

The History of Neuroscience in Autobiography
Volume 14

Edited by Thomas D. Albright
and Larry R. Squire
Published by Society for Neuroscience
ISBN: 978-0-916110-13-0

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pp. 206–245

https://www.doi.org/10.1523/hon.014005

Susan Hockfield



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NIH Postdoctoral Fellow, UCSF Neuroscience Program (1979–1980)
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Susan Hockfield pioneered the use of monoclonal antibody technology in brain research, identifying proteins regulated by neuronal activity early in an animal's life, coincident with activity-dependent neuronal development. She served as Yale University's dean, Graduate School of Arts and Sciences (1998–2002), and as provost (2002–29004). She was elected in 2004 as the 16th president of the Massachusetts Institute of Technology, the first woman and the first life scientist to lead the Institute. As a scientist and academic leader, she championed the convergence of disciplines to accelerate revolutionary advances in biomedicine and other areas of science. Her award-winning book, The Age of Living Machines *(2019), describes the people and technologies of a "convergence revolution," the integration of the life sciences and engineering reshaping the landscape of scientific research and producing new technologies to address the greatest humanitarian, medical, and environmental challenges of our time.*

Susan Hockfield

As far back as I can remember, I have been obsessed with how things work. Early in life, I took apart vacuum cleaners, daffodils, irons, hydraulic door closers, and even acorns to see what was inside. During my scientific career, I peered through electron microscopes and engineered antibodies to understand the structure and function of the mammalian brain. Later, in my administrative roles as dean, provost, and president, I convened colleagues across disparate disciplines—scientists, humanists, engineers, organizational experts, and more—to design new approaches to solve important academic puzzles and challenges. I never planned a formal career path for myself, but instead I had the great good fortune to leap from one insight or discovery to another, driven by my insatiable curiosity. That same curiosity drives me to ask questions and explore new ideas and technologies, often pursuing them without sufficient anticipation of where they might lead.

I was fortunate to come of age as a scientist in the midst of the molecular biology revolution. As an anatomist at my core, I intuitively understood that function could be deduced from form, and I was able to collaborate with many talented scientists to apply the tools of molecular biology to describe novel mechanisms in the brain. From the brain to the boardroom, I found terrific excitement and satisfaction in revealing a new perspective on a problem.

Life is the art of the possible—the convergence of disparate ideas, people, or fields into wholly new and unusual collaborations. To me, science is about discovering something in one domain and a different thing in a different domain, and then discovering that those things are related and that, if you put them together, each can inform the other. We have just begun to pursue all that's promised by the explosive intersection of biology and technology. That convergence of fields promises to lead to brilliant new ways to tackle our greatest medical, environmental, and humanitarian challenges. And just thinking about the possible products of these unexpected convergences makes me smile.

Early Years

My grandparents were immigrants from Eastern Europe, fleeing oppression and persecution. Like many first-generation Americans, they would never talk about their journey. When asked, my paternal grandfather, who arrived in the United States as a 13-year-old with his 11-year-old brother, but without their parents, would simply say, "We're Americans now." I've

often wondered just how difficult life has to be to send your young sons alone on a boat to the unknown. Difficult enough that if you were to stay in your homeland, you would be certain to die. I never met my great grandparents, but I'm ever grateful for their sacrifices to save the lives of their sons.

With an immigrant background and the desire to succeed in a new country came an intense focus on education. My father did what the son of an immigrant was expected to do: He became a studious scholar, electing to study electrical engineering at the University of Illinois. He was extraordinarily analytical and yet extraordinarily warm. His dream, he told me later in life, was to become a communications engineer, but World War II intervened. He joined the Navy and, upon his return from the war, went to law school. He spoke little about that career shift, but it became clear that his war experience on submarines and at the testing ground for the new armaments in the Pacific Theater changed his perspective on life. He finished law school and had a successful career as a corporate attorney while raising his family.

We were a big, busy family. I was one of four girls, spaced a year and a half apart. Our household was loud, boisterous, and regularly on the move. The second in the family, I was born in 1951 in Chicago, but we moved when I was three months old to Danbury, Connecticut, where I spent most of my youth. I have vivid memories of the community nursery school that my mother and her friends founded. Independent play was the educational model of the day, which suited me just fine. I explored by myself and spent a great amount of time taking things apart—dissecting them to figure out how they worked, but with no interest in reassembly.

We moved to Chappaqua, New York, when I was in fifth grade. My meticulous mother had researched the school systems near New York and determined that Chappaqua had the best public schools in the area. Her highest priority, after our safety, was making sure we had the best possible education.

My mother was some kind of magician. The kind of magician who whips up a gourmet meal in less than an hour. The kind of magician who sews colored loops in the toe of each of the household's socks to denote its owner and simplify sock sorting (performed by me and my sisters) for a family of six. The kind of magician who packs four little girls into a car and drives hours to the Pennsylvania Dutch community in Pennsylvania, or Washington, D.C., or New York City museums and concerts to teach her girls about history and to experience culture.

My parents pushed us all to be strong students, although I never considered myself to be one. They fostered my older sister's interest in reading, languages, and history, and encouraged my burgeoning fascination with mathematics and science, as evidenced by all the things I took apart. I don't recall my parents ever yelling at me for taking apart the iron

or the vacuum cleaner or whatever else caught my curiosity, although I do have a suspicion that my father quietly followed behind me, reassembling all the things I took apart.

And I certainly did have a penchant for taking things apart. I was an anatomist long before I knew what that word meant. I took apart flowers and seed pods. I took apart a fan, a lamp, a vacuum cleaner, you name it. If I could take it apart, I reasoned, I could understand how it worked. At eight years old, I dissected my watch—analog at that time, of course. I certainly didn't know what I was doing and, as I removed various components, the tightly wound mainspring suddenly popped out of the case and went flying, along with all the tiny pieces around it. I can still feel that spring bursting apart in my hands. There was no reassembling that dissection.

I had no idea that my methods for satisfying my curiosity were unusual, until one day a friend stopped me in my tracks. I was curiously fiddling with the hydraulic door closer that prevented the screen door from slamming shut, when my friend Lois shouted, "Stop that!" Puzzled, I responded, "Stop what?" I can still recall her impatience and disbelief as she described myself to me: "You're always taking things apart and never putting them back together, so stop it!" I still can feel my sense of incredulity. No one had ever before told me to stop my "experiments."

My interest in how structure gave rise to function continued unabated, encouraged by my parents. On Christmas morning when I was 10 years old, my parents handed me a wrapped box from under the tree. I opened it, and my jaw dropped. What would two intuitive parents give their daughter who dismantled every object she could find? A microscope!

It was a light microscope of my own, complete with premade slides of plants and insects. What a fantastic adventure—I loved it. Perhaps a stereotypical beginning for a scientist, but there I was, hunched over my microscope, examining everything I could squeeze onto that little stage. If I could figure out the structure of a leaf or seed pod or spring, I believed I could also figure out how it performed its function.

My parents interpreted my scientific curiosity as a sign that I would be the doctor in the family. But I had serious reservations about becoming a doctor. To this day, hospitals make me uncomfortable, and sickness and disease break my heart and easily bring me to tears. Other people, including my neuro-oncologist physician husband, have the amazing skill to hold a person's hand during the deepest, darkest times of their life, bringing comfort as well as healing. That is not my skill. Yet it was my predesignated path, so I went into high school with an expectation that I was headed toward med school.

I did not enjoy much of high school. I was painfully shy, and I'm not sure when or how I grew out of the worst of it. At one point, I came home from school unhappily and told my mother that I was lonely and did not

have a group of friends. She brusquely replied, "You're a leader. Start a group!"

I was baffled—start a group? I was no leader. But that magical mother of mine seemed to already know my future. If someone told me then, as a quiet, reluctant high schooler, that I would someday stand up to talk to 10 people—much less several thousand, as I would during my years at Yale and Massachusetts Institute of Technology (MIT)—I would have bet all the money in the world they were wrong.

Chappaqua's Horace Greeley High School was designed to simulate, as nearly as possible, a college experience. Many of the teachers cultivated independent learning, encouraging our curiosity and self-directed study. I had two wonderful biology classes in my freshman and senior years. As a freshman, we started by studying the structures of various animals and tissues using colorful transparencies of layered acetate. With the flip of a page, I moved from skin to muscle to an organ to bone. That experience, flipping deep into organ systems and back out again, was a key moment when my curiosity about how things work brushed up against living systems.

The apex of my high school experience was a senior-year biology class that included an independent study in which each student was assigned an organ system to study in rodents. Like flint to steel, a spark burst forth. I realized I wanted to study living things, to know how they worked. But I still didn't know what to call this love or how to pursue it. That would come later.

At that time, I was still destined for medical school.

College and Graduate School

As a freshman at the University of Rochester in Upstate New York, I chose biology as my major, but my heart wasn't in my coursework. That is, not until my very last semester, when I took a cell biology class with Professor Jerome Kaye. That single class would soon introduce me to the thing I had unknowingly been looking for my whole life.

At the time, electron microscopy was biology's new shiny toy, revealing details about the beautiful structures inside of cells that provided insights into how cells gave rise to the properties of tissues and clues to what went awry in disease. The class introduced me to a recently published book, *The Fine Structure of the Nervous System* by Alan Peters, Sanford Palay, and Henry Webster (1970). The pages included the most wonderful images I had ever seen, obtained by an electron microscope. Electron microscopy unveiled cellular structures at a higher resolution than previously achievable, and that ability would burst open a whole new level of understanding of how the molecular composition of subcellular elements gives rise to a cell's function—the molecular biology of cells. In that class, I realized it

was possible to visually dissect a cell to understand how it works. I was immediately hooked.

One day, Dr. Kaye opened class by posing a question related to the course material. By the conclusion of the class, I thought I knew the answer. So for the first time in my college career, I attended office hours to discuss his question. To this day, I encourage students to go to office hours. Faculty reserve those times for students to bring questions, difficulties, and ambitions. Too few students take advantage of the opportunity of office hours, and—trust me—your faculty are eager to talk to you!

At office hours for the first time, I proposed my solution to the question Dr. Kaye had presented in class. He encouraged my answer, and we kept talking. I conveyed how much I enjoyed his class and confessed my ambivalence about going to medical school.

