The History of Neuroscience in Autobiography
Volume 1

Edited by Larry R. Squire
Published by Society for Neuroscience
ISBN: 0-12-660301-4

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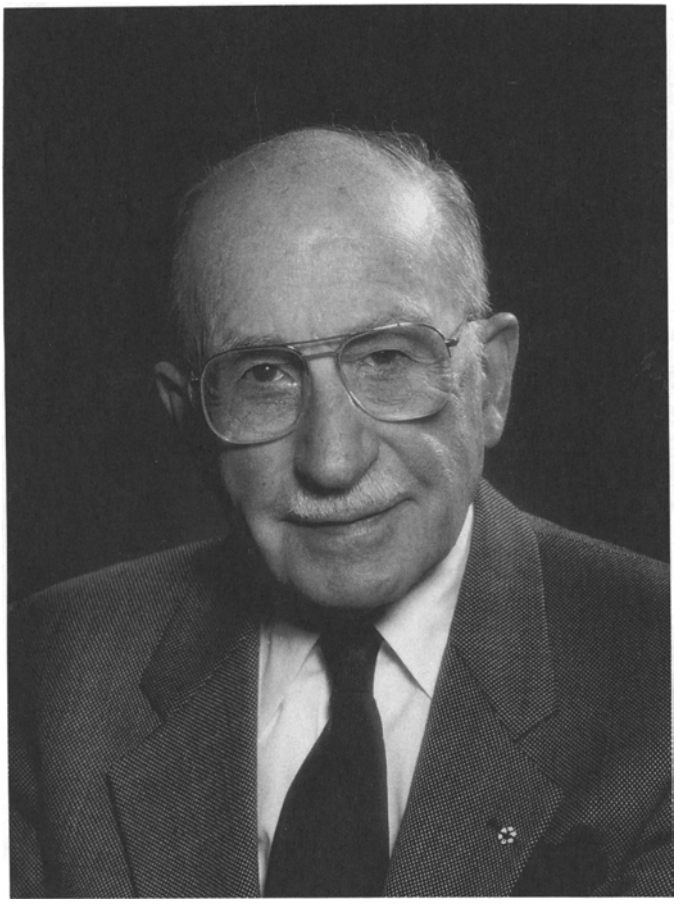
pp. 318–346

https://doi.org/10.1016/S1874-6055(98)80012-1



Herbert H. Jasper

BORN:

La Grande, Oregon
July 27, 1906

EDUCATION:

Willamette University (Philosophy and Psychology, 1923)
Reed College, B.A. (Psychology, 1927)
University of Oregon, M.A. (Psychology, 1929)
University of Iowa, Ph.D. (Psychology, 1931)
University of Paris, D.és.Sci.de l'état (Neurophysiology and Biophysics, 1935)
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HONORS AND AWARDS (SELECTED):

Fellow, Royal Society of Canada (1964)
The William G. Lennox Award for Research in Epilepsy (1969)
Officer of the Order of Canada, O.C. (1972)
Ralph W. Gerard Prize, Society for Neuroscience (1981)
Karl Spencer Lashley Award, American Philosophical Society (1982)
McLaughlin Medal and Prize, Royal Society of Canada (1985)
The Milken Family Medical Foundation and the American Epilepsy Society "Most distinguished prize and award for basic research in epilepsy" (1993)
F.N.G. Starr Prize, Canadian Medical Association (1994)
Member of the Canadian Medical Association Hall of Fame (elected 1995)
The Albert Einstein World Science Award of the World Cultural Council (Mexico City, 1995)

Herbert Henri Jasper dedicated his life to studies of the brain in relation to the mind and behavior. He pioneered the establishment of the electroencephalogram (EEG) for the study of the electrical activity of the brain in relation to states of consciousness, learning, and epileptic discharge. He proceeded to use microelectrodes to record from single brain cells and synapses combined with studies of neurochemical mechanisms involved in the control of brain activity.

Herbert H. Jasper*

Dedication to Brain Research, Willamette University, 1924–1926

I was born in La Grande, Oregon in 1906. Through my father, who was a Protestant minister and religious scholar, I became interested in world religions and social problems. I was in uniform during the first world war as a messenger boy in the Army camp where my father was in charge of social services for the troops. I then went to Willamette University in Salem, Oregon, from which my father had graduated in the school of theology and social studies. I majored in philosophy and experimental psychology and decided to devote my life to studies of brain-mind-behavioral problems.

I delved deeply into the history of philosophy, led by Professor Charles L. Sherman. I was fascinated by the Greek philosophers, Socrates, Plato, and their idealistic point of view. This was tempered, however, by the work of Plato's pupil, Aristotle, and his research in biology and botany that led him to collect species of plants and animals and to speculate about their origin and development, which led him close to the views later expressed by Charles Darwin. My view of Greek philosophy also was tempered by the atomic materialism of Democritus and the Epicurean pleasure principles. I found the origin of medicine in the works of Hippocrates and Galen.

After my initiation into Greek philosophy I proceeded to study the great philosophers of the 17th and 18th centuries—Descartes, Bacon, Kant, Hegel, Spinoza, Berkeley, Locke, Spencer, Hume, and Bergson. I was very interested in Kant's *Critique of Reason* for it seemed that his view of metaphysics and epistemology and his psychological analyses of human behavior and mental processes in relation to brain activity was quite reasonable. The extreme subjectivism of Berkeley impressed me little, because it seemed to me a game of logic rather than an attempt to understand the world we live in.

My favorite of the 17th and 18th century philosophers was Bergson with his view of "creative evolution," continual change and development of truth and the mind. His introduction of the importance of time, with the

*Some highlights of 70 years in neuroscience research.

emergence of new forms and ideas in time, was reminiscent of Einstein's treatment of time as a fourth dimension. Bergson, however, did not agree with Darwin's view of natural selection by chance mutations in heredity as the mechanism of creative evolution. His careful studies of the remarkable development of animal organs, such as the brain and the eye, convinced him of a creative design in nature.

Professor Sherman, however, was more interested in the modern philosophers, particularly the pragmatists like Charles Pierce and John Dewey. We spent much time on the work of William James in *Principles of Psychology*, a truly great work of pragmatic philosophy as well as a comprehensive treatment of his views of the "stream of consciousness" and its relation to the continuous flow of sensations modified by instincts, attitudes, and emotions.

I was introduced to psychology through studies of the functional anatomy of the brain. Professor Sherman gave us a good review of what was then known about the sensory receiving areas of the cerebral cortex; the motor cortex; and the frontal, parietal, and temporal association areas. It seemed that many of the philosophical problems that had been bothering me, and the mechanisms of the mind and behavior as described in psychological terms, might well be understood if we knew more about the brain mechanisms involved.

I met an attractive young lady in our psychology class. Her father was the superintendent of the Oregon State Mental Hospital, located not far from the university, on the outskirts of Salem. I walked her home a few times and then she invited me to tea with her parents in their home, which was a splendid house on the beautiful grounds of the hospital. I was pleased to accept her invitation not only because I was becoming fond of her but also because I was curious about what went on in a state mental hospital.

I arrived on a Sunday afternoon and, walking through the grounds of the hospital, we met several patients who seemed to be roaming around quite freely. One of them spoke to my young lady in a friendly manner. I did not recognize him as a patient and asked the young lady who he was. She replied that he was one of many patients who had the liberty to walk freely on the grounds while being supervised from a distance.

I found the superintendent to be a charming fellow, willing to discuss the activities of the hospital. He invited me to join in some of the regular patient conferences and ward rounds. I did this on many occasions and was astounded by the strange distortions in thought and behavior we encountered in patients for whom there seemed to be little or no treatment, only good custodial care.

In some patients only a thin line separated them from what might pass as normal or only slightly odd. What disturbances in brain function could underlay such tragic derangements in mental activity and behavior, was a question that has haunted me all my life.

These are only a few of the influences that led me to make a firm commitment at about 20 years of age to devote my life to brain research in all of its aspects. As it happened, the late 1920s and the early 1930s proved to be a most exciting period in the development of neuroscience:

1. Joseph Erlanger and Herbert Gasser at Washington University in St. Louis, Missouri, had just developed the use of the cathode ray oscilloscope to visualize for the first time the precise form of nerve action potentials and their differences, depending on the diameter and myelination of nerve fibers.

