



David H. Cohen

BORN:

Springfield, Massachusetts
August 26, 1938

EDUCATION:

Harvard University, Cambridge, MA B.A. Magnum Cum Laude with Highest Honors (1960)
University of California, Berkeley, Ph.D. (1963)
UCLA, Brain Research Institute, NSF Postdoctoral Fellow (1963–1964)

APPOINTMENTS:

Assistant Professor of Physiology, Western Reserve University (1964–1968)
Associate Professor of Physiology, University of Virginia (1968–1971)
Professor of Physiology, University of Virginia (1971–1979)
Chairman, Neuroscience Program, University of Virginia (1975–1979)
Professor and Chairman, Department of Neurobiology, SUNY Stony Brook (1979–1986)
Vice President for Research and Dean of the Graduate School, Northwestern University (1986–1992)
Professor of Neurobiology, Northwestern University (1986–1995)
Provost, Northwestern University (1992–1995)
Vice President and Dean of the Faculty of Arts and Sciences, Columbia University (1995–2003)
Professor of Biological Sciences, Columbia University (1995–2008)
Professor of Neuroscience in Psychiatry, Columbia University (1995–2008)
Vice President and Dean of the Faculty Emeritus of Arts and Sciences, Columbia University (2008–Present)
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HONORS AND AWARDS (SELECTED):

NIH, NHLBI Research Career Development Award
Fellow, Academy of Behavioral Medicine Research
Fellow, Society for Behavioral Medicine
Fellow, American Association for the Advancement of Science
Distinguished Service Member, Association of American Medical Colleges
Distinguished Service Award, Association for Neuroscience Departments and Programs
President, Society for Neuroscience, 1981–1982
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Chair, Association of American Medical Colleges, 1989–1990

Cohen spent the first half of his career developing a vertebrate model system for cellular studies of associative learning. He delineated a necessary and sufficient conditioned stimulus-conditioned response pathway, and unconditioned stimulus information likely converges on every relay of this pathway. This potentially enables training-induced plasticity throughout the pathway. In fact, cellular changes occur as peripherally as the lateral geniculate nucleus. These neurons receive unconditioned stimulus input via an inhibitory noradrenergic projection from locus coeruleus that is necessary for the learning-related discharge change. By no means a simple system, the identified pathways are reasonably well defined and the central processing time sufficiently short, 40–80 msec, that the system can be described as temporally compact.

Cohen left the laboratory in 1986 for university central administration, and much of the second half of his career has involved administrative activities engaging the sciences broadly, both through his university responsibilities as a senior administrator and through his service on the boards of such organizations as Argonne National Laboratory and the Fermi National Accelerator Laboratory. Throughout his career, Cohen has been deeply involved in the affairs of the Society for Neuroscience (from 1971 to the present), including eight years as an officer.

David H. Cohen

I have been intensely future oriented throughout my life, and only now in my mid-70s has the present become as preoccupying as the future. The past was never even a contender. An unintended consequence of this has been a relative inattentiveness to personal history and a general neglect of historical recordkeeping. I cannot always retrieve the years in which various events occurred and may have even lost some events entirely. Therefore, I ask your indulgence as I struggle to fill in blanks, glide over gaps, and hope not to make this brief history too egregiously revisionist.

Early Years

As long as I can recall I have been attracted to mathematics and science. This first expressed itself in any concrete fashion as an interest in chemistry around age 13, when I teamed up with a like-minded fellow down the block. We began assembling a chemistry lab in his attic, assisted by an uncle of mine, Philip P. Cohen, who was a distinguished biochemist at the University of Wisconsin and in the words of the National Academy “a pioneer in studies of transamination reactions and in the investigation of urea production.” He sent us CARE packages of lab equipment and generally cheered us on. This created some minor family friction, because he and my father, Nathan E. Cohen, a distinguished social scientist at Columbia University at the time, were fiercely competitive. My uncle’s generosity and encouragement were likely motivated in part to recruit me to “real science” and to spare me from the “soft social sciences.”

For a time, my friend and I mixed haphazardly following the muse of alchemy, occasionally stumbling across some rather impressive exothermic reactions. These occasions captured our attention and focused us on exploring how to amplify the intensity of these reactions. It was not long before we tripped over concepts such as valences and realized there was some meaningful theory and predictability in chemistry that we would be well advised to study. So we visited the high school chemistry teacher and asked to borrow a textbook, explaining that we were interested in understanding more about the nature of chemical bonds and reactions. He was generously encouraging, talked to us for a short while, and as we were leaving, textbook in hand, said, “It is such a delight to find two young fellows interested in more than making explosives.” We managed never to disabuse him of this undeserved noble view of us.

Come high school, retreating to an attic laboratory lost much of its luster, and my interests expanded from science and math to include technology and engineering, rounding it all out to STEM, an acronym that would not hit the streets until decades later. In real terms for me this involved transitioning from trying to make bigger bangs to trying to make faster cars. Throughout high school, I worked at a local auto repair shop, eventually becoming a credible mechanic. But the place was more than a simple repair shop. The folks there lived for more horsepower and speed. The measure of one's worth was how fast you could get a car to go from zero to sixty. It was an innovative teaching experience for me. If we needed to make fuel-flow more efficient, we would argue for weeks about the optimal angle to grind and reseat the intake valves. If we increased the horsepower of an engine such that any standard clutch would regularly blow out, we would argue about what truck clutch springs should be used in rebuilding it. So, here I was back in the laboratory, but in a Texaco uniform and with grease under my nails.

Eventually it was college application time and my teacher in AP English counseled me to be sure that there was no grease on my applications. My first choice was Harvard, and to this day, I think the dean of admissions at Harvard who interviewed me saw the grease on my application as a plus. The thick letter from Harvard arrived in the mail, and at the dinner table that night my parents were elated. (Perhaps a bit of relevant background is that my father earned both his bachelor's and doctoral degrees from Harvard and my mother her bachelor's degree from Radcliffe.) I casually mentioned that my boss at the garage had nominated me for the new fuel injection school at General Motors and that I was weighing that against Harvard. I looked at my father who was rising out of his seat, I think still flexed at the knee. To this day that evening stands as the most compelling instance I have experienced of levitation. And so, on to Harvard.

College—Harvard University

I arrived in Cambridge with the intent of resurrecting my interest in chemistry. In fact, physical chemistry was where I wanted to be, because reductionism was deep in my DNA. Harvard assigned me an adviser who was a physical chemist housed in a basement office. I met with him a few times and, as superficial as it might sound, I simply did not want to be like this fellow. In retrospect, I find it embarrassing that I had the arrogance to make this fellow the avatar of physical chemistry. But I was 18 years old, not an excuse but a soft explanation.

So, I retreated to mathematics. This was emerging as my default mode and intellectual halfway house. As I wallowed in advanced calculus, partial differential equations, algebraic structures, complex variables, transfinite arithmetic, and so on, it became, clear to me that, although mathematics captivated me, it was not where I could spend my life. I recall a beautiful

spring day in Cambridge when I sat for a final exam. The first question started, "Consider an n -dimensional unit hypersphere." I was not really up for that at the moment and jumped to the second question that was in transfinite arithmetic: "Show that a cardinal infinity is equal to 2 raised to the power of a countable infinity." I looked out the window at the sun and the newly blossoming trees and recollected the story in which Gertrude Stein was taking a course with William James. At the final exam, she was alleged to have written in her blue book, "Dear Professor James, this is simply one of those days I do not feel like taking an exam." James was alleged to have replied, "Dear Miss Stein, I know just how you feel, A." It seemed highly unlikely that dog would hunt for me. So I completed the exam and decided it was time again to regroup.

I began taking courses in probability and statistics, which introduced me to stochastic time series that in turn led me to econometrics. This, of course, alienated my advisor in the math department who felt I was compromising my intellectual purity and was being traitorous by not signing up for such courses as Quantum Theory and Lie Groups. I then found a welcoming home in the Department of Statistics, and I owe a deep debt of gratitude to Professor Frederick Mosteller, the departmental chair, for taking me on. This was an exciting period during which I was introduced to a number of extraordinary faculty and exciting research directions that took me on a journey through mathematical modeling. For example, I spent a semester tutorial working with the distinguished sociologist Professor Samuel Stouffer who was impressive in stifling his amusement when I approached him with the proposal that I take the semester to develop a formal model of societal power structures. I vaguely recollect his asking wryly if I felt this would really take the full semester. Then came my introduction to game and decision theory and the opportunity to read with Professor Duncan Luce, one of the driving forces in developing the area. This work prompted me to begin thinking about behavior itself and not simply how to formalize a set of given behavioral outcomes. From here, it was a natural transition to choice behavior and, while still deeply embedded in such concepts as Arrow's impossibility theorem and Pareto optimality, I began to think ever more seriously about behavior.