I will never forget the most important moment in that conversation—in fact, the most important moment in my scientific life: Dr. Kaye pointed a finger at me, leaned forward, and said, "You should get a job in a lab." I asked how one did that. He suggested I walk across the street to the University of Rochester's Medical Center and see if anyone had an open position.

Driven by my curiosity, I followed his advice. I wandered around the medical school for a while, without any appreciation of what departments I walked through. While looking at a poster on one wall, Professor David Scott, an Anatomy Department faculty member responsible for the poster, walked up. I asked him a few questions about his research, which he generously answered. Somehow, in the course of our conversation, I mentioned that I wanted to work in a lab. He offered me a part-time job as a laboratory technician, and I enthusiastically accepted.

On my first day at the lab, I knew I had found what I was looking for. Gerda Dudley, the lab's highly experienced chief technician, introduced me to machines, specimens, and lab personnel. In my memory, the laboratory was filled with brilliant light, the benchtops were covered with an array of fascinating equipment, and a spirit of adventure buzzed in my mind and heart. To this day, I remember saying to myself, with a heartfelt sigh, "*This* is what I've been looking for." I was home.

I quickly became familiar with the staff, tools, and routines of a neuroendocrinology lab that was using electron microscopy to understand the structure and function of endocrine regions of the brain. Gerda Dudley was a master technician and generously taught me everything I needed to know to get started. It didn't matter if I was preparing slides or washing dishes, I loved every moment in that lab.

We took tiny pieces of the brain and examined them at the high resolution of the electron microscope, mapping how a network of nerve cells in one area communicates with those in another area. It was an era of the biological sciences when researchers were just starting to figure out the extent of

what one could visualize with an electron microscope, and the potential for understanding cell functions felt limitless—especially for anatomists like me, and especially in the brain. The brain, with its wildly diverse types of cells and serpentine communication superhighways, is extraordinarily beautiful, and its performance inconceivably complex and precise.

I worked in that lab for nearly a year, starting after I'd graduated a semester early in the winter of 1972. I then moved to Washington, D.C., where, with my technical expertise, I easily found a job as an electron microscopist at George Washington University Medical School, again in the Department of Anatomy. The lab I joined was technically interesting, although it wasn't hugely intellectually stimulating. So, after about a year, when numerous people asked me when I was going to graduate school, I began considering it. I still did not have aspirations to run a lab of my own, nor did I have any long-term plan about where my studies might take me, but it seemed like a logical next step, so I plunged ahead.

Intent on staying in D.C., I applied to the anatomy graduate programs at George Washington University and Georgetown University. Georgetown offered me a better package and allowed me to begin right away, in the middle of a school year, so I joined the Department of Anatomy and Neuroscience at the Georgetown University School of Medicine in January 1975.

In addition to taking classes, I soon began teaching and lecturing in anatomy and histology classes for medical and dental students. There is no better way to learn material than to teach it, and I especially loved guiding laboratory classes, where the students and I met weekly to dive deep into the material.

The core of my graduate education, however, was not at Georgetown. It was at the National Institutes of Health (NIH). During my very first semester, in the spring of 1975, the chair of the Anatomy Department received a call from Dr. Stephen Gobel, a member of the pain research group at the National Institute of Dental Research (NIDR) at the NIH. The NIDR group focused their investigations on the transmission of pain signals from the periphery (skin, joints, teeth) to the brain. Dr. Gobel had received funding to bring a graduate student into his lab for a summer project. For some reason, perhaps because I had extensive lab experience, the chair asked if I'd like to apply. I went to the NIH and interviewed for the job. The project seemed a bit old-fashioned, using a light microscope attached to a "drawing tube" to reflect the microscopic view onto a drawing surface that allowed a reproduction to be drawn. Having come from two years of electron microscopy, I told him, "It seems kind of boring."

Unsurprisingly, he gave another student the job. But then a second student position became available, and this time the project was fascinating: developing a new technology to map the cellular pathways that carry pain information through the brain. I called Dr. Gobel, apologized for my

lack of initial enthusiasm, and asked (please) for another shot. He generously invited me to join the team.

I am so thankful I received that chance. The NIH became the hub around which my graduate life revolved. The pain research group Steve Gobel was a part of included a remarkably broad and exciting combination of disciplines: Steve was an anatomist and had several anatomist postdocs, and the larger pain research group included neurophysiologists, neurochemists, pharmacologists, and psychologists. Part of the group worked at the NIH's Clinical Center, with access to patients who suffered from chronic pain syndromes and were receiving clinical interventions. To someone used to classically organized academic departments, it seemed a highly unusual—and exciting—mash-up of people with different intellectual backgrounds, but I soon came to understand that it made brilliant good sense. Together, the unique, multidisciplinary team was working to identify and understand the neural circuitry and connectivity that allows us to feel pain, with the goal of designing new approaches to prevent or alleviate pain.

The lab radiated a sense of community: a bustling village of people with different skills and different tools, all focused on the same purpose. My education was no longer predominantly books and lectures, but traversing the busy hallways and laboratories, and attending professional seminars, across the institutes of the NIH, a professional realm where giants in the field were making life-changing discoveries. It was there that I first learned that the discoveries in a laboratory can be the antecedents to a clinical intervention. It was there that I joined the conversation at seminars across a wide variety of disciplines. And it was there that I learned how to share my research with the scientific community.

As my first summer in Steve's laboratory was drawing to a close, he and I decided that I should continue in the lab to complete my experiments. It wasn't long before he asked me to write up a draft of some of our research. I eagerly submitted my manuscript to him but was stunned when, after reading my draft, he made the observation, "You don't know how to write. But I'll teach you." And he did: another important step in joining the scientific community.

Under the tutelage of Steve and the others in the research group, I joined the commerce of being a scientist—reading relevant literature, writing and publishing papers, and participating in the constant discussion of science in journal clubs, seminars, and the ever-present hallway conversations. Steve brought me along to professional meetings, where I began to meet leaders in the fields of pain research and neuroanatomy. I found those meetings exhilarating: the formal talks as well as the informal conversations over coffee breaks and at poster presentations. I had finally found my people—people who shared my curiosity and spoke as animatedly about the brain as I did.

After a while, I was spending so much time at the NIH and enjoying my work there that I decided to do my dissertation project with Steve at the NIDR. I don't recall whether Steve or I first hatched the somewhat unusual idea of carrying out my doctoral research with him. It was far from standard for a Georgetown graduate student to do her doctoral project outside the university. With some gentle nudging, Georgetown acquiesced. Steve was given a Georgetown faculty appointment to allow him to serve as my legitimate doctoral adviser. It was an unusual situation, and one of the greatest gifts I ever received: completing my dissertation in one of the world's premiere pain research groups with the freedom to devote all of my efforts to the work of a scientist.

With Steve, I published a pair of papers on the pathways through which pain signals travel from the face and head through an area of the brain called the nucleus caudalis, a central hub for pain processing. When you have the sensation of a painful stimulus—such as a bee sting on the leg or an iron burn on the tip of a finger—that sensory input enters the nervous system and travels to the spinal cord from where it is then relayed to higher brain centers. But pain input from the head and neck, including from the teeth, travels a more complicated path. For my dissertation, I developed a method for tracing the pathway of pain signals for those inputs, received in the nucleus caudalis, and then relayed to higher processing centers in the brain.

As I was nearing completion of the work for my doctoral degree, I understood the next logical step would be a period of postdoctoral study, usually carried out in a different laboratory. While attending scientific meetings, I met a great many people working in the field of pain research, and, through those connections, I arranged a postdoc in the lab of pain researcher Allan Basbaum at the University of California, San Francisco (UCSF).

Looking back now, I realize I was more devoted to neuroanatomy than to the study of pain. And what I enjoyed most was learning and using new technologies to study the structure and connectivity of the brain. So, it was not surprising that my postdoc in California was abbreviated by the lure of another new technology.

For any scientist, accessing a new technology at the outset of its use is an extraordinary opportunity—a chance to understand problems you're studying in a new light. And, by chance, I was about to have that experience a second time.

Cold Spring Harbor Laboratory

In the fall of 1979, I arrived in San Francisco to begin a postdoctoral fellowship in the Basbaum lab. California was sunny and lovely, but as life would have it, I wasn't there for long.

In the spring of my first postdoc year, Allan suggested I participate in a summer workshop at the Cold Spring Harbor Laboratory (CSHL) on Long Island in New York. The workshop was focused on pain, Allan's field of study, and we anticipated that the experience would help me gain a deeper understanding of the field as I started my postdoc. I had heard of CSHL but had never visited. "The Lab," as it was called, was then under the direction of James Watson. Jim, as we all knew him, and Francis Crick had famously deduced the double-helical structure of DNA in 1953. An independent, nonprofit research center, CSHL was well known and highly regarded for its research programs, meetings, and educational programs, including summer courses and workshop series, all designed to promulgate state-of-the-art technologies and perspectives. Under Watson's leadership, CSHL became a major center for the rapidly developing new field of molecular biology.

In the summer of 1980, I attended the CSHL pain workshop, a multidisciplinary gathering of researchers studying how pain is received and perceived. Soon after I arrived, I met Birgit Zipser, an electrophysiologist and the first neurobiologist to join the CSHL staff, and Ronald McKay, a CSHL molecular biologist who would go on to become chief of the Laboratory of Molecular Biology at the National Institute of Neurological Disorders and Stroke. These two scientists would open the door to the next stage of my career.

Together, Birgit and Ron had recently developed a set of monoclonal antibodies that identified components of the leech nervous system. Monoclonal antibodies, or mAbs, were a brand-new technology, developed in 1975 by researchers at the Laboratory for Molecular Biology in England. At the time, only three or four groups were using them to study the nervous system.

To produce mAbs, mouse immune cells were stimulated to generate antibodies to an "immunizing antigen"—in this case, material from the leech nervous system. The resulting mouse immune cells were separated and propagated as individual "clones." Each clone represented the descendants of an individual immune cell, hence the name "monoclonal." The antibodies generated from each of these immune cell clones bind one specific target, or antigen. This remarkable new technology allowed scientists to understand the cellular—and subcellular—distribution of individual molecules, providing a entirely new level of resolution in biology.