2. Hans Berger had just begun publication of his studies, *Das Elektrenkephalogram bei Menschen*, which, when confirmed by Edgar Adrian of Cambridge, England, and many others, launched the use of the electroencephalogram (EEG) for the study of normal and abnormal human brain activity.

3. Sir Henry Dale in England and Otto Loewi in Germany had begun to establish the chemical transmission of the nerve impulse at synapses and neuromuscular junctions. The dispute between the "Dale School" (soup) and the electrical school (sparks) led by J.C. Eccles, then of Oxford, enlivened meetings of the British Physiological Society in the early 1930s until Eccles himself, with the aid of Arthur Feldberg and Martha Vogt, became a proponent of chemical transmission at synapses throughout the nervous system.

4. Alan Hodgkin and Thomas Huxley had demonstrated the ionic mechanisms of the transmission of impulses in nerve fibers, with the help of J.Z. Young and K.C. Cole, at the Marine Biological Laboratories in Woods Hole, Massachusetts.

5. Francis Schmitt with his brother Otto, at Washington University, had begun their studies of the molecular structure of nerve membranes. Frank had also been brought up in a strongly religious background and thought that many of the answers to understanding the function of the nervous system would come from studies of the molecular structure of nerve membranes. This became known as "from molecules to mind" or the "from bottom up" point of view, in contrast with the "from mind to molecules, from top down" point of view, where I started in my thinking about this old problem.

With these and other exciting developments in neuroscience at that time, there is little wonder that I chose to devote my life to brain research. My father was somewhat dismayed for he wondered how I was going to make a living, because I was neither interested in studying medicine to become a doctor, nor in becoming a university professor!

Reed College and Postgraduate Studies at the University of Oregon, 1928–1929 and the University of Iowa, 1930–1931

I had been saturated at Willamette University with studies in philosophy, psychology, physics, and chemistry. I had been shocked by the suicide of a close friend and classmate, and by exposure to a hospital full of mental

patients. I was ready to move on after three years of study and work on the side to pay my way.

I had become a unionized meat cutter working after school and during summer vacations. I also had organized a landscape gardening company, employing students to take care of properties in Salem, all of which did well to pay for my living and tuition expenses.

My father moved to a parish in Eastmoreland, adjacent to the campus of Reed College, an outstanding small college in Portland, Oregon. To my surprise I was admitted to Reed in 1926 soon after I applied, even though it was only for my final year of college. I was thus able to live with my parents close to campus and to join the graduating class of 1927.

I studied psychology under the excellent tutelage of Professor William "Monty" Griffith, a large friendly practical professor, who was interested in all aspects of behavioral psychology. I took a minor in philosophy under an equally outstanding philosopher, Professor Edward Sisson. Classes were small, there being only about 300 students and 45 faculty in the entire college. We met frequently in our professors' homes.

There were few intercollegiate athletics to distract the students, but there was plenty of physical exercise through intramural sports programs. Students received no grades, so that we would not be distracted from our studies. This caused some anxiety on the part of my father who wrote the president toward the end of the year to inquire about my grades and if I would graduate. The president answered by letter telling him that I would graduate with "sufficient extra space to be able to drive out with a four horse team."

I was required to write a thesis, which was equivalent to a master's thesis in the graduate schools of most universities. My thesis involved questionnaire studies of students from most of the universities and colleges in Oregon. It was published in part in the *American Journal of Sociology* in 1929. Titled "Optimism and Pessimism in College Environments," it was my first publication.

My experiences at Reed made indelible impressions on my future life, even today, 70 years later. The faculty and student environment was delightful. I wrote for *QUEST*, the college paper, and conducted an experiment with other students which had a lasting impression on my future in neuroscience. Professor Monty Griffith had been describing the effects of certain psychotropic drugs on the mind. A few students in the dormitory invited me to join them in an experiment with one of the drugs mentioned in class, mescaline. I was interested but afraid of what I might be getting into. I tried the drug and was astounded by the profound effects of a few drops of injected mescaline. The whole world changed. I was disorientated completely, had hallucinations and delusions, and sensations of floating in air. It was a most disturbing and frightening experience, with some rather pleasant and exhilarating feelings as well.

We discussed our experiments afterward, and Monty Griffith warned us severely about addictive properties and ever trying such experiments again. I have never forgotten the dramatic effect of such a small amount of a chemical substance upon the mind. I was determined to include brain chemistry in my future program of brain research.

Graduate Studies, University of Oregon, M.A., 1929 and the University of Iowa

In 1929 I enrolled in a Master's degree program in experimental psychology at the University of Oregon in Eugene. I found an apartment for graduate students, managed by a kindly middle-aged lady, Celia Hager, who mothered us all.

I had two roommates, one a completely blind assistant professor in charge of the laboratories of experimental psychology, Tom Cutsforth. He had lost sight in both his eyes by an accidental injury when he was a very young child. Tom became a close friend and companion, both in fishing and hunting trips in the mountains around Eugene, as well as in the laboratories, helping me to construct the apparatus necessary for my thesis work. My other roommate was a graduate student in psychology by the name of David Turtletaub.

In a cottage next to ours were two sisters, Constance (Connie) and Eleanor (Cindy) Cleaver. Connie was a lively, attractive young lady who was studying art. By coincidence her parents were friends of my parents in LaGrande, Oregon. I found myself spending more and more time with her as time went on, with delightful consequences at the end of the first year.

Professor Edmund S. Conklin was head of the department of Psychology. He specialized in abnormal psychology and was the author of a popular textbook on the subject. Associate professors included Harold Crossland and Robert Seashore, the son of Dean Carl Seashore, head of the Psychology department and the Graduate School at the University of Iowa in Iowa City.

Professor Conklin's lectures in abnormal psychology were outstanding. I did my thesis under his direction, and with assistance from Robert Seashore and Tom Cutsforth, we designed and constructed some of the apparatus I needed in my studies of perseveration. My thesis was titled "Perseveration and its Relation to Depression and Introversion." It was published in the *Journal of Social Psychology* (Jasper, 1931). Because our results were rather negative, we were anxious to get on with more promising experimental work.

In the meantime, Connie Cleaver and I had fallen in love and her interest and work in art opened up new worlds for me. With the agreement and encouragement of our parents, and the urging of our friends, Celia Hager, Tom Cutsforth, Bob Seashore, and others in the psychology department, we were married on Christmas day, 1928 in LaGrande,

Oregon—my birthplace and the home of Connie's father and mother. Connie's sister, Cindy, was the bridesmaid.

For the first year we lived in Connie's apartment when her sister kindly moved elsewhere. We both graduated in June 1929. Connie earned her masters degree in art and I was awarded a master's in psychology.

With the help of Bob Seashore I applied to graduate fellowships at Ohio State, Northwestern, and the University of Iowa, each of which had a very good department of psychology. I was very surprised to have my application accepted in all three universities, presenting quite a problem of choice.

Bob Seashore advised me to take the position that offered the least salary. I objected that I would need more money now that I was a married man, but he reasoned that the department offering the least salary must be the best because it was able get many good students for less. Bob's father was head of the psychology department as well as dean of the Graduate School at the University of Iowa. The lower stipends made our choice an easy one.

Bob also suggested that Connie should apply for a fellowship in the art department. This seemed to be an excellent arrangement and Connie was awarded a fellowship. The art department had close relationships with the psychology department because of Dean Seashore's interest in the psychology of music and art.

We packed up and set out for Iowa City together during the summer of 1929. Bob Seashore was there to meet us, having returned to Iowa to visit his parents, who helped us find an apartment upstairs in an old house near the center of town. They also invited us to dinner at their home with Bob and his wife. We had a delightful evening with this friendly family, and received much good advice about living in the small university town of Iowa City.

