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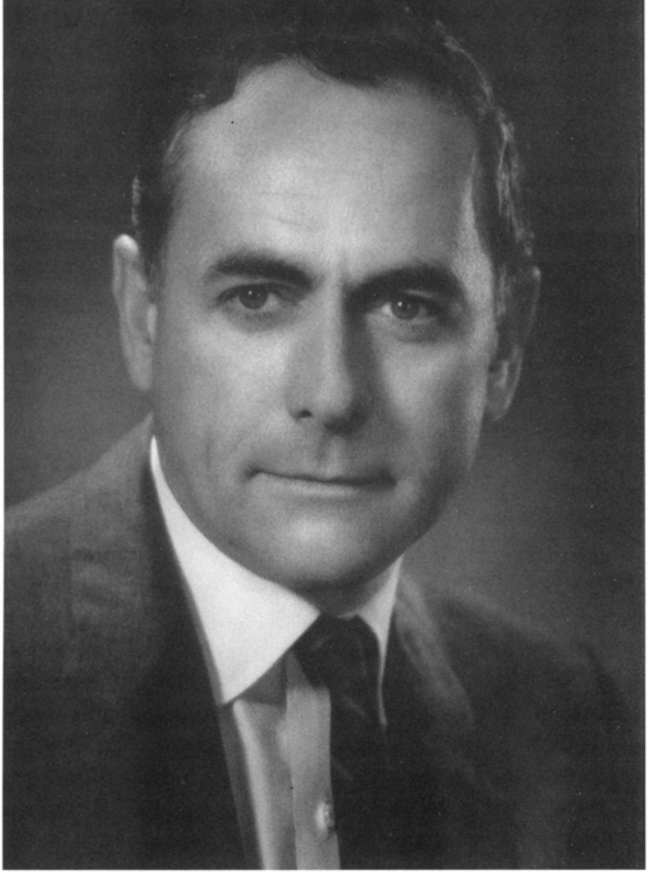

Carlton C. Hunt

BORN:

Waterbury, Connecticut
August 11, 1918

EDUCATION:

Columbia University, B.S. (1939)
Cornell University Medical College, M.D. (1942)

APPOINTMENTS:

National Research Council Fellow, Johns Hopkins School of Medicine (1946)
Associate, Rockefeller Institute (1952)
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Head, Department of Physiology, Washington University School of Medicine (1968)
Visiting Professor, Laboratory of Neurophysiology, College de France (1983)
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HONORS AND AWARDS (SELECTED):

Distinguished Alumni Award, Cornell University Medical College (1996)
Distinguished Service Award, Association of Chairmen of Physiology (1997)

Carlton Hunt began his scientific career in close collaboration with Stephen Kuffler at the Wilmer Eye Institute. He became a leading expert on the muscle spindle, a sense organ that detects changes in muscle length and movement. Working with Kuffler, he showed how the central nervous system regulates the intricate behavior of these sense organs. In later work, he elucidated the spindle's role in sensing the position of limbs in space. A gifted administrator, he also built three excellent departments of physiology at three different universities, all prior to the emergence of neuroscience as an interdisciplinary activity.

Carlton C. Hunt

At the age of 87, I can now look back over more than 50 years since beginning my career as a neuroscientist. Indeed, I entered the field well before it was called "neuroscience." Although several medical and scientific disciplines bore the preface "neuro" in my student days (such as neurology, neuroanatomy, and neurophysiology), the idea of a field encompassing all the complex aspects of the nervous system had yet to evolve.

Chance and timing played major roles in my career. They led to my collaboration with an inspiring mentor who shaped the entire course of my professional life. In 1947, I received a fellowship in neurology from the National Research Council to go to Johns Hopkins. Modeled on the Welch Fellowship in internal medicine, this award provided support for 3 years. My original intention was to do research with A.M. Harvey, who then headed the Department of Medicine. However, Harvey suggested that I spend my first year working with Stephen Kuffler.

Harvey had met him in Sydney during the war when Steve was working with Jack Eccles at the Kanematsu Institute of Pathology. After the 1938 German invasion of Austria, Steve had escaped to England and then moved on to Australia. A recent medical school graduate, he then had no research experience and found laboratory work difficult. Several members of Eccles' group, including Eccles himself, worried that Steve might not succeed in science. Then in 1939, Bernard Katz, a fellow refugee, joined the research group. When Katz found that Steve was, as he put it, "somewhat bewildered" by the complicated terminology and interpretation of electric tracings, he taught Steve what he needed to know for experimental work. An apt pupil, Steve soon mastered the skills necessary to do first-rate research. As Katz later recalled: "Once he had started on his single nerve-muscle fiber preparation, Eccles and I felt he had clearly outrun us."

At the end of the war, Steve left Australia for America where he first worked in Chicago with Ralph Gerard before coming to Johns Hopkins. Although only 5 years older than I, Steve was already an experienced and skillful investigator. His laboratory was located in the basement of the Wilmer Eye Institute. We met, had a good talk, and agreed to collaborate. Thus began 4 wonderful years of experimental work during which Steve introduced me to the challenge and joy of research. Despite differences in our backgrounds, we developed an extraordinarily productive, harmonious, and close relationship. Writing papers together was a collaborative effort. We read our respective drafts back and forth to each other until a consensus was reached. Steve had small hands and remarkable dexterity. His dissections were elegant and swift. He had a light touch in every sense of the word. Steve asked penetrating questions and gave advice that was never heavy handed. In addition, he had a rare talent for designing the experiments most likely to provide clear answers to difficult question.

Steve taught me how to think about biological problems, and there was much laughter between us. A complex person with a subtle sense of humor and an aversion to pomposity, Steve became a lifelong friend. As his scientific reputation grew, he often joked about the distinguished institutions with which he was associated. When Steve was elected a non-resident fellow at the Salk Institute, I asked him what he did during his visits there. His answer came without hesitation: "I sit and salk." I have always been grateful to Mac Harvey for giving me the chance to work with the person who inspired my entire research career.

Early Years

Nothing in my background or upbringing suggested that I would become a scientist. My parents divorced when I was only a few years old. My younger sister died of diphtheria at the age of 3. Her premature death and my mother's profound grief left a deep impression on me. I grew up as the only child of a working mother who was devoted to me. My father appeared only on annual visits. His family's wealth, which was largely dissipated before I was born, had come from investments in the Erie Canal. As a result, several generations of my paternal ancestors had found it unnecessary to work.

Having a mother with a full-time job, no siblings, and an absent father meant that my childhood was rather isolated. But friends and school work kept me occupied, and I learned to be independent at an early age. My faithful companion was a beautiful German shepherd dog who followed me when I rode to school on my bicycle and waited for me at the school's front door until classes were dismissed. Friendship with a local physician proved important to my future. When I was still just a young boy, Dr. Wheaton took me with him on house calls and gave me access to his medical library. These experiences stimulated my interest in medicine.