Birgit had been working on the leech nervous system since her graduate school days, when the leech had become an important, highly tractable model for studying the nervous system. The leech "brain" is organized into a chain of ganglia, and each ganglion is made up of a cluster of roughly 400 neuron cell bodies that relay nerve signals among the other ganglia and between the ganglia and the rest of the body. Each ganglion's structure is highly ordered with clearly visible structures, visible with a relatively

low-resolution microscope. In marked contrast, the mammalian correlate—the neurons and nerve pathways of the brain and spinal cord—are densely packed with neurons and nerve pathways, making it much more difficult to analyze functionally or structurally.

In the leech, electrophysiologists like Birgit had mapped the function of each of those 400 leech neurons: Some carried signals for touch; others for pressure; still others for pain. Now, with a set of mAbs in hand, Birgit was eager to learn if each physiological or anatomical class of neurons would express specific molecular markers, such as unique cell surface proteins, that might be identified with mAbs.

When I arrived for the CSHL summer workshop, Birgit and Ron had just started testing their new mAbs to see what they might learn about the organization of the leech nervous system. I viscerally recall Birgit showing me an early set of photographs of the staining patterns of different antibodies, each of which labeled different sets of neurons. Much to everyone's surprise, the antibody staining patterns did not correlate with neuron classes identified by anatomical or functional characteristics. I was intrigued, thinking, "This is amazing! What molecules could be expressed in these patterns; what could these new patterns mean?" We discussed what the antibodies could be recognizing that didn't follow the established classes of neurons that shared electrophysiological or anatomical characteristics.

Birgit had gotten my attention and, as I was an electron microscopist, I began a study, separate from the pain workshop, to see if I could pinpoint what cellular structures were identified by each antibody. How many different cellular organelles might differentiate one neuron from another? Would the heterogeneity of molecular markers mimic known anatomical or physiological heterogeneity, or might there be an entirely different "molecular identity" that had not been revealed by earlier decades of study? The answer was that the molecular organization revealed by the antibodies had very little in common with previously described anatomical or physiological organizations, posing a fascinating puzzle. I was totally hooked.

As the workshop was ending, Birgit asked if I would consider the possibility of a job at CSHL. I immediately said yes. She said that I would need to meet with Jim. I agreed but explained that I had plans to visit colleagues in Washington, D.C., while I was on the East Coast. I told her to give me a call if and when Jim would like to meet.

I drove six hours to Washington. No sooner had I arrived than Birgit called: "Jim would like to see you tomorrow." So, I got back in my car and drove another six hours back to the laboratory. Sitting on the Long Island Expressway in its perpetual traffic jam, I thought, "I've only just started my postdoc. Can I really leave and take a job?" But my overwhelming sense was *yes*: CSHL was heaven on earth for scientists, with state-of-the-art technology, a huge variety of brilliant people across disciplines, an untold

number of the world's best and up-and-coming scientists coming and going, and an environment that fostered science 24/7. It was a year-round summer camp for scientists.

Upon my return to CSHL, I went to Jim's office. The possibility of a position seemed remote, so I went into the interview without any anxiety—after all, I had not even completed my first year as a postdoc. Jim and I had an unexpectedly wonderful conversation. Jim had remarkably wide-ranging perspectives of the scientific frontier across many fields. He knew that very little molecular biology had found its way to neurobiology—at the time, molecular biology had invaded only a few fields, such as immunology—and he reasoned that the new tools of molecular biology could open a new chapter in neuroscience. Naturally, I agreed.

Jim presented me with a job offer. Many people advised me not to take it. There were many ways it could trip up my career—most obviously by interrupting the postdoc I'd just begun. In addition, the risks of embarking on an entirely new, unexplored research direction were high: There was no guarantee of success. If I failed at CSHL or the job was short-lived, the UCSF had no responsibility to take me back. But I was not wise enough to take those worries seriously, and I was entranced by the prospects of using new molecular tools to reveal as yet unseen features of the brain. I accepted the position.

I went back to UCSF for a few months before my CSHL start date in December. The director of the UCSF neuroscience program was understandably disappointed in my decision and asked if I was sure. I replied that I was. "Well, aren't you afraid?" he asked. "What should I be afraid of?" I replied. "People are going to expect a lot of you," he responded. I honestly, if cheekily, replied, "Well, it's hard for me to imagine anyone expecting more of me than I expect of myself." Ah, the audacity of youth.

Upon my return to CSHL, I was immediately incorporated into the neurobiology team with Birgit and Ron, housed in one of the historic buildings on the lab's campus. My first task was looking into the curious question of why mAbs were lighting up specific, but as yet unidentified, targets in the nervous system. We couldn't yet make sense of why the antibodies were consistently recognizing certain groups of leech neurons with no known physiological or anatomical characteristics shared among them. I performed an electron microscopic analysis of the antibody binding patterns and found that some antibodies bound to the surface of cells, while others recognized intracellular organelles that we couldn't quite identify. There were many more patterns and binding sites than had been previously described—new cellular structures and new classes of neurons! Our carefully constructed characterization of neurons needed revision.

I needed to learn more molecular biology fast. I attended laboratory-wide seminars and made lists of vocabulary words I didn't understand, and

then over lunch asked other junior scientists to explain them to me. It was a crazy way to learn, but it turned out to be a wonderful tutorial.

As a mammalian neuroscientist, I then applied the same technique Birgit and Ron had used in the leech to a mammal. We created 29 lines of mAbs that bound to neuronal antigens in the rodent brain: 16 bound to axons only; 8 bound to axons and cell bodies; and 5 bound to cell bodies only. The patterns, once again, did not match the generally accepted cellular classifications. Overall, the distribution of the antibodies in the central nervous system (CNS) suggested that there was substantially greater molecular diversity among vertebrate neurons than expected, beyond anything before appreciated about the brain's organization.

That initial study was such a new application of the technology it was highly unlikely that a standard journal would have published it and, even if it did, it was unlikely to be published rapidly. Jim, recognizing the value of this technology for changing the way the nervous system could be explored and understood, knew we needed to get the work out fast, so as a member of the National Academy of Sciences, he offered to directly contribute the paper to the *Proceedings of the National Academy of Sciences*. It was published in 1982 (McKay and Hockfield, 1982).

I spent all my time in the lab: morning, noon, and night. Jim had a pattern of walking around in the evenings and stopping by—unannounced—in labs. I was always there, so each time he "dropped in," I'd describe my recent results. Jim recognized early on that neuroscience was ready for molecular biology.

Jim was always looking ahead, in the field and in the lab. One evening he came through the lab and observed, "You have a lot of work going on. You need some postdocs." That had never occurred to me, despite everyone else around me having postdocs. I was a "newbie," just doing my work, head down. I replied to Jim, "I don't have grants yet. How would I do that?" He said he would support two postdocs for the first year, after which I would raise funds to continue to support them. Although I didn't know it at the time, it was the kind of support new faculty would receive as start-up funds from a university. I did not have the experience to have asked for such funding when I started at the lab.

Through that initial hiring and the subsequent addition of two technicians and postdocs, Jim taught me how to grow a lab. I soon received grants from both the NIH and the National Science Foundation (NSF) but, without Jim's initial support, none of the studies would have been accomplished or published.

With my expanding team, I went on to use mAbs to identify specific subsets of neurons and anatomical structures in the central visual system of mammals (Hockfield et al., 1983; Hendry et al., 1984). In addition to lab work, I soon took on another task at CSHL. In my second year on the scientific staff, I encountered Jim during a campus walk. He said he wanted me

to run the Summer Neurobiology Course Program, a set of courses and meetings that drew scientists from around the world to CSHL. I asked why he had picked me. Jim simply replied, "When the phone rings, you pick it up." That was his shorthand that I had skills—being curious, organized, and responsible—that I didn't recognize in myself.

I had no idea how to run the course program, but thankfully I had Terri Grodzicker as my guide. A molecular geneticist and virologist, she started at CSHL in 1972 and continued there for her entire career, serving now as dean of Academic Affairs. There were not many women on the scientific staff at the time, and Terri, while always appearing serious, was also joyful and positive. She set an aspiring example of how a woman could navigate the lab and leadership, engender respect as a scientist, and be an effective leader in a predominantly male environment.

As the director of the CSHL Summer Neurobiology Course Program, I was responsible for identifying scientific leaders from around the world as course instructors, and then persuading them to take on the role. With Jim's help, I identified and reached out to a cadre of researchers who were or would soon be giants in the field, including Eric Kandel, Chuck Stevens, David Van Essen, David Hubel, and Torsten Wiesel. After months of planning, each summer course came together, with participants from across the nation and around the world, working as one group aligned around the ambition of advancing each nascent field. It was my first experience helping to design a science organization and that role was central to my developing a passion for convening people around a shared purpose. I would continue to lead the Summer Neurobiology Course Program for 12 years, until 1997.

Each year since 1933, the Cold Spring Harbor Symposia have played a key role in identifying emerging scientific fields. Scientists around the world would eagerly await the announcement of each year's topic, with the imprimatur of the symposium serving as a powerful accelerant for a new field. Jim's announcement of molecular neurobiology as the topic of the 1983 Cold Spring Harbor Symposium was greeted with great excitement, along with some skepticism about the nascent field.

Jim and Ron McKay organized the Molecular Neurobiology Symposium: 91 speakers and 233 participants. Scientists using the tools of molecular biology to study the nervous system, from physiologists to pharmacologists to anatomists, traveled from around the world to share techniques and ideas. The symposium synopsis later summarized the meeting as "a decisive turning point in the development of neurobiology." I fully agree. It was electric. I gave a platform presentation, as Jim was very supportive of giving young scientists an opportunity to share their work.

Cold Spring Harbor's scientific staff included many remarkable scientists, and the lab population was sufficiently small for everyone to know one another. Informal conversations were easily launched: walking down

Bungtown Road, during lunch at Blackford Hall, before or after the many seminars, or on the tennis courts or running routes around the lab. Through those informal conversations, I had the extraordinary privilege of striking up a friendship with Barbara McClintock. In 1941, Barbara had joined the staff of the Carnegie Institution of Washington in Cold Spring Harbor, the predecessor to the CSHL, and she was still actively engaged at the lab while I was there.