I am unable to describe in this short autobiography the many important experiences I had during my two years in Iowa. My thesis, under the direction of Professor Lee Edward Travis, head of the department of speech pathology, was based on studies of the effects of hemispheric cerebral dominance on the bilateral coordination of movements in normal patients and those with severe stuttering. My thesis was published in the *Psychological Monographs* (Jasper, 1932). There were several other studies published separately. Professor Travis, with his electronic engineer, had developed an electrical recording apparatus for muscle and nerve action potentials, but with which we could not record the slower waves of the electrical activity of the brain, which would come later. I used the apparatus to study the time characteristics of electrical stimulation of nerve and muscle known as "chronaxie" as developed by Lapicque and Bourguinon in France.

More important, perhaps, were the studies carried out with other faculty members in the departments of medicine, physiology, and psychiatry.

It was at Iowa, under Dr. Travis, that John Knott and Donald Lindsley got their start in electrophysiology after I left.

Important also were the contacts I had with the group at Washington University in St. Louis, including Herbert Gasser, Joseph Erlanger, George Bishop, Howard Bartley, Francis O. Schmitt, Lorente de No, and James O'Leary.

Bartley and Bishop were studying the electrical activity of the brain in experimental animals. Gasser and Erlanger were developing the use of the cathode ray oscilloscope for the analysis of compound action potentials from peripheral nerves and their relationship to the diameter and myelination of single nerve fibres.

During the winter of 1929 to 1930 we attended a meeting of the American Physiological Society in Chicago, despite a severe snow storm. Many of the St. Louis group were there and we managed to meet them after their papers were presented. Herbert Gasser then introduced us to a charming young couple from the Sorbonne in Paris by the name of Alexandre (Ali) and Andrée Monnier who were studying in St. Louis on a Rockefeller Fellowship. They were returning to Paris with their cathode ray oscilloscope apparatus they had been building. They asked if I would like to come to Paris to work with them for a couple of years. I agreed, after consultation with Connie, who was delighted at the possibility of continuing her art studies in Paris. With the help of Dean Seashore, Allan Gregg of the Rockefeller Foundation, and Ali Monnier, I obtained the support of the Rockefeller Foundation. We sailed from Oregon on a passenger freighter through the Panama Canal in the summer of 1931. It was a long and delightful trip and we arrived in Paris in time to start the year at the Sorbonne in the fall of 1931. It was a most important turning point in my career.

Enlarged Horizons, Rockefeller Fellowship in Europe, 1931–1933

We were greeted warmly by the Monniers and soon were treated like members of their family. We were also greeted warmly by Professor Louis Lapicque, who was head of the physiology department and worked daily in the laboratories with Madame Lapicque and other members of the staff and students. Everyone seemed to be working on chronaxie in one way or another, which was Lapicque's principal interest. I found it an intriguing theory of *excitabilité en fonction du temps* in the transmission of impulses across synapses and neuromuscular junctions in the nervous system, *isochronism* being a sort of tuning process.

I soon found that Lapicque had another great interest, that of sailing his ocean-going yacht, the "Axon," from his country home near Paimpol in Brittany.

Ali Monnier was a distinguished biophysicist before becoming a neurophysiologist. He was developing a mathematical theory of nerve excitability similar to that being developed by A.V. Hill in London. In addition to our work together with the cathode ray oscilloscope, I was to learn much about the mathematics of nerve excitability and to have experience with chronaxie measurements.

I found that Ali was also a keen sailor and together we sailed his small boat up and down the Seine on summer weekends. I also soon learned to speak fluent French so that I could present some of our work before the French biological society and at other scientific meetings in Paris.

I became acquainted with Alfred Fessard who was also setting up a cathode ray oscilloscope in his laboratories in the College de France. I met Henri Pieron, head of the department of psychology, and visited many other laboratories throughout France and Europe, including many of Ali's friends in Great Britain. I also met neuroscientists from other parts of the world by attending international meetings in Europe and England during my two-year fellowship at the Sorbonne, at the Marine Biological Laboratories in Roscoff, Brittany, where I spent two summers, and at the Marine Biological Laboratories in Naples, Italy, where I met J.Z. Young.

My work at the Marine Biological Station in Brittany was particularly important. I met many leading molecular biologists there and made lasting personal friendships, such as that of Jacques Monod who later received a Nobel Prize for his work on Messenger RNA, and was able to help me in the establishment of The International Brain Research Organization (IBRO) with UNESCO (he too was a keen sailor).

Also important were the lifelong friendships formed during my postdoctoral fellowship, including E.D. Adrian of Cambridge, Frederic Bremer of Belgium, Ragnar Granit of Stockholm, John Fulton of Yale, and Charles Sherrington and John Eccles of Oxford.

Brown University, 1932–1938

In 1932, I returned to the U.S. at the invitation of Drs. Arthur Ruggles and Leonard Carmichael of the departments of psychiatry and psychology of Brown University in Providence, Rhode Island, to establish a research laboratory at the Bradley Hospital in East Providence, adjacent to the Brown campus.

In the meantime, Connie had gone to Oregon to be with her parents for the birth of our daughter, Marilyn. When they arrived in Rhode Island we were put up in an elegant apartment in the hospital. Dr. Bradley, superintendent of the hospital, and his wife and daughter of about the same age, lived next door. Connie was very happy to have the company of Mrs. Bradley and her young daughter upstairs. They were both assisted by a kindly housekeeper.

For my studies, I was given a suite of rooms in the basement which were originally intended for an x-ray department. The walls were lined with lead which would serve as electrical insulation for our recording equipment.

I was soon able to acquire a good electronics engineer, Howard Andrews, from the department of physics. We were soon joined by Margaret Rheinberger, a neurophysiologist who had trained with Dr. John Fulton of Yale. She had experience in electrical recording and in operating on experimental animals. We soon had graduate students and a few colleagues to add to our staff, including Dr. Carmichael himself, who joined us in some of the experimental programs.

I learned from Dr. William Malamud, a German psychiatrist who had moved from the University of Iowa to Harvard, that he had been following the publications of Hans Berger on *Das Elektrenkephalogram* in German and thought that I should consider trying to repeat some of his work. I went to Boston to discuss this with him and to have him translate Berger's publications.

Howard Andrews started immediately to build an apparatus that would be suitable for recording the EEG. We then heard that Adrian had been invited by the American Neurological Association to speak at their next meeting in Atlantic City. I went to the meeting and was pleased to see Adrian again and to discuss his findings and those of Brian Matthews. Dr. Carmichael decided to join us in trying to record EEGs during the summer months of the same year, giving up our vacations for the purpose.

We were successful in obtaining very good records with the splendid apparatus that Howard had created. Howard himself was the first subject, along with other good subjects among the graduate students and staff of the hospital, but his brain waves were difficult to read. We found that Carmichael and I had excellent alpha rhythms which made it possible to confirm many of Berger's findings. We published the first United States paper on the EEG (Jasper and Carmichael, 1935).

We were in close competition with Hallowell Davis and his wife Pauline, and Alexander Forbes from the Harvard laboratories in Boston. They were soon joined by Erna and Fred Gibbs, William Lennox, Donald Lindsley, and Bill Derbyshire. Albert Grass joined them a little later as their electronics engineer. I kept in close touch with their work in Boston by attending many weekly seminars in the physiology department and meetings of the Boston Society of Neurology, where I became acquainted with Houston Merritt, Tracey Putnam, Raymond Adams, and Stanley Cobb.

It was obvious that we had discovered a method of recording the continuous electrical activity of the brain, which was very sensitive to states of mind and consciousness, sleep and waking, relaxation or anxiety, and arousal and attention, with both local and generalized responses to external and internal sensory stimulation of all kinds.

Dr. Adrian had shown that when the alpha rhythm was blocked, the eyes would open even in a completely dark room. We had shown that after-images when the eyes were closed would also cause blocking of the alpha rhythm, even in a completely dark room.

By implanting recording electrodes in cats, we soon observed the generalized slow waves of sleep. When we observed a change to rapid waves we had learned it to be a sign of waking. This proved to forewarn us of the cat needing to use the litter box. We also discovered that students whom we used as normal subjects would often fail to show an alpha rhythm just before a difficult examination, which proved to be a reliable indication of anxiety.