Education and Training

I graduated from high school in 1935 at the age of 16 during the depths of the Depression. At the time, my mother's financial resources were limited, and my father was unwilling to pay for my further education. When I discussed my hopes for college with my father, his reply was unusual for a Yale graduate: "I don't know why you'd want to do that; it never did me much good." Although he was later proud of my accomplishments, my father certainly never pressured me to go into medicine or academe.

Despite my youth, Columbia University offered me a scholarship. To minimize living expenses, my mother and I moved from New Jersey to New York City. A program called "professional options" allowed me to apply to medical school after only 3 years in college. Continuing to live at home was an economic necessity. On graduating, I applied to Cornell University Medical College and the College of Physicians and Surgeons at Columbia. Although my grades were undistinguished, I was immediately accepted at Cornell whereas Columbia insisted that I complete a fourth year of college. Impatient to begin my medical studies, I chose Cornell.

My paternal grandfather (whose own formal education had ended with expulsion from a private secondary school for adolescent misbehavior) agreed to pay the then relatively modest tuition. Cornell University Medical College turned out to be a marvelous place for me. The teaching was excellent. As a result, I became a serious and successful student. Although grades were registered, unless our marks fell below an acceptable level we never knew what they were. There was little overt competition among classmates. Preclinical subjects dominated the first 2 years of study. Basic science fascinated me, and I found the physiology course taught by Joseph Hinsey especially interesting. A forceful and engaging person, he had done important work on the muscle spindle, a sense organ that would later become my major research interest.

Laboratory experiments were an integral part of the physiology course. They were by no means easy or trivial and often lasted well into the evenings. To this day, I can still recall many of them. Although I had been a mediocre student at Columbia, my grades at Cornell were good enough for me to win election to AOA, the medical honor society.

Clinical Training

Although I remained interested in basic science, clinical work became my primary commitment. Internal medicine was especially appealing to me. When I graduated from medical school in June of 1942, I decided to seek an internship in that specialty. David Barr, the head of medicine at Cornell, invited me to join his housestaff. He was an inspiring teacher and clinician, and I enjoyed my first year of training. Interns and residents had a great deal of responsibility for patient care on the wards of The New York Hospital. We worked long, hard hours and did all the laboratory tests ourselves.

Although this experience was satisfying in some ways, from a therapeutic perspective it was frustrating. We could seldom treat life-threatening illnesses effectively. Nonetheless, there were superb clinicians among the faculty who made accurate diagnoses with the limited tools and simple tests at their disposal.

Army Service 1942–1946

Six months after Pearl Harbor, I received my medical degree and became an intern in internal medicine at The New York Hospital. When this internship year ended, David Barr urged me to continue my residency. But I felt the need to participate in the war and so decided to enlist in the army. I joined the 56th General Hospital whose staff was comprised of young doctors from Johns Hopkins and Cornell—some of whom I had known from medical school.

Our group was shipped to Britain as a general hospital. Although there was only one psychiatrist on the staff, the unit was abruptly designated a psychiatric facility. We were all assigned to treat soldiers whose mental illnesses had made them unfit for military service. Our job was to diagnose their psychiatric disorders and to keep these patients hospitalized until the necessary arrangements had been made to ship them home. Despite their serious mental problems, patients were sometimes expected to line up for inspections and stand at attention by the foot of their beds. Making them comply was not an easy task. This bizarre procedure made me wonder who was crazier—the patients or our commanding officers.

Our next assignment was to run a general hospital near Bristol. This period of useful work ended in May of 1944 when we were sent to a town near Liverpool just before the invasion of Normandy. Weeks of enforced idleness—broken by recurrent marching exercises—followed. After landing on Omaha Beach 2 weeks after D-Day, we moved inland and set up a tent hospital in pastures among the few surviving cows. In addition to American soldiers, our patients included German prisoners and French civilians. My work was to triage patients so that they would receive appropriate care as soon as possible. During this time, I became a patient myself due to a disease often fatal then: meningococcal septicemia. Early one morning, I awoke with severe chills and a high fever. Fortunately, I responded to sulfadiazine treatment and made a prompt recovery.

Three months later, we were transferred to Liege, Belgium, to staff a busy general hospital. After VE Day, our unit moved to Rouen in Normandy where we awaited transfer to the Pacific theater. Luckily for us, hostilities ended with the atomic bombings of Hiroshima and Nagasaki. Horrible though these events were, I welcomed the end of the war. Army life, with its mixture of frantic activity and excruciating boredom, taught me the value of being able to control my own time. I was very grateful to return to civilian life.

Starting Out in Research 1949–1952

On returning to my residency at The New York Hospital after the war, I felt severed from my academic roots and decided to seek more experience in the basic sciences. After finishing a year as assistant resident, my friend and medical school classmate Walter Riker helped me get a fellowship in the pharmacology department. Although I enjoyed this experience, I was still attracted to clinical medicine. After seeing a patient with partial muscle paralysis, I became interested in diseases affecting neuromuscular transmission such as myasthenia gravis. At this point, I decided to apply for the National Research Council fellowship that would lead to my collaboration with Steve Kuffler.

Our work together began, with three of us in his basement laboratory: me, Steve, and Bob Bosler, a skilled technician who vastly improved our primitive electronic equipment. At the outset, our setup was barely adequate, and we often borrowed a 35-mm camera from Mac Harvey to photograph our oscilloscope screen. We used an old, continuous-feed camera that jammed often enough to lose data from long and successful experiments. Steve and I investigated the function of small-diameter ventral (gamma) root axons. There had been suggestions, starting with Langley,

Fig. 1. C. C. Hunt and S. W. Kuffler at Johns Hopkins.

that they might provide the motor innervation to muscle spindles. In accordance with this idea, O'Leary, Heinbecker, and Bishop had found that the stimulation of gamma axons, when added to the stimulation of larger motor neurons, produced no significant increase in muscle tension.

Leksell had discovered that stimulating these fibers after most of the larger ventral root axons had been blocked caused an increased firing of what were presumably sensory axons in nerves to muscle. This observation supported the idea that they supplied motor innervation to spindles. In contrast to this mammalian innervation, Laporte, Ransmeier, and Kuffler had shown that small motor neurons in frog innervated tonic extrafusal fibers.

Steve suggested we study the function of mammalian small diameter ventral root fibers by subdividing filaments in ventral roots until we had isolated single small-diameter gamma axons. We developed a preparation using cat in which single sensory axons from muscle were also isolated in subdivided dorsal root filaments. This was not very difficult. We found that stimulation of the small ventral root axons produced no detectable tension in the muscle but increased the afferent discharge in nerves to muscle.

I suggested we try isolating single sensory axons from spindles in dorsal root filaments innervated by our isolated gamma axons. Although Steve was a bit skeptical about my idea, the preparation proved to be relatively easy. We were on our way. Using this technique, we then analyzed the motor responses of muscle spindles to their efferent innervation in detail. It turned out that about 30% of the ventral root outflow in cat was fusimotor and concerned with regulation of muscle spindles, rather than with generation of muscle tension.