At this same time, Professor Mosteller involved me as a research assistant in some projects deriving from a book he and Robert Bush had recently published entitled *Stochastic Models for Learning* (1955). Although this introduced me to modeling with difference equations and Monte Carlo methods for testing models, much more importantly, it also introduced me to learning behavior—a topic that would preoccupy me for many years. I had the memorable experience of spending a summer with NSF support working under the tutelage of Mosteller, Bush, and others, splitting our time between Cambridge and a rental house in Rockport, Massachusetts, that was a hotbed of analytic capability.

During this time I met a classmate at Harvard, Wayne Wickelgren, who was following a similar path to mine. Wickelgren was a fellow traveler who was anchored in mathematics but captivated by modeling of behavior. We teamed up and collaborated on various projects, eventually publishing two papers together based on work we completed as undergraduates. Mosteller, my advisor, also served as Wickelgren's advisor, and he approved of and guided our undertaking a joint undergraduate honors thesis. This was an experimental study of human choice behavior and generated the first of our two undergraduate publications, "A Regression Analysis of Problem Solving in a Binary Choice Task" (Cohen and Wickelgren 1961).

This honors thesis was a watershed event in various ways. It solidified our interest in behavior, although somewhat differently for each of us. Wickelgren became enamored with the processes of human problem solving and migrated toward cognitive psychology. I became interested in learning behavior. We both cooled significantly on the prevailing modeling efforts. After running the experiments for our honors thesis and interacting with our experimental subjects, what became exceedingly clear is that our models paled in the face of the complexity of the ongoing behavior. Also, at the time, it was common practice for researchers in behavioral modeling to write ongoing white papers and circulate them among colleagues. I recall receiving one such white paper from an eminent colleague that was signed, "Sincerely yours in linearity." Linear models, and all our models were indeed linear, simply were not going to cut it. In the modeling business, there often comes a time when one has to choose between the model and the real world. At a young age, I recall my father telling me that there are times in life to abandon your principles and do what is right. This was one of those moments.

Before moving on to the next phase of my professional life, I feel compelled to acknowledge how fortunate I was to have been an undergraduate at a time when one could follow whatever muse was intellectually captivating at the moment. It was a world of opportunity in which "creeping vocationalism" and "credential building" had not yet tainted the scene. I lament that this seems no longer so; what a tragic loss.

Graduate School—University of California, Berkeley

Wickelgren and I put the math behind us and decided we needed to learn about behavior. It seemed that the best way to do this was to go to a leading graduate program in psychology. We had made the decision to remain associated and headed together for the doctoral program in psychology at the University of California, Berkeley. Wickelgren caught me up in his fever of getting to independent research, unencumbered by advisors, as quickly as possible. After all, in the arrogance of youth, we were persuaded that our ideas, which were well out of the mainstream, were compelling and that advisors were barriers to creativity rather than helpful mentors. Operationally,

this translated into getting through the PhD program as quickly as possible. We set the finish line at two to three years and began strategic and tactical brainstorming sessions to craft a plan for achieving this. Well, it succeeded. Wickelgren finished in two years and I finished in two and a half.

Let me offer one example of how this strategizing worked. The doctoral program at Berkeley had a challenging set of preliminary exams that generally took a few years to pass. One exam addressed content and consisted of a large number of multiple-choice questions that tested a daunting reading list of books and journal articles. The brute force approach would be to take a year or two (at the very least) to slog through this material, an unacceptably long time. Research on human memory had drawn a sharp distinction between recall and recognition memory. We hypothesized that, if the literature on recognition memory was indeed correct, we could just skim the background material, covering multiple books in a single evening. Then at exam time, for each question we would simply choose the answer that felt right, making no effort to decide whether it was correct or not. We assembled this large collection of materials, to my wife's displeasure, stacked on our living room floor. We spent some weeks skimming through it all. It worked! Was there a cost to not having mastered all that material at the recall level? That did not concern me at the time and, in retrospect, I seriously doubt it.

Not to appear unduly competitive, I do feel the need to address why it took me six months longer than Wickelgren to finish my PhD. As I mentioned, our interests had begun to diverge. Wickelgren's migrated toward human problem-solving behavior and mine toward learning. For my dissertation, I undertook a study of the decay of short-term memory for absolute brightness values in pigeons. Shortly after I began my experiments, the animal facility director expressed concern about whether the pigeons I was bringing into the animal facilities were free of ornithosis. I was tested for this infection and had a high titer. They would not let me bring in any more birds until I could assure them that they were free of ornithosis. It took me six months to resolve this issue. I located a veterinarian from the CDC working on psittacosis in parrots at a public health facility in San Francisco. We teamed up to develop a prophylactic regime for clearing pigeons of the rickettsia causing ornithosis by embedding chlortetracycline in the feed. So there I was, delayed by half a year, but still within the two- to three-year bogie and having generated my only publication in the *Journal of the American Veterinary Medical Association* (Arnstein et al. 1964).

The path of one's life is in no small measure defined by presented opportunities. One such opportunity for me at this time was working in a lab on a floor amid biologists. A nearby biologist who was involved in comparative studies of the pineal gland saw my pigeons and asked whether he could have the glands of any animals I no longer needed. Wanting to be collegial, but having no idea how to remove the pineal gland, I said, of course, but in the

interests of my time, I would appreciate his coming to get them. He did, leaving me with pigeon heads with partially opened skulls. Within fairly short order, I became proficient at removing the brains, and I began accumulating a number of these brains in jars of formalin. Anyone who has observed operant conditioning of pigeons must be impressed with what impressive learning machines they are. I kept staring at the bottles, wondering how that innocuous looking bit of tissue mediates that behavior.

And thus my passage. I was hooked on the challenge of understanding how the brain mediates the storage of information that enables learning. It was not really a passage into neuroscience, because no such field existed at the time. But it was clearly a passage into the anlage of neuroscience.

Postdoctoral—UCLA Brain Research Institute

As Wickelgren shifted his interests to human problem solving and ultimately headed off to a faculty position in the Department of Psychology at MIT, I explored how best to acquire the skills I would need to study the cellular mechanisms of learning. It was clear that committing to a traditional biological discipline was not the answer and that I needed to find an environment that would expose me to a broad array of disciplines in the context of research on the brain. One of the few places that fit this job description was only a few hundred miles south of me, the Brain Research Institute (BRI) at the University of California—Los Angeles (UCLA).

Rather than identifying a specific mentor and undertaking a defined research project, the more conventional postdoctoral experience, I sought the freedom to audit courses and wander from lab to lab, learning approaches, methods, and what a broad array of distinguished scientists considered to be the most important questions about the brain. I applied for an NSF postdoctoral fellowship, explaining in my application that I was migrating from psychology to a field that had not yet been defined and sought support for a study rather than a research postdoctoral. Happily, NSF bought the plan and I headed to UCLA. Sadly, I cannot imagine this happening today.

What an extraordinary time it was. I sat through neuroanatomy and neurophysiology with the first-year medical students. I learned single-cell recording, basic histological techniques, and animal surgery. The BRI had a rich fare of speakers from around the world, and I did not miss many. And, I did manage to complete and publish a small research project on the effects of lesions of the pigeon optic tectum on brightness discrimination learning (Cohen 1967b).

Among the most valuable products of this time was my ability to clarify my long-term research objective—to develop a vertebrate model system for studying changes that mediate associative learning. During this time, I settled on a model system approach and, consistent with my strong reductionist bias, committed to analysis at the single-cell level. As I look back on

what the catalysts might have been for my defining this direction, I believe it derived from my background in mathematics and formal modeling. The BRI provided a fertile opportunity for conceptualizing it, and my experience there helped shape it into a biological narrative.