Alfred Loomis, a wealthy stock broker, had established important research laboratories in Tuxedo Park, New York. He became interested in electroencephalography and invited us to join him from time to time. On one such occasion he also invited Newton Harvey from Princeton, who brought Albert Einstein with him so that they could study Loomis' brain waves. They put him to sleep, and at first he showed the typical slow waves of sleep. Then the EEG changed to the rapid waves of arousal. He awoke suddenly, asking for a telephone. He called his laboratories in Princeton to tell his colleagues there that he had been reviewing his calculations of the day before and discovered an error which should be corrected. This done, he was able to go back to sleep again. We thus had a dramatic demonstration of the sensitivity of the EEG to mental activity.

The EEG was also sensitive to anesthetic agents and chemical agents, such as benzedrine. Furthermore, it was sensitive to brain metabolism, oxygen tension, pH, brain temperature, local or generalized blood flow, brain lesions such as tumor or trauma, and brain diseases, as in the epilepsies and encephalopathies.

I had been accustomed to using cathode ray oscilloscopes in my work with Ali Monnier in Paris, and I was not about to use any of the ink writers available at the time for fear of missing the more rapid components of the EEG. The precise amplifiers built by Howard Andrews, with direct current amplifiers, Westinghouse mirror oscillographs and photographic recording, assured us of accurate and complete records despite the trouble of getting them.

Woods Hole, Summers of 1932 and 1933

I had completed my research on crustacean neuromuscular systems for my doctoral thesis in Paris by spending the summers of 1932 and 1933 at the Woods Hole Marine Biological Laboratories. At the same time I was able to meet many leading neuroscientists from the United States and other countries who were then working at Woods Hole: Ralph Gerard, J.Z. Young, K.C. Cole, Harry Grundfest, Alan Hodgkin, Herbert Gasser, Francis O. Schmitt and his brother Otto, H.J. Curtis, Ladd Prosser, and a

few others whose names I have forgotten. My many discussions with them were important for my future in neuroscience.

Defending My Doctoral Thesis, Paris, 1935

I sent my thesis in English to Negro Monnier in Paris for translation into French, and defended it in Paris in the spring of 1935. My doctoral thesis was composed of two parts: "recherches sur l'excitabilité et les caracteres de la reponse dans le system musculaires des crustacés. Influences des centres ganglionaires" and "electroencephalographie chez l'homme."

I defended my thesis before a distinguished panel of examiners chaired by Professor Lapicque. The room was full of visitors, including Alfred Fessard, Ali and Andrée Monnier, and many other members of the staff and students. The examining committee seemed satisfied with the principle thesis on the crustacean neuromuscular system, though Lapicque was somewhat dismayed about the work I did with Monnier on "pseudochronaxies." All were enthusiastic about my second thesis on "electroencephalographie chez l'homme." I had to treat everyone to a champagne reception afterward as is the custom.

I stayed in Europe for most of the summer of 1935 in order to visit electroencephalographic laboratories which had sprung up in Europe following the publications by Hans Berger and Edgar Adrian with Brian Matthews. This stay included visits with Alfred Fessard and his co-workers, and Hans Berger and his charming family in Germany, where he was suffering from the advance of the Nazis. Berger was deprived of his position in the university and hospital and put under house arrest for his refusal to ask his students to stand and salute Hitler at every lecture. There was a large picture of Hitler placed on the wall behind his lectern. A few years later Berger went into a deep depression at seeing what was happening to his beloved country and committed suicide.

I also visited the Brain Research Institute of Oscar and Cecile Vogt in "Buch bei Berlin," where Toennies and Kornmuller had developed a very good apparatus for recording electrical activity from the brain in experimental animals. They thought that the pattern of brain waves from different areas of the cerebral cortex in monkeys corresponded to the cytoarchitectonic areas that had been described by the Vogts from their anatomical studies.

We had also documented differences in patterns from different sensory, motor, and association areas of the cortex, although not corresponding so precisely to the cytoarchitectonic areas that the Vogts had described.

In a sound-proofed office, well isolated from the rest of the institute, the Vogts told me that they and the Toennies were far more concerned about persecution by the Nazis than they were about the EEG. Their daughter, Marthe Vogt, asked me what she should do to further develop

her interest in brain research. I suggested that she move to England as soon as possible and concentrate on brain chemistry. She followed my advice, leaving immediately for England to escape the Nazis who were about to close their institute. We were able to get her a good fellowship in England where she made important contributions to the development of chemical transmission of the nerve impulses at synapses throughout the CNS in conjunction with the work of Sir Henry Dale and Arthur Feldberg.

I returned to Paris where I accompanied Ali and Andrée Monnier for a trip to England in their little Citroen car. We visited their friends and attended the annual meeting of the Physiological Society in Oxford, where we stayed with John Fulton and a few of his special friends in his rented manor. It was a remarkable meeting. Ivan Pavlov from Russia and Santiago Ramon y Cajal from Spain were given honorary degrees. Pavlov gave a principle address, primarily in Russian with simultaneous English interpretation, on the theme of conditioned reflexes in the higher cognitive functions of the brain.

The most memorable lecture, however, was given by Sir Charles Sherrington. It was his "swan song" because he was retiring immediately after the meeting. It was a brilliant discourse, mostly on higher level functions of the brain, the cerebellum which "molded body posture by inhibition," and continuous synaptic activity in the cerebrum like "flashing of fireflies on a summer evening." As usual, he gave credit to his students and colleagues for his remarkable contributions to neurophysiology.

We picnicked with Jack Eccles and his family on the Seine and met many British and foreign physiologists at the meeting. We also had a short visit with Pavlov.

We then visited A.V. Hill in London and visiting scientists Ralph Gerard from Chicago and Professor J.P. Fenn from China, who became a life-long friend who came to meet me and Hank McIntosh several times in Montreal.

We then went to the Maudsley Hospital where we met Dr. Golla and W.G. Grey-Walter, a former student of Adrian's who had started a most interesting EEG department. We also met Denis Hill and a young fellow by the name of William Cobb, who later established the EEG department at the National Hospital of Queen Square.

We completed our visits by going to Cambridge to visit Adrian and Matthews, who showed us their pioneering work on electroencephalography and what they called "The Berger Rhythm." Adrian had demonstrated his own excellent Berger Rhythm before the Royal Society in London.

I heard from John Fulton about the meeting of the World Neurology Association at which he was chairing a symposium on the frontal lobes. Carlyle Jacobsen presented his classical work on the frontal lobes of chimpanzees which gave Walter Freeman, a neuropathologist from Washington and Egas Moniz from Portugal, the idea of starting to treat

mental diseases by prefrontal lobotomy. I reviewed this sad but interesting story recently in a book on "Epilepsy and the Functional Anatomy of the Frontal Lobe" (Jasper, 1995).

Montreal, 1937–1965

My colleagues and I had heard of Penfield's observations on the exposed cortex of patients who were being treated for focal epilepsy. We invited him to Brown to give a seminar in the psychology department. We were all impressed by his research on electrical stimulation of local cortical areas in conscious patients during surgery for the treatment of focal epilepsy.

Penfield visited our laboratories at the Bradley Hospital to see our work on electroencephalography for the localization of focii in patients with focal epilepsy. He was skeptical of our work with the EEG, having heard nothing about the EEG before. He agreed, however, to operate on a couple of my patients.

I sent two patients to Montreal whom he considered suitable for operation. I then drove to Montreal on weekends with EEG recording equipment in the back seat of my car in order to be able to record from the exposed cortex on our patients during the operative procedures. Fortunately, Penfield found objective focal lesions in each patient just beneath our EEG localization. He then agreed to take a few more patients, and we continued our commuting collaboration for the rest of the year with satisfactory enough results that he was willing to consider my joining his team in Montreal. However, there was neither money to pay for me and my staff, nor space for our laboratories.

Penfield was able to have my Rockefeller grant moved to Montreal. He then managed to build an addition to the institute, providing good space for our clinical examinations with some room for experimental laboratories. Our electronic laboratories were housed in a storage room in the basement.

