on my radio hobby.

These were my best high school years. I had time for everything, from competitive swimming early in the morning to radio connections in the evening and studying for school in between. Like most of my friends, I became under the spell of hippy culture and pop music. With three of my trusted classmates (trust was a precious commodity of friendship in those days), we often listened to the short-wave broadcasts of Radio Free Europe and BBC, frequently jammed by the Hungarian authorities by frequency interference. Once there was a music quiz on BBC with a prize, which I solved and mailed the solution to the London address provided by the DJ. Two weeks later, he announced my name and address for winning the *Satisfaction* record by the Rolling Stones.

This was the year 1966, the tenth anniversary of the 1956 Hungarian Uprising. Every criticism of the regime was forcefully retaliated. We hated the system, represented locally to us by our communist teachers and most prominently by the school principal. She was a math teacher and occasionally substituted for our beloved young teacher. We often shouted out the solution loudly before she finished scribing a problem on the blackboard. We enjoyed making fun of her, and she was visibly upset by our misbehavior and sent my buddy László out to the corridor (a well-established form of punishment). László walked to the darker end of the corridor (where construction was going on) and sketched a scaffold on the wall with the principal hanging from it. To add emphasis to his anger he added a caption: "This is how every communist will end up." The next day the secret police showed up at the school, taking writing samples from each of us. László was missing from school that day and the following days. He hid in the mountains until he was hungry enough to return home. In the meantime, three of his closest friends were taken to the police station, and we were interrogated for hours and asked to write everything about our connections to the Imperialist West. I was told firmly that they knew everything about me, including the decadent infiltration of my mind by BBC. When I finished, they took the page and asked me to write another, more detailed version. This went on for a few times. Late that night, they showed me a lengthy document with my name on the last page and forced me to sign it without allowing me to read its content. I signed and was released at 6 a.m. At the political trial, I had to read "my confession" but proudly stated to the judge that those sentences were not mine. This upset our principal who was *ex officio* attending the proceedings. The 17-year-old László was tried as an adult and received six years of imprisonment.

My own punishment was not delayed much. In the all-teacher conference, the principal demanded that my grades be decreased in all subjects. I got "good" in physics (that hurt the most), geography, history, and geometry. We also got a grade for social behavior. I got a grade of 2 (the worst was 1), something nobody has ever heard of. Those midterm grades were my worst ever. The principal's punishment aimed to reduce my chances of getting into any university. If this was not enough, the entire math-physics class was disbanded, and we were dispersed into classes with vocational curricula (5+1 or 4+2, meaning one or two days working in a workshop or at a farm). As a result, in my senior year of high school, I learned nothing new. I lost an entire year of my youth at a time when the brain is a sponge and absorbs knowledge at an unprecedented speed. Things improved a bit when the principal retired and was replaced by a jovial successor. He told my mother that if I "behaved well" my future would not be lost. But I did not behave.

By the Prussian tradition of Hungarian education, we had to take final examinations (*Matura* or *baccalaureate*) at the end of high school. It just happened that the first Western pop group that ever visited my town was the British Nashville Teens and their concert was timed for the night before my *Matura*. I could not miss that event! I arrived at the concert with my friends and showed them secretly my small Union Jack banner. When the group played their biggest hit "Tobacco Road," I displayed my enthusiasm by waving the British flag. In no time, I was pulled down from the middle of the theater over the heads of many other spectators by the secret police, who happily tested the efficacy of their rubber batons on my body and drove me to prison.

My mother was demanding but also the tiger protector of the family. She wanted her children to become "somebodies." She worked as a secretary with no high school diploma for most of her life. After moving to Pécs, she started to work as a typist in the Law School, reaching the rank of executive secretary of two consecutive deans. A vital background knowledge here is that in addition to the regular Law School degree, the university also issued degrees to those who attended a "corresponding" class whose members typically showed up only for their oral examinations. Members of this group were mainly cadres of the establishment, such as police and military officers and people with the right party connections. My mom knew all these people because she handled their exam books. Luckily, one of those individuals was a high-ranking officer of the city police. Informed by my sister, who also was at the concert, about my "disappearance," my mother called that person and begged him to release her son. My mother saved me, as she did on several more occasions. The following morning, I was released exhausted. Yet, I got all A's on my *Matura* exam. As an act of congratulation, the teachers offered me coffee. I liked the taste of the coffee with sugar and asked for another double espresso. When leaving the room, I saw our geometry teacher, who also functioned as the party secretary of the gymnasium, rushing toward me. "There will be consequences for your provocation last night," he warned me and left. The coincidence of these several factors triggered a feeling that I never had before. Palpitation, accelerated breathing, stomach cramps, and subsequent insomnia, which were all signs of fear and vulnerability, lasted for several weeks. The positive outcome of this new feeling was that I never drank coffee again, although I am aware that caffeine was only a minor contributor to my anxiety.

With my ham radio obsession, my hope was to continue my studies as an electrical engineer at the Technical University in Budapest. However, my parents argued otherwise, mainly because of the cost of living in Budapest (even though education was free), which they could not afford. So I reoriented myself and applied for medical school. But after my Nashville Teens hiccup, I was unsure I would have a chance. After my oral admission exam in physics, I was told that Professor Ernst wanted to see me. The professor instantly said to me that he had conceived the physics problem and that only 2 of the 800 applicants had a perfect solution. He did most of the talking and at the end, he said, "See you in September." As I learned later that day, Jenö Ernst, the professor of biophysics, was the most powerful professor in the medical school. Thus, the fact that he shook my hand meant my likely acceptance. In November 1944, he and Professor Kálmán Lissák greeted the Red Army when they arrived in Pécs. Lissák was a Nobel Laurate Otto Loewi student in Graz, Austria, and Walter Cannon's assistant at Harvard. He had returned to Hungary to become chair of physiology in the Medical School before World War II. During the Nazification of Hungary, Lissák hid his Jewish friend Jenö Ernst and, in return, Ernst protected Lissák during the postwar communism. Their bond became a significant factor later in my neuroscientist career and served as an example that trust and friendship can override ideological and religious differences.

Medical School and Endre Grastyán

The Medical School of Pécs has produced a remarkable group of neuroscientists, including János Szentágothai, the legendary neuroanatomist. Yet, the two-year anatomy curriculum was just too much for me. While my friends were having fun at the School of Engineering in Budapest, learning exciting stories about radio transmission and electronic oscillators, I spent most of my time studying the unending details of bones and ligaments. But one day during a physiology class, the lecturer Endre Grastyán talked about something truly fascinating. I could not help but wonder about the parallels between the content of that lecture and control operations in electronic circuits. After his introductory lecture on the brain, my life in medical school acquired a new meaning. I wanted to get closer to him and learn more about his creative thinking. After my physiology examination, I applied to volunteer in his lab, a practice similar to the medical doctor/doctoral programs in the United States. The best training in Grastyán's laboratory occurred through my participation in the regular lunch discussions that could go on for several hours. It was during these lunch tutorials that I first learned about the hippocampal "theta" rhythm, which has become my obsession ever since. This was a natural fit for me, given my interest in amplitude, frequency, and single-sideband modulation of radio waves.

I spent most of my time in the Physiology Department. Thanks to Lissák, the chair, the department enjoyed many exceptional privileges. Nearly every faculty member spent one to several years in Western Europe or the United States. Grastyán spent two years in the Brain Institute at UCLA in 1957–1958, and even young faculty could occasionally attend conferences. Lissák encouraged and supported publications in Western journals, which was not a norm in most institutions in Hungary. Even just thinking about the possibility that one day I could cross the Soviet-bloc border was a strong motivation for me to work in the department. Occasionally, we had famous visitors from great institutions, and I had the privilege of being their personal student guide. Sir John Eccles spent several days with us. I had the extraordinary luck of sharing a train ride with the cybernetician and roboticist Grey Walter from the Burden Neurological Institute in Bristol from Budapest to Pécs. I learned about his *Machina speculatrix* tortoise firsthand. The hippocampus researcher Robert Isaacson (from the University of Florida, then the University of Binghamton, New York) visited several times. Bob and I forged a lifelong friendship, and he supported me in many ways when I came to the United States. E. Roy John from New York University also visited for a week. One day, Walter Freeman from the

University of California, Berkeley, gave a seminar on his olfaction maps in our small library. Grastyán, this gentle, polite person, kept interrupting his talk, asking one question after another, drilling the famous U.S. professor for more than two hours. To me, it looked like a boxing match. I wondered why Grastyán invited this person whom he now wanted to execute on the spot. On the way to dinner that night, Professor Freeman told me how much he enjoyed the honest atmosphere of the seminar with sincere questions. I realized then, and there that scientific debate is a reversed truce. You are the defender of truth and only the truth—diplomacy should be put aside. I often think of these moments these days when colleagues congratulate each other about the wonderful lecture they gave instead of offering useful critique. Scientific debate is not personal. It is the essence of progress. I like questions after which I cease breathing in surprise and spend days searching for an answer.

At the end of the year, my mentor Grastyán left for the United States to spend nearly two years in E. Roy John's laboratory at New York University. By that time, I already befriended his disciple Professor György Karmos, who kindly took me in. Karmos had perhaps the most sophisticated neurophysiology lab in Hungary then, including a "computer of average transients." Do not think of a computer but a machine that digitized analog signals and averaged them. My first assignment in the Karmos lab was to examine the variability of sound-evoked responses in the auditory cortex as a function of behavior. I measured the trial-to-trial amplitude variability of the various evoked components and compared them to the baseline before the stimulus. The baseline varied more than the evoked components, however, and we had the "paradox" finding that the early and largest evoked components had the smallest variability. This was the first time I faced the fascinating issues of brain "state," "context," and "spontaneous" activity, problems that remained with me forever. I presented my "evoked response paradox" at a student conference in 1973. I have since seen various iterations of this fundamental observation (e.g., Churchland et al., 2010).

Other than finding a home in the Physiology Department, my six years in the medical school were uneventful. I became a tour guide in my city, worked as a trash collector at a camp site, and was a doorman in an international youth camp for two summers at Lake Balaton. For another summer, I was a bell boy in a hotel. These jobs gave me extraordinary opportunities to occasionally meet people from the Western world and practice my English. A key event of these jobs was meeting Brenda Morrison, an English psychiatrist who spent two days in Balaton on her way to a congress in Vienna. To my great luck, it was pouring rain on those two days, and I had a chance to discuss and compare life in Hungary and the West with her. A month after our meeting, Brenda kindly invited me to spend a month in the Guy's Hospital in London. This was an exceptional opportunity, but I needed to get an exit visa. Again, my mother helped out. Her dean helped me to find the key person and compose a letter to the Ministry of Defense to temporarily suspend my military duty requirement and allow me to travel. To my surprise, I received permission and with the support of our dean, I was allowed to do my elective training in surgery at Guy's Hospital. Brenda's son lent me his bicycle and I discovered many parts of London. The contrast between London and my world was hard to comprehend.

Although I was certain that I wanted to become a brain physiologist (the term "neuroscientist" was not coined yet) from the day when Grastyán gave his introductory lecture, now in my sixth year, I had to think about my real options. Earlier that year, I won the Russian language competition in the Medical School, and as a reward, I could spend my Internal Medicine rotation in Lvov (or Lviv), Western Ukraine. Although I did not gain much knowledge about medicine there, I learned a lot about human relationships. Before, I hated everything that was Russian. But in Lviv, I met many wonderful Russian and Ukrainian students, and I realized that there is a huge difference between people and political formations. One of the greatest things about being a scientist is meeting talented people from so many different nations and discarding stereotypes.

Decisions about jobs in medical school were made in July while I was in Lviv. When I returned home, the bad news awaited me: my hoped-for position was given to the party secretary of our class, and there were not many vacant positions left anywhere in the country. There I was, the first-ever member of my extended family with a graduate degree and, in fact, the second (after my sister) with a high school diploma. By all standards, as a descendant of farmers from both sides of my family, the socialist system was designed for me and similar others of the society. A tiny minority, members of the super-well-organized Communist Party hijacked the otherwise noble intention of the socialist doctrine. Under the banner of equality, inclusion, and moral purity, they rapidly formed a totalitarian bureaucratic state in which the victims became precisely those members of the society for whom their ideology promised prosperity. The implementation of the system failed for obvious reasons. First, there are always opportunistic members in a society, who have an uncontrollable desire to be in a leadership position, irrespective of the ideology. I often amuse myself and silently predict which of my colleagues in the United States would undoubtedly have become leaders in a communist regime, irrespective of their current beliefs. The second peril is "self-censorship." This factor is perhaps the most dangerous because of its large scale and everyday effect. We condemn the actions of colleagues even when we agree with them and praise those we fear and dislike beyond what the authorities expect from us. Third, we elect underqualified persons into leadership positions to show that we are loyalist. In turn, new leaders with insufficient experience hire people with even less experience, which is known as "negative selection." The nondeserving rulers become fearful of their prospect of survival, and their actions focus on preserving their comfort while forgetting their original mandate. These hidden "rules" worked at every level of society, and science was no exception. But politicizing science and redefining history (almost everything was invented by the "Soviet scientists," according to my school books), instead of evaluating and acknowledging scientific contributions strictly based on intellectual merit and societal utility, was a grand failure. Censorship of science is harmful to the progress and well-being of societies, as the collapse of the Soviet system illustrates.

Instead of research, I found one job option: to become a resident in a thermal hospital specializing in rheumatoid diseases in a small town. Another option was a drug company in Budapest (GYKI; the compound GYKI 52466, a widely used noncompetitive AMPA receptor antagonist, was developed there). I learned how to canulate the tail vein of mice, and after hundreds of mice, I could do the procedure blindfolded. I started reading extensively about animal behavior, triggered mainly by the Nobel Prize for ethology in 1973. Detailed observations of the animal's behavior were the needed antidote for Pavlov's passive brain idea and Skinner's recipe to study cognition by lever presses. I returned to Pécs every weekend to continue my experiments, now focusing on behavior. At the company, I pointed out the inadequacies of the testing apparatuses for drug screening and suggested alternative paradigms to my superiors. But I was told that "this is a drug company and we use internationally accepted and verified tests." Well, if no innovation was needed, I wondered, "What am I doing here?"

Military Service

In this low moment, the draft notice from the military was a welcome event. I had to get through with my military training at some point anyway and so the timing appeared perfect. Our sergeant took every opportunity to humiliate the "doctor privates," and he made sure that our uniforms got muddy during the cruelling morning exercises only to get punished if in the afternoon he spotted a piece of dirt on our boots. It gradually sneaked up into my brain that the entire goal of the initial military training was to break our egos and push us to the lowest levels of human existence. It took even longer to comprehend how constructive and valuable it was. Rebuilding ourselves from the ashes was a cathartic exercise. I am grateful to my sergeant and army experience, which made me a better person. I occasionally comment to my students that a month in the gulags would make graduate school look like a piece of cake.

By the end of my military service, I received good news. The party secretary of the Physiology Department "defected" to the Netherlands, and I could fill the vacancy. My monthly salary as an instructor was 2,420 forint (approximately $80).