Initially, Barbara had kindly stopped to talk with me when our paths crossed on the lab grounds. After a few such chats, I began blocking time in my calendar to go to her laboratory to talk. If I blocked 30 minutes, our conversation lasted an hour and 30 minutes. We wandered through many topics, but when the topic was her research, she would pull out examples from her work and talk through the figures. She was both wholly analytical and deeply inspired by nature—she once described one of her methods for assaying her corn crops: She'd walk between the rows of corn she had planted, with her eyes closed, hands spread wide to each side, feeling the plants on either side of her. As an astute biographer once described her, she had "a feeling for the organism." She recounted many of her scientific and life experiences, encouraging me not to be bound by "received wisdom."

In 1983, Barbara won the Nobel Prize for Physiology or Medicine for her 1950s discovery of mobile genetic elements. A press conference was announced, and all the scientists packed into the auditorium. She sat amidst a semicircle of microphones and cameras. The press conference began, with the first reporter asking something along the lines of, "Weren't you mad that you weren't recognized sooner?"

"No," she replied calmly.

Then the second question came, "Didn't it really annoy you that others were recognized before you?"

"No," she replied simply.

When the third reporter asked, "Weren't you really angry that people didn't recognize the importance of your work?" she sighed and said, with a touch of impatience, "You don't understand. When you know you're right, it doesn't matter what anyone thinks."

The small group of reporters laughed, and simultaneously the entire audience of scientists rose as one, cheering and clapping. It was among the most inspiring moments of my life. That was Barbara: It never mattered to her what anyone thought—what mattered to her was being sure her science was right.

Barbara's wisdom helped me navigate a difficult time as I was making plans to leave CSHL. Very few researchers pursued their full career at the lab, so after five years, it felt like the right time to consider moving to a new academic setting, especially to an environment with a larger neuroscience community for the next stage of my career. I began to accept invitations to speak in the context of job searches. At one university, one

of the faculty questioned the integrity of my results, quizzing me not only about the interpretation but also about the basic results themselves. I was taken aback. I thought my work would be received as new and exciting, as it had been in other talks and publications. But this time, it was viewed as *too* new, too avant-garde.

That reaction to my work was deeply unsettling, even humiliating. I hadn't told anyone about it, but when Barbara asked me how the trip had been, I confessed what happened. She nodded her head, sagely saying, "Yes. It will always be that way when you do something new. And it's worse when you're a woman. But don't let it stop you." Those words gave me courage at a difficult time, for which I have always felt immensely grateful. After mustering the courage she advised, I continued giving lectures at many top-ranked programs and was soon recruited to become a faculty member at Yale University.

Yale University

I arrived at Yale in 1985 as an assistant professor in the Section of Neurobiology in the School of Medicine, attracted by the university's robust activity in neuroscience across the campus.

It was exactly the kind of appointment I was looking for, with neuroscience faculty deploying a wide range of methodologies in a wide range of organisms, from invertebrates to humans, from basic cell biology to clinical studies. Central to my faculty appointment was the expectation that I would develop a robust research laboratory. Two of my CSHL postdocs, Sam Zaremba and Beth Friedman, continued with me at Yale and were instrumental in helping set up the lab and launch our research program. We continued to use mAbs to explore the molecular composition of the nervous system and to identify new cells and new organizational features of the CNS. Using an mAb, we identified a previously unrecognized class of neurons that appeared to play a unique role in the circuitry of the cerebellum (Hockfield, 1987). It was the beginning of an exciting era when the tools of molecular biology provided new ways to identify cell classes and determine novel subtypes of cells by virtue of their shared molecular features.

One great advantage in having my lab and teaching responsibilities anchored in the heart of the Medical School was that it brought me into contact with both medical and graduate students, as well as physician scientists, many of whom joined the lab and later went on to have brilliant careers, including Daniel Geschwind, Mustafa Sahin, Jane Minturn, Kathleen Martin, Peter Kind, and Robert Kalb. The lab was a constantly evolving group of people with an array of academic backgrounds, skills, working styles, and career ambitions.

It was a wonderfully vibrant, intellectually collaborative group. Everyone had a different project, yet each of them shared the lab's central

thesis, to advance the molecular characterization of the components of the brain and follow them over the course of development. Dan Geschwind, for example, one of my students pursuing both MD and PhD degrees, became interested in another new technology, two-dimensional electrophoresis, which allowed for the identification of individual proteins at high resolution in two dimensions. Using it, he was able to observe changes in protein expression over the course of brain development (Geschwind and Hockfield, 1989). Mustafa Sahin, also pursuing both MD and PhD degrees, used mAbs to study the organization of the mammalian cerebellar cortex and describe a very rare cell type, the Lugaro cell, showing that it was molecularly and morphologically distinct from Purkinje and Golgi cells (Sahin and Hockfield, 1990).

During these early years, Beth Friedman, a postdoc, initiated a project with Patricia "Patty" Pedersen, a postdoc from the lab of the brilliant neuroscientist Gordon Shepherd on a project identifying specific classes of neurons in the olfactory bulb. That lab connection would soon alter my life in a way that had little to do with science. Patty, with her deep understanding of people, was something of a matchmaker. Patty's husband, Joe Piepmeier, a neurosurgeon, had worked with a highly respected neurologist and neuro-oncologist, Thomas Byrne, during their training at Yale. Patty arranged a dinner for the four of us, but on the day of the double date, Patty called to say that their infant son was not well and they'd need to postpone. It was a relief: I was busy preparing for my departure to teach a summer course at CSHL. But, soon after Patty's call, Tom called to suggest, "Why don't we have dinner together anyway?" I initially declined, but he managed to persuade me that I had plenty of time to "just have dinner." He arrived at my apartment, and when I opened the door, a strange and unprecedented thought crossed my mind: *I'm going to marry this man.*

And I did. Four years after we met, we were married at Yale's Battell Chapel. Our daughter, Elizabeth, was born soon after. Marriage and a child came as a delightful surprise to me. For years, I was clear about the life I wanted as a scientist, but I did not have the same decisiveness about the direction of the rest of my life. I knew I wanted a full, rich life, but I did not have any plan for what it would look like. Turns out, it looked like Tom and Elizabeth, the loves of my life.

Another unexpected thread began to run through my life during those first few years at Yale: I found myself following a path that would lead—without intention—into university leadership. It started with the graduate students. In addition to my laboratory research, I taught graduate and medical students in a neurobiology course and in weekly laboratory sections. Supplementing the didactic lectures, the small-group lab experience was hands-on, exploratory, and provided a wonderfully deep dive into brain anatomy and function with students who really needed to

understand the intricacies of the nervous system. It also put me in conversation with graduate students.

Many of the graduate students I spoke with were disaffected. Unlike my own graduate experience with Steve Gobel at the NIH, where I felt fully included as a member of the research team, many of the Yale graduate students felt outside the center of discussions, and they often had no idea how to access mentors, advice, or the kinds of insight that would help move their projects along.

Eager to see them succeed, I often offered advice. For a student who might be stuck on an experiment or a project, I'd recommend a faculty member or an experienced technician for that student to talk to. Often, they'd be surprised that they could do that; I assured them that they didn't need permission to engage in research conversations with faculty or more senior students and postdocs, and I encouraged them to ask questions in seminars—to get involved in the intellectual commerce of their field of interest.

My openness to engaging with graduate students precipitated the chair of the Section of Neurobiology, Professor Pasko Rakic, to ask me to take on the role of Director of Graduate Studies for our section. I readily agreed. My own graduate experience had been so exhilarating and productive that I was eager to improve the experience of the graduate students. In addition, serving as Director of Graduate Studies exposed me to interesting research projects beyond the perimeter of my own field, and the regular gatherings of Directors of Graduate Studies from across the university broadened my community and my awareness of research across Yale's wide range of disciplines.

Even with my new administrative role, I was still planning for science to be the center of my intellectual world. I loved the lab. I even enjoyed writing grants: The intellectual discipline required to write a coherent, concise grant application is an excellent exercise for clarifying one's thoughts. My lab's research continued to be novel and exciting.

We used an antibody we produced, Cat-301, to explore how spinal motor neurons develop during the first weeks of postnatal life. We discovered that the antibody binds to a cell surface molecule (that we identified as a proteoglycan) whose expression requires normal patterns of neuronal activity during development (Zaremba et al., 1989; Kalb and Hockfield, 1988). We next determined that expression of the Cat-301 proteoglycan specifically requires two signals—one from the fastest nerve fibers, the large-diameter primary afferents, which carry information from the periphery (Kalb and Hockfield, 1990a), and a second from the brain. We showed that neuronal activity influences the expression of Cat-301 in parallel with the normal development of motor neurons (Kalb and Hockfield, 1994). We also discovered that the NMDA receptor, a glutamate receptor (now known to play an integral role in changing the strength of connections between neurons,

a process called synaptic plasticity), regulates Cat-301 expression, therefore identifying NMDA as a modulator of activity-dependent development (Kalb and Hockfield, 1990b).

Cat-301 also provided glimpses into the developmental plasticity of mammalian visual pathways, providing a molecular marker for one of the most fascinating features of the brain—that activity during key periods early in development can determine the mature properties of the brain (Hockfield and Sur, 1990; Hockfield et al., 1990). Activity-dependent development (such as the ease with which languages can be learned accent-free by children but rarely by adults) now had a molecular signature.

Overall, our explorations provided a unique, fascinating new view into how neurons develop and change, depending on how they are activated in early life (Kalb and Hockfield, 1992). The lab grew with many other projects and an expanding set of students and collaborators. Beyond brain development, we also explored an extracellular matrix protein in the CNS that is a molecular marker for—and potential drug target for—glioma, an aggressive brain cancer (Jaworski et al., 1996; Nutt et al., 2001; Viapiano et al., 2005).

As I reveled in my lab and my family, my career took another unexpected turn. In 1998, the dean of the Graduate School of Arts and Sciences stepped down, precipitating a search for the next dean. On the day the *Yale Daily News* announced that the search committee had presented its short list of top candidates to Yale's president, I ran into the colleague whom I expected to become dean.

"Are you on the short list?" I asked him, excited.

He looked at me askew. "How would I know?" he responded. "But I do know that *you're* on the list."