I moved to Montreal in 1938. We opened our EEG department with a symposium in February 1939. Most of the leading EEG workers from the United States and Canada were present for the opening ceremonies. After visiting the institute and observing an operation by Dr. Penfield on an epileptic patient with direct electrical recording from the exposed cortex, we held a short symposium before taking off for the Laurentian mountains to spend the weekend in a ski resort. There, at Domaine d'Esterel near St. Marguerite, we continued our scientific meeting and took time out for skiing. We also had a downhill race in which most participated, including Dr. Penfield. This proved to be the first of annual ski meetings by the Eastern EEG Association which have continued ever since.

My time with Wilder Penfield and his family, in which I became an adopted member, working with his splendid enthusiastic staff and hun-

dreds of colleagues and students from all over the world who worked with us, was certainly the most pleasant and productive 27 years of my life.

I began with ward rounds each Monday morning, which were attended by all members of the staff and students, with active participation of all departments of the institute. It was Penfield's dream to create a multidisciplinary neuroscience institute in which the basic sciences worked closely with the clinicians and the laboratories of radiology, neuropathology, neurochemistry, neuroanatomy, neuropsychology, and, of course, with electroencephalography and neurophysiology, in a fusion of clinical and basic research. This was a forerunner of what soon became what we now know as neuroscience.

I was delighted to take part in the realization of Penfield's dream, which soon became my own as well; it became for me an international as well as an interdisciplinary dream.

I had two excellent assistants for setting up and starting work in the EEG department. The electronics engineer, André Cipriani, was extraordinarily competent. He was able to make all the recording equipment himself with the aid of a well-equipped electronics and machine shop. He made three sets of four-channel ink writing oscillographs with their appropriate, well-designed amplifiers. André had honors degrees from Jamaica in physics and electronics and a medical degree from McGill. He was also a delightful, good humored fellow.

I was also able to get Dr. Penfield's chief scrub nurse from the operating room, Mary Roach, to take over the management of our neurophysiological laboratories so that our experimental operating rooms would meet the standards to which she was accustomed. She left during World War II to take charge of Army hospitals, going with the front line troops during the invasion of Italy. She returned from the war with some Italian assistants who worked with her in our neurophysiological laboratories.

Dr. John Kershman, an accomplished neurologist and neuropathologist from London, England, also joined me in the EEG laboratories, conducting detailed studies of more than 1,000 epileptic patients whom we reviewed in gathering material for our paper (Jasper and Kershman, 1945).

Our review included a clinical history and examination, together with radiographic studies, on over 1,000 epileptic patients, quite a few of whom had been operated on by Dr. Penfield.

It was in 1941 that I also published my first comprehensive treatment of "electroencephalography" as a chapter in a book by Penfield and Erickson (Jasper, 1941). Ted Erickson was chief resident in neurosurgery at the time. In that same year I published another paper with him on (Jasper and Erickson, 1941) using apparatus made by Andy Cipriani some time before (Jasper and Cipriani, 1940). This was the first of a long series of neurophysiological experiments we conducted with neurosurgical resi-

dents as part of their graduate training. They were all excellent workers. In 1941 I also published a review on "Electrical Activity of the Brain" (Jasper, 1941).

During my first two years in Montreal we were examining 2,000 to 3,000 patients in the EEG department, in addition to having weekly epileptic conferences and several operations with Dr. Penfield. I worked with so many patients that I felt I needed more medical training, and so I started medical school in 1940 as a regular student, while also being on the faculty teaching some of the courses myself. World War II had begun and the medical course had been reduced to three years, but we had begun some war research as well.

I fell in love with one of the nurses, Margaret Goldie, who was a formidable opponent on the tennis court (Connie and I had divorced). Goldie and I were married in 1940 in her hometown of Guelph, Ontario, with Robert Pudenz as my best man and Montreal Neurological Institute (MNI) fellows as ushers at the wedding. Goldie, as she was known, settled into a new house and helped me with my medical studies. She continued to work in the EEG and neurophysiology departments and to perform clinical and operative work with Dr. Penfield and his staff. Goldie and I have two fine children, Stephen and Joan.

With Goldie's help and that of several fellow students I was able to pass most of the examinations in medical school, even though I was not able to attend all the classes. However, after a couple of years of maintaining such a hectic schedule, I came down with lymphocytic choreomeningitus, a form of nonparalytic polio, and was hospitalized for several weeks. I missed graduation with my class of 1943a and had to join 1943b for my final year. I then went immediately to Army camp to train to be a captain in the Canadian Army Medical Corps, while Goldie became a Red Cross nurse, both of us in uniform.

Through the remainder of the war I conducted war research for the Army (air transport of the wounded and antibiotics for treatment of wounds, electromyography of nerve injuries), the Navy (development of antiseasickness pills), and the Air Force (studies in the physiology of pilot blackout during battle maneuvers and their protection with a blackout suit). We were also engaged in the use of electroencephalography for the selection of pilots for the Air Force.

Dr. William Cone, who had been involved in our studies of the action of antibiotics on the brains of experimental animals, organized a neurological hospital in Basingstoke, England. John Kershman joined the Air Force and Andy Cipriani was appointed chief of the Atomic Research Laboratories near Ottawa.

We recruited K.A.C. (Allan) Elliott, a distinguished South African brain chemist trained in England, who was working at the University of Pennsylvania. Allan joined us to help improve our treatment of brain

swelling and edema in the many casualties who were returning from combat with severe head injuries.

Allan established the Donner Laboratories of neurochemistry adjacent to the neurophysiology laboratories. We were soon in close collaboration which enabled me after the war to branch into brain chemistry. We were joined by Donald Tower, Niko Van Gelder, Hannia Pappius, Leon Wolfe, Hugh McLennan, and others. Allan also became a close personal friend; we took many ski trips in the mountains and we sailed on Lake Memphramagog together.

The Post-War Surge of Research Fellows, 1945–1955

We had more than 100 research fellows during the 10 years following the war. The first was Jan Droogleever Fortuyn, a distinguished neuroanatomist from The Netherlands. He had received a Rockefeller Fellowship before the war, but had been interned by the German occupation. He and his wife had suffered terrible hardships and persecution and the loss of so many of their Jewish friends during the Holocaust. His wife was a psychiatrist and well-known poet, and had helped the government to solve labor and bilingual problems between the Flemish and Dutch cultures in Holland. After a period of readjustment, Jan and his wife made a big contribution to our research program at the institute. Also of particular importance was the arrival of Cosimo Ajmone-Marsan from Italy, Jerzey Olszewski from Poland, and many others.

Space does not permit me to mention many of the publications done with these colleagues in this brief review. I will include a few in the references at the end of this chapter. I would like to mention two highlights which occurred in 1954 which gives a summary of some of this work: book published with Wilder Penfield on *Epilepsy and the Functional Anatomy of the Human Brain* (Penfield and Jasper, 1954, Boston, Little Brown & Co. pp 1-896) and *Brain Mechanisms and Consciousness* by Adrian E.D., Bremer F., Jasper H.H. and Delasfresnaye J.F., 1954, Oxford, Blackwell Sci. Pub. pp:vii–556.

The International Congress of Physiological Sciences was held in Montreal in 1954. Dr. Penfield and I had just completed our book summarizing much of our work together (Penfield W.G. and Jasper, 1954) with a summary of some of my work with other colleagues. I met many of the leading neuroscientists from around the world, including many from the Soviet Union.

Another event of great importance in 1954 was the Congress of Physiological Sciences, where I had the pleasure of being associated again with many neuroscientists in a satellite symposium. The Congress was held in the Laurentian Mountains near Montreal and our symposium was titled "Brain Mechanisms and Consciousness" (Adrian, E.D., et al., 1954). It has also become a classic in the literature. I served as chairman and

principal organizer of the Congress, with papers and discussion provided by H.W. Magoun, J. Olszewski, W.J.H. Nauta, D.G. Whitlock, W.R. Hess, F. Bremer, M.A.B. Brazier, A.E. Fessard, E.D. Adrian, H. Gastaut, W. Penfield, R. Jung, W.G Grey-Walter, D.O. Hebb, K.S. Lashley, L.S. Kubie, and D. McK. Rioch. I also presented my own paper, "Functional Properties of the Thalamic Reticular System."

The papers, and especially the discussion, of this international symposium on consciousness were most exciting and certainly provided a landmark in the development of neuroscience research into basic mechanisms of consciousness, with many more to follow.