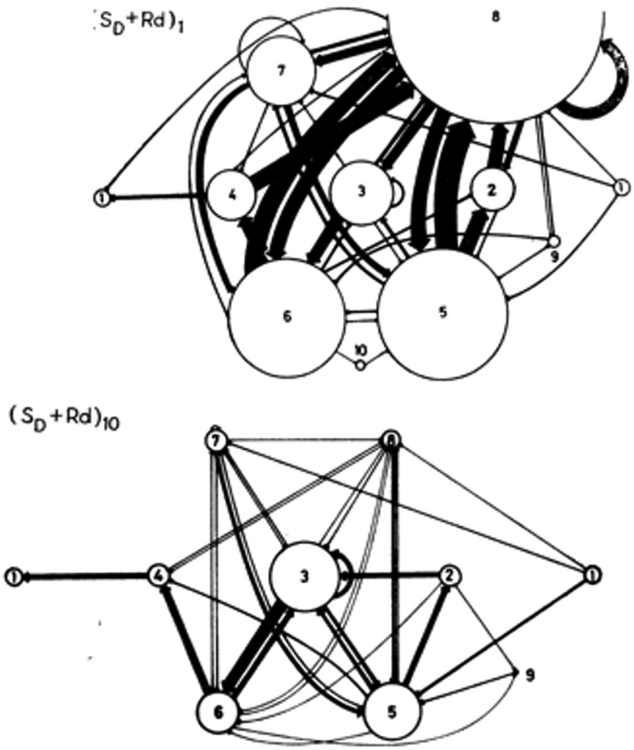

Fig. 2. Ethograms of learning experience in the rat. Top: Frequency of occurrence of different behaviors and sequential relationships among behaviors in session 1. Bottom: Session 10. The diameter of each circle is proportional to the frequency of behaviors. The width of the connecting arrows is proportional to the number of occurrences of each sequence type. 1, drinking; 2, run to bar; 3, bar pressing; 4, run to drink; 5, turn left; 6, turn right; 7, rearing; 8, sniffing; 9, grooming; 10, stop or freeze. (Buzsáki et al., 1979).

Department of Physiology, University of Pécs

Besides teaching, I was busy in the laboratory with my ethological experiments. I was lucky to be able to borrow a videotape recorder, and I spent long hours replaying every detail of rat behavior, from ear pricks to grooming syntax (Figure 2). In 1977, I had the good luck of spending a few weeks at the Nencki Institute of Experimental Biology (founded by Jerzy Konorski) in Warsaw. I trained two dogs to go to place A or B, signaled by loudspeakers from either location. They learned it in a day. The next day, I placed one loudspeaker above the start position and two tones indicating locations A and B. To my disappointment, the performance of both dogs remained poor even by the end of my term in Warsaw. Back in Pécs, I designed an analogous experiment for rats and found similar results.

I made a discovery, or so I thought. The rabbit's fur always coincides with the meat in the appropriate ecological niche, so the fox chasing the fur will get the rabbit's meat every time. Only in the lab can we change this contingency. Soon, I realized that others had already made similar observations (Breland and Breland, 1961; Seligman, 1970).

In the East, one of Pavlov's students, Piotr Stevanovitch Kupalov observed that his freely behaving dogs extensively investigated the metronome (CS) placed in the window sill even at the expense of losing food (US) placed at another part of the room. In the West, the literature on "autoshaping" also argued that the CS does not become a substitute for the US but rather becomes a new goal (Brown and Jenkins, 1968), which acquires significance for the animal through its active investigation. The brain is not a passive absorber but an investigator. My first contribution to this topic was translating part of Kupalov's work into English. A year later, I was delighted to receive an invitation from the editor of the *Journal of Experimental Analysis of Behavior*, founded by B. F. Skinner, to write a review on East versus West views on behavior. The main message of my first-ever review was that the doctrine that any stimulus can be linked equipotentially to CS (Pavlov) and that any behavior can be shaped by the reinforcer (Skinner) is simply false. Instead, signals and reinforcers provide an affordance for implementing innate tendencies. We do things to perceive our surroundings. This old message remains important today, with the revival of the Skinnerian philosophy under the label of "reinforcement learning" (*anything* can be linked and shaped by the reinforcer), the rapid spread of head-fixed preparations, and using virtual reality "environments" (Grastyán and Vereczkei, 1974; Buzsáki, 1982, 1983).

To my surprise, many bright students wanted to work in my lab. They were only a few years younger, and we shared similar worldviews. To relate my newly learned knowledge about evolutionary constraints to behaviors, I designed an experiment that mimicked our feeling of "imprisonment" behind the Iron Curtain. Rats had to press a lever for food at intervals longer than 10 seconds. The timer was reset if they pressed at, say, at 9.8 seconds. This is a difficult task (known as DRL) even for humans. Doing nothing for food is a counterprepared behavior. We built a six-compartment apparatus, in which the "rooms" contained various things, such as a running wheel, wood shavings, and toys. Half of the rats could mediate time by visiting the rooms at will (freedom group). The other half was confined to one compartment with just a lever. Members of the freedom group learned the task well, adopting stereotypic visits to the different compartments between presses. The performance of the confined group remained consistently poor. After the asymptotic execution of the task, we changed the contingencies. By enjoying the new freedom, the performance of the previously confined rats increased rapidly. In contrast, the now-confined animals pressed the lever prematurely and kept jumping out from the lever-containing compartment.

The calory cost of their jumps and presses was several times higher than the calory gain obtained with the few pellets during each session. Subsequently, we repeated the experiments in animals that underwent fimbria-fornix lesions. Surprisingly, the lesioned group's performance was less affected by the freedom-confinement change. The sad conclusion of the experiment was that brain damage is the solution to tolerating confinement. I presented our findings at an Intermozg (Russian for Interbrain) meeting in Pécs, and Ezras Astratyan, the founder of the Institute of Higher Nervous Activity, Moscow, was my chair. Nobody asked a question after my talk but my friends warned me: "Be careful, they understood you." It took a long time to write up this story, and another four years from submission to publication (Acsádi et al., 1986). My ethologist "career" culminated in my writing a little book on animal learning (in Hungarian; Buzsáki, 1984).

Parallel with the behavioral experiments, I continued to work with Grastyán's lab on the relationship between hippocampal activity, locomotion, eye movements, CS responses, and my refined behavioral parameters. I already had a mini question to address the existing debate in conditioning. Reward and US were believed to be the main motivators of behavior but how they exerted their impact on behavior was a mystery. The dominant theory was "stimulus substitution." But I thought that a cat or dog is not so stupid to recognize that the loudspeaker is not palatable. Others promoted the view that the CS-US contingency offers an affordance to investigate and learn about the CS. Clever people on both sides of the debate designed various experiments to "convincingly" support one or the other hypothesis, but without an objective arbiter, the arguments remained just arguments. With electrodes in the brain, however, we could ask the brain. During eating, drinking, and other times of consummatory behaviors theta oscillations were absent. Conversely, when the animal investigated the signal, theta was always present, clearly revealing that the CS was not a substitute for US. I realized how unending psychological arguments can be a shortcut by looking at the brain, the source of all behaviors. Brain activity is an objective judge. I should examine not only behavior but also the brain. By studying the electrical activity of brain networks, I could also satisfy my curiosity about information transmission beyond the Morse code and sender-receiver communication.

Electrophysiology Is Cool

I inherited Karmos's 12-channel pen recording electroencephalogram (EEG) after he left to head an Institute in Budapest. Following Grastyán's pioneering work on theta oscillations and the orienting reflex, other groups linked this prominent hippocampal pattern to attention, arousal, and information processing. Those ideas could be contrasted with another influential hypothesis, advanced by Cornelius (Case) Vanderwolf that theta oscillations in the rat occur during intentional or voluntary movement. To "defend" my mentor and "disqualify" Vanderwolf's view, I designed various paradigms to demonstrate that theta can be induced in the immobile cat in response to the CS. Of course, there always remained the possibility that the cat was prepared to move, and with such reasoning, the problem was shifted to the territory of "intentionality" and volition. Moreover, the cat is not a rat. The latter factor was my main motivation to switch to rodents, despite the technical challenges associated with the smaller head of the rat. By the time I was performing those experiments, virtually every term in the psychological vocabulary had already been claimed to relate to theta. Thus, to come up with the ultimate single correct correlate seemed hopelessly remote. I suspected that those behavioral-cognitive terms were hypothetical constructs with fuzzy boundaries, which may not correspond to any particular brain mechanism. Perhaps it would be safer to work on something more tangible.

I was already familiar with Per Andersen's (University of Oslo) (see volume 4) beautiful evoked field response/population spike experiments in rabbits, and I had just found out that such experiments could also be done in the waking rat. I read the reprint by Winson and Abzug (1977) at a barber shop while waiting for my haircut. I got so excited that I left before my turn and rushed back to the lab to check what components I needed to start recording evoked responses from the rat hippocampus. I had to build head stages from dual MOS-FET buffers to use smaller diameter but higher impedance electrodes. With the afforded input-output measurements, I could study the impact of sleep and waking, learning, and the interactions of various hippocampal inputs on output functions in the paradigm that I designed for the autoshaping experiments. We soon replicated and expanded Andersen's experiments with entorhinal input stimulation, identifying the sequence of fiber volley potential, extracellular field PSP (input), and population spike (output). To our surprise, when we used the same approach by stimulating the hippocampal commissure, although the CA1 responses behaved as expected, we did not see a population spike in the dentate gyrus, no matter the stimulus intensity. That baffled me, and I began to think that the commissural fibers also terminated on interneurons and, in turn, such fast inhibition could have overridden the anatomically known excitatory inputs on the granule cells. However, this hypothesis required further investigation.

Finding Treasure in Pécs and Making Contacts with the Academic World

As a young man, I pretended I had a free life and never thought of anchoring myself to a place or person. That is until I met the most beautiful and most brilliant woman in the world. When you find someone whose soul resonates with yours, and you realize that every moment you spend with her feels like a gift of life, you are sure you found true love. Veronika Solt and I got married in October 1978. My life as a scientist did not change but acquired a different meaning. A better one.

By the late 1970s, Hungary began to depart from other Soviet-bloc countries in various ways, allowing visits to Western countries, at least for artists and scientists to "represent the achievements of the socialist system." I hitchhiked to Florence, Italy, to present our work on the interaction between the commissural and entorhinal inputs in the hippocampus at the European Neuroscience Association meeting (a predecessor of FENS). My first truly international meeting was an extraordinary experience to meet authors of papers whose works I have already read. Among them were Menahem Segal and Bruce McNaughton, who have remained my friends and fierce competitors to this day. Bruce was going through every panel of my poster and told me that he was working on a manuscript on the same topic, an event that happened many times over the years. There is a term for this today: "being scooped." For me, it was delightful news that someone at the other end of the world found something similar. What a relief it was.

My key link to the world of neuroscience was Magdeburg in Eastern Germany. There, Professor Hansjürgen Matthies was the director of the Magdeburg Medical Academy. Because of his party connections, he was allowed to organize an annual symposium on learning and memory, where most of the speakers were from beyond the Iron Curtain. Here I met Helmut Haas, Cornelius (Case) Vanderwolf, Joe Martinez, James (Jim) McGaugh (see volume 4), Rodolfo Llinás (see volume 5), and Susan Sarah—all friends who made a big difference in my later life. After the Berlin Wall was brought down, I was invited back to Magdeburg to deliver the Hansjürgen Matthies Honorary Lecture in 2015.

Although my rat experiments were going well, I was ready to learn more and asked Grastyán to recommend a suitable lab in the United States. My plan A was to work in the laboratory of W. Ross Adey. Plan B was any other lab, anywhere. Adey was the head of the Space Biology Laboratory at UCLA, also a ham radio operator, and the first neuroscientist to use digital computers for the analysis of the EEG, a perfect match for my interests. Grastyán wrote to Ross, but a few months later, he told me that Adey's lab may not fit me well. I had not thought much about this comment for decades, but in 2021, I received a package from an old colleague in Pécs, who saved some of the relics from Grastyán's desk. In it was a letter from Adey, explaining that he was eager to host me: "I can promise him excellent experience in a very new and exciting area of molecular biology … of learning and memory"). For reasons I will never learn, Grastyán did not mention Adey's response to me. Perhaps my mentor thought that I would have a better opportunity. Yet, I occasionally wonder how my scientific trajectory would have evolved if I had the chance to join Adey's lab, especially because we are currently working on the impact of GHz radiation on brain activity, a specialty of Adey's lab in the 1960s. In 1979, Grastyán received a letter from Eduardo

Eidelberg, a neurosurgeon, from the University of Texas at San Antonio. Eidelberg and Grastyán overlapped at UCLA in their younger years and had mutual respect. I immediately wrote to Professor Eidelberg and thanked him for his offer. There was no offer, of course, and he had never heard of me, but Grastyán's word was enough for my acceptance. I still had to undergo several clearance hurdles until I eventually received an exit permit stamp on my passport and the U.S. J-1 visa.

Another rewarding event in 1980 was the International Union of Physiological Sciences (IUPS) meeting in Budapest, with 6,000 scientists (Figure 3). I attended many memorable lectures and met several of the speakers. I had the opportunity to show my single-unit recordings to Per Andersen, who said that he had never seen such nice large amplitude extracellular units in the hippocampus. This praise meant a lot to me. He introduced me to Carol Barnes, who with Bruce McNaughton, was spending sabbatical time with him in Oslo. I invited Carol for dinner at a restaurant with great gypsy music. Carol brought two other gentlemen with her, whose name I could not quite make out during the introduction. They asked me about my work and I explained to them that I just finished an experiment on long-term potentiation (LTP) of the commissural input to CA1. I politely enquired whether they were familiar with LTP and related plasticity phenomena at which point Carol clarified that her friends were Tim Bliss and Graham Goddard (Carol was Graham's student). You can imagine the complex mixture of my embarrassment and pleasure meeting such giants.

Fig. 3. IUPS meeting in Budapest, 1980, with my wife, Veronika Solt, and Evgeny Sokolov, the most prominent Soviet researcher on the orienting response.

During the meeting, I managed to exchange enough U.S. dollars and Deutschmarks from my acquaintances to purchase a plane ticket from London to New York City. I could pay for the London flight with Hungarian currency. The stopover in the United Kingdom allowed me to participate in the Fourth European Neuroscience Meeting, Brighton in September 1980. There, I met Fred (Rusty) Gage, marking the beginning of our lifelong friendship. When I arrived in New York City, I figured out how to use the coin telephone and called Bob Isaacson, hoping that he would explain to me which subway I should take to find him. I did not know that New York City and New York State were different, and only there I realized that Binghamton was a seven-hour bus ride from the city. Never mind, I found the 42nd Street Bus Terminal and took a red-eye Grey Hound bus ride to Binghamton. The next day, I visited Bob's lab, gave a seminar, and met the students and postdocs who gave me a crash course about life in the United States. Bob was generous enough to purchase a plane ticket for me to San Antonio, Texas. My life in the United States began to take shape.