I was stunned. It had never occurred to me that I would be considered for the role. I didn't even want it: I was a scientist. Being a full-time administrator was the last thing I wanted. "No, I'm not a candidate," I said.

"Yes, you are," he countered.

An hour later, my phone rang. Regina Starolis, the president's able and wonderfully decorous secretary, was calling to ask if I was available to meet with Yale's president, Rick Levin, that afternoon. I assumed he wanted to ask me about the candidates and how they might engage with what I thought were the pressing issues for the Graduate School, so I happily complied. Much to my surprise, Rick said he'd received the list of candidates from the search committee, and he knew all of them personally except for me, and he thought we should meet.

I still believed that I did not have the remotest possibility of being selected: President Levin would never name an unknown neuroanatomist from the Medical School, with no previous experience in university governance, as the dean of the Graduate School. So, it was natural to respond to his questions frankly, and I shared my ideas of how to repair several of the

operations I had found not to be working optimally at the graduate school. He asked if I would be interested in the role of dean, and I said no. I may have even scoffed. It was not the direction in which I was headed.

I went home, however, and after a conversation with Tom, I reconsidered my response. I called Rick and apologized for being short-sighted. I said that if he thought I could help Yale as dean, I would welcome learning more about how he envisioned the role. We left it at that, and Tom, Elizabeth, and I went to Washington, D.C., for a spring-break vacation. While away, we had our first night's dinner at a Chinese restaurant. The dinner ended, of course, with a presentation of fortune cookies. I cracked open a cookie, peeled out the fortune, and read: *Your life is going to change.* Because Elizabeth was eager to eat there again, we returned to the same restaurant the next evening. That night, my fortune read: *Everyone agrees you're the best.* I have mounted those two fortunes in a frame that I have kept in my office.

I returned from our short trip in a different state of mind—I was now intrigued. Upon my return, Rick invited me to his office and offered me the role. I was an unexpected choice, and it was extraordinarily courageous of Rick to make that decision. He saw something in me that I didn't see in myself. But he had been dean of the Graduate School, and he knew what the role required.

I agreed to take on the role for three years, hoping to be able to fix some of the problems I had seen in the graduate school, and then I'd go back to the lab. Rick and I agreed that I would spend "most of most Fridays" in the lab with my research team. The lab, thankfully, was mature enough that they didn't need my daily input. So, in 1998, I became dean of the Yale University Graduate School of Arts and Sciences.

It was a fascinating, exhilarating, and challenging experience. My immediate goal was to make the graduate experience as fruitful and productive as possible for our students. Among the first changes we made included improvements for graduate students who engaged in teaching. Many graduate students served as teaching assistants as part of their education, yet few received any training in teaching. I hired Bill Rando to design and oversee a teacher education program, which became the Yale Teaching Center. I also hired two associate deans: Richard Sleight to oversee graduate programs in the sciences, mathematics, and engineering, and Pam Schirmeister to oversee the programs in the humanities and the social sciences. Both were wonderful educators and collaborators; they worked closely with departments and programs to improve the experience of graduate students and their faculty. Pam's daughter and Elizabeth were similar ages, and we became close friends.

With a terrific team in the Graduate School, and the support of the university leadership—President Rick Levin, Provost Alison Richard, Secretary Linda Lorimer, General Counsel Dorothy Robinson, and many

others—we designed more opportunities for graduate students to be involved and feel central to the university. I appointed Lisa Brandes as director of a beautifully redesigned social space in the Hall of Graduate Studies for graduate students, encouraging graduate students across the campus to gather, share their experiences, and learn from one another. The MacDougal Center was generously funded by Yale alumni donors, and it rapidly became a welcoming, active site for a wide range of social and cultural events to enhance graduate student life.

I also hired my first assistant, Diane Hovey, who promptly taught me how to have an executive assistant. The first time I walked into her office with some papers in my hand, she immediately demanded that I hand them over to her. When I resisted, she explained, "*I* do the filing, because later, *I'll* be the one who needs to do the finding." With that, Diane and I had a deal.

Slowly but surely, I built a team. I learned how to do so from Rick, who became one of my great life mentors. Rick was expert at gathering a set of colleagues, each with a different background and different responsibilities, and spurring them to work together toward a shared purpose. Once each week, the president, provost, dean of the college, and dean of the graduate school met for lunch. We'd each lay out the problems, issues, or concerns in our domains, and discuss them as a group. Those meetings were important not only to resolve the problems of the day but also to develop a sense of cohesion and common purpose among the university's leadership. Our discussions were frank and open, and we each contributed different perspectives and information from across the university.

One ongoing concern that consumed a significant portion of my time and attention as dean at Yale was a graduate student unionization drive. Long before I was dean, there had been unsuccessful efforts to unionize the Yale graduate students. At campuses of state universities, the ability to form a graduate student union was governed by state labor laws, and graduate students at some state universities had successfully formed unions. However, at private universities, unionization was governed by the National Labor Relations Act (NLRA), and in the late 1990s, the NLRA held that graduate students at private universities (like Yale) were not employees: They were students and could not form unions.

Yale was targeted to contest the legal standing of graduate students as employees under the NLRA. Yale's history of labor issues suggested that it could be vulnerable to a campaign to bring a case to the National Labor Relations Board to change the status of graduate students to that of employees with the possibility to unionize.

The university's core purpose is to foster human curiosity and its fulfillment and advancement, and to do so requires a fertile environment for the pursuit of learning and discovery. Those goals are not fostered in an environment of factions embattled against one another. Yet, instead

of finding joy in the pursuit of a common purpose to expand knowledge, many graduate students at Yale were clearly unhappy. It was a wake-up call. When I spoke with graduate students leading the unionization drive, they often described their relationships with their advisers and professors as contentious, not productive. And, in practical matters, the students had legitimate concerns, such as the lack of availability of health care for the families of graduate students.

To better understand the issues, I visited graduate students in their departmental programs across the university. Although each department had its own set of issues, some graduate student concerns were shared. I began to understand where the central administration could make a difference. Early in my service as dean, Rick and I modeled the cost of providing health insurance for the graduate student families and decided it was something we could and should do.

The unionization drive was a tense, difficult time, with passions running high. It was not uncommon for me, when walking on campus, to turn a corner and find myself confronted by several dozen angry students. I became more careful about when and where I walked on campus. I continued to do what I could to improve the graduate school experience. When a vote over unionization was eventually called, the union was not approved by the majority of graduate students.

During my service as Yale dean, we mounted a collaborative effort to strengthen a sense of community for graduate students. To underscore the integral role of graduate education in the Yale ethos, we initiated a lecture series called, "In the Company of Scholars." That name was chosen from a well-known and well-loved quotation by Yale historian George W. Pierson: "Yale is at once a tradition, a company of scholars, a society of friends." I embraced that characterization, and a plaque with Pierson's quotation hung prominently on the wall in my dean's office.

Once a month, I invited a Yale professor to give a lecture attended by graduate students and their faculty from any discipline. Each month, hundreds attended each lecture and the post-lecture reception. We also started a graduate school newsletter as a regular source of news and communication and formed a graduate student council to represent them on campus. Cumulatively, the efforts did, over time, change the tenor of the graduate student–university relationship.

When I took on the role of dean, Tom stepped up at home, handling many of Elizabeth's day-to-day needs on top of his busy neuro-oncology practice. At that time, it was unusual for a husband to commit to being the major at-home parent, but it was important to both of us. There were not many women in leadership positions then—or even now—and we offered an example that women could be leaders and have a family, too.

And, of course, I loved and treasured my time with Elizabeth. One of my favorite parts of the day was evening bedtime. We'd talk as she got

ready for bed, and I'd lie on her bed trying not to fall asleep. We'd read stories together until she was in fourth or fifth grade. She had become a very able, independent reader, and so our story time morphed into catch-up time, a reserved and treasured time to talk about the day and share our stories.

After four years as graduate school dean, Yale's Provost Alison Richard was recruited to Cambridge University, United Kingdom, as vice chancellor. Rick Levin invited me to follow Alison in the role as provost. By then, we had a higher-functioning, improved graduate school and programs, and students felt more empowered and better connected to the life of the university. My younger self's plan of going back to my lab research was no longer top of mind. The late 1990s and early 2000s were an important time for Yale and for higher education in general, and I found that I could again be helpful, finding ways for Yale to better serve our students, faculty, and staff. I had a strong team of associate provosts and was once again building a team and fostering a productive, innovative academic environment. I'd found my second calling.

Unbeknownst to me at the time, my tenure as provost would be short-lived. For years, beginning shortly after assuming the deanship, recruiters from other universities regularly called. I had no intention of leaving Yale—I loved working with Rick, I loved my team, and I loved the students. When I became provost and the cadence of the calls increased even further, I told my assistant that I was done with search consultants and would no longer take calls from search firms. I was not looking for another job. But that did not prevent another job from looking for me.

About a week before I got the call that would lure me away from New Haven, I was chatting with a Yale English professor. He was participating in a new "Program for Humanities in Medicine," a series of talks and discussions to explore medicine through the lens of the humanities. I asked how the program was going. He said he'd resigned from it because he was "tired of being beaten up about truth." I was astonished and—as a scientist and deep believer in "truth"—personally offended. I have long held the belief that an individual's purpose in this world is the pursuit of truth. The truth is what we seek; truth is at the core of why universities and academia exist. And, yet, here was a professor sickened by a discussion of the pursuit of truth. It was disheartening.

A week later, the phone in my office rang. I still don't know how someone named Jim Champy got my direct number, but he was calling as chair of the search committee for MIT's next president. As I said to others, I told him that I wasn't interested. But Jim replied, "Just let me tell you a bit about MIT. First of all, MIT is a place that believes in the pursuit of truth." I literally pulled the phone away from my ear and looked at it. Truth? How did he know I'd been stewing over that English professor's complaint? Jim went on to say that, in its pursuit of truth, MIT believed in meritocracy,

which had drifted from center stage for most of academia. When he asked if I would come to Boston to have a conversation with the search committee, I had to agree.