Simple systems such as the *Aplysia* (Kandel and Spencer 1968) in many ways were exceedingly compelling, and it seemed almost foolhardy to tackle the complexity of a vertebrate brain. It was not even clear at the time whether it would be possible, with the available methods, to map the relevant pathways for any learned response. That said, what drove my decision was a deep concern that simple systems were too simple behaviorally. Indeed, over the ensuing years, the struggle to demonstrate associative learning in *Aplysia* offered testimony to this concern. So intrepidly, perhaps a euphemism for naively, I forged on toward trying to develop an effective vertebrate model system for cellular analysis of associative learning (Cohen 1969).

Unbundling the Different Paths of My Career

Before describing that work, I need to address some organizational matters for this essay. My active research career spanned the years 1964 to 1986 at three venues—the Department of Physiology at Western Reserve (now Case Western Reserve) Medical School (1964–1968), the Department of Physiology at the University of Virginia Medical School (1968–1979), and the Department of Neurobiology and Behavior at the State University of New York (SUNY) at Stony Brook (1979–1986). Beginning in 1971, however, administrative activities began looming increasingly large in my life. A first phase involved activities related to the development of neuroscience as a field. These included neuroscience program development at the University of Virginia and Stony Brook (1971–1986) and, particularly prominently, broad and deep involvement with the SfN (1971–present). A second phase began at the close of my research activity in 1986, when I moved into central university administration and became increasingly involved in national activities, most often science related (1986–2008).

Research Years (1964–1986)

My program to develop a vertebrate model system for cellular studies of associative learning began in earnest when my postdoctoral ended and I moved to my first faculty position as an assistant professor in the Department of Physiology at Western Reserve University Medical School.

Developing the Behavioral Model

A first task was to select an experimental animal. Having worked with pigeons both as a graduate student and postdoctoral fellow, I had become

impressed with how easily they could be trained in associative learning tasks. They were inexpensive, readily available, easily maintained, and easy to handle. From a behavioral perspective, this turned out to be a particularly fortuitous choice, as I will describe shortly, less so with respect to delineating the neural circuitry. I expected that to be quite challenging for any mammalian species, but it turned out to be particularly daunting for the pigeon because the avian neuroanatomical literature was so limited. There was also a price to be paid for working outside the mammalian mainstream. On the positive side of the ledger, it permitted ongoing contributions to comparative neurology, and I take great pride in my publications over the years in the *Journal of Comparative Neurology*. This also began a long-term association and friendship with Harvey Karten, the contemporary maven of avian neuroanatomy. Although we only occasionally published together (e.g., Cohen and Karten 1974), we maintained an ongoing dialogue about a broad range of topics in avian and evolutionary neurobiology.

The next task was to identify an appropriate behavioral paradigm. Constraints on the behavioral model were that it had to show robust associative learning that developed in a nonmoving animal in a single training session of at most a few hours. An obvious candidate was a classically conditioned autonomic response, and because heart rate is so easily measured and quantified, I began there. There was a significant literature on classical conditioning of heart rate in various mammalian species. The developmental time course and robustness of the conditioned response were attractive in some but not all mammals (e.g., the rat), but such conditioning had never been demonstrated in the pigeon. By 1966, I had developed a standardized training paradigm in which a 6-sec pulse of whole-field illumination, the conditioned stimulus (CS), was followed by a mild, 500-msec foot-shock, the unconditioned stimulus (US), which elicited cardioacceleration, the unconditioned response (UR). A conditioned cardioacceleratory response (CR) of predictable dynamics begins developing within 10 light-shock pairings and is asymptotic by 30 pairings (Cohen and Durkovic 1966).

This work began defining three themes that carried throughout my research years. The dominant theme, of course, was pursuing a cellular analysis of associative learning. Both comparative neuroanatomy and neural control of the cardiovascular system became significant subsidiary themes. Although these subsidiary themes were of genuine interest to me, they also generated support for the laboratory during the long march to develop the model to the point at which single-cell studies could be undertaken. For example, neural control of the cardiovascular system enabled a successful application to the National Heart, Lung, and Blood Institute at NIH for a Research Career Development Award and, over the years, the laboratory generated a number of publications relating to neural control of the cardiovascular system (e.g., Macdonald and Cohen 1973; Cohen and Cabot 1979; Cabot and Cohen 1980; Cohen and Randall 1984).

Mapping the Circuitry

During my four years in Cleveland, I consolidated the behavioral model and began the daunting task of mapping the neural pathways involved in mediating the learned response. I approached this by heuristically defining four segments of the system: the visual pathways transmitting the CS; the somatosensory pathways transmitting the US; the descending pathways mediating expression of the CR; and the efferent pathways mediating the UR. I then began at the periphery of each segment and systematically mapped the pathways centrally using multiple techniques. It was assumed that such an analysis of the input and output segments of the system would lead to sites of convergence of the CS and US pathways, as well as to their sites of coupling with the CR.

This mapping program was pursued vigorously until I left the lab in 1986 and, over this period, I did in fact succeed in gaining a first approximation to a necessary and sufficient pathway from the eye to the heart (Cohen 1980, 1985). A number of students contributed significantly to this effort: John Cabot, Thomas Duff, Russell Durkovic, Paul Gamlin, Charles Gibbs, Michael Gold, Robert Leonard, Robert Macdonald, Lawrence Pitts, Theresa Ritchie, Adrian Schnall, James Schwaber, Doris Trauner, and John Wall.

With respect to transmission of CS information, it involves three ascending parallel pathways: a thalamofugal pathway homologous to the mammalian geniculo-striate system and two tectofugal pathways perhaps homologous to mammalian tecto-thalamo-extrastriate pathways. Each of these three pathways can transmit effective, although not necessarily redundant, CS information, and it is only with their combined interruption that formation of the CR is precluded. A number of studies from 1967 onward were necessary to define these CS pathways (e.g., Cohen 1967a; Cohen and Trauner 1969).

To characterize the descending pathways mediating expression of the CR, I began at the final common pathway, the sympathetic and vagal motoneurons. We initially showed that the response, at least within the 6-sec conditioned stimulus period, is mediated entirely by the extrinsic cardiac nerves; there is no hormonal component. Although the vagus nerve mediates the shortest latency component of the CR, the major contribution is mediated by the sympathetic cardiac innervation (Cohen and Pitts 1968; Cohen 1974a).

Given a description of the final common path, we launched a comprehensive study to identify the central pathways that control heart rate and blood pressure, assuming that some subset of these would converge on the cardiac motoneurons to mediate the CR (Macdonald and Cohen 1973). We ultimately identified a descending system in which electrical stimulation elicits striking pressor-accelerator responses and lesions prevent expression of the CR (e.g., Cohen 1975; Cohen and Macdonald 1976; Cohen and Goff 1978a). The most rostral component of this pathway is the avian amygdalar homologue, which is the most cardioactive structure of the avian telencephalon.

There is then a well-defined amygdalar projection to the posteromedial hypothalamus in which a pathway originates that traverses the ventromedial brainstem at mesencephalic and rostral pontine levels. In the caudal pons it shifts to a ventrolateral position that is maintained through the medulla. At medullary levels, fibers project dorsomedially from the pathway to access the dorsal motor nucleus of the vagus, while the spinal continuation of the pathway occupies a position in the lateral funiculus and ultimately accesses the sympathetic preganglionic cardiac neurons through one or more spinal interneurons.

Given that the CS pathways ascend to the telencephalon and that the CR pathway involves the amygdala, it seemed reasonable to hypothesize that the relevant visual areas of the telencephalon influence the amygdala through intratelencephalic circuitry. An extensive series of anatomical experiments identified intrinsic telencephalic cell groups and their connections that could serve this function (Ritchie and Cohen 1977, 1979). These cell groups, including a critical subnucleus of the amygdala, are all responsive to varying degrees to whole-field illumination (the CS), and preliminary behavioral experiments suggested the necessity of this circuitry for acquisition of the CR.

Rather early in the program we described the peripheral components of the US and found that activation of A-delta and C fibers is required to elicit a CR (Leonard and Cohen 1975c). Subsequently, we completed a cytoarchitectonic analysis of the pigeon spinal cord, described the patterns of dorsal root termination, established some of the properties of the intraspinal circuitry by which the US influences sympathetic postganglionic neurons, and generated preliminary data implicating the dorsolateral funiculus in the transmission centrally of the US (Leonard and Cohen 1975a, 1975b, 1975c).