Fusimotor stimulation facilitated the response of spindle afferents to muscle stretch. Up to six gamma motor axons could be isolated onto a single spindle and stimulated without producing any significant muscle tension. The fusimotor axons, in turn, showed reflex motor responses very different from those produced in motor axons to skeletal muscles. This finding opened the way to a number of studies on the motor innervation of spindles. The double single fiber technique we had developed was widely used.

Muscle spindles are the principal sense organs for detecting changes in muscle length. We learned that the small gamma fusimotor axons regulate that sense organ exclusively. They provide feedback to the control of muscle tension and also impart information about muscle length to the nervous system. This research turned out to be of great importance to the understanding of mammalian spinal cord physiology. Many years later, I was delighted to learn that these studies were among Steve's favorite experiments.

After 3 years, Steve began studying ganglion cells in the cat retina and left the spindle studies to me. This pattern was typical of Steve's approach to research. He would open a new area with the collaboration of a younger colleague and then, after several years, he would open another field of inquiry—leaving the problem he had first explored to his collaborator. Steve was a brilliant and generous scientific explorer who was happy to let others answer the questions he was the first to ask. Through his unique research style, he had a profound impact on neurobiology, not only through his own work but also through the research of the many scientists he trained, inspired, and influenced. In my time with him, Steve liberated me from my clinical interests and made me a seasoned researcher.

During my fourth and last year in Kuffler's laboratory, two lively young researchers arrived who would later win the 1981 Nobel Prize: David Hubel and Torsten Wiesel. To begin with, they shared space with me and Steve. Writing in Volume 1 of this series, Hubel noted

> Steve's main influence on Torsten and me was by example. He did an experiment every day, and he did virtually everything himself—dissections, recording, and writing the papers.

In their joint memoir, *Brain and Visual Perception,* each discussed Steve's effect on their collaboration. Hubel commented

> what guided and sustained us was the attitude of Steve Kuffler... (who) played a role that was crucial for both Torsten and me, in terms both of its day to day importance and the fact that it was sustained over some forty years.

Wiesel wrote of his wish to provide young scientists with "the opportunity that Steve Kuffler so generously gave David and me, the freedom to explore, the freedom to fail, the freedom to follow where the experiments lead you."

When I spoke at Steve's memorial service in 1980, I mentioned the Latin phrase used by Dutch historian Johan Huizinga to describe the basic human instinct for play "Homo ludens." Steve brought this element to research, which always made collaborating with him both intellectually rewarding and great fun. As Bernard Katz wrote in his Royal Society memoir of Steve: "(he) had the great good fortune to find endless excitement in his life's work"—an excitement that Steve transmitted to many others.

The Rockefeller Institute 1952–1955

In 1955, I felt it was time to strike out on my own. A meeting with Herbert Gasser and David Lloyd at the Rockefeller Institute led to my being offered a position there. Lloyd was an important figure in spinal cord physiology. His work provided the essential bridge between Sherrington's largely

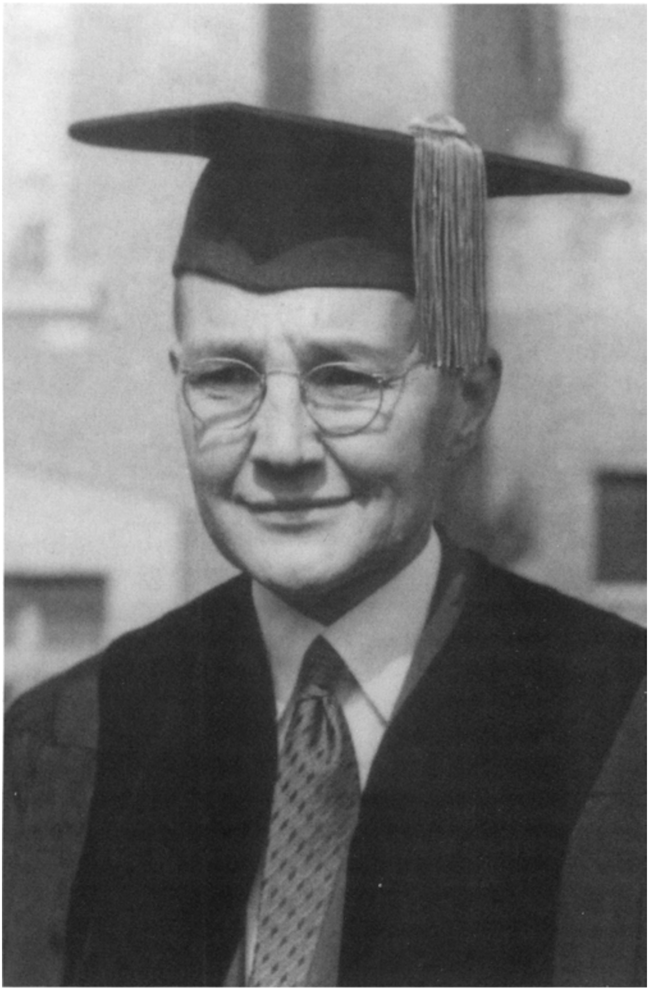

Fig. 2. H. S. Gasser at the Rockefeller Institute.

behavioral studies on spinal reflexes and subsequent research by Eccles and others using intracellular recording.

Herbert Gasser had won the 1944 Nobel Prize in Physiology or Medicine with Joseph Erlanger for their studies of nerve function. He was a remarkable man of rare modesty and high integrity. Typical of his character was his comment about winning the prize: "Dismay rather than elation was my immediate reaction." So alien was self-promotion to Gasser that when he was nominated in 1938, he refused Erlanger's request to provide copies of his papers to the Nobel Committee. His rationale was uncompromising.

> It must be well known in Stockholm that nomination for the Nobel prize coming to hand with full information must be made with the cooperation of the nominee. One is thus forced into the position of appearing, at least in some measure, to nominate oneself... my considered opinion about becoming a party to the proposal impels me into not consenting to do so.

Gasser had first encountered Joseph Erlanger as his professor of physiology at the University of Wisconsin. In his autobiographical memoir, he recalled: "the subject matter Erlanger presented differed so widely from what was anticipated that it amounted to a revelation." Wisconsin then offered only 2 years of medical school. Gasser believed that Johns Hopkins School of Medicine, with its scholarly focus, was the ideal place to finish his medical education. For financial and personal reasons, his father wanted him to stay in the middle west. However, as Gasser wrote of himself, "(his) determination was unshakeable." He eventually managed to gain his father's support and graduated from Johns Hopkins in 1915.

Two years later, Erlanger offered Gasser a position in the Department of Physiology at the Washington University School of Medicine. The crucial technical breakthrough for their research came in 1920 with the development of the cathode ray tube sufficiently sensitive to record nerve potentials. Writing some 40 years later, he recalled, "The most difficult step in opening up a new field had been taken. Ever afterward, Gasser never had any doubt about the direction he would follow."