First Postdoc: University of Texas Health Science Center, San Antonio

During our first meeting, I explained my research plan on the hippocampus to Professor Eidelberg (or Eduardo after the first five minutes). He told me that his grant was for spinal cord research, but he had an extra room where I could conduct my experiments, provided that I prepare single fibers from the dorsal root of the spinal cord twice a week. I should call him when I had the fiber with electrophysiological proof so he could take over the experiment. That was a generous deal, but he warned me, "young man, the only thing you will be able to show is that Ramón y Cajal was right." For me, that was not such a bad outcome. He had no student or postdoc but two technicians, who occasionally helped me with histology and developing my 35-millimeter films.

I continued the work I started in Pécs searching for direct proof of feedforward inhibition. Steve Fox and Jim Ranck (1975) had already hypothesized that fast-firing cells in the hippocampus were inhibitory interneurons. My aim was to record these putative inhibitory cells and show that they respond before activation of granule cells or pyramidal cells. Collecting the needed instruments took a while, but I had my lab up and running in a few weeks.

I missed my wife and family. They had no telephone at home so exchanging letters was the only way of communication. The content of the second letter from my wife was devastating. Her exit visa application was denied. My secret plan was to work in North America for a few years but certainly not without my wife, and I contemplated returning to Hungary before the end of the year. To distract from my anxiety, I spent more and more time in the lab. On Christmas Eve, I found four beautiful interneurons, each firing 2–3 milliseconds before the population discharge of the pyramidal cells.

In my excitement, I overdeveloped the film, and the spikes were barely visible. For today's readers, the evoked responses were displayed on the oscilloscope screen, and for documentation, we took screenshots with a camera (later by Polaroid). I continued to search for putative interneurons in the coming months, showed that they fired rhythmically with theta waves, and collected convincing evidence that excitatory inputs excite both interneurons and principal cells; however, either because the synapses on the interneurons were stronger or because the charge time of the membrane of interneurons was faster, interneurons fired first and, therefore, could prevent or delay the discharge of their innervated principal neurons. In addition, I could show that activation of interneurons by commissural stimulation attenuated the entorhinal input-evoked responses. In the papers published with Eduardo, we concluded that "interneurons of the hippocampal formation can be directly driven by various afferent inputs," and also that "the excitability of the principal cells depends upon the relative strengths of the inputs to these two cell types" (Buzsáki and Eidelberg, 1982; p. 603; Buzsáki 1984; p. 145). Within a year, Roger Nicoll's (see volume 10) group at the University of California at San Francisco reported that in the hippocampal slice preparation, orthodromic stimulation always evoked higher amplitude inhibitory postsynaptic potentials (IPSPs) in CA1 pyramidal cells than antidromic stimulation, another strong support for feed-forward inhibition. With the emergence of immunocytochemical identification of interneurons, anatomical evidence for feedforward inhibition was firmly provided. The case was settled. Feed-forward inhibition has become a standard part of today's neuroscience, rarely referring to the original papers. Yet, to my great honor, a citation of my Ralph Gerard Prize (2020) reads: "As a young scientist in the early 1980s, he introduced the concept of feedforward inhibition, which is now a widely recognized property of neural circuits." Roger Nicoll was a member of the jury. He remembered, and this mattered more to me than anything else. After all, the biggest honor one can get in our profession is acknowledgment from those handfuls of people whose work we admire.

The year 1980 marked the beginning of *Glasnost* and *perestroika* in Eastern Europe. Perhaps because of such an "opening," my wife's appeal was granted, and she got an exit visa in January. She bought a ticket to New York City, I went to the Hospital's Credit Union, borrowed $1,200, and bought an old used car, a Fiat 128, which looked very much like my father-in-law's Soviet Lada. On the way to New York, I visited William (Skip) Levy and Oswald Stewart at the University of Virginia in Charlottesville, and Barry Komisaruk at Rutgers University, Newark, New Jersey. In New York City, I met Jim Ranck, Steve Fox, and John Kubie (whose works I knew by heart) and Jon Winson at Rockefeller University.

Jon Winson put me up in the Abby Aldrich Hall guestroom at Rockefeller, next to the library (a reminder: I was just a postdoc from behind the Iron Curtain). That place was more elegant than the Presidential Suite of a Westin Hotel. When my dear Veronika arrived, she created the false impression that her husband won the lottery. Only the battered Fiat car was a reminder of my postdoc status. While driving in Brooklyn the next day, I hit a pothole and lost a tire. Never mind I had a spare. On the way back to San Antonio, we had to change the front tires four more times. It turned out that the pothole collision broke the car's suspension. Luckily, we did not know it until we reached Texas, so we did not panic and cancel our travel plan. That plan included a lot. We visited the Isaacsons, James (Jim) Sprague (see volume 1) at the University of Pennsylvania, whom I had met earlier; Fred King, the director of the Yerkes Primate Center in Atlanta, Georgia; and Morton Mishkin at the National Institutes of Mental Health (NIMH) in Bethesda, Maryland. Mort was already a superstar, so I was surprised to learn that his office was in the basement with water dripping through the ceiling. Mort also spent some time at the Nencki Institute, Warsaw, so he was well aware of the financial situation of East Europeans. After my seminar, he handed me an honorarium of $100 in cash. That was enough for a Peking duck dinner with my wife, gasoline expense to Texas, a night in New Orleans, and $40 reserve for a potential emergency tire replacement. Later, I learned from several of my colleagues on our trip that the Federal Bureau of Investigation (FBI) had inquired about a potential spy from the Soviet bloc. Bob Isaacson jokingly told to Fred King that the FBI was not after the Hungarian but rather that Fred was the target of a covert investigation. These days, FBI agents occasionally visit my lab and ask me about potential spies working in my lab.

At the University of Texas Health Science Center (UTHSC), Veronika became a nonregistered medical student but had the same clinical responsibilities as the regular students. This was a super-generous gesture of several professors, for which we are eternally grateful. During the summer, we drove to California with our Fiat. In San Diego, I visited Larry Squire (see volume 11), Stuart Zola-Morgan at the VA Hospital and Max Cowan (see volume 4) at the Salk Institute, who introduced me to Larry Swanson (see volume 11) and David Amaral. I was impressed by the high density of neuroscientists in a single city. The scientific highlight of my trip was UC Irvine. Both Jim McGaugh and Charles (Chuck) Ribak invited me to give two separate seminars. Jim put us up in the luxurious Laguna Beach Hotel and invited us for dinner at his house. In my Anatomy Department seminar, I met strong resistance to the idea of feedforward inhibition. Our intense debate electrified Chuck to devote his next several years to providing the needed structural evidence with my colleague László Seress from Pécs, whom he invited as a postdoc. The last leg of our trip was the San Francisco area. At my seminar at UC Berkeley, there were only six people: Walter Freeman, the olfactory physiologist; John Garcia, the discoverer of "taste aversion learning"; Mark Rosenzweig (see volume 5), a pioneer of neuronal plasticity; my host Joe Martinez; and his two students. I reminded myself that quality overrides quantity. We also drove to Stanford and visited Richard Thompson, the dominant neurophysiologist of learning and memory of the day. He kindly invited me to join his lab as a postdoc, but his lab was a bit of a disappointment in terms of technical sophistication. Moreover, by that time, I had already committed to joining Brian Bland's lab in Canada. Just a week before my departure to the United States, I had received a letter from Brian (a student of Case Vanderwolf) to join his lab for two years. Brian had just returned from Per Andersen's lab, full of energy and ideas, and he had received excellent funding from the Alberta Heritage Foundation. While in San Antonio, I kept corresponding with my university at Pécs and the authorities to allow me to stay abroad longer.

Second Postdoc: Canada

In retrospect, it is truly amazing how much support I received from my U.S. colleagues. It is unheard of today that a postdoc is invited to give a departmental seminar for an honorarium and stay with the family of the faculty. Perhaps, it was my exotic status of being an Eastern European or a salutation to Grastyán that secured me those privileges. We crisscrossed the country, assuming that our trip was perhaps the last one in the United States. We had one more long drive ahead of us though. We packed our belongings in the Fiat, said goodbye to our friends, and arrived in Calgary on Canadian Thanksgiving Day and stayed with the Blands. Veronika and I slept in the next day, and Brian served us breakfast in bed! Unfortunately, later that day, he had to deliver devastating news. The Alberta Heritage Foundation has terminated its postdoc program. The implication was that I had no funding. Yet, I did not panic. I was trained under uncertainty, and, importantly, we were young. I planned to contact as many Canadian neuroscientists as possible Brian and I knew and offer my services. The first and last person I called was Case Vanderwolf, whom I met in Magdeburg. Case agreed to take me in. So we sat back into our Fiat and drove on the Trans-Canada Highway for five days from Calgary to London, Ontario. I spent a good part of each day fixing our car.

University of Western Ontario

The Department of Psychology at the University of Western Ontario (today Western University) was in a modern building surrounded by old houses in the architectural style of Oxford. Case had no student that year, only a technician, and I was the only postdoc. But to my great luck, Lai-Wo (Sten) Leung had just joined the faculty in the Department of Physiology and helped me a lot with novel quantification methods. Case's lab had a pen-recording EEG, two oscilloscopes, stimulators, and a most precious four-channel FM tape recorder. Luckily, the psychology student lab had more equipment, some of which I could borrow. I have already learned how to sharpen and insulate tungsten microwires from Eidelberg, I just had to build an impedance meter and a microdrive to record single units from behaving rats. I trained rats to run several turns in a running wheel to activate a sound, which signaled the availability of a water reward at the opposite end of the box. I wanted to study single-neuron correlates of the theta waves so I needed long theta trains that occurred reliably during running. For a while, I was scared to write Grastyán that I was working with Case Vanderwolf, whose views on the hippocampus were diametrically opposite to his. Case was smart in different ways from Grastyán. He was a detail-oriented careful investigator. Although he was acutely aware that the brain serves cognition, he refused to accept any claim on memory or spatial map until all other motor, autonomic, and hidden variables were excluded. I agreed with him that it is super important to monitor those variables, but I had already formed the opinion that action and cognition are not completely separable. Searching for "pure" cognition in an animal that does not behave was perhaps a hopeless endeavor.

Working with freely moving, drug-free rats increased my confidence in my previous findings. For any comparison, the same animal retained its own control, and this within-subject design has remained my main approach throughout my entire career. Identifying putative interneurons and pyramidal cells was much easier because the repeatable behavioral correlations provided additional help to identify waveform and firing pattern differences. The recordings were inspected on the oscilloscope screen and broadcasted through a loudspeaker. The ear is a Fourier analyzer, and the smallest changes can easily be detected. I first extended my previous observations on the relationship between firing patterns of different hippocampal neuron types and theta oscillations while simultaneously studying their correlations with fast (gamma band) rhythm. But the most fascinating pattern was a strong, synchronous population pattern, which I called sharp wave burst and later sharp wave-ripple. I have been enthralled by the beauty and power of this pattern from the first moment I heard its buzzing sound. I felt as if I had been listening to a group of orchestra musicians idly tuning their instruments. The next moment they united in the thrilling harmonies of Beethoven's Fifth Symphony. Studying the neuronal correlates of sharp wave ripples, I learned why synchrony is a unique brain operation. It can bring about enormous power surges at no extra energy cost. If one listens to one instrument at a time in an orchestra, even the first violin, the actual melody of the piece may be hard to identify. This is more so in the brain. When listening to the spike sounds of individual neurons, we cannot detect the occurrence of a sharp wave ripple. But when we listen to many neurons, one cannot miss it. I wondered what the function of such collective action might be. The brain must have a good reason to go to the trouble of producing such unusual population activity. My first thought was that it was a teaching signal to induce plasticity in the neocortex, but it took four decades of further work in many laboratories to get a clearer picture. Instead of writing three papers on theta, gamma, and sharp waves, I decided to publish the three interconnected stories in a long paper with 21 figures (Buzsáki et al., 1983).

During the summer of 1982, two key things happened. Veronika was allowed to visit Canada by the Hungarian authorities for a month. Second, Bob Isaacson wrote me a kind letter, offering me an assistant professor position. This was an exceptional opportunity, and we discussed it extensively with my wife. The stakes were high. If we both stayed, she had to start all over her medical studies. If I stayed in the United States and she returned to finish her remaining year and a half, there was a good chance that the authorities would classify me as a "dissident" with no option to return to Hungary. She would not be able to return to the United States. We chose a third option. She flew back to Hungary at the end of the summer, and I also returned at the end of the year. Did we really make a decision or float on the waves of life?

Case Vanderwolf was an exceptionally warm-hearted mentor. I often spent the weekend at his house where he and his wife, Judy, prepared meals based on medieval recipes. Every summer, they made a canoe trip in Northern Ontario and invited us along with Case's daughter and a couple from Toronto to their annual trip. The seven of us were flown by a seaplane and landed on a big lake in the middle of nowhere. Only Case and Judy knew how to handle a canoe properly and only Case had an idea (and a map) of where we were. He always warned us ahead of time when a waterfall was expected, at which point we had to portage our canoe and gear (i.e., our tents, food, clothing) on our heads from the upstream to the downstream parts of the river. After sunset, we pitched a tent, made fire, and cooked dinner. The backpack with the food had to be placed as far as possible from the tent in case hungry bears would come by. And they did. One night, my wife left the tent and faced a sizable bear from a close distance. Luckily, the bear had another agenda than to attack an innocent Hungarian. After the third day, my wife and the couple from Toronto were discussing how to kill Case for all this torture, but we postponed carrying out the plot because of our fear that we would never make it back to civilization without him. With time, however, we adapted to the daily routine, and at the end of our venture, we all concluded that that was the best trip in our lives. Case's training reminded me of my good sergeant in the army. And one more detail: On the first day of our trip, Case declared that the rule of wilderness is skinny-dipping and he showed the way. We justified our following by thinking that there would be one less thing to dry each day. I am sure if I did such a thing with my lab members today, I would appear on the front page of the *New York Times* and lose my job. But back then and there, this was completely acceptable with no prejudice, and things functioned just as well.

Leaving Canada

In October, one day after submitting our manuscript, I packed the few things I had and drove the Fiat back to the United States to visit Bob Isaacson and tell him my decision. From there, I drove to New York City, bought a ham radio transmitter, I gave away my faithful Fiat wreck to a Hungarian acquaintance, and ordered a car from the Schiphol Airport, Amsterdam. In those days, transporting cars was the best way to convert dollars to Hungarian currency because citizens spending more than one year abroad were exempt from paying the 60 percent tax.

In New York City, I developed a fever and by the time I arrived in Amsterdam, it got worse. I picked up the car, an Audi diesel (the cost of diesel was half the regular gasoline in Hungary), and drove to Hungary in one stretch. At the Hungarian border, the "authority" informed me that, as a Hungary citizen, I was not allowed to drive a car with a foreign license plate inside the country. I asked for tools to remove it but he gave me a hateful look and told me to return to him after removing the plate. It was the middle of the night and raining and I was shivering in fever. After a good while, a Belgian truck arrived and the driver helped me to remove the license plate. I arrived in Budapest the following day and spent a week in bed in my in-law's apartment with pneumonia. It was not a welcoming beginning to my restart in Hungary.