At this point, we had begun to appreciate that most, if not all, central structures along the CS-CR pathways receive US input. Given this understanding, we made the tactical decision to shift our focus from trying to map the US pathway from the periphery centrally to mapping it back from the nuclei along the CS-CR pathway. A particularly compelling example of the merits of this approach will be described shortly.

Specifying the pathways that mediate the cardioacceleratory to the US was of low priority, and this response is almost certainly quite diffuse and can be mediated at many levels, including spinally. Consequently, we did not make a systematic effort to delineate this segment of the system.

Temporal Properties of the Information Flow

The CR has a latency of approximately 1 sec and persists for 6 sec (Cohen and Goff 1978b). The central processing time for this response, however, need not be of the same order of magnitude. Characterizing the discharge properties of the cardiac motoneurons during the conditioned response allows specifying that response in a neurophysiological time domain that

excludes delays at the motor periphery. Determining the temporal properties of the retinal response to the CS then permits a reasonably precise estimate of central processing time. That estimate, in turn, establishes the temporal boundary conditions for interpreting neurophysiological data from central structures.

The sympathetic cardiac postganglionic neurons respond weakly to the visual stimulus before training. The latency of this orienting response is approximately 100 msec, and it consists of a short burst of action potentials followed by a brief period of depressed discharge before a return to maintained activity levels. This response habituates rapidly. With CS-US pairing, the probability of occurrence of this phasic response and its duration increases rapidly. An important feature of this CS-evoked sympathetic discharge is that it consists almost exclusively of a transient response at CS onset with a latency of approximately 100 msec and a duration of 300–400 msec. Thus, the central processing time for the 6-sec behavioral conditioned response does not exceed 400 msec, indicating a highly nonlinear input-output relation between the sympathetic postganglionic innervation and the heart (Cohen 1982, 1985).

The cardiac neurons in the dorsal motor nucleus of the vagus also respond to the CS before training, but with decreased discharge. Because the vagal cardiac innervation is inhibitory, this contributes to the CR. Like the sympathetic response, the vagal response has a prominent phasic component, with a more modest tonic component lasting 110 msec. Associative training enhances the magnitude of this decreased discharge, as well as shortening its latency to 60–80 msec at asymptotic performance (Gold and Cohen 1981, 1984).

At the input to the system, retinal ganglion cells respond to whole-field illumination as a rather homogenous population, bursting briefly at light onset and largely ceasing activity during the remainder of the sustained illumination. This “on” burst has a minimal latency of 18 msec and a maximum duration of 80 msec (Duff and Cohen 1975). These response properties are not affected by associative training (Wild and Cohen 1985).

Calculating the differences between the modes of the response histograms of the retinal ganglion cells and the cardiac motoneurons gives 105 msec for the central processing time for the vagal component of the CR and 135 msec for the sympathetic component. The minimal central processing times for the vagal and sympathetic components are approximately 40 msec and 80 msec, respectively. This significantly delimits the system and, although certainly not a “simple system,” it might be described as a “temporally compact system” (Cohen 1982, 1984).

Sites of Neuronal Modification—the CS Pathways

So, some 15 years after the initiation of the program to develop a model system, it had advanced sufficiently to permit cellular study of potential

sites of plasticity. An obvious starting point was the CS pathways and, beginning at the periphery, we first showed that neither the CS-evoked nor maintained activity of the retinal ganglion cells change as a function of conditioning (Wild and Cohen 1985).

We then focused on the most prominent of the three ascending pathways transmitting CS information: the retino-tecto-rotundo-ectostriatal pathway, and initially studied its telencephalic target, the ectostriatum. If these neurons were invariant with training, we could infer that the subtelecephalic components of the pathway also would be invariant. Certain classes of ectostriatal neurons clearly showed modification of their CS-evoked discharge during conditioning. An examination of the thalamic relay of this pathway, the nucleus rotundus, similarly demonstrated classes of neurons whose discharge changed as a function of associative training (Wall et al. 1985).

This raised the possibility of training-related plasticity as peripherally as the first central synapses of the retinal ganglion cells. Given the structural complexity of the avian optic tectum, we turned our attention to the thalamofugal pathway, the avian homologue of the mammalian geniculostriate system. Studying these retinorecipient neurons permitted both assessment of training-induced plasticity in a second of the three involved ascending visual pathways and determination of whether such change occurs as peripherally as the first central synapse of this visual pathway. The results clearly showed robust training-induced changes in the avian equivalent of the dorsal lateral geniculate nucleus (LGN) (Gibbs et al. 1986). There were no changes in maintained activity, and only the short latency phasic response showed modification. The short latency and duration of the modified discharge make it highly unlikely that it is driven by feedback from more central structures. At this point, we felt confident in concluding that at least two of the three visual CS pathways undergo associative modification and that this modification can occur as peripherally as the retinorecipient neurons.

The modifiable geniculate neurons respond initially to both the CS and US, demonstrating the expected need for convergence of these two inputs for associative learning. The associative training paradigm then enhances the initial CS-evoked discharge of the modifiable neurons, whereas in a nonassociative control paradigm, this discharge habituates. The discharge enhancement is largely restricted to the short latency, transient visual response, which is consistent with the pattern of training-related discharge changes observed in the cardiac motoneurons. Thus, the modifiable neurons simply seem to do more of what they initially do in response to the visual stimulus, and the time course of this discharge modification over training parallels that of the motoneuronal discharge changes.

CS-US convergence is necessary for associative modification, but it is not sufficient. Modifiability seems to depend on the nature of a neuron's

response to the US. Only LGN neurons whose discharge decreases in response to the US show associative modification. Neurons that are either unresponsive to or excited by the US show discharge attenuation under either associative or nonassociative paradigms. Thus, the properties of the US input are more critical in determining a neuron's modifiability than the properties of the CS input. Modifiable LGN cells can show either increased or decreased discharge in response to the CS, and therefore the direction of the CS-evoked and US-evoked discharges need not be the same in the modifiable cells.

The obvious next step was to investigate the source of the US input to the LGN, and a series of neuroanatomical and neurophysiological experiments implicated a projection from the locus coeruleus (Cohen et al. 1982; Gibbs et al. 1983; Broyles and Cohen 1985; Elmslie and Cohen 1990). Histochemistry and immunohistochemistry clearly demonstrated the presence of norepinephrine and serotonin in LGN, and iontophoresis of norepinephrine can inhibit the firing of LGN neurons. Recording from cells in locus coeruleus indicated that a substantial proportion are responsive to the US at latencies consistent with relaying timely US information to the LGN. Furthermore, electrical stimulation of locus coeruleus can decrease the discharge of LGN neurons, and interruption of the coeruleo-geniculate projection largely eliminates the population of LGN neurons showing decreased discharge to the US. Under these conditions, such neurons are no longer modifiable by conditioning. Finally, electrical stimulation of locus coeruleus can serve as an effective US. Consequently, it seems reasonable to tentatively conclude that the coeruleo-geniculate projection is both necessary and sufficient for relaying effective US information for associative modification of LGN neurons.

Brief Overview and Forks in the Road

It was now the mid-1980s, and the state of play was as follows: We had a highly effective behavioral model and had specified a necessary and sufficient CS-CR pathway, which in some segments such as the visual system involved parallel pathways. We had good reason to suspect that US information converges on every relay along this CS-CR pathway, although the source of that input need not necessarily be the same for different levels of the pathway. This would suggest the potential for training-induced plasticity at all relays along the pathway, giving an anatomically localized system with widely distributed sites of plasticity. Furthermore, this system, although by no means simple, is "temporally compact," such that it may well be possible, admittedly with challenging effort, to ultimately describe the input-output relations at each relay of the system.

In pursuing the most peripheral site of plasticity, a somewhat unexpected finding was that clear training-induced change occurs in the retinorecipient

neurons of the thalamus, the avian lateral geniculate homologue. Equally surprising was that the necessary and sufficient US input to the geniculate is via a projection from the locus coeruleus. This was one major fork in the road at this time.

I say it was a fork because this potentially could have taken the lab in the very different direction of focusing on an *in vitro* model with a slice preparation, using electrical stimulation of the optic nerve as the CS and iontophoresis of norepinephrine as the US. At the same time, I was reluctant to abandon such other possible directions as gaining an understanding of the role of such structures as the amygdala (Cohen 1975). To pursue the many opportunities would have meant a substantial expansion of the laboratory, an unattractive prospect as I had always preferred a modestly sized enterprise.