Although he already saw himself primarily as a physiologist, in 1921 Gasser agreed to become head of the Department of Pharmacology. A close colleague and collaborator, Dr. Helen Graham, vividly described him in those years:

> To Dr. Gasser, an integral aspect of research is discussion and, in those days, when he had the right partner, discussion never seemed to weary him... his indifference to time and his ability to make colleagues ignore it not infrequently prolonged the Monday afternoon seminars.

In 1931, Gasser accepted a position as head of the Department of Physiology at Cornell University Medical College. Four years later, he agreed—with some reluctance—to become the second director of the Rockefeller Institute. Gasser received assurances that an experienced staff would allow him time for research. He was told that the director's most important function was "the maintenance of complete freedom to the investigators. In favor of this essential goal he had strong convictions." The strength of this conviction would mark his 18-year tenure as Director. Gasser's scientific interests influenced the Institute's research focus. For the first time, the structure and function of the nervous system became an important subject of study there.

By the time I arrived at the Institute in 1952, Gasser had been the director for 17 years and was just a year away from retirement. Despite his administrative duties, he had continued laboratory work. I found that Gasser's incisive intellect was combined with an unusual openness to new ideas. As George Corner noted:

> Investigators who went to him for counsel always left impressed by the intellectual vigor and scientific knowledge he brought to their problems, no matter what their field of science.

Gasser attributed his method of choosing research subjects to Dr. J.J. Abel (his professor of pharmacology at Johns Hopkins) who told him: "there are two times for working on a problem; before anyone has thought of it and after everyone else has left it." His colleague David Lloyd remarked: "As a result, Gasser was always the innovator or the finalist."

He became my ideal of a leader with complete integrity. As Gasser's friend and colleague Joseph Hinsey wrote, "Integrity was a passion with him and he was impatient with sham and pretense." At first meeting, he seemed rather reserved and austere. But as I came to know him, his warmth and graciousness emerged. On becoming Director of the Institute, Gasser had been given the use of an elegant apartment on the upper East Side. He enjoyed entertaining visitors there or in good restaurants. Deciding where to go for dinner involved his carefully perusing an extensive collection of restaurant guides.

A unique feature of the Institute at that time was an elegant faculty dining room. It was a high-ceilinged rectangular space with large windows overlooking the East River. Jean Lous David's splendid double portrait of the chemist Lavoisier and his wife presided over one end of the room. Tables set with white cloths and napkins accommodated 8 to 12 people. Uniformed waitresses served the meals. Members of the Institute tended to congregate in luncheon groups with a common research interest. But some, including me, preferred to move around to various tables and become acquainted with researchers in other fields. I can still recall listening to lively discussions with Maclyn McCarthy and Peyton Rous.

Central to Gasser's directorship was a belief that scientists should have absolute freedom in choosing their research subjects. One of his major concerns was that accepting government grants might compromise this freedom. As a matter of principle, he refused federal funds to reimburse the awardees for their laboratory expenses. Gasser's comments at the time reflected his commitment to intellectual freedom:

> The product of the Rockefeller Institute is new knowledge. It cannot be forecast and it cannot be achieved through administrative direction. All that can be done is to create optimal conditions for its production... this means fostering individuals and allowing them freedom... grants are made in the interest of defined projects... Projects, by definition, are not consonant with free inquiry.

On his retirement as director in 1953, Gasser returned to the laboratory on a full-time basis. Concerned that his experimental approach might be out of date, Gasser turned to new techniques such as electron microscopy. This allowed him to accurately measure the size of unmyelinated axons. During these years, he made important discoveries about how nerve impulses were conducted. Gasser found that the speed of conduction speed was directly proportional to the size of the axon coming from the sense organ in both myelinated and unmyelinated sensory fibers. Writing, as usual, in the third person, Gasser gave a detailed account of how he chose what to study in retirement:

> between the cessation and resumption of (his) experimental activity, the outlook in neurophysiology had greatly changed. A brilliant new school of nerve physiologists had grown up with its attention focused on single fibers... Belatedly to gain proficiency in the new techniques... had no attraction.
>
> At the interruption of the experiments on the differentiation of nerve fibers there was much left unfinished and much calling for reassessment... Here was left a sector... where he could enter without encountering much competition, one for which he had the due insight and training, and one to which he had long been dedicated.
>
> He started along the old line without the aid of a research associate. Thus his freedom to pick up... the loose ends left from the past was unrestricted by any obligation to the welfare of an assistant.

Since our laboratories were close together, I frequently visited him to compare our data. Gasser occasionally came down to talk with me. His keen observations and critical intelligence made these informal encounters extremely worthwhile. During my time at the Institute, I studied the diameters of sensory axons in muscle spindles and Golgi tendon organs. I also worked on spinal cord physiology using the cellular approaches David Lloyd had developed.

Archie Mcintyre came to our laboratory from New Zealand. He collaborated with Lloyd and me on muscle spindle and spinal cord studies. An exceptionally able investigator, Archie later became an internationally respected authority on sensory receptors. His insight into scientific problems and grasp of new research techniques later made him a major influence on the development of neurophysiology in New Zealand and Australia. Archie loved experimental work and, like me, enjoyed Herbert Gasser's visits to our laboratory. Gasser once casually mentioned the fact that there was little known about the properties of the cutaneous sense organs innervated by peripheral nerves. As a result, Archie and I performed a series of experiments to explore this question and wrote three papers on the subject.

A letter Herbert Gasser wrote to me in 1960, just a year before he suffered a devastating stroke, reveals his ongoing interest in our research. His comments were those of an interested and supportive colleague, rather than those of a world famous expert.

> I've been considering your and Archie's very impressive papers. They have a bearing on ideas I am now entertaining in an attempt to straighten out the size-velocity tangle.... At the moment, the velocities indicated in the composition of whole nerve potentials seem to be as full of deceptions as mincemeat is full of raisins...I would love to know where the top velocity comes in your estimation...

The use of a cooking metaphor to describe a complex scientific problem was typical of Gasser's unpretentious practicality.

In his introduction to the second edition of Erlanger and Gasser's book *Electrical Signs of Nervous Activity,* David Lloyd wrote

> The milestones of the past and those of the present are markers of progress, and a milestone passed is nonetheless of enduring value to those who would follow the path of learning.

When his career was drawing to a close and mine was beginning, Herbert Gasser gave me a sense of where the path of learning might take me.

The Albert Einstein College of Medicine 1955–1957

In 1955, Henry Lawson invited me to take a position in the Department of Physiology at the newly founded Albert Einstein School of Medicine.

After spending the previous years at well-established institutions, I thought this job would be an exciting adventure. The school opened its doors just after my arrival. When classes began, many buildings were still unfinished. As winter approached, we had to wear coats in the unheated lecture rooms. To demonstrate intracellular potentials for the students, we used micropipettes fabricated in another building and carried to the classrooms.