The meeting was held in secret at the Algonquin Club, a storied Boston intellectual gathering place on Boston's historic Commonwealth Avenue. I entered a room with a square table large enough to accommodate the two dozen members of the search committee. Many of them sat with their hands clasped on the tabletop, prominently displaying a "brass rat," the large and distinctive MIT school ring. I immediately relaxed, realizing I was at no risk of getting this job as I was not one of them. I was just there to hear what they had to say. I had not prepared a script, and I knew very little about MIT. Much to my surprise, what followed was a lively, insightful, forward-looking conversation about higher education.

From the car heading back to New Haven, I called Tom. He asked how the meeting went. "Unfortunately," I replied, "it went very well." Even so, I knew that if I were to actually pursue the position, I would start with three strikes against me: I was not from MIT; I was not an engineer; and I was a woman. MIT had never had a female president nor a biologist president.

My initial Boston visit was followed by additional conversations and visits. Elizabeth was in seventh grade at the time, and when we dropped her off at summer camp, her parting words to Tom and me were, "When I come home from camp, don't tell me we're moving to Boston." We kissed and hugged her, and sent her off to enjoy the summer.

When she arrived home in August, we told her we were moving to Boston.

Massachusetts Institute of Technology

Between the late August announcement of my selection as MIT's next president and my early December official start in the role, I shuttled back and forth between New Haven and Cambridge to wrap up my duties at Yale while starting to immerse myself in MIT.

Walking around MIT's urban campus was quite unlike Yale's leafy streets and Gothic architecture. In contrast, MIT's somewhat imposing central campus dates to 1916. Its "Main Group" of formal, statuesque limestone buildings surround a formal central courtyard, Killian Court. A short walk away, the more recently built campus includes the eye-catching Stata Center, designed by architect Frank Geary, with its unusual angles and colors. MIT has an intricate urban campus, with buildings known by their numbers and connected by underground tunnels. Once again, I found myself in a future I had not imagined, and it was exhilarating.

Following in the footsteps of Jim Watson and his evening strolls through labs to chat with staff, I've learned that my own "style" of

administration leans heavily on conversation. As I engaged with faculty, staff, and students in formally scheduled meetings or simply walking through the campus, I found something new, unexpected, or delightful in each conversation. I quickly learned that MIT people were my kind of people—explorers of the as-yet unknown who collaborated across disciplines with ease and insatiable curiosity.

After my unusual graduate school experience in which I was free to roam the NIH campus—an institution organized by purpose rather than discipline—and then cultivating intellectual convergence across disciplines at Yale, I no longer cared what department someone was in. I wanted, even needed, to gather people across disciplines. In many of my conversations, I recognized examples of convergence—often unanticipated or unexplored intersections between previously unrelated disciplines—as a powerful way to build a whole far greater than the sum of its parts.

On August 26, 2004, the day that MIT announced my presidency, I gave a short speech to an overflowing audience in MIT's central lecture hall, 10-250. Of course, knowing only a little about the Institute and its people, I could not yet know in what directions we might go. However, I had learned through my many conversations that MIT cultivates a commitment to service. In introducing myself to the MIT community, I tried to capture that ethos: "I want MIT to be the dream of every child who wants to make the world a better place." The packed audience, resonating with my message, rose to its feet in ovation; I was welcomed with that reassuring start. Many of my decisions as MIT's president would be driven by that idea—that MIT would be a place for people who wanted to change the world.

Shortly after the announcement of my appointment as MIT president, I received a phone call from a good friend who was then chair of Neurology at Massachusetts General Hospital. She offered the briefest of congratulations, and then burst out, "Can Tom come to work for us?" That solved an important problem, and Tom would continue his practice of neurology and neuro-oncology and teaching medical students in a flourishing career at Mass General and Harvard Medical School for the next 20 years. Elizabeth's transition was slightly harder. Moving schools midyear in seventh grade is a challenging time for any 13-year-old girl, but she handled it with grace. By May of that first spring, she surprised me one evening by thanking me for making us move, because her new school "was so much better than my other school." Whew—another problem solved!

As for myself, I decided not to move my lab, despite an offer from a colleague in MIT's Brain and Cognitive Sciences Department to help set one up. By then, it felt incompatible to be entirely devoted to university leadership *and* to have responsibility for a lab needing my attention. So, with some regret, but in appreciation of my central responsibilities, I shed my lab coat and focused entirely on university leadership.

Tom, Elizabeth, and I established our home in the historic Gray House, built in 1917 as the MIT president's house, and I moved into the president's office, located in one of the original Main Group buildings in the center of campus. One of my earliest decisions was hiring a spectacular executive assistant, Leslie Price, who had spent many years in that role for business leaders in Boston's entrepreneurial community. Leslie has stayed with me during my entire time at MIT, managing the office and navigating a perpetually complex and over-full calendar. In addition to her skills as a master of office logistics, her natural sociability makes her a wonderful connector of people. She presented a welcoming, helpful presence to visitors to the office, as well as establishing cordial channels to facilitate our work across the campus and beyond into the Boston business community.

Leslie and I do not look alike—at least to those who know us. However, the anomaly of having the first woman as MIT's president walking the hallways of the offices of the Institute's leaders, led to Leslie (another new face on campus) not uncommonly being greeted as me. Happily, as I became better known, and as the Institute's leadership included an increasing number of women walking our administrative corridors, the cases of mistaken identity became vanishingly rare.

I was formally inaugurated as the 16th president of MIT on May 6, 2005, wearing the cardinal and gray formal academic regalia of my new home. Entering Killian Court with Elizabeth and Tom at my side on a magnificently sunny day, I received a wonderful welcome from the MIT and greater Boston communities. One tradition in the inauguration festivities for college and university presidents is a welcome speech from a currently serving university president. I invited my former Yale colleague, Alison Richard, who was then serving as vice chancellor of Cambridge University (the equivalent of a US university president) to provide that traditional welcome speech. Alison opened with the observation, "Never again will anyone have to stand up here and welcome the first female president to MIT." The crowd applauded her clever statement. For some, I'm sure my appointment was a triumph, while for others it was an uncomfortable anomaly. In truth, I was less worried about being accepted as the first woman to serve as MIT's president and far more concerned with the MIT community's reception of the first biologist to serve as their president.

Seventy years before me, MIT President Karl Compton had insisted that the physical sciences must be squarely required within the MIT curriculum. And here, in 2005, I focused my inaugural address on cultivating the convergence of engineering and the life sciences. While most of the world recognized MIT for its engineering prowess, the MIT Department of Biology would have been a crowning jewel at any other university. That department boasts numerous Nobel Laureates, including Phillip Sharp, David Baltimore, and Salvador Luria. Notable faculty and alumni include eminent, paradigm-shifting biologists such as Susan Lindquist, Victor

Ambrose, Cynthia Kenyon, Allan Spradling, and many, many more. With that powerful foundation in mind, I told the assembled community at the inauguration that my generation of leadership must design new ways to blend research across the life and the physical sciences and engineering, as I had heard that theme frequently in my conversations across the campus.

Getting to know the MIT community, I quickly adopted as a conversation starter, "What do you love about MIT that you wish we did more of?" Two main themes emerged. The faculty cared about finding solutions to the world's growing energy needs and about the opportunities emerging from a convergence of biology with the physical sciences and engineering.

However, MIT had a serious challenge that constrained how aggressively we might take on these two important opportunities: MIT was carrying a large—and growing—financial deficit. When I assumed office, MIT had an annual operating deficit of more than $100 million. My first challenge, and one that would take a multiyear effort to resolve, was to design and stick to a budget to close that gap. As I had learned from Rick Levin, I immediately gathered a group of faculty members to help design a plan. By making large but targeted reductions in the budget, by 2008, our general institute budget was brought into balance for the first time in a decade or more.

And it was not a moment too soon, because in the fall of 2008 the global financial crisis hit. MIT's endowment, which provides more than 20 percent of the Institute's operating budget, dropped 20 percent in a year. As a result, we ultimately cut even more expenses than we had originally planned: roughly $130 million over two years. Again, the strategy was developed through conversations across the campus, including visits by me and Institute leaders to departments, schools, and programs, all reporting to a central team to carefully deliberate the cuts.

Instead of asking the central team what we *should* cut, I first asked what we should *never* cut. We assembled a list with a hierarchy of priorities. Two top priorities rapidly emerged, activities at the very core of MIT which—as long as we could manage—we would *not* cut: undergraduate financial aid and faculty recruitment. First, I refused to lose, even for one or two years, students who were highly qualified to come to MIT but needed resources to make it happen. Second, MIT did not stop hiring faculty, but continued to recruit through economical challenging years, allowing us to hire top faculty with very little competition.

Those two priorities demonstrate how MIT, at its core and in my heart, was always about the people. If you have beautiful buildings bursting with shining resources, but don't have the right people, nothing important will happen. I still get emotional about it.

From early in my introduction to MIT, I structured several ways to get to know faculty and to foster interactions across departments and schools. One important effort was the convening power of the president's house,

Gray House, designed to accommodate large gatherings. We hosted many such events—often several in a week—to foster cross-department, cross-school interactions. These included an annual event for women faculty and another for minority faculty that gave them a chance to meet faculty from across the Institute, providing an important sense of community beyond an individual's department or program.

I remember the first night I hosted MIT's women faculty. A magnificent circular staircase connects the downstairs coat room to a large first-floor reception area in Gray House. When I arrived to welcome the faculty, I stepped to the top of the stairway, stood at the railing, and gazed down at a stairwell full of brilliant, famous MIT faculty—all women. I'm rarely overcome by emotion, but my eyes filled with tears. The force of their presence, knowing their professional accomplishments, and the possibility of what that force would bring to the world, was overwhelming. Every year it was a wonderful night, and it helped create a community that identifies and addresses shared challenges.

Although addressing the financial deficit was one of the first challenges during my tenure, it certainly was not the last. Some were difficult, but minor (often involving a single faculty member), while others were more global. Many could not be resolved through standard MIT channels. However, when it came to navigating and resolving challenges and crises, I did have a secret weapon—my "three wise men": In addition to Yale's President Rick Levin, the trio included Stanford's President Gerhardt Caspar, and former Brown President Vartan Gregorian.