I never resolved this dilemma because of another entirely unexpected fork in the road, the prospect of moving into university central administration. I will tell that story a bit later.

Society for Neuroscience (1970–Death Do Us Part)

Permit me to backtrack now to 1970 when my involvement with the SfN began, an involvement that continues to this day. The Society has had an extraordinary impact on the development of neuroscience as a field. Early on after the start of my professional career, it was clear to me that, to play off Pirandello, I was a neuroscientist in search of a field. Thus, when the fledgling Society was hatched, there was no question that I would be willing to commit deeply to working for its success. Indeed, I traded significant potential research time for this endeavor and have never regretted that decision. My involvement with the Society has been one of the most rewarding experiences of my career, and I would like to think that I contributed materially to the development of this truly uncommon organization.

I have organized this section by functional activities of the Society that I had a significant hand in developing, and to the extent possible, I have tried to order these chronologically. A first period, the pre-officer period was 1970–1975. I had the honor to be elected secretary in 1975 and to serve in that position until 1980 when I was elected president. I then served as president-elect, president, and past-president until 1983. This defines the officer period of 1975–1983. The post-officer period that began in 1983 continues to the present.

Pre-Officer Period (1970–1975)

During this period, I was involved with the Society primarily through its chapter structure. In 1968, I left Western Reserve to accept a position in the Department of Physiology at the University of Virginia (UVa) School of Medicine as an

Associate Professor. At that time, UVa Medical School had received substantial federal funding to invest in building its basic science departments. It was importing whole departments, and I was part of the wave migrating from the Department of Physiology at Western Reserve. Western Reserve émigrés also populated the Department of Pharmacology, while the revitalized Departments of Biochemistry and Microbiology came from Johns Hopkins.

Not long after my relocation to UVa, I learned about the existence of the SfN, a newly formed organization defined by an organ rather than by function or methodology as were the traditional disciplines of anatomy, biochemistry, pharmacology, and physiology. I cannot recall how I first learned of the Society, but I do recall that I resonated immediately to the concept. At the time, I was already a member of the American Association of Anatomists and the American Physiological Society and, in addition to attending the annual meetings of these organizations, I would less regularly attend meetings of various behavioral and pharmacological associations. This was necessary to cover the span of the multidisciplinary needs for my research program. The Society was clearly an enterprise I needed to become involved with and give my all to support.

Early on, the Society developed a local chapter structure, an insightful decision, as it created local receptor sites for institutional program development. I took advantage of that to establish a chapter at UVa in 1970, I believe one of the earliest chapters of the Society. In 1971, I was invited to serve on the Society's Committee on Chapters, which I then chaired in 1974–1975. While on this committee, I approached the Grass Foundation, which already supported the Grass Lecture, seeking support for a traveling scientist program that would cover the costs of invited speakers to visit chapters. The objectives were to strengthen the chapters locally by enabling a major event to coalesce local neuroscientists, to incentivize chapter development, and generally to make the Society more attractive to its evolving constituency. The Grass Foundation encouraged me to submit a proposal. I did, it was successful, and the program launched in 1972. I directed it until 1975, when I was elected secretary of the Society. (Ironically, many years later in 2005 I joined the board of the Grass Foundation, and during the economic downturn a few years after found myself in the uncomfortable position of listening to discussions about whether the annual grant to the Society for the Grass Traveling Lecture Program should be reduced, if not eliminated. Appropriately, I had no alternative but to recuse myself.)

Officer Period (1975–1983)

My involvement with the SfN expanded explosively during this period, and I take great pride in having played an instrumental role in the expansion of the Society's mission to encompass various new and critical areas. It was during this time that the Society adopted advocacy as central to its mission,

and advocacy was one of the critical stimuli for our committing to an ongoing effort to communicate to the public. The Society took its first step to becoming an international organization during this period. It undertook its first long-term strategic planning effort, and it was during these years that external events thrust the Society into the vigorous defense of the role of animals in research. All of these areas are now structurally embedded in the organization and are fundamental elements of its mission.

Advocacy and Public Information

On being elected secretary, my involvement with the Society broadened and deepened. I became a member of Council, serving from 1975 to 1983. As the growth of the Society was becoming a national phenomenon, it increasingly was called upon by various organizations to join in advocacy efforts, and it was not long before this was incorporated into the mission of the Society. I represented the Society in most of these early relationships, such as with the National Committee on Research in Neurological and Communicative Disorders and the Coalition for Health Funding. The Society's involvement in advocacy was formalized in 1977 with the establishment of the Government and Public Affairs Committee, which I started with Floyd Bloom. I chaired this committee for many years. Also, beyond acting as liaison with other advocacy groups, I regularly wrote for the Neuroscience Newsletter about the funding environment, provided congressional testimony, met with senators and representatives who sat on key committees, and maintained ongoing contact with the leadership of our principle funding agencies.

I do need to digress to share with you one of my favorite anecdotes of these advocacy years, a situation that was both amusing and disconcerting. During the Carter administration I managed to schedule a meeting with an individual rather high in the executive branch. I made some introductory remarks about neuroscience and its funding needs. Following that, he asked whether I was aware that Rosalyn Carter had an interest in mental health. I responded that, yes, we were aware of that and were deeply grateful for her interest. He then asked whether I also knew of her interest in learning disabilities. I said, no, I did not, and he followed that by asking whether learning disabilities had anything to do with the brain. That left me momentarily speechless and, on recovering, the only response I could muster was, "Sir could you possibly suggest an alternate organ?"

This experience with advocacy offered significant civics lessons with respect to how Washington works (or does not), including the role of lobbying. One of my most important takeaway lessons, beginning with the previous anecdote, was the monumental task we confronted to educate both our elected officials and the public about brain research. Congressmen continually reinforced this message, and still do, telling us to get out there at the local level. Many others in the Society understood this need for informing the

public and this, like advocacy, was incorporated into the mission of the Society and catalyzed the establishment of the Public Information Committee. I served on this committee as a member beginning in 1983.

One of the early efforts in this context was an effort to establish connections with leading science journalists. We organized a meeting at Airlie Conference Center in Warrenton, Virginia (I do not recall the year), in which a group of neuroscientists and distinguished science journalists spent a few days together getting to know each other and exploring how we might more productively interact. I suspect the neuroscientists learned considerably more than the attending journalists. I was disabused of my assumption that science writers were failed scientists. On the contrary, I learned they are talented and accomplished reporters who could just as adeptly be covering the crime desk or foreign affairs. In the spirit of full disclosure, I put my sister, Susan Cohen, in that category. We also learned that attempting to convince a journalist to include the salient historical background and key scientists enabling any discovery they were reporting on was not a battle we were likely to win, any more than making the case for caveats in reporting a result. It was in this context that we learned the frequent lament of journalists that they wanted to meet a one-handed scientist who does not tell them, "on the other hand." Their goal was to get their article on the front page, and one-handed scientists were simply not going to capture their attention. All this said, my sense was that we walked away from that meeting wiser about the media and having established substantial good will with the attending journalists.

Perhaps our most important advocacy partner was the Association of American Medical Colleges (AAMC). At the time, the AAMC was the biggest game in town in advocating for support of medical research. Our first formal contact with the organization was in 1978 when we were invited to join the Association's Council of Academic Societies. I served as the Society's representative and, in 1984, was elected chair of the Council. Beyond serving as the Society's point of contact with the AAMC, out of personal interests deriving from my positions on medical school faculties, I became deeply involved in AAMC activities, serving on its various committees; ultimately, I was elected chair of the AAMC in 1988.

Going Global

The seeds of international neuroscience were clearly evident in the establishment of the International Brain Research Organization (IBRO), incorporated in Canada in 1961. Indeed, IBRO was seminal in establishing early on an international network of neuroscientists and initiating important activities, including international congresses.

In its early years, the Society was appropriately focused domestically, but it was not long before it began to look beyond U.S. borders. It could hardly do otherwise, as it was becoming clear that the Annual Meeting of the Society,

from its inception in 1970, increasingly was attracting foreign attendance and was becoming the dominant neuroscience meeting globally. As of 2016, I believe, approximately 40 percent of the Society's membership is foreign.