Without ever having been an associate professor, I received an appointment as a full professor and was invited to join the Executive Faculty. Among its outstanding members were Irving London in medicine, A.I. Gilman (father of A.G. Gilman) in pharmacology, and Ernst and Bertha Sharrer in anatomy. A.S. Paintal came from New Delhi to collaborate with me. We studied the reflex activity of gamma fibers to muscle nerves and called them fusimotor axons. The term was generally adopted, and the nomenclature remains far better known than our research on this topic.

The University of Utah, 1957–1964

In 1957, I received an offer to head the Department of Physiology at the University of Utah. I had never given much thought to taking such a job. When I asked Herbert Gasser's advice, he said: "You know, Cuy, you are really pulling the cart in that position."

Although the medical school's facilities were poor, and state support inadequate, I found this position attractive. There was a strong faculty, including Max Wintrobe in medicine, Louis Goodman in pharmacology, Emil Smith in biochemistry, and Bill Carnes in pathology. The western environment, with its clear air and beautiful mountains, seemed healthy and refreshing after the decade I had spent in Baltimore and New York. A chance to recruit able scientists and to create an ambiance conducive to new directions in physiology was appealing. I agreed to take the job.

Soon, the easy availability of federal funds made the minimal level of state support less of a problem, and the department began to thrive. The group I recruited was highly productive. As Bob Martin wrote of that period

> The scientific activity in the physiology department at Utah between 1958 and 1965 was amazing. In the beginning, Cuy Hunt, Ed Perl, Carlos Eyzaguirre, and myself formed the core of the neurophysiology group. Charles Edwards and George Eisenman later joined the faculty as well. The list of visiting scientists and postdoctoral fellows included Preet Gill, Motoy Kuno, Alberto Mallart, Archie Mcintyre, Autar Paintal,

> Guillermo Pilar, Akira and Noriko Takeuchi, and Koji Uchizoni. During that period we contributed more than forty papers to *J. Physiology,* the journal of choice for cellular physiology.

Ed Perl did pioneering studies on nociception. Martin and Pilar worked on the ciliary ganglion, which developed into a major field of inquiry. Lynn Landmesser came as a postdoctoral fellow and worked with Pilar. Dick Wylie joined me to study reptilian spindles. One memorable symposium I organized brought a galaxy of neuronal types to a mountain lodge for an informal scientific meeting combined with excellent skiing.

Our physiology course was unusual. Each faculty member taught the material he or she found of particular interest. In order to demonstrate photoreceptor responses, we had limulus shipped from Wood's Hole. This personal style of teaching physiology introduced students to the excitement of laboratory research.

Despite my administrative responsibilities, I continued to work in the laboratory. I analyzed the effects of axotomy on frog sympathetic ganglia using electron microscopy to study changes in structure and electrophysiology to study changes in function. The department's atmosphere was stimulating, and Bernard Katz's periodic visits from London were memorable.

In 1962, I decided to take a sabbatical and work in Bernard Katz's laboratory at University College. Most of the fellows there were Americans, and we worked in pairs. My partner was Phil Nelson who was then visiting from the National Institute of Health (NIH). He and I were given a small lab and a research problem that Katz supervised during our stay. Every weekday morning, he made rounds and reviewed our progress. Birks, Katz, and Miledi had described the changes in amphibian neuromuscular junction after presynaptic denervation. We studied the changes in structure and function of frog sympathetic ganglion cells that resulted from cutting their presynaptic nerve fibers. Phil and I then compared these findings with those that occurred in a nerve-nerve synapse. This sojourn gave me a brief but welcome respite from administrative responsibilities.

The Yale School of Medicine 1964–1967

When I returned to Salt Lake City after my sabbatical year, I wanted to make a fresh start. In 1964, I received an offer from the Yale School of Medicine to head its Department of Physiology. The Search Committee seemed eager to hire a person capable of creating a first-rate department, and the position was an open-ended appointment. A tradition of excellence had never taken root in this department, and some of its tenured professors were of distinctly dubious quality.

I was determined to recruit new and promising faculty members. However, the complications involved in effecting this transformation were not immediately apparent to me. Chief among them were academic politics and the medical school's position as a stepchild of the university. It had always been far less prominent and considerably less well endowed than Yale's undergraduate college. These factors were to make my task far more difficult.

My first appointment was a superb administrative associate, Harriet Batchelder. She was a seasoned veteran, a wise woman who knew Yale well. Politically savvy, deeply loyal, and highly organized, she quickly became essential to the smooth functioning of the department. The first and most important faculty member I recruited was Joe Hoffman. I thought general physiology, membrane transport, and cellular neurophysiology would make good intellectual combination. Joe was a leader in the field of membrane transport. He also had excellent judgment about science and scientists. Although it took me some time to persuade him to leave the NIH for Yale, I finally succeeded in doing so. He was a great asset to the department during my time at Yale and remains so to this day.

The medical school's administrative rules required that only department faculty members of equivalent or higher rank could vote on new appointments. With Joe's arrival, five full professors of physiology had voting rights. As a result, when I proposed new appointments, we were often faced with three negative votes to our two positive ones. Nevertheless, within 2 years of my arrival, Joe and I had managed to recruit a number of excellent people. These included Bob Martin, John Nicholls, Knox Chandler, Peter Curran, Carolyn and Clifford Slayman, and Dennis Baylor (then a postdoctoral fellow in another Yale department).

Along with running the department, I continued my laboratory work. At Gerhard Geibisch's suggestion, I invited Peter Heistracher to come from Vienna and collaborate with me. We studied contractile activation and deactivation of muscle fibers using short extrafusal fibers in snake skin. He and I also did preliminary studies on cat tail muscles that proved to be important to my later research.

Bill Betz, who was then a medical student at Washington University, wanted to do research in the department, so I invited him to come. As a result of this interval, Bill completed a doctorate in physiology and eventually became head of the physiology department at the University of Colorado. Another young scientist, Tony Ridge, came from Bristol to work in my lab and would later return to collaborate with me at Washington University.

Although my effort to upgrade the department looked successful to the outside world, reactions within the department were considerably less enthusiastic. My standards and administrative style were very different from those of my predecessors. A number of long entrenched professors felt threatened by the influx of new faculty members. And though I had been appointed chairman to upgrade the department, certain senior faculty members felt I was pursuing that goal far too aggressively.

The school of medicine was a highly complex institution with many overlapping areas of responsibility. For instance, a university-wide committee oversaw appointments and promotions within the medical school. The most dramatic dispute during my tenure as chairman involved the promotion of a longtime faculty member with strong Yale social connections and a highly controversial research focus. Jose Delgado had come to the department from Spain in 1950 at John Fulton's invitation. For some years, he had been implanting brain stimulators to modify behavior in animals and, more ethically questionable, in mental patients.