Starting a New Life in Pécs

The three and a half years I spent in Pécs after returning from the United States were perhaps the most formative years of my life. Being aware of the financial and technical limitations, I deliberately designed experiments that were difficult technically and could not be done faster, even in a large lab in the United States. One of those experiments was to identify the state-dependent changes in the distributions of the current sinks and sources involved in hippocampal theta oscillations. The first step was to collect high-spatial-resolution data with a stationary (control) electrode in the CA1 stratum oriens and from all other layers successively. I used my running track paradigm to "clamp" behavior and compare theta during level pressing and running. Each experiment took an entire day with frequent failures because cerebrospinal fluid short-circuited the drive or the rat was not motivated enough to run beyond 50 trials. Case Vanderwolf donated his four-channel FM tape recorder to me (my most expensive piece of equipment) so I could record everything on tape. The analysis step was an even bigger challenge. Although we received our first computer, a PDP-11, from Roy John, New York University, we did not have an analog-digital converter. I found a solution at the Kodaly Institute of Music in Budapest, dedicated to preserving folksongs in digital format. So I carried my tape recorder to Budapest on a train, converted the data onto punch tape, and used the punched tape in Pécs to feed our PDP-11. Even with this clumsy process, our paper was the first current-source density description of hippocampal theta oscillations. We also showed that theta waves are asymmetric and that the degree of asymmetry varied with speed in a layer-dependent manner.

By the time I resumed my position in the Department of Physiology, Grastyán had replaced Kálmán Lissák as the director. He hated his director role and often cited Plato as an excuse: "One of the penalties for refusing to participate in politics is that you end up being governed by your inferiors." One such "inferior" was a "Moscovite" (this was a reference to Hungarians who obtained a doctorate in the Soviet Union), who got confused about ion concentrations inside and outside the neurons during his talk at a summer course. After the talk, a young participant commented on the talk in a genius way. Everyone in the room, except the Moscovite speaker, realized that the young participant made fun of him. He was Peter Somogyi, who already held appointments in Budapest and Oxford by then. After the talk, we immediately bonded and have remained trusted friends.

Our plan to return to the United States got a further push when Veronika learned after her graduation that she did not get any of the jobs she applied for in Pécs, despite being a top student. Grastyán helped out (again!) by offering a temporary instructor position for my wife in the Physiology Department but allowed her to work in the Internal Medicine Clinic. In early 1983, I received a letter from William (Chip) Levy from the University of Virginia offering me a research associate position. At the U.S. Embassy, however, the Consul firmly told me that she issue me only a J-1 visa, which meant that we had to return to Hungary for two years before changing our visa situation. This unexpected verdict felt devastating then but, in retrospect, we should have been thankful because this event forced us to follow plan B, which was planning our future in Hungary. From the sales of the Audi and our small apartment and loans from our relatives and friends, we bought a share of a house in the nice, hilly part of Pécs. We had enough room to host journal clubs with my students regularly. Our beautiful daughter, Lili, was born a year later.

Grastyán Turns 60

Endre Grastyán turned 60 in 1984. Case Vanderwolf and I thought organizing a hippocampus meeting in his honor would be a lovely birthday present. This was the first-ever meeting I organized. The speakers included William Levy, Fernando Lopes da Silva, John Kubie, Jim Ranck, Ian Wishaw, Brian Bland, Stan Leung, Klaus Reymann, Bob Muller, Steve Fox, Case Vanderwolf, John O'Keefe, Bob Sainsbury, Vincent Bloch, Richard Morris, Rob Sutherland, and Tim Bliss (Figure 4). This team was the cream of hippocampal physiology research of the day (Per Andersen could not attend). I also invited a dozen Soviet scientists but nobody came because a year in advance was too short for passing through the multilevel bureaucracy. The scientific highlight of the meeting was Jim Ranck's premier presentation of his discovery of head direction cells in the postsubiculum, neurons that fire only when the head points in a certain direction in the environment, regardless of the animal's location. We recorded all the discussions, which I transcribed and published along with the submitted manuscripts in a volume Case and I edited a year later (Buzsáki and Vanderwolf, 1985). There was an interesting discussion about the theta phase relationship of spikes. Whereas I observed that, on average, spikes of pyramidal cells prefer the trough of the theta cycle, John O'Keefe argued that place cell spikes could occur at any phase, perhaps escaping from the background control. Case emphasized the importance of two types, cholinergic and noncholinergic, theta oscillations. In turn, O'Keefe commented that perhaps we should "speculate about the possibility that in fact there are various phase relationships that can occur between these two EEG patterns, and may be part of the function of the EEG is to create interference patterns as a function of the different phases of these two theta waves" (Buzsáki and Vanderwolf, 1985, p. 386). Nine years later, O'Keefe cracked the puzzle with the discovery of spike "phase precession." A memorable comment on my work came from Tim Bliss: "My point about sharp wave was that it is a naturally occurring phenomenon which satisfies the two conditions, high frequency and high strength" (Buzsáki and Vanderwolf, 1985, p. 395). All speakers referred to Grastyán's pioneering work, and I was happy to see how much he enjoyed interacting with all the participants.

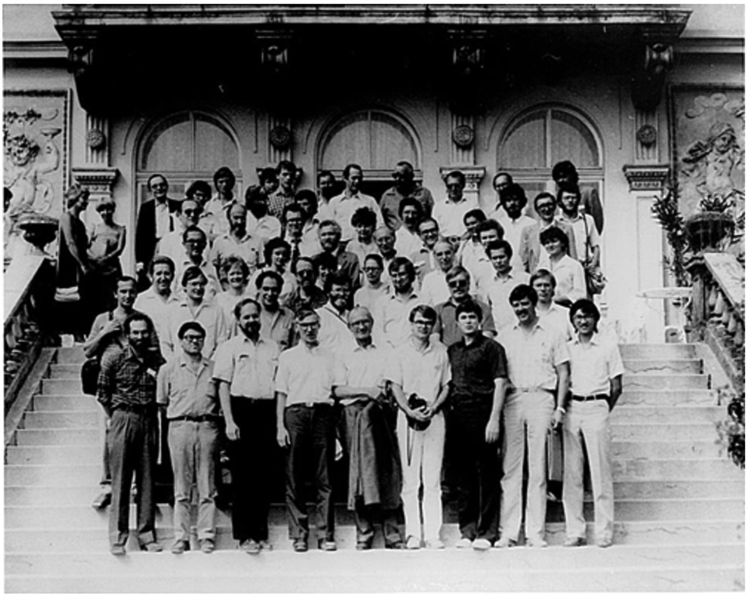

Fig. 4. Symposium for Endre Grastyán's 60th birthday. Vasváry Villa, Pécs. First row, William Levy, Fernando Lopes da Silva, John Kubie, Jim Ranck, Endre Grastyán, György Buzsáki, Ian Wishaw, Brian Bland, Stan Leung; second and back rows: Klaus Reymann, Bob Muller, Steve Fox, Case Vanderwolf, John O'Keefe, Bob Sainsbury, Vincent Bloch, Richard Morris, Rob Sutherland, Ferenc Kekesi, István Mészáros, József Czopf, Lajos Vereczkei, László Molnár, Mark Molnár, and others. (Missing from the picture: Eric Halrgen and Tim Bliss.)

University of Binghamton, New York

Although I could not accept the job at the University of Binghamton, Bob Isaacson invited me back to teach a graduate course on human neuroanatomy. It was planned as a full semester course, but the graduate school allowed me to condense it to two months. The month before my expected trip, Antonio Giuditta (Universitá di Napoli, whom I met in Magdeburg) sent his postdoc, Adolfo Sadile, to my lab to study whether DNA synthesis was involved in LTP. As a control, we used low-frequency stimulation (1 Hz), and we found long-term depression of the evoked responses: "The decrease in PSA [amplitude of population spike] became significant 5 min after the stimulus trains" (Sadile et al., 1991, p. 85). During the second week of our work, I was summoned by the military, as a reservist, for an exercise. After we built a tent hospital in the Bakony mountains and were expecting casualties from an imagined "attack by the imperialists," a political officer summoned me for an "interview." The officers in the room presented the conspiracy theory to me that NATO sent Adolfo Sadile to spy on secrets of the Hungarian Army through me. When I asked how NATO would know in advance that I would be drafted, they were puzzled but not much. I was scared to mention that I had a planned trip to the United States and just hoped that the communication between the military and the secret police was poor. It was. Two days before my planned departure, the exercise ended and all reservists were released.

I went to the Pathology Department and requested a cadaver brain for my course. At JFK Airport in New York City, the customs officer asked about the content of my carry-on bucket, and I told him I was carrying a human brain for teaching purposes. I showed him a certificate that the specimen contained no dangerous substances, and he was satisfied. My students in the course were overwhelmed with happiness being able to inspect, handle, explore, and cut a real brain and to learn about blood supply, the Willis circle, the meninges, ventricles, cranial nerves, and of course the Brodmann areas of the brain.

University of Lund, Sweden: Brain Tissue Transplantation

Our lives with Rusty Gage intersected again. Rusty felt a bit isolated at Texas Christian University and went to work with Anders Björklund (see volume 10) in Lund, Sweden. The Björklund lab was already a leader in neuronal sprouting and transplantation. Their work ushered many clinical trials with tissue transplants in Parkinsonian patients. Rusty introduced me to Anders at a meeting. They explained how they removed the fimbria-fornix to access the vascularized surface of the thalamus, which is an excellent substrate for transplanted embryonic tissue. In my lab, I was also removing the fimbria-fornix but to examine the impact of subcortical inputs on hippocampal oscillations.

We immediately formed a plan to determine whether embryonic hippocampal or septal transplants in the fimbria-fornix cavity could restore hippocampal function. Anders kindly offered me a visiting scientist position that allowed me to visit his department and learn their techniques. I made several trips to Lund, my income tripled, and I was able to buy our first color television. Rusty taught me his already perfected surgical methods, I replicated them in Pécs, completed the physiological investigations there, and brought the brains back to Lund for histological analysis. Every experiment offered something unexpected, and we published several interesting papers about the electrical activity in the transplanted tissue and the restored or malfunctioning patterns in the host brain. I thought this collaboration with Anders and Rusty would continue forever, giving me a comfortable life (and income) in my Pécs laboratory.

An important collateral of my Lund days was my introduction to Tamás Freund (see volume 11). Both Anders and Rusty knew Tamás, who at that time was working with his mentor Peter Somogyi in Oxford. I mentioned to Tamás that epileptiform activity always develops in the transplanted hippocampal tissue, a sign of impaired inhibition. Tamás was eager to investigate and soon reported a discovery to me. The axon-initial segment of transplanted pyramidal cells was devoid of chandelier interneuron boutons. The spike output of the pyramidal cells was without control, which explained the epileptic activity. In the discussion of our paper, we made the tentative (and incorrect) suggestion that epileptic activity kills chandelier interneurons. I corrected this conclusion decades later at Tamás's 60th birthday party. By that time, we had learned that GABAergic interneurons are born in the ganglionic eminence and chandelier cells migrate to the hippocampus in late embryonic days. Because we transplanted the hippocampus at embryonic day 15, our tissue probably did not contain a single chandelier neuron.

One night in Lund, Rusty got a call from Robert (Bob) Katzman, who had just become the chair of the Neuroscience Department at the University of California, San Diego (UCSD). Bob offered Rusty a tenured job. By then, we had established our sincere friendship and started thinking about how we could continue working together. After returning to Pécs, I found a flyer in our library in which the National Academy encouraged young researchers to apply for scholarships working abroad and even listed a couple of foundations. That was quite a policy change. So I started to apply to every foundation I could find in the United States without much success. I also tried the new JD French Alzheimer's Foundation (JDFAF). I had no credentials in Alzheimer's disease (AD), but after learning that the foundation was dedicated to the neurosurgeon John (Jack) Douglas French, the first director of UCLA's Brain Research Institute, whom Grastyán and Eidelberg knew well, I became hopeful. I already heard about his wife, Dorothy Kristin French, the New York Metropolitan Opera diva. When Jack was diagnosed with AD, Dorothy devoted her life to the cure of the disease, and with her Hollywood friends, she founded the JDFAF. My application about AD was pretty vague, but it explained how cholinergic inputs may affect hippocampal and thalamocortical operations. Rusty and Eidelberg kindly wrote letters of support. When I met Mrs. French later, she told me that Eidelberg also wrote her a personal letter saying a few things about me, but she liked the sentence that "this crazy Hungarian likes to dance." To this day, I want to believe that this was the reason why I got a $100,000 grant, which allowed me to join Rusty at UCSD in 1986.

Wonderful UCSD Years

We spent four unforgettable years in San Diego. Our daughter Lili and the Gages' daughters Cheka and Alex were similar in age. My wife and Rusty's wife, Mary Lynn, became close friends. We spent many weekends together, celebrated many family birthdays, and our daughters attended the same Montessori school.

UCSD was a very welcoming environment. To my great surprise, in the first week after my arrival, a nice gentleman, Theodore (Ted) Bullock (see volume 1) was looking for me and told me that he had read my ethology papers. Learning that someone at the other end of the world read Eastern European journals was a strange feeling. But Ted was an exceptional person. He became my unsolicited mentor on physiological issues at UCSD. He introduced me to Bob Galambos (see volume 1) (who discovered echolocation in bats) and Reginald (Reggie) Bickford. Reggie was a student of both Lord Edgar Adrian and W. Grey Walter. Reggie retired in 1980 but kept his lab gadgets in a shipping container, which became my treasure box. The most important piece he donated to me was a computerized 16-channel EEG mapping device with a printer. This was precisely the equipment I was dreaming about. Case Vanderwolf had already shown that cortical electrical activity has both cholinergic and noncholinergic components, and my JDFAF grant was about the impact of the nucleus basalis on the thalamocortical system. I had to build a running wheel apparatus and a 16-channel miniaturized preamplifier. This innovation was made possible by the great electronic shop in the Physics Department at UCSD, where I spent a great deal of time. Quad-MOSFET chips had just become available, and I could stack them into a cube that was easily carried by an adult rat. Sixteen screws in the skull above the neocortex provided a two-dimensional map, and I also recorded unit firing from the cortex, thalamus, or the nucleus basalis (NB). I could record only up to 30 seconds of data with Reggie's EEG machine, but that was state-of-the-art. I did depth profiles of the neocortical LFP (local field potential), combined with lesions of NB or parts of the thalamus. I showed that "damage to the NB resulted in a dramatic increase of slow delta waves," whereas thalamic lesions affected sleep spindles (Buzsáki et al., 1988, p. 4007).

After the paper was accepted, I got a call from Mircea Steriade from Laval University, Quebec City, who was one of the reviewers: "Why does a Hungarian want to destroy my career?" was his opening sentence. Mircea, a Romanian, was the undisputed leader of thalamocortical rhythms back then and believed that the thalamocortical projection was solely responsible for the neocortical arousal. After our telephone discussion, he invited me to visit his lab and we wrote a 61-page chapter on the basal forebrain cholinergic system (Steriade and Buzsáki, 1990). The results and the methods I established were suitable for asking questions in aged animals, especially because cholinergic impairment was believed to be a cardinal problem in AD. With my undergrad student volunteers, we could record cortical maps from the same animal for up to 16 months and found that the most reliable correlate of aging was a thalamocortical rhythm, which I called high voltage spindles (HVS).