During my officer years, the first non-U.S. chapters in Canada and Mexico were chartered. I was particularly involved in the establishment of the Mexican chapter, and I developed personal relationships with the Mexican neuroscientists leading its development. I was also involved in the Society's establishing formal relations with IBRO. I served as the liaison to the IBRO Central Council from 1978 to 1982 and served on the U.S. National Committee for IBRO of the National Academy of Sciences from 1980 to 1986.

The exciting growth of the field during this time was penetrating a number of countries, as it had the United States, and neuroscience organizations were blooming globally. To get some sense of what was happening internationally and on behalf of the Society to express solidarity with these developing enterprises, as president-elect, I convened the presidents of the various national neuroscience societies in Malaga in 1982 and again, as president, in Paris in 1983. Also, during my presidential year, I visited various countries to offer encouragement for national neuroscience society development and to offer what consultative advice I could.

Strategic Planning

The explosive growth of the Society and its expanding menu of activities had a Wild West character as I entered my presidential year. The reactive nature of our growth stimulated me to initiate a long-range planning effort, not to control our trajectory but rather to get some sense of priorities for our ongoing activities and to identify other projects and areas in which we should be engaging. I appointed an ad hoc committee of "tribal elders" to look into our future. It was a rather informal exercise, different from the stylized strategic planning exercises of today. The enthusiasm of the ad hoc committee I believe signaled a collective sense of our having reached a milestone in the development of the Society. Although I cannot assess the impact of the ad hoc committee's report, I have been told that subsequent planning exercises have validated its findings and recommendations.

Animals in Research

As I indicated earlier, much of our activity in governing the Society in these, its late teenage years, was heavily reactive. That was certainly the case for our initial involvement in the issues of animals in research. Some of you may recall the Ed Taub case, in which People for the Ethical Treatment of Animals, still a fledgling organization, infiltrated his laboratory and released films allegedly documenting mistreatment of research primates. This broke in the press during the annual meeting where I was assuming the

presidency of the Society. The membership was outraged and demanded that the Society defend Taub. This was a classic instance of the adage, “where you stand depends upon where you sit.” As a colleague, I was prepared to go to the barricades for Taub, but as president of the Society, I had no choice but to hold back and tell the membership that we needed facts before we could take a public position. This was the shot heard round the vivarium that embedded us deeply as advocates for the use of animals in research, a cause that has become deeply institutionalized in the Society.

Concluding Comments on the Officer Years

Over this period, I spent a significant amount of time each week on Society activities, beyond the more formal responsibilities of an officer or as a member of standing committees. For example, I chaired the Grass Lecture Selection Committee for five years, sat on the Gerard Prize Selection Committee, served on the Subcommittee on By-Laws, and was the liaison to a number of organizations both domestic and international. Additionally, I spent many hours on the phone each week with the executive director of the Society, Nancy Beang, and with the other officers. Also, with the growing recognition of neuroscience and of the Society as the dominant professional organization of this explosively emerging field, I was asked to serve on what seemed like countless advisory committees for NIH, NSF, the Department of Defense, and the National Academies, as well as being invited by numerous institutions to consult on how they should move forward in their local development of neuroscience.

It was a great honor to be elected to these offices. It put me at the center of the Society’s governance during one of the most exciting periods of the organization’s development and of the emergence of the field as an integrated discipline. I had the extraordinary privilege of being in a leadership position as the Society grew into adulthood, broadened its missions, and gained enviable recognition nationally and internationally.

Post-Officer Period (1983–Present)

My involvement with the Society following my officer years focused largely on its financial affairs. As an officer, I served ex officio on the Finance Committee from 1976 through 1982. At the time I was elected president, the financial state of the Society was sound, if not thriving. I took it as a serious responsibility of my office to look toward ensuring the organization’s long-term financial stability. It seemed to me that, beyond responsible management and governance, this demanded focusing on two objectives: developing diverse revenue sources and building a financial reserve. Too many of our fellow societies were overly dependent on one or at most two dominant revenue sources, such as dues or publications, and almost none had developed a strategy for establishing a significant financial reserve.

We clearly had a healthy start with respect to revenue channels. Membership was growing substantially each year, and it had become evident that foreign membership was becoming a significant unanticipated revenue source. The annual meeting was generating sizable surplus revenue annually because the meeting was well managed and had become a “must attend” rather than a “nice to attend” event both domestically and internationally. This of course generated strong exhibitor revenue. The journal recently had been launched and represented yet another potential revenue channel for the future.

With respect to building a financial reserve, I cannot recall our cash situation at the time. If memory serves it was perhaps \$2 million plus parked in a mattress. So I began to consider ways we could deploy these funds more productively to launch a reserve fund. We began by engaging an investment advisor and adding annual surpluses into an investment pool. Before long, it became clear we needed a better structure than a single advisor informally dealing with a few of us. So, I drafted an investment policy, and in 2002, we constituted an Investment Committee and engaged a custodian to manage our accounts and advise the committee. I undertook the responsibility of chairing the committee and early on recognized the need to bring on some members who were investment professionals. I had relocated to Columbia University in 1995 and by 2002 had built a rolodex of Manhattan-based financial people who could assist us in assembling a powerful advisory structure. Since then, we have enjoyed the benefits of having on the committee at any given time one or more truly impressive investment professionals. I chaired the committee from its inception until 2011, and I am highly gratified that we now have a reserve exceeding \$50 million (not including the significant equity in our building) that has had a solid annual return on investment—adding yet another revenue source.

I again served on the Finance Committee from 2000 to 2006 and continued ex officio until 2011 in my capacity as chair of the Investment Committee, giving me in aggregate 17 years on the committee. Over these years, we have developed an extraordinarily productive relationship between the Investment Committee, Finance Committee, and the Council with respect to how best to ensure the financial stability of the Society. This is testimony in my estimation to the impressively thoughtful and effective governance of the Society over the years by its Council and elected officers.

The finance story does not end here. When the Society constructed its own building, I was asked to serve on the Building Committee. This was in 2003, and my role on the committee primarily focused on the building’s financing. We were fortunate in getting gold-plated bank ratings, in no small measure because of our financial reserve, and this gave us exceedingly favorable financing. So, with annual dues, annual meeting revenue, journal revenue, investment revenue, and then real estate revenue, we are enviably diversified. As I write this, that looms large as our environment becomes increasingly challenging financially.

And, my story with the Society does not quite end yet, although my window into the Society now sadly narrows to the Investment Committee on which I continue to sit as vice chair.

Institutional Program Development (1971–1986)

As the Society was undergoing its extraordinary evolution, exciting developments also were being realized locally at members' institutions. These were the venues of the actual research and training that represented the "on the ground" development of our emerging interdisciplinary field of neuroscience.

The UVa chapter of the Society that I initiated in 1970 flourished, as did neuroscience at UVa. John Jane, the chair of Neurosurgery, a strong advocate for basic neuroscience and a dear friend, was an essential colleague in this development. Neuroscience flourished sufficiently that in 1975 we established a formal, degree-awarding program in neuroscience that I chaired until 1979 when I relocated to SUNY Stony Brook. Mounting a new degree program at UVa, as at most public universities, required state approval that in turn required a convincing statement of need. Successfully addressing the question of need was, indeed, an interesting and highly productive exercise that catalyzed some of my evolving views of the field and stood me in good stead in advising colleagues at other universities on program development, in my leadership roles in the Society, and in our advocacy efforts in Washington, DC.

My first direct involvement in any formal entity that recognized our emerging field occurred in these years, the early 1970s. This was serving on the NSF's Panel for Neurobiology from its inception in 1972 until 1975. I believe this grant review panel was the first formal federal acknowledgment of neuroscience as a field. Jim Brown from NSF was responsible for making this happen and for recruiting our merry band that included, among others, Gary Lynch, John Hildebrand, Jeff McKelvy, and later Lorne Mendell. NIH reviewed brain science proposals at that time primarily through disciplinary committees, such as the Neurology A and B Study Sections. I served on the Neurology A Study Section from 1977 to 1987, chairing it from 1983 to 1987.

What lured me away from UVa was the proverbial offer that could not be refused from SUNY Stony Brook. The offer was to develop a full-service Department of Neurobiology and Behavior that, if memory serves, would have about 20 full-time faculty, primarily new hires. The committed space, start-up resources, departmental budget, and graduate student support were beyond generous. The department was responsible for teaching undergraduates, medical students, and graduate students. Although not formally in the medical school, we had close ties and, as the department developed, there were an increasing number of joint appointments with medical school departments. For example, although my primary appointment was in the

Department of Neurobiology and Behavior, I also held appointments in both the Department of Physiology and the Department of Anatomy.