Delgado's most famous experiment took place in Cordova, Spain. He implanted a brain stimulator in a bull, brought the animal into an arena, and waved a cape in its direction. When the bull charged, he pushed a button, thereby activating the stimulator to stop its forward motion. In 1965, under the headline "Matador with a Radio Stops Charging Bull," *The New York Times* reported this sensational feat in detail. Two photos of the event—one showing the bull charging, the other picturing the animal stopped in its tracks—were published in a number of journals. However, a keen observer noted that shadows in the two photographs indicated a time lapse of hours rather than seconds between the two events.

I was reluctant to promote Delgado to the rank of full professor for several reasons. I had doubts about his judgment and qualifications for the position. A third, and more serious, reservation was the dubious ethical goal of his work. Delgado wanted to use brain stimulators as a means to control human behavior in a broader social context. He stated "Functions traditionally related to the psyche, such as friendliness, pleasure or verbal expression, can be induced, modified, and inhibited by direct stimulation to the brain." Delgado also asserted that he had been able to "play" monkeys and cats "like little electronic toys." Some years later, he wrote a book whose title made his ambition clear: *Physical Control of the Mind: Toward a Psychocivilized Society*.

After several prominent scientists I contacted had sent negative evaluations of his work, the Promotions Committee decided the wrong people had been asked for advice. They obtained more positive letters from behavioral scientists. I threatened to resign if Delgado were promoted over my objections. In the end, a rather unusual compromise was reached. He was transferred to, and promoted in, the Department of Psychiatry.

In 1966, I was asked to chair an Ad Hoc Committee on Departmental Reorganization. After extended deliberations, some members suggested that anatomy, microbiology, and physiology be merged into a single administrative entity. Although this proposal was eventually rejected, it made me feel quite uneasy about my future as a department head at Yale. In addition, Dean Vernon Lippard, who had recruited me, retired. In his first speech to the faculty, Dr. Lippard's successor stated that, under his leadership, the Yale School of Medicine would not become a center for what he called "subhuman biology."

I suspected that such a negative view of basic science might bode ill for the future. The introduction of rotating chairmanships in the preclinical departments also hastened my departure. My strengths were to recruit promising faculty members and to foster an ambiance to a productive and collegial group—tasks that could only be accomplished in the long term. Being rotated out of a position at which I excelled did not appeal to me.

So, after leading a revolution that lasted just 3 years, I accepted an offer to head the Physiology Department at the Washington University School of Medicine. Some Yale faculty members were openly incredulous that I was willing to leave an Ivy League school for a position at what they considered an obscure midwestern institution.

The Washington University School of Medicine 1967–1983

Over the years, I had known a number of excellent professors from the Washington University School of Medicine including Herbert Gasser, George Bishop, Jim O'Leary, Henry Schwartz, and Bill Landau. In 1967, it was an institution with a great scientific tradition and a refreshing lack of pretension. In contrast to Yale, the Washington University School of Medicine was more prominent and better funded than the undergraduate college. An Executive Faculty, composed of permanent department heads, governed the school and elected the dean on an annual basis.

The Department of Physiology, where Erlanger and Gasser did their pioneering studies on nerve, had a dwindling faculty and antiquated facilities. However, by the time I came, plans for the McDonnell Basic Sciences Building had been finalized. The new space it provided would be ample for incoming faculty members. Unlike Yale, there were far fewer vested interests to contend with in upgrading the department. And although St. Louis was a much larger city than New Haven, the social atmosphere there was far more informal and friendly.

The dean, Ken King, was a most unusual person. A highly intelligent man with a dry sense of humor, keen powers of observation, and quiet integrity, he enjoyed the trust and respect of the Executive Faculty. We all had great confidence in his judgment. As a result, he was reelected Dean annually for over 20 years.

After my experience at Yale, I found the attitudes of my fellow members of the Executive Faculty quite amazing. They actually made me feel at home in practical ways. For example, Oliver Lowry, the head of pharmacology, suggested that since the Department of Physiology was in such dire need of renovation and expansion, I should be given a larger portion of institutional funds. This was a great help both in building new space and in renovating laboratories that had long been in a dilapidated state.

On my arrival in 1967, the "spirit of the school" as Carl Cori had once called it, was still exceptional. Department heads were concerned about issues beyond their own immediate spheres of influence. They cared about the medical school's general welfare. At that time, the Executive Faculty was in the midst of a generational change. Several other new department heads came at about the same time I did. Roy Vagelos had already succeeded Carl Cori in biochemistry, and just before my arrival, Max Cowan was appointed to be the head of anatomy. Max and I became close colleagues and good friends. We shared mutual interests in science as well as common goals in building our respective departments. So, we decided to collaborate by organizing joint seminars and choosing faculty members from both our departments to serve on search committees. In the essay, that Max wrote for Volume 4 of this series, he made mention of our collegial relationship, crediting me for removing "all the usual barriers that so commonly divide academic departments." I would only add that this was a mutual effort.

As had been the case at Yale, choosing the right administrative associate proved crucial to my success as a new department head. I hired a young woman, Jackie Baker, who was highly competent and absolutely trustworthy. I knew that with her help the department would run smoothly. My first academic appointment was Mordy Blaustein. A graduate of Washington University Medical School, his familiarity with the institution was a real asset to me. Mordy contributed in important ways to the department's growth. He gave me astute advice about scientific developments and the outstanding people in various fields. Despite Washington University's unfashionable midwestern location, the department steadily grew and improved. As excellent new faculty members arrived, persuading other people to move became easier. In addition to Mordy, my recruits included Paul De Weer, Luis Reuss and Elsa Bello-Reuss, Dale Purves, Nigel Daw, Josh Sanes, and Jeff Lichtman. When federal funds were not available, Ken King provided me with funds from the Dean's Office to purchase new equipment for incoming faculty.

The varied interests among new faculty members broadened the department's range of research. Mordy Blaustein worked on synaptosomes and later on $Na^{+}Ca^{2}$ exchange, Paul De Weer did research on the sodium/potassium pump, Luis Reuss studied epithelial transport, Elsa Bello-Reuss worked on renal tubules, Nigel Daw focused on vision, Dale Purves did research on neurons in the sympathetic chain, Josh Sanes studied molecules in basal lamina that determined sites of neuromuscular innervation, and Jeff Lichtman (who received his Ph.D. from our department) worked in cellular neurophysiology.

Roy Costantin was an outstanding addition to our group. He had received his M.D. from Columbia and then worked with Richard Adrian in the physiology laboratory at Cambridge before joining the faculty at his alma mater. Roy had done excellent research on muscle and was clearly a scientist of great promise. I offered him an appointment that he quickly accepted.

A short time later, Roy called me and suggested that because he had just received a diagnosis of malignant melanoma, I might want to reconsider hiring him. I assured Roy that this made no difference in my desire to have him join the department. Within a short time, he gained his colleagues' affection and admiration. Before the disease recurred, Roy did superb work in both teaching and research. His papers remain classics in the field. Roy's untimely death was a tragedy for his family and a great loss to many friends and colleagues.