The amount of experience among those three, and an open invitation from each to share their insight with me, was invaluable during my MIT presidency. When things headed in a difficult direction (e.g., when the wheels came off the bus), I called each of them. Their insight and observations about how they had addressed similar difficult situations often had not occurred to me. With their help, I could view a dilemma from many sides and formulate the best possible strategy. In retrospect, one of my greatest regrets is that I did not call on them more regularly. Mentors are a source of insight and encouragement that make both the bad and the good times better. Everyone should have a small posse of trusted mentors.

One of the good times was the formation of the MIT Energy Initiative, or MITEI. When I asked MIT faculty about their top three concerns or areas of interest, virtually everyone I spoke with urgently mentioned the desire to contribute to solving the energy crisis. Oil and gas prices had been climbing with no end in sight, and global warming as a result of fossil fuel use was a growing concern.

Across MIT, scientists and engineers were already pursuing ambitious alternatives to oil and gas, and economists and political scientists were designing policy interventions. From the seeds of those efforts sprang MITEI, an interdisciplinary, Institute-wide effort to advance energy innovation for a

clean energy future and to promote an energy transition. Among areas of interest were strategies to accelerate the use of solar power and critically important technologies for energy storage.

I had the great good fortune to recruit two brilliant leaders for MITEI: Ernie Moniz from the Physics and Engineering Systems Departments and Bob Armstrong from the Chemical Engineering Department. Together, Ernie and Bob compiled an inventory of ongoing energy-related work at MIT, planned a series of high-level colloquia on campus, and built an industrial liaison program to bring industry perspectives and partners into the initiative. It was clear that the energy crisis would not be solved by academia alone but required close collaboration with industry, and we recognized the critical importance of building those partnerships from the start. Bob was our brilliant interlocuter for industrial partnerships, while Ernie, who later went on to become the US secretary of energy in the Obama administration, cultivated government relationships. They were a dream team and, through their efforts, MITEI was enormously successful. MITEI and its industry collaborators supported hundreds of research projects and produced comprehensive analyses for policy makers and regulators, including our "The Future of . . ." report series that examined the routes by which a variety of energy sources could meet future energy demands. MITEI also supported energy education, creating programs in the classroom, field, and online for students to conduct energy research in diverse fields, including a new MIT minor degree in Energy Studies (that could be attached to any MIT major) and a graduate Society of Energy Fellows. Over time, more than 25 percent of MIT faculty members participated in the initiative—and I can tell you from experience that it's vanishingly rare that 25 percent of the faculty agree on anything.

As part of MITEI and other activities, I frequently found myself in Washington, D.C. Bill Bonvillian, astute director of MIT's Washington, D.C., office, served as my guide, briefing me before meetings about our goals and intentions. Visits with politicians and committees gave me an appreciation of the power of individual voices in helping to direct and influence the direction of leaders in Washington. In fact, in 2009, when President Barack Obama decided to give a major address on US energy policy, he chose to give that address at MIT, where I gave him the first tour of an MIT laboratory a sitting US president had ever received. I'll admit, it was a highlight of that year.

Another highlight was the wonderful, dramatic acceleration of our activities at the convergence of biology and engineering—the one I had anticipated in my inaugural address. When I spoke with biologists at MIT, many of them had ongoing projects with engineers or were developing new technologies with physicists. There was clearly a shared ambition to cross disciplines to reach a deeper understanding of biological mechanisms and to turn those biological insights into new technologies to improve lives.

By the early 2000s, molecular biology had given us tools to do that sort of technical work in the biological sciences. With new molecular tools, we could apply engineering methods and techniques on the scale of cells and proteins. The opportunities at that domain of convergence were huge. And among those collaborations between biologists and engineers, one area of focus kept turning up repeatedly—cancer.

Since the founding of the MIT Center for Cancer Research in 1974, MIT had contributed important discoveries that increased our understanding of the biology of cancer, revealing biological pathways that could be pharmacologically blocked. More recently, a grant program tested out these types of cross-cutting collaborations, but I aspired for something bigger and broader. We started by promoting collaborations across departments and institutions, but we quickly realized that we could better foster these new collaborations by gathering collaborators into the same physical space—an idea that may sound commonplace but was unprecedented at the time.

MIT had a wonderfully engaged and enthusiastic donor in philanthropist and devoted MIT alum, David Koch, a longtime leader at Koch Industries. David and his family had dealt with cancer, and he was committed to helping to find new approaches. He had always been generous to MIT as well as other world-class institutions and organizations. Perhaps unsurprisingly, as an MIT-educated chemical engineer, he found compelling our proposal to bring engineering and biology together to battle cancer.

David's enthusiasm raised my confidence that MIT could further amplify its ability to make a meaningful difference in cancer. On October 9, 2007, we announced the launch of a major new initiative in cancer research supported by David Koch's $100 million gift. The initiative included a new building that would physically bring together cancer biologists with chemists, engineers, computer scientists, and more. The David H. Koch Institute for Integrative Cancer Research was completed and dedicated in 2011; filled with cutting-edge technology enabled by David's further gift of $60 million. I cherish the photograph of David with almost a dozen Institute leaders holding shovels, ceremonially "breaking ground" for MIT's new approach to our fight against cancer. With programs that included collaborations with oncology colleagues across the river at the Massachusetts General Hospital, the Dana Farber Cancer Center, and Brigham and Women's Hospital, MIT had a unique opportunity to invent a new approach to cancer research.

Originally, the Koch Institute was designed with biologists and engineers on alternating floors. As our faculty members came and went over time, even those distinctions blurred, until all the floors became fully mixed across disciplines. Together, faculty that were once labeled by their different home departments are now simply considered part of the Koch Institute. And they have made fantastic advances, such as a Food and Drug Administration (FDA)–approved onco-fluorescent agent and handheld

imaging tool to improve surgical outcomes in breast cancer, now approved by the FDA, and related strategies to localize and prevent ovarian cancer; novel gene-editing tools and organoids for tumor modeling; machine learning tools to accurately assess risk of breast and other cancers; novel immunotherapy vaccines; and many more.

At some point after the Koch Institute was in full swing, we invited David to join us for an afternoon of presentations from graduate students and postdocs. One at a time, students stood up and described the motivation for their work at the Institute. Many had a story of a family member or friend with cancer. Most shared a profound intellectual curiosity to fix things. Both David and I were teary-eyed. Here were the young people who were going to change the world.

MITEI and the Koch Institute embodied the best of what MIT could be. MIT is a university that is uniquely strong in both the physical and life sciences, and we now had the physical layout, enthusiasm, and sense of freedom for people to cohabitate beyond physical or academic boundaries.

Yet even over the decades while MIT was bustling and thriving inside, its exterior was rough around the edges. Early in my presidency, one morning I drove to the Institute and pulled into an overgrown, dirt parking lot surrounded by a collapsing chain-link fence. When I arrived, unsettled, at the office, I asked my staff, "Who owns that terrible lot?" A staff member replied, with a bit of embarrassment, "We do." I asked why it was in such terrible condition. She said that when something breaks at MIT, someone runs out and wraps some yellow police tape around it, and there it would sit. MIT did not have the discipline—structurally or financially—to repair the campus. I requested that the lot be paved and the fence repaired. After that, I became vigilant in noticing yellow tape "decorations" all over campus.

The original MIT campus was designed and constructed on the newly land-filled bank of the Charles River in 1916. When I arrived as MIT's president, our buildings and grounds were showing the wear and tear of age. Buildings inside and out were in tremendous need for updating. But that was easier said than done, especially with the annual fiscal deficit we were already committed to addressing.

Here, again, my lessons learned at Yale helped. Like MIT, Yale's campus had long been in need of renovation, but the cost of such a project was more than daunting. Yale's president, Rick Levin, made the brilliant and helpful observation that the "physical debt" of the campus had been accumulated over many generations, so it would be misguided to think a single generation should bear the cost of revitalizing 100 years of deferred maintenance. Instead, he advocated and instituted an economic model to carry out those renovations over an extended length of time, along with a plan for maintaining the campus going forward, so that future generations would not be burdened with the same kind of debilitating fiscal and physical debt.

In 2006, it was time for MIT to adopt a similar campus development program, and I knew just the person for the job. I called Terry Stone, an MIT Sloan alumna and member of the MIT Corporation with a keen analytical mind paired with a convivial personality. Terry was planning her retirement, but I knew that she would make an outstanding executive vice president and treasurer for MIT. With the encouragement of several members of the MIT Corporation's Executive Committee (on which she served), I called Terry and boldly asked if she would like to come work with me at MIT. She replied, "No, I'm retiring. I don't need another job." I said I would call her back in a couple months.

Several calls and conversations later, Terry agreed to come onboard to oversee MIT's finances and operations, and thank goodness she did. With her leadership, the "MIT 2030" project swung into action: a long-term plan to repair and maintain MIT's physical resources. It involved the fiscal planning that would require three-quarters of a billion dollars, the construction of three major academic buildings—including a new home for the MIT Sloan School of Management—housing for 550 graduate students, renovations in the School of Science and the School of Engineering, and new parking facilities and infrastructure.

In addition to improving our campus, we also fostered commercial and community development in Kendall Square. This effort also began with a personal anecdote: Shortly after we moved to Cambridge, Tom stepped out of the President's House (only a couple of blocks from Kendall Square) one Saturday morning to get a haircut. He walked from our residence into Kendall Square and found the neighborhood almost deserted: no barber shops, no restaurants, no people on the streets, nothing. Based on my experience in New Haven and Yale, I recognized that a university and its neighborhood must live not only in harmony but also must bolster and support each other. MIT and Kendall Square needed to coexist and grow together for the area to become an appealing place to study, work, and live. The future of each depended on the other.

I asked the MIT Investment Management Corporation, which oversees MIT's real estate developments, for a plan to help develop Kendall Square. By working closely with industry partners, over time Kendall Square has transformed from a semi-industrial, somewhat dangerous area with acres of dirt parking lots into a gleaming, bustling hub of innovation with more IT and biotech firms per square mile than anywhere on the planet, including dozens of MIT start-ups alongside global giants like Google, Pfizer, Biogen, Novartis, and Microsoft.