The development of interdisciplinary training programs in neuroscience was accelerating, but few of these programs had yet transformed structurally into departments. It was exciting to lead the development of one of the first departments of neuroscience in the country, and with 20 or so dedicated faculty, it was indeed a substantial department. Recruiting was relatively easy, given the concentrated presence of neuroscience colleagues and the ample space and start-up resources of the department. The department grew and flourished. Early senior faculty included Harvey Karten, Murray Sherman, Lorne Mendell, Jeff McKelvy, and Paul Adams. All newly recruited faculty secured substantial grant support, and it was not long before our graduate program competed successfully for an NIH training grant. We then moved into the rarefied atmosphere of Hughes and MacArthur awards. Meanwhile, with all the recruiting dinners, I gained 10–15 pounds.

From my experience in program building at UVa and department building at Stony Brook, I was becoming increasingly involved with neuroscience education both at the ground level at home and in responding to requests for advice and consulting from colleagues at various universities. The absence of a forum or organization to assist emerging programs to develop and to facilitate communication among them became acute. Education was not really on the Society's agenda in its early years. Given the growth velocity of the Society, it simply did not have the bandwidth to stretch its missions even further at that time. I was familiar with the organization of chairs of other medical school disciplines, such as the chairs of physiology departments, and I became convinced that a similar structure might well serve neuroscience program directors and departmental chairs.

This was 1980, and I had just been elected president of the Society. At the annual meeting in 1981, I convened an informal meeting of a group of directors of neuroscience programs to get a sense of their views about launching a chairs-type organization. The responses were mixed but on balance constituted a go-ahead in my view. Thus was born the Association for Neuroscience Departments and Programs (ANDP). I served as its first president in 1981–1982 and as past-president in 1982–1983. Joe Coulter was an essential collaborator in launching this effort. Validating the organization has been its sustainability, indeed significant growth, over many years until it was formally incorporated into the Society in recent years, giving the Society a firm presence in neuroscience education.

Post-Neuroscience Career (1986–2008)

My time at Stony Brook, 1979–1986, was a special time in many respects. Beyond building one of the first neuroscience departments, being elected president of the Society, founding the ANDP, and enjoying one of my most

productive periods scientifically, I married Anne Remmes, a neurologist I met at Stony Brook, had a fourth child and gained a stepdaughter to bring us to a total of five, all of whom lived with us in a house we bought that was built in 1750. It was indeed a frothy time. But what caught me by surprise were pressures that were not immediately apparent and began to steer my life along a rather new course, one different from the traditional academic life.

The first of these pressures was an offer to become the director of the National Institute of Neurological Disorders at NIH. I was seriously tempted by the offer, but it simply was not the right time of my life to move. Anne and I were trying to integrate families, and I felt it was premature to leave the department I had committed to build. I began to sense that this offer signaled something about possible new, unexpected directions, but this path still felt reasonably close to my home base of neuroscience. I could understand that I met the science test and had gained experience with advocacy groups, had some experience on the Hill, and had had deep involvement with NIH. Thus, although I sensed this offer might mean more, I had not yet appreciated that I was on a path leading to a rather radical change in my life.

Not long after I received the offer from NIH, I began receiving occasional feelers about senior administrative positions at universities. I had never aspired to be a university senior administration and was quite content with my life at Stony Brook. One prospect did catch my attention, however, vice president for research and dean of the graduate school at Northwestern University. Neuroscience, defined by an organ, is an amalgamation of many fields. It was not a large intellectual leap to imagine playing a role in overseeing and cultivating an even broader array of scientific disciplines and catalyzing cross-disciplinary efforts. There was an appeal to viewing science more broadly from a higher altitude.

I agreed to a visit and was impressed immediately by four things: the unexploited scientific potential at Northwestern, the structure of the position that implied it could indeed have a high impact, the richness of the scientific environment in the Chicago area, and the president of the university—Arnold Weber. The search was on a fast track, and I visited again in two weeks. By then it had become clear that the demands of the position simply would not permit operating a laboratory and, in fact, Weber made this quite explicit. First, I was surprised I would even consider this. I had just had my principal grant renewed for five years, and my research program was at a significant inflection point. I did not have the luxury of agonizing over the decision because the search was on such a fast track. I honestly cannot say where I would have come out if I had had more time to consider. But the position was intriguing, and I took the leap.

Did I have subsequent regrets about abandoning the model system when it had just reached such a potentially productive stage? Aside from

an occasional pang when talking to old colleagues, I really did not with one exception: no one was able to pick up the baton of the model system. I have wondered at times to what extent the choice of a nonmammalian vertebrate was an almost insurmountable disincentive. Beyond this, my life at the time became increasingly relentless between the demands of my university position and a full dance card of growing outside responsibilities that I describe shortly. I must admit, however, that as I write this autobiography and recount my research life, I have had some sharper than usual pangs.

In my first six years at Northwestern, 1986–1992, I was embedded in educating myself about a broad range of sciences and vigorously promoting science at the university. The research volume in dollars at Northwestern tripled during these six years. Significantly contributing to this growth were interdisciplinary research efforts through centers, some of which involved collaborations with other universities. For example, we competed successfully for an NSF Science and Technology Center in Materials Sciences in collaboration with the University of Chicago and the University of Illinois. We also were awarded an NSF Science and Technology grant for a Circadian Rhythms Center in collaboration with UVA. Another initiative was to bring focus to and advance what had been a low-key Technology Transfer Office. This was an enormously instructive and exceedingly satisfying time for me, as well as being a highly productive time for the university.

During this same period, requests to serve on an array of scientific boards filled my dance card to overflowing. I joined the Life Sciences Research Advisory Board of the Air Force Office of Scientific Research, serving until 1991. I served on the board of Argonne National Laboratory, one of the large general purpose laboratories of the Department of Energy from 1986 to 1994, and I was privileged to chair the Scientific and Technical Advisory Committee of that board from 1988 to 1994. This committee was responsible for in-depth reviews of all programs at the laboratory. During my tenure as chair, the Advanced Photon Source was constructed at Argonne, a major national facility of broad benefit to science and in particular to structural biology.

This kind of activity became epidemic, and in 1987, I was invited to join the board of the Fermi National Accelerator Laboratory, a special purpose Department of Energy Laboratory and, until the Large Hadron Collider at CERN went online in 2008, the most powerful accelerator in the world. I served until 1995 and chaired the Administrative Committee of the Board from 1993 to 1995. This role immersed me in high-energy physics and cosmology, and as the only nonphysicist on the board, I must admit to feeling at moments like I was having an out-of-body experience. This feeling reached a peak when for a brief time I served on the oversight committee for superconducting magnet development for the Superconducting Super Collider. Additionally, I sat on the Executive Committee of the Scientific Advisory Committee of the Governor of Illinois from 1989 to 1995.

Between my role at Northwestern in broadly facilitating research and these outside responsibilities, I underwent 6–10 years of “total STEM immersion.” I deeply value that experience of viewing science from 30,000 feet. To this day, I can understand at least three research articles in each weekly issue of *Science*.

In 1992, the provost at Northwestern stepped down and I was asked to fill that position. On arriving at my office at 7:30 a.m. on my first day, there were already two phone messages. One was from the general counsel of the university and the other from the football coach. It was immediately clear that my intellectual life was going to move from foreground to background. Indeed, my first administrative act was to sign off on a policy banning rollerblading in university buildings. So much for the life of the mind.

My administrative experiences up until this time, from departmental chair at Stony Brook to vice president for research at Northwestern, had substantially broadened and deepened my exposure to science and developed my skills in research administration. On becoming provost in 1992 and going forward, I began to acquire rather different skill sets. These skills are perhaps best described as general management and budget skills and at Northwestern I was trained by a master, President Arnold Weber. He is the toughest and most effective manager I have ever encountered. An MIT-trained economist, Weber had served as a deputy director of the federal Office of Management and Budget under George Schultz, chancellor of the University of Colorado system, and as director on many Fortune 500 boards. One particular testimonial has stayed with me, a comment made by one of our trustees at Northwestern, a Fortune 500 CEO. He said, “Weber could run anything, including World War III.”