Some months later, we organized a symposium in his memory. Roy's chief interest was basic problems of muscle physiology. Scientists from both sides of the Atlantic, many of whom had collaborated with him, were invited to attend. Richard Adrian and Andrew Huxley came from England for the occasion. Rather than simply being a memorial service, this gathering was also a working scientific meeting. References to Roy and his contributions to the field were woven naturally into each speaker's talk. The bitterness of losing him far too soon was tempered by a sense of Roy's ongoing influence and the enduring affection of his friends.

One veteran faculty member joined our group. Albert Roos arrived at the medical school in 1947 to work in the division of anesthesiology for the legendary head of surgery, Dr. Evarts Graham. Although he had long held a joint appointment in anesthesiology and physiology, Albert's laboratory was located across the street from our department, near the Department of Surgery in Barnes Hospital. I found Albert extremely bright and very engaging. One day in 1970, I met him in the elevator and invited him to join the physiology department on a full-time basis. He agreed to do so.

This move turned out to be an excellent idea both for him and for us. Albert savored his new environment and flourished among my new recruits. In turn, we were stimulated by his vibrant presence. His younger colleagues' cellular approach to physiology influenced Albert's research. Although Albert was over 50 at the time he joined the department, he opened an entirely new area of research. Within a few years, he became the leading authority on intracellular pH. Albert's outgoing personality and enjoyment of vigorous discussion enhanced the department's collegial atmosphere.

Another senior appointment was John Heuser, who came to Washington University from the University of California School of Medicine in San Francisco. His delicate laboratory equipment had to be moved from the west coast with extreme care. Ken King provided the funds necessary to pay the substantial expenses for having this done.

Several professors who were in place on my arrival remained in the department. Len Banaszak and Scott Matthews did excellent work in x-ray crystallography. Charlie Molnar and Russ Pffeiffer continued their studies on the cochlea. Charlie, who had helped develop the LINC computer, also headed the Computer Systems Laboratory.

In addition to administration, I managed to keep my own research going. Yas Fukami came with me from Yale, and we worked, sometimes together and at other times separately, on muscle receptors. I was fortunate to have Sherman Beacham, who was an outstanding medical student while I was at Utah, work in my lab for several years. Later, Bob Wilkinson joined me. We had a highly productive and enjoyable collaboration studying isolated muscle spindles. Trained as a physicist at Rice, Bob brought quantitative skills and knowledge to our research. In the mid-1970s, I spent three summers working in Stockholm with David Ottoson. During these visits, we made considerable progress in developing an isolated mammalian spindle preparation.

By the late 1970s, both Roy Vagelos and Max Cowan had begun to consider leaving the medical school for other positions. Strenuous efforts were made to change their minds. Chief among them was the establishment of a University Division of Biology and Biomedical Sciences. Roy served as its first director, and Max then succeeded him. The purpose of this innovation was to keep Roy and Max at the university a few years longer and also to unite the medical school's basic science departments with related departments on the undergraduate campus. Despite this effort, Roy and Max both left the medical school soon thereafter.

A Sabbatical in Paris

By 1980, I was ready to take a 6-month sabbatical in Paris to work with an old friend and colleague, Yves Laporte. Yves and I had met in the early 1950s when he was doing research at the Rockefeller Institute.

Thirty years later, Yves had become head of the Physiology Laboratory at the College de France and was elected administrator of this august institution. Several of his laboratory colleagues were then studying muscle spindles: Francoise Emonet-Denand, Julien Petit, and Lena Jami. In contrast to my isolated preparations, they used whole animals for their research. Laporte's team was highly skilled in this approach, and the results of their experiments were very interesting. By working with them, I hoped to gather information that could be useful in my isolated receptor studies. And I had always wanted to live in Paris.

My old medical school professor of physiology, Joseph Hinsey, heard about my sabbatical plans and suggested that I get in touch with his friend Dr. William Dock who lived in Paris. His father, George Dock, had served both as the head of medicine and dean at the Washington University School of Medicine. Bill began his career as a pathologist and later became an eminent cardiologist.

He possessed a remarkable grasp of medical history combined with a love for and knowledge of Paris. Although in his 80s, Bill was a indefatigable walker and a superb guide. His stories about medicine were fascinating, his narrative style was delightful, and his sense of humor was irreverent. One of Bill's favorite phrases was "a death to make your mouth water" which he applied to those fortunate enough to die quickly and without suffering. But his basic approach to living in Paris as a widowed octogenarian was "joie de vivre." Spending time in his company taught me a good deal about how to age gracefully.

Return to Washington University 1981–1983

By the early 1980s, the medical school had changed considerably. The faculty and physical plant had grown substantially, as had the budget. Most important to me, the Executive Faculty was no longer a small, cohesive group with a common concern for the school's welfare. It had become a large group of disparate individuals whose interests centered on their own departmental agendas. Whereas the dean had formerly controlled most institutional funds, clinical departments had by then begun to accumulate large amounts of money. When some department heads took positions I considered self-serving or when they ignored obvious conflicts of interest, I reacted negatively. My refusal to support some of their decisions led certain colleagues to see me as uncooperative and overly conservative.

In 1983, the National Academy published a survey of Research Doctorate Programs in Biological Sciences, the first such assessment undertaken since the 1960s. Physiology was the only department at Washington University to be ranked among the top ten in the country. Yale's Department of Physiology, which had retained many of my recruits, was also in this group. To my surprise and delight, Carl Cori sent me a handwritten poem to mark this occasion:

> Chancellors, deans, colleagues and students Washingtonienses
> Examine important message in Science with critical lenses.
> In a contest wide, in Academia's lofty towers,
> Rate programs for graduates, no flowers!
> Rate giving Ph.D. degrees in Physiologia
> To be honest, this is not the casa mia.
> The answer came 2000 strong
> The verdict was as clear as a gong.
> Top rated at Washington U. is Physiologia.
> Where are the Hunts—Madonna mia.

They now have a banner with which to picket,
But instead to Paris they bought a ticket.
Hurray for the Hunts.

Ironically, these findings were published just as the new Division of Biological Sciences was in the process of absorbing our graduate program.

In September of that year, when I was due to retire, members of the department organized an excellent symposium in my honor. Although being celebrated at an occasion like this is rather like going to one's own funeral, the organizers made it a happy and memorable day for me. The theme was "Biophysical Approaches to Physiology." Nine of the eleven speakers had been members of my previous departments and were by then full professors or department heads: Carlos Eyzaguire, Arthur M. Brown, Knox Chandler, Joe Hoffman, Bob Martin, Mordy Blaustein, Motoy Kuno, Guillermo Pilar, and Edward Perl. In addition, Yves Laporte came from Paris and David Barker from England. It was an elegant and interesting occasion that gave me the pleasure of seeing many old friends and colleagues.