I made many friends at UCSD. Phil Groves and his postdoc, Jim Tepper, taught me their way of intracellular recordings. Bob Terry was my adviser on AD-related issues, Marta Kutas (of Hungarian origin) and Steve Hillyard on cognitive topics. Larry Squire, David Amaral, and Stuart Zola-Morgan were my hippocampus buddies. Their super-energetic student, Wendy Suzuki (I was a member of her committee) organized Hippocampus Pizza dinner discussions at the Salk Institute. Francis Crick and the neurophilosopher Pat Churchland (see volume 13) were frequent partners in these debates. Pat sent her graduate student Adina Roskies (now a distinguished professor of philosophy at Dartmouth College, New Hampshire) for a rotation in my lab to learn how to record neuronal spiking activity from hippocampal neurons. Ted Bullock introduced me to his collaborator Walter Heiligenberg, a student of Konrad Lorenz. Walter and Ted studied the jamming avoidance of the electric fish Eigenmannia. The key thing I learned from Walter is that sensation starts with an action. Walter's wife, Zsuzsa, was a piano teacher from Budapest, and the organizer of music life in San Diego. Famous musicians who gave concerts in the San Diego Symphony often ended up for dinner in Walter's and Zsuzsa's home in Del Mar. We spent many nights together with Walter, Zsuzsa, and their three children, who mixed English, German, and Hungarian, during the party conversations. Walter's good friend was Mark Konishi (see volume 6) from CalTech who often came down to cook dinner for the parties. Mark liked to reminisce about his early life in Japan, which resonated with my childhood in Hungary in multiple ways. Unfortunately, Walter was killed in a plane crash while on his way to deliver a lecture at the University of Pittsburgh. He was 56.

IBRO Satellites in Pécs, 1987

The Second World Congress of Neuroscience (IBRO) was held in Budapest in August. I submitted two applications for organizing satellite events in

Pécs. The first proposal was about synaptic plasticity in the hippocampus with Helmut Haas (then in Zürich), whom I met twice in Magdeburg, and the second was about brain tissue transplantation with Rusty Gage, Anders Björklund, and Albert Aguayo. When both applications got accepted, I started to panic. How do I tele-organize such important events from San Diego? My ex-technician and two of my ex-students in Pécs helped me out. I arrived in Hungary with $5,000 cash in my pocket (*nota bene*: it was illegal for Hungarian citizens to possess foreign currency) and brought two micromanipulators, which were ordered by my colleagues in the Department of Physiology, worth $6,000, for which I expected to receive Hungarian forints from the university. Helmut brought 10,000 Swiss francs in his briefcase. We expected about 120 participants, all major players in the plasticity field for the first satellite. Helmut and I arrived three days earlier to check out everything from hotel rooms to the planned concert in the cathedral. But one major item was missing: poster boards. After calling every person I knew for information, one lead was to the House of Armed Forces and I even got the name of the chief officer. Entering his room, I explained that this international conference reflected the salutation of the scientists of the world to the achievements of our great socialist system. I also placed an envelope on his desk. He opened the envelope (it contained 10,000 forints, approximately a month of salary), slid it into his drawer, and asked how many boards I needed. Helmut was shocked to see military trucks in front of the conference hotel and soldiers setting up the poster boards. Helmut and I paid for the hotel rooms, lunches, and dinners, including the gypsy band, with cash for the three days and expected to be arrested by the authorities at any time.

A good part of the symposium was dedicated to long-term plasticity. One of our originally scheduled speakers was Graham Goddard, who had drowned early that year while attempting to cross a storm-swollen river in New Zealand's Arthur's Pass National Park. Graham was 48 years old and had already established his fame with the discovery of kindling. Helmut and I thought it appropriate to dedicate our satellite event to his memory and asked his friend, Tim Bliss, to give a lecture in Graham's honor. Tim's emotional talk discussed the different forms of plasticity and their relationship to epilepsy. Tim admitted that he never liked the term he coined (LLP, long-lasting potentiation), and other abbreviations like LTE were equally difficult to pronounce, whereas LTP (long-term potentiation), the term introduced by Graham, was so much easier to utter. He suggested switching from LLP to LTP honoring Graham Goddard and LTP became the standard abbreviation after the Pécs satellite. That memorable moment of the meeting was worth the effort of organizing it. Today's neuroscience students only know about LTP.

We ended the satellite with an excursion to the Villány wine region, where I paid for the dinner and wine with the money I borrowed from my parents. I had not yet received the reimbursement for the micromanipulators from the university and ran out of cash. Yet, I still had to run another satellite after the main IBRO event in Budapest. One great talk at the main meeting was by Rodolfo Llinás, who talked about low threshold Ca^{2+} spikes and how thalamic neurons can spike not by excitation but by rebounding from inhibition. That was really cool, I thought. After his talk, Peter Somogyi and I managed to "steal" him from the audience, and Peter shared his homemade sausage-filled sandwiches with us while we were picnicking on the grass. In the afternoon, I bumped into Tamás Freund, who was planning to come to our transplantation satellite, and I told him that I had no money to support the event. Tamas ran home, brought back his family's entire savings, handed me a big envelope, and said: you pay it back when you can. He not only saved our symposium but also taught me about trust and the real meaning of friendship.

The transplantation symposium was designed to be a small event, mostly discussing strategies. One speaker from Havana, Cuba, showed a movie in which Fidel Castro watched Parkinson's patients who received adrenal medulla transplants and were playing basketball after their recovery. Many questions were asked but few answers were given. Rusty had to fly back to the United States early the next day and the Cuban speaker offered her services. Picture this: an American neuroscientist, whose father was a decorated U.S. Navy pilot commanding officer, riding in a Soviet Lada car, property of the Cuban Embassy. I survived both meetings and was happy that I ventured into them.

Grastyán told me how much he enjoyed the plasticity satellite. Yet, I was surprised that he did not ask questions and a bit disappointed that he did not come to our social events. He suggested that I submit my Doctor of Science degree to the academy because he wants me to become his successor. Although I felt that nothing was a bigger honor than hearing this gesture from him, I politely responded that he should perhaps think about others as well in case my candidacy was not approved. He then gave me a long explanation of why he returned to Hungary from the United States, despite the opportunities he was offered. That was the last conversation with my dear mentor. He died of lung cancer in a year. He was a chain smoker. A few months later, Rusty and I invited Jan Bures from Prague to San Diego and I talked to Jan about my bad feelings regarding my last conversation with Grastyán. Jan put it plainly: "You are obliged to your talent and have to exploit it. Von Neumann may have ended up as a high school math teacher in Hungary." The comparison was exaggerated, but I got the point.

My First Promotion

Parallel with the nucleus basalis and thalamic studies, we continued our transplant experiments with Rusty. We observed that after the fimbria-fornix lesion, chronic epileptiform events inevitably emerged in the hippocampus. It seemed that such a preparation was suitable to study the impact of neuronal transplants on hippocampal excitability. Nearly all tissue transplants made things worse and induced seizures. Eventually, we succeeded in suppressing epileptiform events by transplanting embryonic locus coeruleus directly into the deafferented hippocampus. Despite this small success, I was confused about my research direction. I did not quite understand the structure of academia in the United States and had uncertainties about my future. The grant proposals we had written with Rusty were successful. Yet, Rusty also recognized that this direction alone was not a sustainable research goal. His group had rapidly switched to studying nerve growth factors and applying genetic engineering methods. I felt that my focus on neuronal circuits and the *in vivo* methods I tried to improve were not interesting enough. Job advertisements were almost exclusively for researchers using *in vitro* methods, the dominant electrophysiology technique of the day.

Rusty had a more optimistic view, and he sponsored my green card (permanent U.S. residency) application. At the same time, he suggested to Bob Katzman to offer me an in-residence faculty position. I was heartened when the faculty unexpectedly recommended me the title of associate professor, a jump from a postdoctoral scholar. This was great news.

Lucky Number 13

My new title did not come with a startup package or new space but with the responsibility to raise funds for my salary and research. But when I began applying for grants, one rejection followed the next. One NSF reviewer's main comment was that the "sole goal of this application is to secure a faculty position to G.B. in the US." I did not understand why this was a crime. Bob Katzman kindly included me in their AD program project renewal with just one experiment and a small budget, yet the reviewers recommended removing me from the project. I applied for a grant from the Epilepsy Foundation of America. I did not receive it, but the rejection letter included an encouraging line that my application was scored high and, therefore, I should reapply. I did at the next round, only to get a second rejection letter. My JDFAF grant was about to run out. My wife worked as a volunteer in the Department of Medicine with no salary. One night, after reading a bedtime story to my daughter, I was watching my sleeping beauty and, with the prospect looming of not being able to pay the mortgage in a few months, was thinking, "What kind of a father I am, who cannot provide support for my family?". Such desperate moments are never forgotten but become part of you, making you stronger after each failure. I shared my desperation with Roger Traub, who was visiting me at UCSD from IBM. Roger encouraged me by saying that my work should be funded eventually. His emotional support coincided with my 12th rejection.

Justin Zivin, a neurologist colleague, submitted a program project to NIH, and I was part of that proposal. My project received the best score from the site visit team, and I felt encouraged. Then, during the spring of 1989, I received good news. Our application with David Amaral and Peter Rapp to study the behavioral, physiological, and anatomical changes following intraventricular infusion of nerve growth factor in aged monkeys was funded by the Alzheimer's Disease and Related Disorders Association (ADRDA). A month later, I received the "pink sheet" from NIH, indicating that the score on my RO1 application was in the fundable range. It was the same proposal that had been rejected by the Epilepsy Foundation twice. Our program project with Justin was also funded by the summer. Things miraculously rebounded. The important lesson I learned from these and many other similar experiences is that resilience to defeat and humiliation is the most crucial ingredient of a scientist. I have written two books, each with 13 chapters. My current laboratory at NYU is on the 13th floor. Our apartment in New York City is also on the 13th floor (Okay, it says 14th in the elevator because 13th floors do not exist in residential buildings). I like the number 13 (and I am not Judas).

Two-Stage Model of Memory

Amid my uncertainties about research directions, there were occasional sunny days. The denervated (fimbria-fornix lesion) hippocampal preparation offered some unexpected advantages. The sharp wave ripples were transformed into large amplitude interictal spikes and single-pulse stimulation of the entorhinal inputs evoked a unique spatiotemporal event with multiple population spikes that I could study with the 8 or 12 recording sites implanted along the long axis of the hippocampus. Following tetanic stimulation of the entorhinal input, the response amplitudes did not uniformly increase, but a new, unique spatiotemporal pattern was induced. The genuinely unexpected thing was that after such induction, the spontaneously occurring population events ("exaggerated" ripples) in the CA1 region were virtually identical to those of the evoked responses, except that no dentate responses preceded the spontaneous CA1 events. Reversing the stimulus polarity and recruiting a different set of perforant path axons induced another unique spatial-temporal constellation of spontaneous events. The repeated spontaneous patterns were present only during immobility and nonREM, the same brain states that promoted sharp wave ripples. This was a eureka moment, and I knew I was onto something important. I saw a potential caricature of what happens during learning and memory. The neocortical information during learning may modify the synaptic connectivity in the CA3 recurrent system, and the spontaneous sharp wave ripples replay the newly formed pattern over and over during nonREM sleep. This was a potential explanation for the puzzle why episodic memories can last forever after a single experience. I shared my excitement with Rusty and rushed to the third floor to outline my new idea to Phil Groves. Phil became so excited that he organized an impromptu seminar the following week, and I used the blackboard to outline my future two-stage memory model. The small audience included Phil, Reggie, Bob Livingston, Pat Churchland, the freshly arrived faculty Terry Sejnowski, Steve Hillyard, and Francis Crick (Rusty was out of town). We went on from 10 A.M. until lunch with exciting discussions and diversions, and I knew that I had to write about the discovery soon, although I was unsure about the format. Pat suggested a review, which I eventually did. She and Terry generously devoted a chapter to the two-stage model in their book *The Computational Brain* (Churchland and Sejnowski, 1992).

Trip to Oxford

Tamás Freund received a fellowship from the Italian pharmaceutical company FIDIA to study the protective effects of gangliosides in the ischemic brain in the Neuropharmacology Unit, Oxford, with A. David Smith and Péter Somogyi. Although several "static" experiments have been performed before and after global forebrain ischemia, there was no information about the dynamic changes that occurred in the brain during and following the ischemic challenge. I built the necessary equipment in San Diego and carried it to Oxford. In two days, we were recording before, during, and after ischemic attacks. In addition to electrical activity, we recorded local tissue O_2 level and temperature with a microprobe developed by the company (Ottosensors) of my engineer friend from Vienna, Otto Prohaska. Before leaving Oxford, I gave a seminar discussing my ideas on sharp wave ripples and the two-stage model of memory formation. Because Tamás and I worried a bit that some of the variability of the results was due to anesthesia, Tamás flew to San Diego, where we replicated the critical aspects of the experiments in freely moving rats.

Shortly after my return to UCSD, I received a letter from David Smith. David, the founding editor of the IBRO journal *Neuroscience*, invited me to join the editorial board. This was an unexpected honor. In addition, David suggested that I submit my planned review of my memory model to *Neuroscience*. I already had an outline, but the final product took another year to finish while I had to attend to other priorities.

Hesitation and Choice

After receiving three grants, my financial security looked okay, at least for a while. I had one graduate student, Melissa Hsu, who was interested in ischemia. I volunteered to teach a course for a group of underrepresented minority students, was an adviser for two medical students, and became the chair of admissions for graduate studies (thanks to David Amaral), my first major administrative duty. I published 12 papers in 1989 and received an accelerated promotion from the department. Everything looked good for me. The situation was somewhat different for my wife. Veronika kept working as a volunteer even after she passed the Educational Commission for Foreign Medical Graduates (ECFMG) exams (today they are called USMLE). In her residency interviews, she was encouraged to rate the University of Southern California highest to "match" their program. We discussed many variations of how to handle our family affairs, mainly because Lili was about to enter elementary school. Living separately in two cities or living together halfway between San Diego and Los Angeles were not attractive options. Until that point in our marriage, my wife supported my ventures, following me around the world. It was my time to give back to her.

Paula Tallal and Ian Creese from UCSD were offered to direct a new center at Rutgers-Newark, and my friend Jim Tepper suggested they recruit me. I liked their vision of the center and the proximity of the University of Medicine and Dentistry of New Jersey (UMDNJ), a potential workplace for Veronika. She applied to the psychiatry program at UMDNJ and had to submit her match rankings in April. One day before her deadline, I called Ian, asking when I should expect a written offer from Rutgers, and explained our urgency. He told me there were routine administrative steps to be followed, but he assured me everything was on track. I was not sure what "everything" meant because we had not even discussed my salary, space, or title. Veronika matched shortly at UMDNJ, and my options came down to one choice, the outcome of which depended on Ian's and Paula's handshakes. When I arrived at Rutgers in 1990 as a full professor, I received everything they offered me verbally.