In parallel with my emerging academic management skills and likely because of them, I found myself being increasingly exposed to business, first through my activities directing Northwestern’s Technology Transfer Office and then with enterprises outside of the university. In 1990, I was asked to serve on the board of Zenith Electronics and continued in this post as director until 1995 when LG Electronics acquired Zenith. I have been serving on boards of private and public companies ever since.

When Weber retired from the presidency of Northwestern in 1994, I decided it was time to explore new directions. As a sitting provost at a major research university, it was not a surprise to be invited to look at various university presidencies. The problem here was being offered presidencies I did not want and not being offered presidencies I did want. Also, given my service as chair of the AAMC and other activities in academic medicine, it also was not a surprise to be invited to look at medical school deanships. After exploring two such possibilities, I decided this was not a direction I wanted to pursue.

What did come as a surprise, however, were contacts from outside academe. One direction that perhaps should not have been unexpected,

given my scientific background, management experience, and growing involvement with business, were inquiries about CEO positions at start-up biotech companies. Two other offers in particular stick with me because they seemed rather discontinuous with my background. One was finding myself on the short list for the secretary of the Smithsonian. The more deeply I explored that prospect, the more exciting it became and had I been offered the position I would have accepted. The other was even further afield. It was a phone call asking would I be willing to be a candidate for the presidency of the Federal Reserve Bank of Chicago. My initial response was to tell the gentleman on the phone that he had the wrong David Cohen. (I assumed that somewhere in the country there was a David Cohen who might be qualified to head a Federal Reserve Bank.) He asked if I were the provost at Northwestern. I said, yes, but I was a biologist and, while I would enjoy sitting on the Federal Open Market Committee setting interest rates, I had no idea how to clear checks. He said what the bank currently needed was a strong manager, and I now apparently enjoyed that reputation. The lesson for me was that I had unknowingly earned my way into a labor pool of “free-range chickens” and was fair game for possibilities I never had imagined.

At the end of the day, I decided two things with respect to my next career move. I belonged at a university and I wanted to be deeply embedded in the academic action in a senior operating position. I did not want to spend my days (and evenings) fund raising and attending grip-and-grin events. About this time I was asked by Columbia University to consider the position of vice president and dean of the faculty for Arts and Sciences. It is difficult to describe the position because it defies all principles of sound management. It is probably closest in structure to the dean of the faculty at Harvard, but with a somewhat expanded portfolio. All of the Arts and Sciences chairs were direct reports, as were the deans of Columbia College, the Graduate School, the School of General Studies, Continuing Education, the School of International and Public Affairs, and the School of the Arts. And, somewhere in the range of 30–35 centers and institutes were also in the portfolio. On paper, this gave approximately 65 direct reports. One could not be more involved in the day-to-day academic action. The vice president for arts and sciences was also a member of the president’s cabinet, giving broad exposure to university affairs. Finally, it was an opportunity to return to Manhattan, the mother country for me. So, I relocated to Columbia in 1995.

Even though administration at Columbia was a body contact sport and the job was relentless, I stayed with it for eight years, significantly outlasting the tenures of previous incumbents. I recall at the end of my first Arts and Sciences faculty meeting declaring that I would not return for another such meeting until I had a dais that covered my entire body. I retired from this administrative position in 2003 on turning 65. Those eight years were exceedingly exciting and satisfying with respect to enhancing many of the

26 departments in the Arts and Sciences. Many of these departments were highly ranked on my arrival at Columbia, and we managed to bring almost all into the top ranks through an innovative and highly rigorous process of program review. At the same time, we improved faculty salaries and eliminated a longstanding structural deficit, balancing the Arts and Sciences budget for the first time in more than a decade.

It is difficult to describe how intellectually stimulating it was to be so deeply involved with the broad array of Arts and Sciences departments, many of which were among the best in the world. We also substantially strengthened the School of the Arts and connected it to the undergraduates. It was a priceless education. And, I was hardly isolated from neuroscience, given the depth of neuroscience faculty at Columbia, including four who had been or would become presidents of the Society—Eric Kandel, Gerry Fischbach, Mickey Goldberg, and Carol Mason.

I returned to the faculty in 2003, but I had accrued sufficient leave to almost carry me to my full retirement from the university in 2008 at age 70. (Columbia has an extraordinarily generous leave policy that I critically looked at early on when planning how to balance the budget. That look lasted about 500 msec when I quickly realized there was not enough political capital on the entire planet to survive touching that third rail.) During this five-year period, I became increasingly involved in the business of education. I joined the boards of various companies involved in education-related enterprises and became an advising partner in a private equity enterprise involved in education. This carried me into my retirement life.

Retirement

I have now been retired from Columbia for seven years. During that time, I have continued to serve on boards, not all of which are related to education. Over time, however, the number of boards I serve on is steadily declining. I do continue to serve on the Investment Committee of the Society, and I try to attend as many of the past-president lunches at the annual meeting as I can. Until rather recently, the seating arrangement at these lunches had been chronological, based on the year of your presidency. When some of us more elderly presidents pointed out that each year we were getting closer to the end of the table, SfN compassionately moved to open seating. That removed a disincentive to attending the lunches. I do remain deeply interested in the affairs of the Society. It has been a truly significant part of my life.

A main event of my current life is serving pro bono as provost of a fully online, tuition-free university, the University of the People. This nonprofit university was established in 2009 with the mission of offering access to affordable, quality degree-granting programs online to any qualified student. I joined the institution soon after its launch. We were

accredited in 2014 and currently have approximately 2,000 students from more than 150 countries. Since receiving accreditation, the university is vigorously ramping up its enrollment and expanding its degree-granting programs. This all keeps me as busy as I wish to be, because I have every intention of fully enjoying my now rural life on our property that sits at the eastern border of the Hudson Valley and the western border of the Southern Berkshires.

Conclusion

I appreciate that this autobiography has been a bit atypical, if not disjointed, as neuroscience was the dominant theme of only the first half of my professional career, and this is a series dedicated to the history of neuroscience. On the other hand, had I restricted this to only that period, my autobiography would have ended at age 48 and that seemed rather peculiar. Hopefully, the second half of my career, although not bearing directly on the history of the field, might be of some interest as a case study of a neuroscientist's evolution to platforms that view science from an increasing altitude. After all, science did loom large in the second half of my career, but from a different perspective than the lab bench.

Throughout, however, the SfN has continued to be woven into the fabric of my life. I feel we came of age together as neuroscience established itself as a field. It has been extraordinarily rewarding to have deeply experienced this period in the history of science and, hopefully, to have made some contribution to it. As part of this, I want to take this opportunity to say how privileged I have been to have worked closely with all three executive directors of the Society. In the beginning was Marjorie Wilson. Were we blessed in having her. Wilson was responsible for steering us and launching us on our incredible growth path. She was indeed an uncommon woman. Wilson was succeeded on her retirement by Nancy Beang. Beang skillfully brought us through our adolescent years and into adulthood, providing essential support throughout my years as an officer. And then there is Marty Sage, who has impressively professionalized the Society, a critical need as we became an 800-pound organization. Indeed, I believe no scientific society is better managed or governed than the SfN.

To close, in 1993, I was invited to deliver the keynote address at the Annual Meeting of the Association of Neuroscience Departments and Programs, an organization I had founded some 10 years earlier. In preparing my remarks for that address, it occurred to me that a simple acronym, DRG, wove through life as a changing metaphor. In my student years it stood for dorsal root ganglion. Then, as I was launching my laboratory its meaning shifted to the Division of Research Grants at the NIH. As I became deeply involved with the AAMC in the 1980s and engaged with broad issues in academic medicine, including reimbursement for services, it transformed

to Diagnostic Related Groups. On moving to Columbia in the 1990s and adding the humanities and social sciences to my portfolio, DRG came to signify the budget code for the Department of Religion. That was as far as I took the metaphor at the time of that keynote address. Now, as I become longer in tooth, this metaphor continues by fully cycling back to its first incarnation, dorsal root ganglion, as I become more and more preoccupied with the expected aches and pains of aging. Since that begins to sound a trifle whiny, I will quit here—but not without saying it has been a great run. And I would leave you with two bits of gratuitous advice that have stood me in good stead throughout my career. Make your mistakes slowly and never forget the whimsy of it all.

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