Brain mechanisms of consciousness and unconsciousness have been a central theme of my life in neuroscience research. Studies of the "basic mechanisms of the epilepsies," which we have made the subject of a series of international symposia with the help of the National Institutes of Health (NIH), have dealt with this subject in the context of epileptic seizures beginning with loss of consciousness. These have been called "centrencephalic" or "cortico-reticular seizures," with reference to hypothetical brain mechanisms involved. I discussed this question in some detail in a 1991 guest editorial in the *EEG Journal.*

Neurochemical and Microelectrode Studies

After studying thousands of human EEG records, hundreds of electrocorticograms from the exposed human cortex during operations with Dr. Penfield, and extensive research on the electrical activity of different brain regions from implanted electrodes in experimental animals, I became dissatisfied with the limitations of these techniques. I was convinced that we needed to know far more about the firing of individual brain cells in various cortical and subcortical regions in waking animals and humans. I was also convinced that we needed to know much more about the neurochemical basis of neuronal excitability and the transmission of impulses at synapses.

Neurochemical Studies

With the help of Allan Elliott, an outstanding neurochemist, we were able to tackle some of these neurochemical problems. We wanted to make use of the samples of epileptic cortical tissue from epileptic patients that had been extracted during operations. Dr. Elliot and Donald Tower first sought excitatory substances such as acetylcholine (ACH), but they were unable to find consistent changes in ACH metabolism in focal epileptic tissue.

We then wondered if a lack of an inhibitory substance controlled the excitability of the brain. We invited Dr. Ernst Flory and his wife Elizabeth from the California Institute of Technology to join us because he had discovered, in a brain extract, a substance with strong inhibitory effects on

the crustacean stretch receptor, which he called *Factor I* (for inhibition). We worked together with Allan Elliott on the analysis of Factor I.

We had heard of children who had seizures because of a lack of some substance in their commercially prepared milk. It proved to be vitamin B6, which is essential for the production of an amino acid known as GABA (gamma amino butyric acid). Injection of this vitamin would control the seizures. We then thought that Ernst Flory's Factor I might be GABA.

We had great trouble isolating GABA from the inhibitory factor in brain extracts, but with the aid of an analytical chemist from the Merck Company, we succeeded in purifying a crystalline substance that proved to be GABA. Confirmation of its inhibitory action on cortical tissue in animals was then carried out with success (Jasper, 1984).

We began systematic studies of all the amino acids that were released or extracted from the cortex in experimental animals during sleep and waking. Following experimental epilepsy, we performed chemical analysis on cortical tissue and on the fluids extracted from the cortex by a superfusion technique using a specially designed perfusion chamber that we installed in the skull, resting upon the cortical surface after removing the dura and penetrating the piarachnoid covering with microelectrodes. This made it possible to sample the local fluid in the cortex in animals without general anesthesia, during sleep and waking, during learning experiments, and following local experimental epileptogenic lesions.

We performed the analysis with the help of Leon Wolfe and a superb analytical chemist from Japan, Ikuko Koyama. We began these studies at MNI and continued them after I moved to the University of Montreal, where Ikuko worked closely with me and with Nico Van Gelder. We also collaborated with Dr. Tomas Reader, a postdoctoral fellow from Buenos Aires, Argentina.

We performed an analysis for the presence of ACH, all of the amino acids, and finally for the monoamines noradrenaline, serotonin, and dopamine.

We also conducted ACH determinations with an Italian postdoctoral fellow, Gaston Celesia, who later became editor of the *EEG Journal*. With Celesia we found that the liberation of ACH was increased three to four times in concentration when the cats were awakened from natural sleep, or during electrical stimulation of the brain stem reticular formation to awaken or arouse the animals (Celesia and Jasper, 1966). Arousal consistently produced a marked increase in ACH liberation as compared to slow wave sleep.

We also found local epileptiform spikes produced from cortex treated with eserine or physostigmine when the animal was aroused by stimulation of the brain stem reticular formation.

Later, with another fellow at the University of Montreal, J. Tessier, we found that there was an increase in the liberation of ACH during rapid eye movement (REM) sleep as compared to that in slow wave sleep (Jasper and Tessier, 1971). We discovered with J.H. Ferguson that ACH, when

applied to the cortical surface, after preventing its hydrolysis with eserine, resulted in a very large sustained epileptic discharge which could be triggered by weak sensory stimulation in the cat. We concluded that ACH was a powerful convulsant agent when applied under certain conditions (Ferguson and Jasper, 1971).

In summary, we found that changes in the concentration of glutamic and aspartic acids, with sometimes an increase in taurine, and with a decrease in GABA, were characteristic of epileptogenic cortical tissue. Arousal caused an increase in glutamic acid as well as ACH, with a decrease in GABA. Sensory stimulation seemed to decrease the liberation of the monoamines. Microiontophoretic studies showed important interactions between ACH and the liberation of monoamines, suggesting a presynaptic effect of ACH on monoamine terminals.

For my 80th birthday celebration in 1986, and to my great pleasure, my colleagues at the University of Montreal, Tom Reader and Bob Dykes, and Massimo Avoli and Peter Gloor at the MNI of McGill University, organized a remarkably fine comprehensive symposium titled "Neurotransmitters and Cortical Function: From Molecules to Mind." I collaborated with them in writing an overview of this splendid symposium with the title "Molecular Controls and Communication in Cerebral Cortex," (Jasper et al., 1988) from which I will quote a few excerpts:

> **Significance of cortical neurotransmitters**
> The contributors to the present symposium have provided many fascinating and important highlights of recent research on the many neurotransmitters or modulators which have played a leading role in the remarkable advances being made during recent years in our understanding of chemical and molecular mechanisms involved in the organization of cortical function. In this final chapter we shall attempt to present some of our impressions of the overall importance of these developments with an emphasis on the subtitle of this book, "From Molecules to Mind."
>
> **Amino Acids**
> It would seem that the only good candidates for the chemical synaptic mediation of the rapid transient transmission of excitatory and inhibitory actions on specific information processing, cognitive, and specific motor functions of cerebral cortex are amino acids (glutamic and aspartic acids), which are universally excitatory, while GABA is the major, if not the only, generally active inhibitory substance in cerebral cortex. All of the other neuroactive substances found in cerebral cortex have slower and longer lasting effects, modulating excitability and the action of

other neurotransmitters. Some may be "cotransmitters" as shown by Jones in this volume with his immunocytochemical studies of glutamic acid decarboxylase (GAD, the enzyme for GABA synthesis from glutamate) and certain peptides.

It would seem to be of considerable importance that the metabolism of glutamate and GABA are so closely interrelated, GABA being produced by the decarboxylation of glutamate by means of a specific enzyme GAD, together with the coenzyme pyridoxine phosphate (vitamin B6). Rate limiting steps in the synthesis of both GABA and glutamate are also closely related as shown by Szerb in this volume.

The fact that the most important excitatory substance can be the immediate precursor of the most important inhibitory substance suggests that these interrelationships may be relevant to the maintenance of a balance in excitatory and inhibitory controls in synaptic mechanisms involved both in information processing as well as in integrative motor control. Defects in GABA mediated inhibitory controls may lead to epileptic discharge as described by Avoli, and may abolish pattern discrimination in cells of visual cortex, as shown by Sillito and Murphy. Dykes et al. have shown that blocking of GABA action by bicuculline enlarges and blurs receptive fields of single cells in somato-sensory cortex.

Thus, GABA may play a leading role in all higher integrative functions of cerebral cortex in which patterns of excitation are being molded by inhibition.

The specific ionic channels mediating the excitatory properties of glutamate and aspartate have not been clearly elucidated, but they probably involve both Na and Ca conductances. The ionic mechanism of inhibition by GABA involves Cl channels. GABA receptors are very closely related and coupled to benzodiazepine receptor sites as described by Lambert et al. The barbiturates may also act, in part, via the GABA system, further increasing the importance of GABA in such physiological phenomena and pointing to its importance in neuropharmacology.