Bob Katzman retired from his position as chair, and the department was in search of a new leader. Leon Thal was one of the applicants, and he promised me tenure in case he became the new chair. Ted Bullock was super generous and offered half of his lab space for me at Scripps. During my years in Newark, I often fantasized about how much easier it would have been to recruit postdocs with a lab right on the beach. Despite such attractions, we decided to move to the east coast. Rusty threw an unforgettable goodbye party for us. At that point, we had coauthored 29 papers and several additional chapters, but life laid new paths before us.

Twenty Peaceful Years at Rutgers University, 1990–2011

Rutgers-Newark is ethnically the most diverse university in the United States in the heart of this old city in New Jersey. When I arrived in 1990, I had three rooms in three different buildings, a graduate student (Melissa Hsu) and a postdoc (Zsolt Horváth). Melissa remained a UCSD student. Zsolt drove his family in his old Oldsmobile (we called it

Zsoltmobile, which was passed onto two more generations of postdocs) from San Diego to New Jersey. I soon recruited another postdoc from Pécs, a neurosurgeon from China, and Anatol Bragin (a collaborator of Olga Vinogradova from Moscow), and the six of us started the lab. I had already given up on the transplant work and wanted to focus on circuit analysis, where my engineering mind felt comfortable. This required expanding in two directions: intracellular recordings and large-scale monitoring of spiking activity.

First 10 Years

The Intracellular Project

Our intracellular program was conceived in San Diego, when Nobuaki Tamamaki (then from Kyushu University) visited UCSD. He mentioned a new Japan-supported program, the Human Frontier Science Foundation (HFSP) that called for international collaboration. I invited Tamás Freund, Helmut Haas, and Richard Miles (from Paris) to join Nobu and me. The main goal was to characterize the physiological properties of morphologically identified inhibitory interneurons in the hippocampus. We got the award, my first one at Rutgers, which, with renewal, kept us together for six years. My job was *in vivo* characterization. I trained a neurosurgeon postdoc to impale neurons, examine how their properties were related to various LFP patterns, label the neurons with neurobiotin, and reconstruct the entire dendritic and axonal tree. Beginner's luck was with us, and soon we labeled a chandelier neuron in CA1. I faxed a few pictures of the axon arbor to Peter Somogyi, who got on a plane to visit us and trained us to reconstruct the axon connections with a neurolucida tube, and we wrote the outline of a paper, all in a week. Our neuron was the first chandelier cell in the rat. It contacted more than 1,000 pyramidal cells, exceeding previous estimations based on Golgi studies by several-fold.

The second project aimed to reconstruct the axon arbors of pyramidal neurons and granule cells in different hippocampal regions in both the dorsal and ventral hippocampus to quantify the distribution of axons in different layers and estimate the magnitude of convergence and divergence of pyramidal neurons. This four-year project has remained a key reference for intrahippocampal pyramidal neuronal connectivity. Luck struck again when we found a peculiar, never-before-seen interneuron, which we called the "back-projection cell" (Figure 5). The neuron's cell body was in the CA1 region, but its axons crossed the hippocampal fissure and projected back to the CA3, and possibly dentate regions. This pattern was opposite to the well-known trisynaptic excitatory traffic in the hippocampus. Floyd Bloom, the editor-in-chief of *Science*, highlighted our findings in his Society for Neuroscience Plenary Lecture and, subsequently, many types of long-range interneurons have been described in the forebrain.

The project for interneurons yielded several new types and new papers. However, the search for interneurons in the apical dendritic layers failed. Instead of healthy-looking interneurons, postdoctoral fellow Anita Kamondi (today professor of neurology at Semmelweis University, Budapest) recorded only variable amplitude, often wide waveform spikes from mystery structures. We first thought the variable amplitude reflected poor impalement and leakage of a dying neuron. One afternoon, the fire alarm went off, and we had to evacuate. An hour later we returned and the recording still showed the same pattern with burst of spikes during sharp wave ripples. Suspecting that recordings were from dendrites, postdoc László Acsády, who came from the Freund lab, carefully examined the reconstructed pyramidal neurons, and each time we found that activated glia attached to the dendrite at the estimated depth of penetration. These were the first anatomically verified intradendritic recordings in the hippocampus. The results suggested that "sharp wave bursts facilitate dendritic electrogenesis" (Kamondi et al., 1998; p. 3925) and that Ca^{2+} spikes in distal dendrites contribute significantly to the extracellularly recorded theta currents. With time, we became experts with intracellular recording *in vivo*, and it remained our auxiliary technique for many years to come. Postdoc Darrell Henze succeeded in recording simultaneously intracellularly and extracellularly (with tetrodes or silicon probes) from the *same* neurons. These recordings included our first widely shared dataset that served as ground truth for perfecting unit clustering programs in many laboratories (https://crcns.org/data-sets/hc/hc-1) and biophysical modeling of extracellular spikes with Christoph Koch's lab at CalTech. Over the years, we collected intracellular data from more than 450 neurons in the hippocampus, entorhinal cortex, and neocortex. Add three more decades, and we could study the membrane properties of CA1 pyramidal cells in freely moving mice.

Progress in a dozen or so labs on hippocampal interneurons matured enough by 1995 to write the first synthesis. Tamás Freund, Richard Miles, and I discussed an outline of a potential review. This was boosted by the first interneuron meeting I organized with Phil Schartzkroin (University of Washington) and Tamás Freund. It was a satellite event of SfN's annual meeting in San Diego on a Mississippi steamboat in Mission Bay. The text Tamás and I wrote on the anatomy and physiology of interneurons, respectively, got longer and longer, and we contemplated turning it into a book. David Amaral, the founding editor of the *Hippocampus* journal, came to our rescue by offering the pages of an entire issue of his journal. Tamás came to Rutgers for a month, and we worked day and night to meet the short deadline. Wiley brought 400 copies to the SfN meeting in 1996 to promote the journal and large color posters featuring our cover design. All copies and posters were gone in two days and our monograph became a citation classic (Freund and Buzsáki, 1996).

By this time, Peter Somogyi had already done fundamental observations of hippocampal interneuron types *in vitro* and planned to expand

Fig. 5. Celebrating our "back-projection" interneuron paper (Sik et al., 1994). Front: Daniel Carpi. Left to right: Maan-Gee Lee, Zoltán Nádasdy, Arne Ylinen, Markku Penttonen, Attila Sik, G. B., and Melissa Hsu (partially visible is James Chrobak).

his remarkable expertise to *in vivo* reconstructions. He sent Thomas Klausberger to my lab to learn intracellular recording and labeling. Thomas worked super hard, but finding interneurons routinely and labeling them reliably proved not to be a high-throughput direction. We were also doing juxtacellular labeling of neurons, a method also with compromises. When Thomas returned to Oxford, he perfected the juxtacellular method and carved out a remarkable niche in the interneuron field with many admirable experiments.

Large-Scale Neuronal Recording Project

Our large-scale neuronal recording program started with meeting Jerry Pine, a great physicist and maverick from CalTech who sought me out with a proposal to build and test a "cultured neuron probe," a combination of the neuronal transplants, electrode-neuron interface, and control device. It was a silicon structure with 16 wells into which dissociated fetal septal or hippocampal neurons could be placed one by one before implanting into the hippocampus with the hope that the graft neurons could contact the host brain and that, through stimulation of the well neurons, the host brain could be affected. Anatol Bragin, an avid tennis player, loved to ping floating neurons into the probe wells, imagining that he was playing "microgolf" under the microscope. During our collaboration, Jerry introduced me to Ken Wise. Ken, a Bell Labs alumnus, was a professor of electrical engineering and a creator of the Micro-Electro-Mechanical Systems (MEMS) program at the University of Michigan. Ken envisioned silicon micromachining to replace traditional microwires with implantable silicon electrodes for neuronal recordings. I put my bet on this emerging new method, knowing that a metal microwire has only one useful spot, the tip, whereas the rest of it is just bulk that replaces neuronal tissue. In a silicon probe, virtually the entire surface can be recorded from hundreds of sites. Ken and I found a remote table at a CalTech dinner and came up with four-shank and six-shank designs that night. All the early probes were designed for acute experiments and were wire-bonded to a large printed circuit board (PCB). At Rutgers, I cut and filed the PCB to its smallest possible size, soldered the leads under the microscope, and attached it to a microdrive so that it could be chronically implanted in a rat. The next time I met Ken and showed him the "mutilated" PCB and my stacked 24-channel MOSFET preamp, he appeared in disbelief. This started our 30-year collaboration and friendship. The new probes allowed us to record up to 50 neurons at a time, a leap from the typical one to three neurons of the day, and investigate the "time-varying organization of neuronal populations" during sharp wave ripples. With the new approach, we had our first major paper 18 months after I joined Rutgers, which demonstrated that "powerful depolarization of the excited interneurons results in high-frequency firing of these cells which, in turn, determine the precise timing of pyramidal cell discharge" (Buzsáki et al., 1992, p. 1027). Ken's group created the "Michigan probes," which turned into the NeuroNexus company, and his technology is the direct precursor of today's Neuropixels.

The linear silicon probes also allowed for the simultaneous recording of LFP in different layers in cortical structures and quantifying the direction of current flow between extracellular compartments (i.e., current source density analysis). We could zoom into the neuronal substrates and mechanisms of theta, gamma oscillations, sharp wave ripples, neocortical spindles, slow oscillations, and other network patterns. We studied information exchange across brain regions assisted by the various brain oscillations and their phase-amplitude coupling and published a barrage of papers on these topics. Xiao-Jing Wang (from the University of Pittsburgh) spent the summer of 1995 in my lab, and we published an influential computational model on gamma oscillations (Buzsáki and Wang, 1996). The large-scale recording project has expanded in several ways and over the years has become the strength of the lab.

To exploit our geographical situation and be part of a bigger community, Bruce McEwen (see volume 8) from Rockefeller University and I founded the NY/NJ Hippocampus Club in 1991. In our monthly events, students, postdocs, and lab leaders from the New York City and New Jersey labs gathered either in our center at Rutgers or Rockefeller. The presenters were students or postdocs and the presenting lab paid for the drinks and pizza (which we brought from New Jersey to save money). The club survived and prospered for eight years. Bruce and Ian Creese (my codirector) nominated me to become the chair of the Biomedical Section of the New York Academy of Sciences, which became another excellent opportunity to interact with neuroscientists beyond our small community at Rutgers-Newark.

To our great happiness, our second daughter, Hanna, was born in 1992, just before my wife started her child psychiatry rotation. She received a two-week (!) break for her maternal leave. Her residency program kept her busy, so sharing the responsibility of parenting was a must, which was eased a lot by the long visits of either my mother or my in-laws. I hardly ever discussed my work, grant, and paper rejections with my wife those days. When I arrived home, there were other duties and problems to solve while we were working hard on two careers and raising children and trying to fit into a society so different from the one in which we were brought up kept us busy. Driving kids back and forth to school and afternoon programs are familiar exercises to parents in the United States. I often wondered how much more productive young people could be if preschools were available at the workplace and school kids could rely on efficient and safe public transportation, as are the norms of life in other industrial countries. This parental dependence was so different from my childhood, with all the freedom I had. More importantly, as kids, we learned much more about each other outside school than during classes, and that early imprinted experience was an essential preparation for understanding the true nature of friendship and human interactions later in adult life.

My first featured lecture invitation was the Bottazzi Lecture at the 43rd Annual Meeting of the Italian Physiological Society in Sorrento, just before the presentation of the venerable John Zachary (J. Z.) Young (see volume 1) from University College London. J. Z. was an honorary member of the *Stazione Zoologica* in Naples and a lover of Italy. He made me swear that I would bring back my family to Sorrento someday. A few years later, after visiting Adolfo Sadile in Naples, we spent a few days on the Amalfi coast with my family, staying in a beautiful hotel with mosaic floors and frescoes on the wall. My elder daughter (12 years old at the time) was not happy with the surroundings and complained that the hotel was "such an old fashion" place and so different from the spectacular hotels in Atlantic City, New Jersey, and Las Vegas. That night, my wife and I decided we needed to provide some European education to our daughters. Just before Hanna entered elementary school, my wife paused her job (already working in private practice), and we left for my sabbatical in Paris in 1999.

Sabbatical in Paris

If it were up to me, I would make it mandatory that every person in every country should spend time in another culture. In Paris, our daughters discovered in no time that the cultural and fashion center of the world was not Millburn, New Jersey. Hanna went to a French school, while Lili attended an elegant international high school on Avenue Victor-Hugo. My hosts, Alain Berthoz at Collège de France and Yehezkel Ben-Ari at Port Royal Medical Center, treated me extremely well. Alain had a huge machine shop, and with the engineer, we designed our C-shape super-light microdrive prototype, which we proceeded to use for a decade. For years, Ben had worked on a phenomenon known as giant depolarizing potential (or GDP), an in vitro network-driven membrane oscillations with superimposed fast action potentials. I reasoned that GDP must be the *in vitro* equivalent of sharp wave bursts *in vivo*. Ben's argument was that they were distinct because GDPs disappear after a week in the postnatal life of rats. I thought the best way to settle the issue was to join his lab and perform *in vivo* experiments on rat pups. Ben's lab was full of activities. My desk was next to our rig, surrounded by offices of students and postdocs, including Rosa Cossart and Christophe Bernard. Rustem Khazipov was my main experimenter buddy. He applied his patch-clamping experience *in vitro*, and we soon succeeded in demonstrating the first patch-clamp recordings *in vivo*, coupled with silicon probe recordings. The earliest pattern we observed in the hippocampus was a sharp wave burst reminiscent of giant depolarizing potentials observed *in vitro*, and it persisted beyond the first week of life. Theta oscillations emerged during the second week, as Brian Bland had already shown, but ripples appeared only three weeks postnatally. The reviewers of our manuscript liked our story, but two of them complained that they expected that work with Buzsáki as the senior author should have been done in the behaving pup, whereas our experiments in Paris were performed under anesthesia. When I returned to Rutgers, Xavier Leinekugel from Ben's lab came with me. We spent a year replicating our results in behaving pups and the resubmitted work was accepted (Leinekugel et al., 2002).

Although sharp waves were the earliest first pattern in the hippocampus, they were often coupled with a rhythmic neocortical pattern, as we observed with Rustem, who also joined us at Rutgers. We termed this rhythmic pattern "early spindle" because it was the earliest organized neocortical activity. In the somatosensory cortex, each spindle was invariably triggered by muscle twitches, limb jerks, or whole-body startles: "the interaction between movement-triggered sensory feedback signals and self-organized spindle oscillations shapes the formation of cortical connections required for sensorimotor coordination" (Khazipov et al., 2004, p. 758). With this work, we provided evidence that the baby kicks in the womb, which is important for normal development of the fetus by teaching the brain about the shape and size of the growing body. Rustem continued his work as a group leader at the Institute of Mediterranean Neurobiology (INMED) in Marseille and founded the Institute of Neuroscience of Kazan State Medical University in Kazan, Tatarstan. After Xavier and Rustem, more than a dozen French researchers joined my lab in subsequent years. My sabbatical time was well invested.