As we helped develop Kendall Square, we also developed the livability of our campus. Through an ambitious, "Campaign for Students," from 2006 to 2011, we attracted more than $575 million to increase funding for scholarships, fellowships, and student life. By expanding the number of undergraduate dormitories, and reopening and renovating dining halls,

we increased the number of young scholars we could welcome by almost 10 percent. As we renovated campus and helped develop our neighborhood, our students, faculty, and staff enjoyed improved work and study spaces, safer streets, and a stronger sense of a campus community.

Post-presidency

After eight years at MIT's helm, I decided to step down. Following the Institute's commitment to the major initiatives of my administration, it was ready to move on to a new set of priorities, and I recognized that it was the right time for a new leader to step into the role.

We had accomplished so much during my presidency, including $3 billion in fundraising and the launch of MITEI and the Koch Institute for Integrative Cancer Research, two cross-disciplinary programs addressing critically important technical and societal issues that are still going strong today. We also improved resources for student life and learning; we renovated and expanded housing; and we improved dining and socializing facilities. With a view of the long term, we established a financial structure with greater fiscal discipline, expanded the Institute's global educational and industry connections, and made great progress on a comprehensive infrastructure framework for the future of campus and Kendall Square.

I announced my resignation on February 16, 2012. In July that year, I passed the mantle to my successor, L. Rafael Reif, who had served as MIT's provost since 2005. Learning of my plan to leave the MIT presidency, Graham Allison, director of the renowned Belfer Center for Science and International Affairs at the Harvard Kennedy School, invited me to spend the next academic year at their campus. The Belfer Center generously welcomed Leslie and me into their midst, providing office space and camaraderie in an intellectually stimulating environment. Discussions at the Belfer Center shifted my attention from affairs of academia and MIT to those of the political and economic currents in the nation and the world.

Thanks to the generous intercession of Vartan Gregorian, president of the Carnegie Corporation of New York (where I had served as a trustee), I received an invitation to spend a month during the spring of 2013 at the American Academy in Rome, accompanied by Tom. The American Academy is a magnet for scholars from around the world, with interests ranging across the centuries. The scholars in residence and visitors at the Academy met together for lunch before heading off in our different directions—to study, to tour, to explore libraries and archives—all amidst Rome's magnificent historical artifacts.

Here again, like the Belfer Center, conversations ranged widely across the world's history and geography. In addition to the intellectual delights of the Academy, we had Rome at our doorstep. Tom and I walked the city, enjoying the fruits of our self-imposed discipline to explore only a single

destination each day (along with anything we might encounter en route). The luxury of a daily singular focus, rather than our usual tourists' race to visit as many sites as possible, provided a more rewarding experience than our previous trips to Italy. At the Academy, we enjoyed participating in conversations and seminars with the other scholars.

We returned to Cambridge relaxed and happy. Tom was thriving in his medical practice at Mass General Hospital and also teaching at Harvard Medical School. Elizabeth was enjoying college life and her studies at Harvard, where she had decided on medicine as her future direction. Following her Harvard undergraduate studies, she continued in the academy as a Rhodes Scholar at the University of Oxford. She returned to Boston, to Harvard Medical School, following in her father's footsteps. She has the best traits of both of us, including my insatiable curiosity to understand how things work and Tom's compassion and commitment to helping those in need.

In 2014, as I was concluding my visit at the Harvard Kennedy School, Graham invited me to present the Godkin Lecture, a major speech that had been delivered annually at the school since 1903. I spoke on my perpetual passion for opportunities made possible by new and nonstandard collaborations between usually separate disciplines. More specifically, I described the coming technologies arising from the convergence of biology with engineering and the physical sciences.

I described how recent scientific revolutions in biology—the advent of molecular biology and genomics—had identified what I've called the "parts list" of biology: DNA, RNA, and proteins. Engineers, always ready to invent new technologies, are drawn to parts lists. The biology "parts list" has enabled a convergence of biology with engineering, in which biological "parts" can be assembled into new technologies.

In my Godkin Lecture, I described this new era of biology and provided examples of how the convergence of disciplines will transform our lives, from health care to clean energy and food production. I advocated for our country to establish a national movement to support innovation and advance these newly emerging technologies. "A future we have not yet imagined is upon us, and the possibilities of convergence are truly beyond our imaginations," I said.

After the lecture, Graham tracked me down, accosting me in a manner that reminded me of the life-changing conversation I had with my biology professor at the University of Rochester. Graham directed me, "You *must* write a book." I laughed and told Graham that I had no idea how to even start that process. He instantly replied, "First, you'll need an editor. I'll introduce you to mine." And he did.

As a graduate student, Steve Gobel had taught me the responsibility of writing up one's findings to share my work with the scientific community. Now, Graham was encouraging me to share my ideas about the future of

science and technology with an even broader community in a new format—a book not for scientists themselves, but for a scientifically curious, lay audience.

With Graham's help and encouragement, I spent most of the next year writing the text for what would become *The Age of Living Machines*: *How Biology Will Build the Next Technology Revolution* (2019). Writing a book was a new endeavor for me, and I had to develop a writing schedule to develop the content and purpose, and to put the ideas behind the book onto paper. I spent many days walking the Cape Cod shoreline, marinating ideas, and doing my best to follow the writing schedule I had set up for myself.

When the book was published, I had the privilege of sharing it on a book tour. If there was a single strong piece of evidence that my shy, younger self had fully grown into an eager conversationalist, this was it. It was tremendously fun to describe the book's central ideas and examples of anticipated devices to store energy, diagnose disease, and purify water, along with more general science and technology issues. The book was well-received and precipitated numerous lecture invitations, interviews, and more. Most surprising and most humbling was receiving the American Institute of Physics' 2020 Science Communication Award, citing my breadth of experience and optimism as key elements in "speaking against anti-science and reaching the public."

After my year at the Belfer Center, I continued as a tenured professor of neuroscience at MIT, with an office at the Koch Institute. I have loved being at the Koch Institute, in the heart of collaboration, where I can observe and participate in the power of bringing biology and engineering together to fight cancer. It also provides ready conversation with top-caliber scientists and engineers—and their students—who are inventing new approaches to prevent, detect, and cure cancer.

At a regional biotechnology awards ceremony in 2018, I had a conversation with Sangeeta Bhatia and Nancy Hopkins, two of my MIT faculty colleagues. Sangeeta is an inventor, a serial entrepreneur, and one of the key biomedical engineers at the Koch Institute. Nancy is an MIT molecular biologist of the generation just before mine, who wrote a landmark report documenting widespread discrimination against female faculty at MIT. That report, released to MIT faculty in 1999, rapidly spread across the country, motivating Yale and every top university to reexamine its appointment and promotion practices.

At the awards event, the three of us shared our concerns about how few female professors founded biotech companies. Sangeeta had just taken a sabbatical to focus on a start-up company, and she had noted that virtually all the investors to whom she pitched her company were men; men held the keys to the capital she needed. We pointed out to each other how few women attended the awards ceremony and how few women received the biotechnology awards. But vague impressions are not data, so the three

of us decided to gather real data to get a better picture of MIT women faculty's participation in biotech.

With support from the Sloan Foundation, we enlisted experts to gather data about the participation of MIT women faculty in start-ups. Our study focused on only seven of the fourteen departments in MIT's Schools of Science and Engineering, but even so, it arrived at a remarkable conclusion: Had the women in those departments participated in founding companies at the same rate as the men, they could have started roughly 40 additional companies to help predict, prevent, and treat disease.

I saw that statistic and immediately thought, "Those are squandered assets." Women, who are as academically qualified as their male counterparts, aren't starting companies that could advance innovations in health care, energy, and an array of technologies to improve lives sustainably. We needed to do something about it.

In 2021, we cofounded the MIT Future Founders Initiative to increase the number of female faculty members who start companies. The initiative kicked off with a series of bimonthly talks from academics who had successfully started companies. We developed a series of workshops on launching a start-up, drawing in regional leaders in biotech to guide our faculty. We saw a need for, and set out to build, the infrastructure for women to connect and network, so that when two friends are having lunch and one describes great research results, the other can say, "You should start a company. I'll introduce you to my funders."

We continued to select a new group of faculty every two years, providing mentorship, education, and connectivity. In our first year, eight of the nine women selected went on to develop their companies. In its first few years, 21 faculty members participated, leading to 14 start-ups that raised more than $40 million in seed funding.

In addition to the MIT Future Founders Initiative, the late David Schmittlein, then the dean of MIT Sloan School, invited me to co-instruct an MIT Sloan School course, "Corporations at the Crossroads." The course calls on a remarkable roster of company CEOs to participate in the class. Through student questions and CEO reflections, it focuses on the role of CEOs and their path to executive leadership. I got to know many industry leaders, MIT Sloan students, and professors through the class; the learning and connection never stops. The class became among the top-ranked classes at MIT Sloan.

During more than a decade since stepping down as president, I have also enjoyed making myself accessible to undergraduate and graduate students, who often come to chat in my office. Carrying on the tradition of the mentors who once helped me, I've helped and encouraged students in any way I can. Every student should feel agency in their education, where coursework and lab hierarchies are not constraints but opportunities for growth and seeking insight.

Conclusion

It has been a uniquely challenging task to look back through my career by writing this chapter. From my early days of taking apart vacuum cleaners and hydraulic door closers, to mapping the structure and revealing the function of molecularly defined pathways in the brain, to traversing the halls of some of America's oldest and grandest institutions, I have been blessed by a career that has been as unpredictable as it has been fulfilling.

It has been an honor to have a hand on the steering wheel of extraordinary universities, places full of amazing people with world-changing ideas. The role of the university is to create possibility, not constrain it. It's a place to discover things outside oneself and inside oneself. Few things are more exciting in life than being part of that world of discovery and opportunity.

I have had the great good fortune to have learned from exceptional mentors, students, colleagues, and collaborators, and I am thankful to every one of them. Most important, I can never sufficiently thank my husband Tom and our daughter Elizabeth for joining me on this wild ride with enthusiasm and encouragement.

Today, when I walk into a lab or stride down the halls of the Koch Institute, I feel profoundly the joy and gratitude of a life that unexpectedly found me. My life and career have been a product of convergence—of the people, ideas, and events that made me who I am, more than the sum of my parts. For each part, I am grateful. For the whole grand thing, well, just thinking about it makes me smile. I have the deepest thanks and gratitude for all my companions that made it possible, my "company of scholars and society of friends."

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