Kris Krnjevic described microelectrode studies of the excitatory action of Acetylcholine on cortical cells. He concluded that ACH does not act like a classical neurotransmitter at cortical synapses. Cellular impedance is increased instead of decreased with excitation, and the resulting excitation has a slow onset and prolonged duration. The effect is thought to be due to blocking of K^+ channels, thus prolonging the action of other transmitters. A second messenger such as cyclic GMP might be involved acting via some form of protein phosphorylation.

ACH plays a most important role in regulating the state of reactivity of cortical cells, as in sleep and waking and attention, as well as in the reinforcement and prolongation of cortical and hippocampal synaptic activity important in mechanisms of memory, as suggested by its deficiency in Alzheimer's disease.

Peptides

The discovery and localization of over 60 neuropeptides, together with their specific receptor proteins in the brain has presented us with complex problems of understanding their functional significance, since such significance has been determined in only a few of them (e.g., substance P, enkephalin, somatostatin, and endorphin).

Microelectrode Studies

I had already begun microelectrode studies of single neuronal cell firing in various depths of cerebral cortex and, with stereotaxic techniques, from subcortical structures in basal ganglia, thalamus, and brain stem (Li and Jasper, 1953), but we needed to develop techniques for single cell recording in conscious behaving animals and humans.

With the help of a former student, David Hubel, who was then working at the Walter Reed Army Medical Research Laboratories in Washington, D.C., I learned how to make tungsten microelectrodes for single cell recording, which I developed further in Montreal with recording chambers and micrometer controls that enabled not only the recording of the electrical activity from single cells in waking experimental animals and humans, but also the collection of fluid from local areas of cerebral cortex, and from subcortical regions, for biochemical analysis.

In collaboration with Dr. Gilles Bertrand, we were able to perfect a stereotaxic microelectrode technique for recording from single cells in the human thalamus and basal ganglia during operations for the treatment of Parkinson's disease (Jasper and Bertrand, 1966a, b, and c).

I also carried out intracellular microelectrode studies in collaboration with Costa Stefanis, a postdoctoral fellow from Athens, Greece, who eventually became professor of psychiatry at the University of Athens, and president of the mental health division of the World Health Organization (Stefanis and Jasper, 1964; Jasper and Stefanis, 1965).

The use of microelectrode techniques to study states of sleep and waking, epileptic discharges, attention, conditioning, and other states served to open many new vistas of neuroscience research that were to occupy me for the rest of my professional life.

With the help of two excellent postdoctoral fellows, Gianfranco Ricci from Rome and Ben Doane from Hebb's department of psychology at McGill, we studied unit firing from many areas of the cerebral cortex in

monkeys during avoidance condition to specific frequencies of photic stimulation (Jasper, et al., 1960). We presented our results at the Moscow Colloquium in 1958 (Jasper and Smirnov, 1960).

There were 49 official members of this colloquium hosted most generously by the Academy of Science. There were also many nonparticipating guests who came mostly from the Soviet Union. Participants were seated around a large oblong table. We heard the presentation of many exciting papers and had animated discussions. I was privileged to share with Academician I.S. Beritashvili from Tiflis, Georgia, the position of honorary president. Acting presidents were Professors H. Gastaut and V.S. Rusinov.

I quote from the welcoming remarks of Academician A.V. Topchiev, the vice president of the Academy of Science of the USSR, as follows:

> We hope that this colloquium will not only be an important landmark in the further development of the scientific problems you are elaborating, but that it will consolidate the friendly ties between the participants of the colloquium, based on reciprocal contacts and exchange of experience. This will, in turn, further the noble task of establishing mutual understanding and friendship among the nations of the world.

At the close of the sessions we passed a unanimous motion to continue and enlarge the international collaboration we had so much enjoyed by sending a delegation to UNESCO to form a permanent international interdisciplinary brain research organization to promote continued international and interdisciplinary collaboration throughout the world. Professor Alfred Fessard and I headed this delegation.

With the help of UNESCO and the Council for the International Organization of Medical Sciences (CIOMS), together with an interdisciplinary organizing committee, we were successful in establishing a truly international and interdisciplinary brain research organization known as the International Brain Research Organization (IBRO) in 1960, which was officially incorporated by the Parliament of Canada as an independent international body with headquarters in Paris.

I moved to Paris with my family for one year and then spent four years, almost full time, getting IBRO established. We undertook many successful projects, with UNESCO publishing bulletins telling of our activities: fellowships, symposia, workshops, etc., and an important survey of neuroscience in many countries. These national surveys resulted in the establishment of many national and regional organizations for brain research or neuroscience.

The survey in the United States was aided by the National Academy of Sciences and the National Institutes of Health following a special visit by Dr. V.G. Longo of Italy and me to Washington, D.C. This visit resulted

in the formation of the Brain Research Commission and, eventually, of the Society for Neuroscience (SFN) 10 years later, in 1970.

Celebration of the 25th anniversary of the Society for Neuroscience in San Diego in 1995 was preceded by a celebration of the 35th anniversary of IBRO, which had become the world federation of neuroscience, with about 35,000 members.

I gave an opening address at this Fourth International Congress of IBRO, held in Kyoto, Japan, under the presidency of Professor Masao Ito. This was another outstanding experience of my life, which I enjoyed with my wife, Mary Lou, and with the excellent hospitality of our Japanese colleagues.

In my opening address I recalled the Moscow Colloquium, the birthplace of IBRO, and the resolution that we would try to make IBRO not only a medium for the international and interdisciplinary collaboration of neuroscientists, but that we should use our personal contacts with neuroscientists worldwide to improve friendly relations among nations. After the close of the Moscow Colloquium in 1958, I had had a private conference with the president of the Soviet Academy of Science which resulted in his agreement to cooperate with our international efforts in this direction.

I suggested that brain research might be an excellent channel for the promotion of better international relations because so many of these problems are based on malignant mental attitudes that might respond to scientific studies of brain function as a determinant of social behavior. I feel strongly that modern neuroscience, with all of its advances during recent years, should be used to apply knowledge and techniques to the understanding and prevention of such malignant mental attitudes that form the basis for so much conflict.

Time and space do not permit continued elaboration of the rapidly growing field of neuroscience research. Such discussion does not belong in my autobiography anyway, since I have been struggling to follow the rapid advances in this field as shown in a recent publication titled "Early efforts to find neurochemical mechanisms in epilepsy" (Jasper, 1992) in which I described these developments as "a new ball game."

Acknowledgments

I would like to acknowledge the many friends, colleagues, and students I have had over these many years who have contributed largely to the success of my research endeavors and to the pleasure of my career in neuroscience. I am especially indebted to the hundreds of staff members and students who collaborated with me for 27 years at the MNI and to the staff members and students who collaborated with me over the past 30 years at the University of Montreal, a collaboration which is still continuing today.

I also would like to pay tribute to the work of Francis O. Schmitt who organized the Neuroscience Research Program (NRP) in which I took an

active part for many years (1962–1975). Of particular importance in relation to the present *The History of Neuroscience in Autobiography* is the first such history organized to celebrate Frank Schmitt's 70th birthday at the Massachusetts Institute of Technology in October 1973 (Worden et al., 1975). Twenty nine of the leading neuroscientists contributed chapters to the book. My own chapter was titled "Philosophy of Physics—Mind or Molecules."

I was particularly involved in the final NRP Colloquium on "The Organization of the Cerebral Cortex," held at Woods Hole, Massachusetts in 1979 (Schmitt et al., eds., 1981). I was a contributing editor in the section on "The Role of the Cerebral Cortex in Higher Brain Function." My chapter was titled "Problems of Relating Cellular and Modular Specificity to Cognitive Functions: Importance of State Dependent Reactions."

Prizes and Awards

I have recently received numerous prizes and awards, which manifest the support I have had from many sources. I will mention only a few: the Ralph Gerard Prize of the Society for Neuroscience; officer of the Order of Canada; the McLaughlin Medal of the Royal Society of Canada; the F.N.G. Starr Award of the Canadian Medical Association; election into the Canadian Medical Hall of Fame; the Albert Einstein World Science Award of the World Cultural Council, received in Mexico City, December 1995; and Le Grand Officier de l'Ordre National du Quebec, June 1996.

I would like to share these prizes with my father, who was my original inspiration, and with the hundreds of colleagues with whom I have lived and worked for more than 70 years of dedication to research on the brain and its relation to the mind and behavior.