Building Our House

My sabbatical in Paris divides my two decades of tenure at Rutgers University. But there was another dividing event as well. Before moving to Paris, we sold our house and bought a run-down ranch in a nearby town with a good public high school. In Paris, my wife and I began to read extensively about contemporary architecture and interior design. One late night, visiting the book section of the Virgin Megastore on the Champs-Elysées, we found a book on glass houses by Peter Forbes, a student of Ludwig von Mises, and we knew immediately what kind of house we wanted to build. Peter Forbes is based in Florence, Italy, and has a reputation for designing ecofriendly structures. Peter alerted me to one of his trainees, who was an architect teaching at Rutgers, and the three of us designed our new home. I became a contractor, handyman, electrician, plumber, and interior designer, all at the same time. My wife and I built a kitchen, two bathrooms, stairs, and furniture in the living room and our study, and we helped the workers in every possible way. It was a great teamwork. The original ranch house was ugly, and my daughters would not even give their address to their classmates and did not invite anyone to visit. But once the new house was shining, my younger daughter overheard the conversation between a young girl and her father in the street saying, "This is the coolest house" (https://www.youtube.com/watch?v=tiLW-K935rI). From then on, both my daughters loved their new palace.

Close to the finish line, I was carrying two buckets full of tiles while climbing a ladder. I almost made it to the top when I lost my balance, flipped backward, and landed on the concrete floor head first. My wife and Hanna left for food shopping minutes earlier. When I regained consciousness, I could move my left hand to my ear, and I felt something flowing. I mentally traveled back to a surgery lecture in my medical school, when our professor explained that hemorrhage in the ear canal is a signature of a *scala media* fracture and is deadly. Fearing that the end was coming, I crawled on one side to the telephone in the living room and called 911. Firemen arrived and broke through the front door. The next thing I remember was seeing many shoes around me and, sad faces of my wife, daughter, and strangers. I broke six vertebrae, and my left otolith and damaged my shoulder. Only two days later when I recovered from spinal diaschisis and had my first urination in the emergency room I felt alive again. I learned how to sleep (or at least rest) with my eyes open because when I closed them the whole world started to spin around me. After I was discharged from the hospital, my lab came to my rescue. America Online internet service had just become available in New Jersey. Anton Sirota, my faithful graduate student, worked hard to establish a connection between my bed at home and the lab. When I started walking again, I told myself that from now on every day is a gift of life. I should be nice to everyone and grateful for everything. But of course, with time, I just got back to my old self and continued doing everything the same as before my second head concussion. Yet, I noticed that my creativity improved. Of course, this may be just my imagination and the alternative to head concussion-induced improvement is that I started to cherish previously unnoticed moments of life with increased optimism.

Second Decade at Rutgers

At about the same time when I moved to Rutgers, David Tank became the group leader at Bell Labs in Murray Hills, New Jersey. David's house was only a few minutes from ours, and we have spent several parties together. David put together an extraordinary team, including Winfried Denk (the codiscoverer of two-photon laser scanning microscopy), Karel Svoboda, Michael Fee, and David Kleinfeld. My monthly visits to Bell Labs and the unique opportunity to spend time with these super-bright physicists were the intellectual highlights of my regular life. When they moved to *in vivo* preparations, Karel often showed up in our lab to learn about intracellular recordings. I spent many days in his lab to learn about the new imaging technique that catapulted them to fame in just a few years. Thinking that imaging is essential for progress in systems neuroscience, postdoc Hajime Hirase built our two-photon rig, with the generous help of Rafael Yuste (Columbia University). Although the original plan was to monitor neurons, we had better luck with imaging pericytes and astrocytes. Our astrocyte imaging was the first *in vivo*, and we showed that population bursts of neurons can selectively affect some but not other processes of astrocytes. Hajime took his experience to his future lab at RIKEN, Tokyo, and devoted his career to studying glia.

When Bell Labs was shamefully discontinued by corporate interests, the researchers spread. Karel moved to the newly built flagship institute of the Howard Hughes Medical Institute, the Janelia Research Campus in Virginia, and invited me to become a visiting scientist. I had a small experimental room there for six years, with full support of Jeff Magee with whom I had already interacted for a decade. My official funding from HHMI was for a project to enable chronic recordings from at least 64 sites using multiplexed technology. This was an important goal because mice rapidly became the species of choice in neuroscience. Postdoc Sebastien Royer (now a professor at KIST, Seoul, Korea) moved from the Rutgers lab to Janelia, and I traveled to visit him and Jeff biweekly or monthly. Soon after the discovery of optogenetics, Sebastien, Jeff, and I thought that perturbing circuits in a neuron-specific manner is what systems neuroscience needs. We learned how to thin and sharpen glass fibers with hydrofluoric acid (a fatally dangerous chemical) and to attach the fibers to the shanks of our silicon probes. In less than a year after the landmark optogenetics paper of Ed Boyden and Karl Deisseroth, we were making light-driven artificial place cells, followed by specific silencing of parvalbumin and somatostatin-expressing interneurons. These experiments demonstrated that "perisomatic and dendritic inhibition have distinct roles in controlling the rate, burst and timing of hippocampal pyramidal cells" (Royer et al., 2012, p. 769).

When miniature signal multiplexers became available, my postdoc Antal (Toni) Berényi built the first 64-channel system for mice and soon a 512-channel device for rats. With that, our expensive 128-channel recording systems with myriads of cables and occupying half of the room became obsolete overnight and was replaced by Toni's system, which was smaller than a desktop computer and cost only a few thousand dollars. With Toni, we also asked how we could affect epileptic seizures noninvasively by transcranial electric stimulation (TES): "Closed-loop TES can be an effective clinical tool to reduce pathological brain patterns in drug-resistant patients" (Berényi et al., 2012, p. 738). Upon his return to Hungary, Toni Berényi founded his company Neunos for epilepsy management, which was recently acquired by BlackRock, Inc.

One day, a mathematician and graduate student in robotics from University College London, Ken Harris, visited Rutgers' String Theory group and my lab in exploration of postdoc options. After graduation, Ken got a job at Lloyd Bank, a typical career path for math students. A few months later, he asked me whether the postdoc position was still available in my lab. Ken joined us soon, accepting a budget cut from his salary of more than 60,000 British pounds to $28,000. To me, this indicated his dedication! I told Ken that the most important problem in neuroscience was the separation of extracellularly recorded units. Ken immediately got to work and perfected József Csicsvári's unit cluster-cutting algorithm. Postdoc Michaël Zugaro and his partner Lynn Hazan, a software engineer, were already working on our NeuroScope development. A good part of our weekly lab meetings was devoted to questions and wishes of lab members to perfect both NeuroScope and Ken's program, which became KlustaKwik. After extensive testing, we shared both programs with the neuroscience community, which were improved further by the many contributing labs.

In the meantime, Ken absorbed a tremendous amount of neuroscience knowledge and realized that the clustering of neurons was not the most important problem. We worked on a variety of topics until Ken zoomed in on a key question, the mechanisms of cell assembly formation. We found that spike timing of "pyramidal cells can be predicted more accurately by using the spike times of simultaneously recorded neurons" within the "temporal window [that] matches the membrane time constant of pyramidal neurons, the period of the hippocampal gamma oscillation and the time window for synaptic plasticity" (Harris et al., 2003, pp. 552; 555).

Anton Sirota (now a professor at Ludwig-Maximilians-Universität, Munich, Germany), Kenji Mizuseki (now a professor at Osaka Metropolitan University, Japan), Kamran Diba (now a professor at University of Michigan), Shigeyoshi Fujisawa (now a professor at RIKEN, Tokyo), and George Dragoi (now a professor at Yale University) overlapped with Ken and

Eva Pastalkova, marking an especially productive epoch of the lab. Kamran observed that neuronal sequences during sharp wave ripples are a palindromic game; playing the same place cell sequences either forward before exploring the environment for "planning upcoming trajectories" or reverse "run sequences at the end of the trial could facilitate backward associations" (Diba and Buzsáki, 2004, p. 1242). A further boost for the functional significance of sharp wave-ripples came from the experiments in which we showed that their elimination led to serious memory impairment (Girardeau et al., 2009). In addition, the high spatial resolution of the silicon probes allowed us to realize that the compact cell body layer of the hippocampal CA1 region was in fact two layers as if the layers of the neocortex were squeezed into a narrow layer. In parallel, Anton's work demonstrated how hippocampal theta oscillations entrain local gamma rhythms in wide neocortical areas.

The replay experiments lead to frequent lab discussions of whether the brain starts out fresh as a *tabula rasa* and gets modified postnatally by experience or whether there are functionally preconfigured modules from which experience can choose. Harime's, Ken's and George's experiments were more compatible with the idea that brain dynamics create a "realm of possibilities" and experience can match some of these preexisting patterns. A fascinating question was whether the activity of hippocampal place cells is determined by sensory inputs spike by spike or generated by an internal program. Postdoc Eva Pastalkova and I designed a two-choice memory experiment in which the rat was trained to run in a wheel for 15–20 seconds between trials and to keep in mind which part of the maze it planned to go. Based on these experiments, we hypothesized that "during recall, imagination, or action planning, the sequence identity is determined by the intrinsic dynamics of the network" and that "neuronal algorithms, having evolved for the computation of distances, can also support the episodic recall of events and the planning of action sequences and goals" (Pastalkova et al., 2008, p. 1327). We performed parallel experiments in the prefrontal cortex (Fujisawa et al., 2008), which showed similar sequentially organized neuronal assemblies, and we also found that the hippocampus and prefrontal cortex work hand-in-hand in action planning and memory tasks. Because assembly sequences reliably predicted elapsed time, we hypothesized that "mechanisms that evolved for spatial navigation also support tracking of elapsed time in behaviorally relevant contexts" (Itskov et al., 2011, p. 2828). Our observation prompted Howard Eichenbaum (Boston University) to call hippocampal neurons "time cells," initiating a novel branch of hippocampal research.

Rhythms of the Brain

In research papers, scientists write to other specialists often in language understandable only to the superspecialists. However, experimental findings from the laboratory gain their power when they are understood by people outside the trade. As brain oscillations have begun to be understood by their spiking contents, there was a need to bring together two large but independently studied areas: the microscopic spiking and the mesoscopic local field potentials. I felt the urge to do it but did not quite know how to start. To support my sabbatical visit to Paris, I applied for a Fogarty International grant. After the initial rejection, the revision was approved but only after I had already returned to the United States. The foundation kindly agreed to split it into halves, and I spent parts of two summers in Ben-Ari's lab, now in Marseille. Ben kindly let us rent his medieval apartment in the historic harbor of Marseille. While my family and visitors were playing cards each night, I began to outline a book about brain rhythms, six years before it eventually got published. Halfway through the writing, I received an invitation from *Science* to write a review about brain oscillations. I was already prepared and Andreas Draguhn (University of Heidelberg) and I submitted the manuscript in six months (Buzsáki and Draguhn, 2004). The published book (*Rhythms of the Brain*, 2006) was well received. Its punchline summary, that "brains are foretelling devices and their predictive powers emerge from the various rhythms they perpetually generate" (p. vii) resonated with neuroscientists, cognitive scientists, physicists, and engineers alike. Predictive coding became a household phrase in brain research. Justification of the role of neuronal rhythms in brain communication was an uphill battle. In a general sense, communication is an agreement between a sender and receiver. This "agreement" is called the cipher known to both parties. A key aspect of the cipher is a procedure by which the messages are broken down into smaller chunks. Chunking of messages by agreed rules allows the coding and "deciphering" of virtually infinite combinations from a finite number of chunks. This small set of rules, usually referred to as syntax, "governs the combination and temporal progression of discrete elements into hierarchical relations that allow the generation of messages" (Buzsáki, 2010, 365). I reasoned that the potential substrate for a neuronal syntax in the brain is the constellation of neuronal rhythms. Honestly, I never dreamed about the combined impact that the book and the review had on the field. But I am happiest about the moments when a young researcher tells me that the reason why they chose to study neuroscience is because they read the rhythm book.

One person who read the book was Richard (Dick) Tsien (see volume 11) at Stanford University. Dick already had a reputation for his formidable memory and his ability to summarize one's complicated talk in three short sentences. My first lengthy conversation with Dick was at a Jacque Monod Conference in Roscoff, France, in 2005. After my plenary talk, Dick asked me to sit next to him at dinner and warned me to get prepared. Dick expects yes or no answers to his questions, which are often not straightforward in neuroscience. At the end of dinner, we promised each other that we would continue our conversation. And we did so on multiple occasions, including an event in Shanghai. On a walk to the conference venue, Dick was explaining something enthusiastically, immersed in thought, and only watching the red light turning to green but not the traffic. A cab rushing through the already red light was approaching Dick at a high speed at which point I instinctively grabbed his jacket and pulled him back to the sidewalk. Dick likes to think that I saved his life, and I like to believe it. Independent of this incident, Dick and I were already discussing how wonderful it would be to work together in the same institute. Not knowing about each other's intentions, both Dick and I were invited to interview for the director position at the Helen Wills Neuroscience Institute at the University of California, Berkeley. At the same time, Rodolfo Llinás recommended both of us to replace his director role at New York University. Dick was offered both jobs and turned down Berkeley. I was the runner-up at Berkeley and was supported by both Mu-ming Poo (see volume 13) and Robert Knight who represented two ends of the scientific spectrum in the institute. The prospect of leading this extraordinary institution felt great for my ego. As an alternative, Dick suggested to join him to build the new Neuroscience Institute at NYU. My hesitation between the prestige in California and the opportunity to work in New York City peacefully lasted only for a weekend. Monday, I withdrew my application from UC Berkeley and accepted the offer at NYU.

Lab Life and the Challenge of Recruiting Colleagues

Our small center at Rutgers-Newark was a far cry from the large neuroscience community of UCSD, the Salk Institute, and the Scripps Research Institute. Recruiting good students and postdocs was a challenge. The best strategy has proven to be reaching out to my colleagues and begging them to send their young to my lab. I met József Csicsvári when he was still a high school student and told him to study engineering and computer science. He did and applied to our graduate school at Rutgers after he got his degree in Budapest. Anton Sirota was referred to my lab by a Russian scientist who spent a year in my lab at Pécs. Both József and Anton spent eight years with me as graduate students and postdocs. Hungary, France, Finland, and Japan, and more recently Spain, China, Germany, and Iran are the countries from which I recruited most of my lab members. Personal-level recruitment has always been the most effective strategy for me. During my second visit at Collège de France in 2008, I shared a large desk with several students, including Adrien Peyrache (now a professor at McGill University) and Gabrielle Girardeau (now group leader at Institut National de la Santé et de la Recherche Médicale, Paris) who joined my lab after they graduated along with Lisa Roux (now a group leader at Université de Bordeaux) and several other French scientists. I met Antonio Fernandez-Ruiz (now directing his own lab at Cornell University) at a summer course I organized in Barcelona, and who recruited several more Spanish investigators to the lab. I have always helped postdocs coming from abroad to obtain their own support from national and international grant agencies. In this process, they learn the challenge of grant writing, and acquire stronger self-confidence and independence when they arrive and start their projects.