Retirement from Administration and Four Years in Paris 1983–1987

As my 65th birthday approached, then the age for mandatory retirement from administrative positions at Washington University, I felt it would be less awkward to be elsewhere during the search for my successor. I had headed the physiology department for 16 years (1967–1983). During that time my young recruits became able senior faculty. An important aspect of my administrative style had been to serve as a buffer between my faculty members and medical school politics. Retiring as department head substantially diminished my influence on the Executive Faculty. There seemed little point to my retaining a lame-duck position.

In 1983, with support from the Fondation de France, I took an extended leave to work again in Yves Laporte's laboratory at the College de France. On my departure, Nigel Daw became acting head. A national search for my successor resulted in an internal appointment: Philip Stahl, a cell biologist who turned the department in that direction. There was no interest in sustaining the strength I had established in neuroscience.

Three leading faculty members, Dale Purves, Josh Sanes, and Jeff Lichtman, moved to the Department of Anatomy and Neurobiology, then headed by Gerry Fischbach. Gradually, other senior faculty accepted positions as department heads elsewhere: Mordy Blaustein at the University of Maryland, Paul De Weer at the University of Pennsylvania, Dale Purves at Duke, and Luis Reuss at the University of Texas Medical Branch at Galveston.

A Second Return to Washington University 1987–1994

Four years in Paris gave me ample time to work with Yves Laporte's research group. During those years, we published a number of papers on muscle spindles. Despite my love of Paris, by 1987 returning to the United States seemed a good idea. My son had entered college, and my daughter was born in June of that year. Perhaps becoming a father again at the age of 69 impaired my ability to slip gracefully into retirement.

Mordy Blaustein kindly offered me space in his department. But because I still had so many friends and colleagues at Washington University, it seemed easier to return there. I planned to reestablish my laboratory for further study of fusimotor effects on muscle spindles. However, after a 4-year absence, obtaining space in my former department proved far more difficult than I had anticipated. Because I had spent so many years renovating and arranging space for other people, I found this situation both awkward and ironic.

Luckily, Bill Landau, who then headed neurology, offered me space in his department. Michael Chua joined my laboratory there. He had received his Ph.D. from the Australian National University and had been a postdoctoral fellow with Bill Betz at the University of Colorado. Mike brought many quantitative skills to our work. As a result, the isolated spindle research received a real boost. We used a number of fluorescent dyes to stain components within the spindle, particularly the sensory terminals. Using confocal microscopes enabled us to obtain beautifully clear images. These images could then be used to make three dimensional reconstructions.

Moving to the neurology department proved only a temporary solution to my space problem. After Bill Landau retired as head in 1991, it became obvious that I would have to seek space and a position at another institution—not easy to do in one's 70s. Although a number of my former associates at Yale were willing to provide me with some lab space there, that failed to materialize. Fortunately, Ed Perl, my former colleague at the University of Utah, offered me a position in the Department of Physiology at the University of North Carolina. The chairman, Stan Froehner, was amenable to this idea. I moved there in 1995 and continued my research on spindles with Michael Chua.

The following year, I received an award of distinction from my alma mater, Cornell University Medical College, for "notable achievement as a physician, scientist, educator, and administrator." In 1997, the Association of Chairmen of Departments of Physiology gave me its Distinguished Service Award "for outstanding service and dedication to the discipline of physiology." At that time, the organization's president was Paul De Weer, who had been one of my young recruits at Washington University. It was a particular pleasure for me to receive this honor from him. In a generous tribute, Paul noted that over 20 of my associates had become department heads, a number he called "unique in the annals of physiology."

Six years later, in the fall of 2003, I was asked to speak on the occasion of Mordy Blaustein's retirement as head of physiology at the University of Maryland. He had been my first recruit to the Department of Physiology at Washington University. Hearing tributes to Mordy's 25 years of service made me realize how fortunate I had been to hire him early in his career. I also realized that when my once promising young recruits began to retire, it was a sure sign of age—both theirs and mine.

A Retrospective View

Looking back over a long career, I feel fortunate to have started out in neuroscience when it was a small and friendly enterprise—and at a time when the field was undergoing an exciting intellectual transformation. The relatively few neuroscientists then were likely to know each other and to have some familiarity with one another's work. The Society for Neuroscience, founded in 1969, now lists some 35,000 members. The range of research interests—indicated by the number of new journals—has also grown enormously. Although both these changes are signs of progress, I am glad to have started out when neuroscientists were a more cohesive group, funding was generous, and there was less pressure to tailor research for a particular agenda.

Timing was certainly a critical element in my career. For an aspiring young scientist, the chance to collaborate with Steve Kuffler was a remarkable piece of luck. And although I did not collaborate with Herbert Gasser, my contact with him proved invaluable. These two outstanding scientists, Gasser born in 1883 and Kuffler in 1914, represented consecutive generations of excellence in neuroscience. To have known them both early in my career was a privilege and an inspiration.

The availability of NIH funds during the years I was a chairman enabled me to build three fine departments. Was all this department-building in three different academic settings really worthwhile? Despite the occasional difficulties I encountered, I would say yes. To a large extent, I enjoyed the challenge of building new departments in very different places. The fact that I was able to do so more than once was a source of great satisfaction.

Although the quality of teaching concerned me, I was not known as an exciting lecturer. One medical student said: "Dr. Hunt gave the lectures in cardiovascular physiology, but his heart wasn't in it." My most significant accomplishments were to recruit talented scientists and to create environments that encouraged their research. Watching them flourish was a great pleasure.

Although individual achievement was important, the atmosphere in my three departments was always collegial. Faculty members had a genuine interest in each other's research topics and enjoyed sharing ideas about them. Enduring friendships developed as a result. Several of my former colleagues have told me "the best years" of their lives were those spent in one of my departments. I take their comments as a high compliment.

Given current fiscal constraints, it would be difficult, if not impossible, to build three departments of similar quality today. A common recruiting style these days is to hire established "stars." This is far more expensive and certainly less rewarding than it is to nurture the development of promising young scientists.

With the current stringency of government funding, grant applicants must predict the research results to be obtained and also demonstrate that the proposed experiments are likely to yield them. The effect of these requirements on scientists has been exactly what Herbert Gasser feared: a loss of the intellectual freedom to follow the path of discovery wherever it might lead. I was fortunate to begin my career when one could choose a research topic at will, on its own merits, and without concern about fitting it into a predetermined scientific agenda.

When he was director of the Medical Research Council, Peter Medawar wrote: "I construe my function...as mainly to create the kind of environment conducive to the advancement of learning...this is all a director...can do." His views remind me of Herbert Gasser's remarks while he was director of the Rockefeller Institute: "New knowledge cannot be forecast and cannot be achieved through administrative direction. All that can be done is to create the optimal conditions for its production." Like them, my constant goal as an administrator was to create the best possible environment for the advancement of learning. During some 25 years as a department head, I hope to have done so.

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