Finally, I would like to express my most sincere appreciation to the Society for Neuroscience which, for 25 years, has been a constant source of inspiration and information about progress in all branches of neuroscience. I would particularly like to thank the Society's executive director, Nancy Beang, for her personal friendship and her remarkable organization of annual meetings and other activities of the Society, together with a dedicated series of presidents, councilors, and committee members.

I also thank Larry Squire and the SFN Publications Committee for this opportunity to contribute to the first volume in this series, *The History of Neuroscience in Autobiography*, with such a distinguished group of colleagues.

I also express great appreciation to my wife, Mary Lou McDougall Jasper, for her constant support in these strenuous endeavors and for her help in the preparation of this manuscript.

Selected Publications

Adrian ED, Brémer F, Jasper HH, Delafresnaye JF. Brain mechanisms and consciousness. Oxford: Blackwell Scientific Publications, 1954;v–556. (Published simultaneously by Charles C. Thomas, Springfield, IL, and Ryerson Press, W. Toronto, Canada.)

Avoli M, Reader TA, Dykes RR, Gloor P. Neurotransmitters and cortical function: from molecules to mind. New York: Plenum, 1988;v–619.

Celesia GG, Jasper HH. Acetylcholine released from cerebral cortex in relation to state of activation. *Neurology* 1966;16:1053–1064.

Ferguson JH, Jasper HH. Laminar DC studies of acetylcholine activated epileptic discharge in cerebral cortex. *Electroencephalogr Clin Neurophysiol* 1971;30:377–390.

Jasper HH. Cortical excitatory state and synchronism in the control of bioelectric autonomous rhythms. *Cold Spring Harb Symp Quant Biol* 1936; 4:320–338.

Jasper HH, Cipriani A. A method for the simultaneous recording of focal cerebral blood flow, pH, electrical activity, and blood pressure. *Am J Physiol* 1940;128:485–492.

Jasper HH, Kershman J. Electroencephalographic classification of the epilepsies. *Arch Neurol Psychiatr* 1941;45:903–943.

Jasper HH, Droogleever-Fortuyn J. Experimental studies of the functional anatomy of Petit Mal Epilepsy. *Res Publ Ass Nerv Ment Dis* 1947;26:272–298.

Jasper HH. Charting the sea of brain waves. *Science* 1948;108:343–347.

Jasper HH Diffuse projection systems; the integrative action of the thalamic reticular system. *Electroencephalogr Clin Neurophysiol* 1949;1:405–420.

Jasper HH, Ajmone-Marsan C. Thalamocortical integrating mechanisms. *Res Publ Ass Nerv Ment Dis* 1952;30:493–512.

Jasper HH. Unspecific thalamocortical relations. In: Field J, Magoun HW, Hall VE. *Handbook of Physiology; Neurophysiology II.* Washington, D.C.: Am Physiol Soc, 1960;1307–1321.

Jasper HH, Ricci G, Doane B. Microelectrode analysis of cortical cell discharge during avoidance conditioning in the monkey. In: Jasper HH, Smirnov GD, eds. The Moscow colloquium on electroencephalography of higher nervous activity. *Electroencephalogr Clin Neurophysiol* 1960;Suppl 13:137–155.

Jasper HH, Smirnov GD, eds. The Moscow colloquium on electroencephalography of higher nervous activity. *Electroencephalogr Clin Neurophysiol Suppl* 1960;13.

Jasper HH, Stefanis C. Intracellular oscillatory rhythms in pyramidal tract neurones in the cat. *Electroencephalogr Clin Neurophysiol* 1965;18:541–553.

Jasper HH, Bertrand G. Recording from microelectrodes in stereotaxic surgery for Parkinson's disease. *J Neurosurg* 1966a;24:219–221.

Jasper HH, Bertrand G. Stereotaxic microelectrode studies of single thalamic cells and fibers in patients with dyskinesia. *Trans Am Neurol Assoc* 1966b;79–82.

Jasper HH, Bertrand G. Thalamic units involved in somatic sensation and voluntary and involuntary movements in man. In: Purpura DF, Yahr MD, eds. *The thalamus*. New York: Columbia University Press, 1966c;365–390.

Jasper HH, Tessier J. Acetylcholine liberation from cerebral cortex during paradoxical (REM) sleep. *Science* 1971;172:601–602.

Jasper HH. Philosophy or physics—mind or molecules. In: Worden FG, Swazey JP, Adelman G, eds. *The neurosciences: paths of discovery*. Cambridge: MIT Press, 1975;401–422.

Jasper HH. The problem of relating cellular or modular specificity to cognitive functions: importance of state dependent reactions. In: Schmitt FO, Worden FG, Adelman G, Dennis SG, eds. *The organization of cerebral cortex*. Cambridge: MIT Press, 1981;375–393.

Jasper HH. Margaret Goldie Jasper, a tribute. *Electroencephalogr Clin Neurophysiol* 1983;56:534–535.

Jasper HH. The SAGA of K.A.C. Elliott and GABA. *Neurochem Res* 1984;9:449–460.

Jasper HH, Reader TA, Avoli M, Dykes RW, Gloor P. Molecular controls and communication in cerebral cortex: an overview. In: Avoli M, Reader TA, Dykes RW, Gloor P. *Neurotransmitters and cortical function: from molecules to mind*. New York: Plenum, 1988;593–605.

Jasper HH. Current evaluation of the concepts of centrencephalic and corticoreticular seizures. *Electroencephalogr Clin Neurophysiol* 1991;78:2–11.

Jasper HH. Early efforts to find neurochemical mechanisms in epilepsy. In: Avanzini G, Engel J Jr, Fariello RG, Henemann G, eds. *Neurotransmitters in epilepsy*. Amsterdam: Elsevier, 1992;(*Epilepsy Res* Suppl No 8)1–8.

Jasper HH, Riggio S, Goldman-Rakic PS. Epilepsy and functional anatomy of the frontal lobe. *Advances in neurology*, Vol. 66. Philadelphia: Lippincott Raven Press, 1995.

Levi-Montalcini R. NGF: an uncharted route. In: Worden FG, Swazey JP, Adelman G, eds. *The neurosciences: paths of discovery*. Cambridge: MIT Press, 1975;244–265.

Li CL, Jasper HH. Microelectrode studies of the electrical activity of the cerebral cortex in the cat. *J Physiol* 1953;121:117–140.

Magoun HW. The ascending reticular activating system. *Res Publ Assoc Nerv Ment Dis* 1952;30:480–492.

Morison R, Dempsey EW. A study of thalamo-cortical relations. *Am J Physiol* 1942;135:281–292.

Mountcastle VB. An organizing principle for cerebral function: the unit module and the distributed system. In: Edelman GM, Mountcastle VB, eds. *The mindful brain*. Cambridge: MIT Press, 1978;7–50.

Mountcastle VB, Edelman GM, eds. The mindful brain: cortical organization and the group selection theory of higher brain function. Cambridge: MIT Press, 1978.

Penfield WG. Epileptic automatism and the centrencephalic integrating system. *Res Publ Assoc Nerv Ment Dis* 1952;30:513–528.

Penfield WG, Jasper HH. Epilepsy and the functional anatomy of the human brain. Boston: Little Brown, 1954;1–896.

Schmitt FO, Worden FG, Adelman G, Dennis SG, eds. The organization of the cerebral cortex. Cambridge: MIT Press, 1981;v–592.

Schmitt FO, ed. Macromolecular specificity and biological memory. Cambridge: MIT Press, 1962.

Sharpless S, Jasper HH. Habituation of the arousal reaction. *Brain* 1956;79:655–680.

Stefanis C, Jasper HH. Intracellular microelectrode studies of antidromic responses in cortical pyramidal tract neurones. *J Neurophysiol* 1964;27:828–854.

Worden FG, Swazey JP, Adelman G, eds. The neurosciences: paths of discovery. Cambridge: MIT Press, 1975.

Young JZ. Sources of discovery in neuroscience. In: Worden FG, Swazey JP, Adelman G, eds. *The neurosciences: paths of discovery.* Cambridge: MIT Press, 1975; 15–46.