The most important ingredient of a successful working environment is that all members feel safe. Feeling safe, giving, and receiving moral support from others is especially critical in an environment where young and often naïve people gather together from different parts of the world with different cultures and value systems. By design, I never had a lab technician in my lab. Instead, the chores are divided among us. Each person is a "mini-boss" in charge of the computer network, surgery room, histology, machine shop desk, organizing lab meetings, seminars, and journal clubs, so that they can reprimand the violators and can expect to be reprimanded by others in case of misbehavior. Perhaps the most difficult thing for a lab leader to do is to "implant" ideas into the heads of others so that the student or postdoc believes that the idea is their own. Scientists are the best workforce in the universe because they work on their ideas and therefore are willing to venture into never-explored territories and tolerate high levels of uncertainty. I worked with them to write their own grant proposals whether it was for a fellowship or a K99. I designed my lab at Rutgers so that everyone's desk was in a single large room, next to the labs and my office, and I always kept my door wide open, as an invitation to walk in. I have had lunch with lab members every day. Participation is completely voluntary without any pressure or agenda. Animated lunch discussions in which we all feel like equal partners are the best and easiest way to learn about each other, the difficulties of everyday private life, frustrations, and dreams. In addition, this is an opportunity to address bigger questions of our science, which rarely enter the discussion of our papers but which offer a broader perspective or framework within which we can conceive new experiments and discuss what we find. Lectures and seminars are tailored for an abstract average person with the naive assumption that the audience retains all the details and follows the logic of the lecturer. In contrast, the essence of small-group conversation is to question the fundamental logic, a quest for clarification and simplification, and a search for explanations and answers without a rigid agenda, in which the focus is not on covering large chunks of material but on understanding the critical details.

Accepting a new member to my lab is an unwritten contract between that member and me that I will take responsibility for nurturing and guiding them to the next level of their lives whether it is a postdoc position, faculty job, or industry. Members typically "calibrate" their abilities in comparison with others during their tenure in the lab and there is rarely a discrepancy between their expectations and my judgment. Our weekly lab meetings, lasting longer than two hours, are the best forum to evaluate each other. The presented progress reports and the fierce but honest discussions are our most constructive activities. I sincerely hope that no lab member has ever left a lab meeting feeling that they were ridiculed or even slightly ashamed. Instead, the feedback provided by these "many eyes see more" discussions often help to guide useful modifications and improvements of the strategies.

I evaluate people slowly perhaps because I often witnessed the pattern that a student's or postdoc's performance may appear "mediocre" for two or three years only to rebound unexpectedly with a discovery. This is the nature of scientific investigation. I never had to fire a student or a postdoc. Perhaps I could not. It is tempting for a lab leader to team up with people with complementary skills for a planned project. Yet, in my experience, good collaborations emerge from social interactions. Taste for the same type of movies, playing tennis or other sports, or just drinking coffee or tea together provide the foundation of trust that can lead to more efficient collaboration than rational logic. Feeling comfortable with a fellow lab member at any moment inside and outside the lab is far stronger a drive than the potential benefits of the collaboration. Failing together is often as much a reward as succeeding. Several marriages and lifelong friendships have been sparked from such interactions in the lab over the years. Nothing pleases me more than seeing that my "kids" get along with each other (Figure 6). For me, the success of mentoring is not only measured by the academic ranks achieved by my mentees but also by their happy family lives and the photos they send me about moments of their private lives. Science is a social endeavor.

Fig. 6. Nine lab alumni (now professors around the world) dining in Verona, Italy, May, 2023. Left to right: Antonio Fernandez-Ruiz, Sebastien Royer, Kamran Diba, Gabrielle Girardeau, Adrien Peyrache, Karim Benchenane ("honorary member"), George Dragoi, Lisa Roux, and Anton Sirota.

An Unexpected Call

Returning to my room from the conference hall at the Santa Fe Institute in February 2011, I found a message from Denmark to call back the number. I recalled that a few months earlier I had submitted a recommendation for my friend Peter Somogyi for the new Brain Prize from the Lundbeck Foundation. Secretly hoping that my nominee got the prize, I called back the number. As I was listening, I got a bit confused as to why the caller congratulated me, but it slowly sank in that I was one of the three winners. I wondered who had nominated me. As I learned later, nobody did. However, when the committee's selection came down to two names, Peter Somogyi and Tamás Freund, some members felt that I also belonged to that group. Rewarding our trio was a nod to the excellence of Hungarian neuroscience over a century.

New York University Langone Medical Center

Our move from Rutgers to NYU happened overnight. Several lab members already lived in New York City and they established our first "colony." Within a month after we all moved to NYU, we were running experiments.

NYU offers a friendly collaborative environment. Postdoctoral fellow Dion Khodagholy developed the "NeuroGrid," an organic, conformable recording device to record from many cortical sites (Khodagholy et al., 2015). After testing the functionality of NeuroGrids in rodents, in virtually no time, we were able to test them also in patients with epilepsy, thanks to our fruitful collaboration with neurosurgeon Werner Doyle and neurologist Orrin Devinksy. We forged new collaborations with several NYU colleagues, including Dick Tsien, Robert Froemke, Dayu Lin, Rodolfo Llinás, Michael Long, Bernardo Rudy, Bob Machold, Anli Liu, Sage Chen, David Heeger, John Rinzel, Tony Movshon, Xiao-Jing Wang, and Dan Sodickson. We continued our tool developmental efforts with the engineering group at the University of Michigan, now headed by Professor Euisik Yoon, leading to innovative μLED probes that allow for optogenetic probing of a single neuron or a few neurons as well as several other devices.

Our New Framework

After I received the Cortical Discoverer prize from the Cajal Club in 2001, I was invited to write a review for a prominent journal. I thought that the best way to exploit this opportunity was to write an essay asking whether the dominant framework in neuroscience is on the right track. My main argument was that many terms in neuroscience are inherited from folk psychology and are often used in two ambiguous ways: both as the thing-to-be-explained (*explanandum*) and the thing-that explains (*explanans*). I warned that a framework dictated by human-centric introspection might not be the right roadmap for neuroscience and argued that there should be another way of carving up the brain's "natural kinds." A month later, I received the rejection letter: "Dear Gyuri, … I hope you understand that for the sake of the journal we cannot publish your manuscript." Yet, the issues I exposed in the manuscript kept bugging me and after moving to NYU, I decided to expose my views to a broader audience in the form of a new book (*The Brain from Inside Out*, 2019). Ever since the time of Aristotle, thinkers have assumed that the soul or the mind is initially a blank slate, a *tabula rasa* on which experiences are painted. This view has influenced thinking in Christian and Persian philosophies, British empiricism, and Marxist doctrine. In the past century, it has also permeated psychology and cognitive science. This "outside-in" view portrays the mind as a tool for learning about the true nature of the world. The alternative view—one that has defined my research—asserts that the primary preoccupation of brain networks is to maintain their own internal dynamics and perpetually generate myriad patterns of neural activity. When such preexisting nonsensical patterns are linked to actions and potentially useful environmental signals, they acquire significance to the organism. Thus, learning is more of a matching process rather than a *de novo* computation. In addition to receiving feedback about the influence of the book from neuroscience colleagues and beyond, a cherished bonus was the 2020 Prose Award from the Association of American Publishers.

Balance, symmetry, and normality dominate our thinking and culture, perhaps because of their simplicity. Similarly, in neuroscience, we seek for and tend to present "typical" or "representative" neurons and dynamic patterns. But when with our large-scale recordings we began to collect data by the hundreds and thousands, and quantified the results, a very different picture emerged. We and others found that at many physiological and anatomical levels in the brain, "the distribution of numerous parameters is in fact strongly skewed with a heavy tail, suggesting that skewed (typically lognormal) distributions are fundamental to structural and functional brain organization" (Buzsáki and Mizuseki, 2014, p. 264). The brain is not composed of numerous similar components and is not as plastic as it previously seemed, but rather component diversity and nonegalitarian organization ride on the backbone of a preconfigured connectivity and dynamics characterize neuronal networks. Such preexistence is often referred to as attractors, manifolds, or neural schemata. This insight not only had implications for how we now collect, analyze, and interpret observational data but also gave rise to a different type of thinking.

During the past decade, we operated mainly under this new framework. Although the centerpiece of our work has remained the hippocampus, we reached out to its partner structures to better understand its computation. Expanding our hippocampal work to the head direction system was a rewarding experience, and the findings provided evidence that mechanisms that appear to be controlled externally are in fact self-organized and present even during sleep (Peyrache et al., 2015). A series of experiments was devoted to examine the extent to which apparently learned and newly synthetized neuronal patterns were preconfigured (Grosmark and Buzsáki, 2016; McKenzie et al., 2021; Valero et al., 2022). The new thinking led to our investigations of the embryonic origin and constraints of neuronal connectivity and physiological interactions in the adult brain. In contrast to the dominant framework that portrays neural systems in which sensory inputs sculpt connectivity and dynamic patterns through activity-dependent plasticity ("neurons that fire together, wire together"), we showed that "neurons born together, wire and fire together" (Huszár et al., 2022, p. 1210) is an alternative scenario. Even more surprising revelations were ahead of us. While many laboratories have investigated sharp wave-ripples as a cognitive biomarker, in reality, sharp waves evolved much earlier than the complex neocortex that reads out the mnemonic content of hippocampal sharp waves. We discovered that this very pattern also serves to reduce interstitial glucose in the body, and thus "two seemingly distinct processes—cognition and whole-body metabolism—are linked together" in the brain by hippocampal sharp wave-ripples, and "mnemonic and metabolic processes are regulated simultaneously within an organism" (Buzsáki and Tingley, 2023, p. 204).

I tend to believe (or at least pretend) that the most recent manuscript we happen to be working on is the most important. After working on hippocampal sharp wave ripples for decades, our recent work finally seems to make sense of this remarkable brain pattern. A general wisdom is that experience needs to be somehow tagged during or shortly after learning, but the brain mechanisms responsible for such triaging have yet to be searched for in earnest. Now we found that the sharp wave ripple is the selecting mechanism. Only when sharp wave ripples tag a particular experience will that experience be replayed over and over during subsequent sleep. Sharp wave ripples are thus a post hoc editing mechanism that selects aspects of experience to be preserved for future use (Yang et al., 2024). Thus, my excuse for not mentioning potentially important events in our lab life during the past several decades and omitting names of numerous talented students and postdoctoral fellows and colleagues whose hard work was so important to my scientific life is that not enough sharp waves emanated in my brain during critical events or when attempting to recall those events. As a substitute, I refer the reader to a 2019 photograph (Figure 7), when most previous and current lab members came together to celebrate our friendship (the excuse being my 70th birthday). Further sources of information include the Neurotree (https://neurotree.org/neurotree/tree.php?pid=5038) and our lab alumni web page (https://Buzsákilab.com/wp/labmembers/group-photos/).

Fig. 7. Group get-together in 2019. First row: Mihály Vöröslakos, Kamran Diba, Xiao-Jing Wang, G. B., Azahara Oliva, Anna Conti, Farnaz Sharif, Ekin Kaya, Veronika Solt (my wife). Second (intermittent) row: Xiaoyang Long (guest), Kenji Mizuseki, Anton Sirota, Manuel Valero, Karim Benchenane, Shengjia Zhang (guest), Andrea Cumpelik, Yiyao Zhang, Laura Green, Jolin Chou, Stephanie Rogers, Rachel Swanson, Maria Asplund (visitor), Derek Buhl, Dirk Isbrandt, Viktor Varga. Third row: David Sullivan, David Tingley, Andres Grosmark, Jennifer Gelinas, Yoshikazu Isomura, Lisa Roux, James Chrobak, Zifang (Frank) Zhao, Fan Wu, Darrell Henze, Luke Sjulson, Heather Read, Gabrielle Girardeau, George Dragoi, Antonio Fernandez-Ruiz, Behnam Tayebi (guest), József Csicsvári. Fourth row: Ken Harris, Dion Khodagholy, Yuta Senzai, Thomas Hainmüller, Erik Schomburg, Ipshita Zutshi, Peter Rozman, Dan English, Roman Huszár, Dan Levenstein, Xavier Leinekugel, Kathryn McClain, Eliezyer Fermino de Oliveira, Omid Yaghmazadeh, Asohan Amarasingham, Adrien Peyrache, Sebastien Royer.

Missing alumni from the picture: Gyula Acsádi, László Acsády, Costas Anastassiou, Péter Barthó, Mariano Belluscio, Antal Berényi, Liang Cao, Angus Chadwick, Boldizsár Czéh, András Czurkó, Ronny Eichler, Shigeyoshi Fujisawa, Tomoyuki Furuyashiki, Caroline Geisler, Oleg Godukhin, Palmer Greene, Lynn Hazan, Esther Holleman, Bo Hu, Ingrid Inema, Gábor Jandó, Julien Chuquet, Anita Kamondi, Roustem Khazipov, Svenja Kiljan, Charles King, Thomas Klausberger, Lianne Klaver, Bernát Kocsis, Marie Lacroix, Maan-Gee Lee, John Long, András Lörincz, Lisa Marshall, Dun Mao, Sam Mckenzie, Michaele Migliori, Lenäic Monconduît, Sean Montgomery, Zoltan Nádasdy, Simal Ozen, Edit Papp, Eva Pastalkova, Jagdish Patel, Markku Penttonen, Peter Petersen, Michele Pignatelli, Jonathan Platkiewicz, Marie Pollard, Pascale Quilichini, Michael Repucci, David Robbe, Florbella Rocha-Almeida, John Schulman, Attila Sik, Zoltan Somogyvári, Marisol Soula, Eran Stark, Marie Vandecasteele, Lajos Vereczkei, Brendon Watson, Lucia Wittner, Aarne Ylinen, Michaël Zugaro. For new members since 2019, see (https://Buzsákilab.com/wp/labmembers/group-photos/).

Postlude

Describing one's life story is like illustrating evolution in a one-minute movie as if there was a well-thought-out plan from viruses to invertebrates to humans, without the myriads of branch points and dead ends. In an autobiography, we tend to ignore those blind alleys to save pages and emphasize only the rare positive highlights. Yet, I am grateful for every bit of hardship I have encountered in my life. I know those "negative" experiences made me a better person and offered opportunities to take alternative, often better, paths. Of the highlights, the most memorable event for me was my plenary lecture celebrating the 50th Anniversary of the Society for Neuroscience in Chicago in 2019. Nearly all of the 7,000 seats in the lecture hall were taken, and the fire marshal prevented more people from entering, marking the highest attendance in the history of the society, according to the organizing committee. A bonus was that "Buzsáki lab" was the most frequently tweeted name of the meeting. What can be a higher honor than so many of my respected colleagues devoted 60 minutes of their lives to my presentation?

I have lived the life of a gypsy, moving from one place to another and building 17 labs. Once a Hungarian friend from Transylvania, who had spent his prime years of education in Canada and now lives in the United States, asked me the question: where is home? I answered that home is what you build with your sweat and tears, like the one I built for my family in New Jersey. But that house is sold, and I have become a full-time New Yorker. Perhaps the correct answer is that my home is the lab. There are only two difficult things in life. The first is to figure out what keeps us moving. The second is to be able to make a living with that hobby. I succeeded in both, mainly because I found a partner who supported me in my ventures and I have two lovely daughters, without whom life would be just emptiness.

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