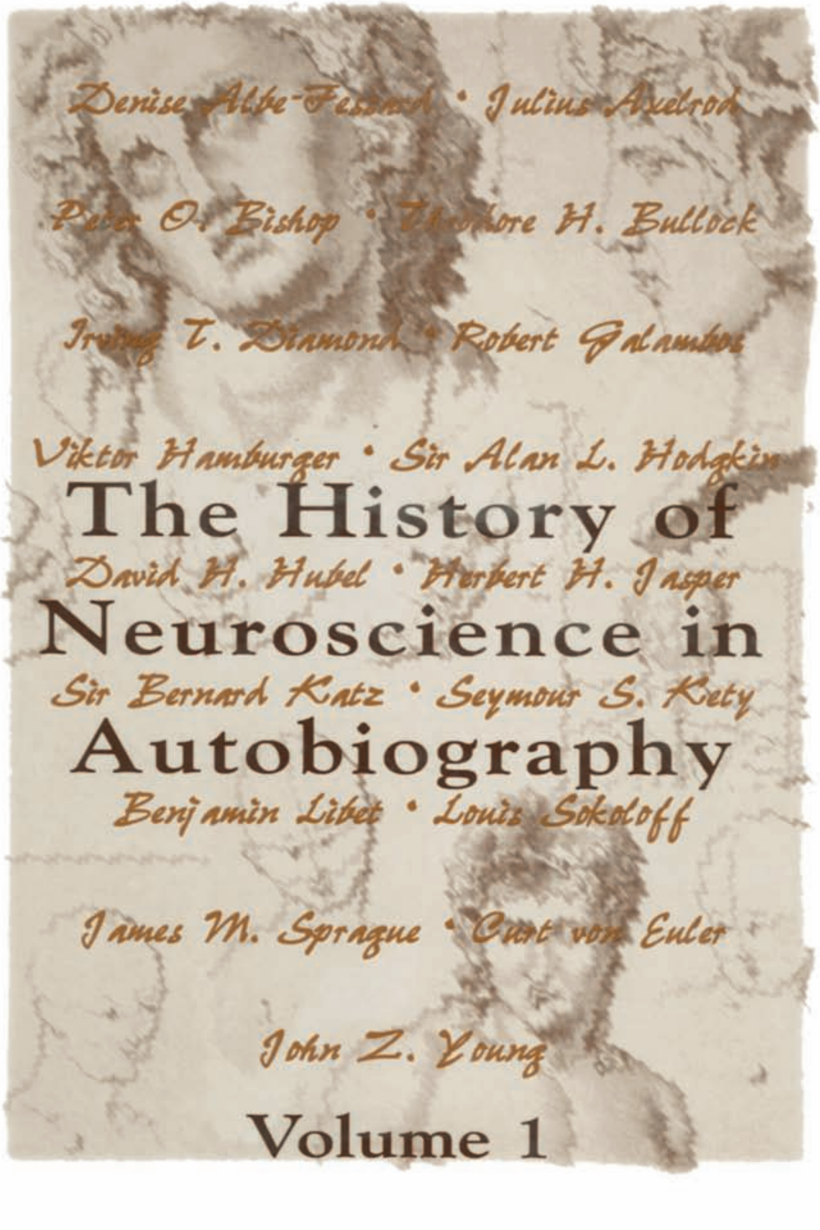

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The History of Neuroscience in Autobiography

Volume 1

Edited by Larry R. Squire

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Contents

Denise Albe-Fessard 2

Julius Axelrod 50

Peter O. Bishop 80

Theodore H. Bullock 110

Irving T. Diamond 158

Robert Galambos 178

Viktor Hamburger 222

Sir Alan L. Hodgkin 252

David H. Hubel 294

Herbert H. Jasper 318

Sir Bernard Katz 348

Seymour S. Kety 382

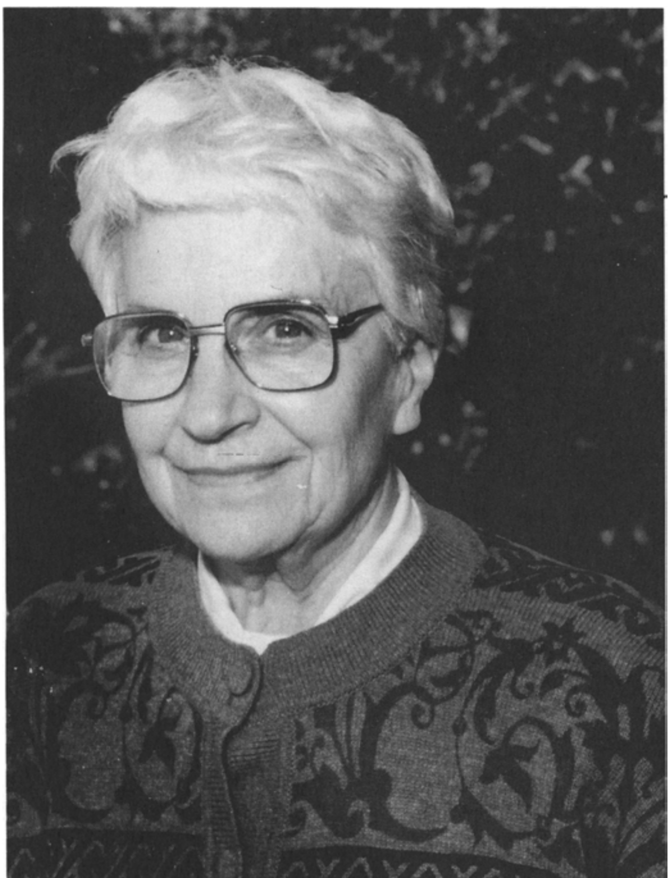
Benjamin Libet 414

Louis Sokoloff 454

James M. Sprague 498

Curt von Euler 528

John Z. Young 554



Denise Albe-Fessard

BORN:

Paris, France
May 31, 1916

EDUCATION:

School of Physique et Chimie de Paris (Engineering, 1937)
Paris University, Doctor és Sciences, 1950

APPOINTMENTS:

Sorbonne, Université Pierre et Marie Curie (1957–1984)

HONORS AND AWARDS (SELECTED):

Chevalier de la légion d'honneur (1973)
Officier de l'ordre du mérite (1978)
International Association for the Study of Pain (First President, 1975)

Denise Albe-Fessard has carried out fundamental neurophysiological work on the organization of central nociceptive pathways. Her major contributions have centered on distinguishing between separate medial and lateral thalamic centers in nociception.

Denise Albe-Fessard

Childhood and Training, 1916–1939

I was born in Paris in 1916 during World War I (the Great War). Although I was quite young, I remember sheltering in a cellar at night when the zeppelins bombed Paris. In 1918 when Big Bertha began to fire on Paris, my parents sent my siblings and me to live in the south of France with my mother's family. My parents, both from Languedoc, had lived in southern towns close to my father's work after their wedding. An engineer for the railways, my father was mobilized and was involved in the construction of military lines that carried troops and munitions to the front. He was employed by the railway company of the Midi before the Great War and was responsible for the construction of tracks linking isolated mountain villages in the Pyrénées and then in the Cévennes.

My parents came from peasant families. My mother's paternal grandparents were market gardeners in a village near Toulon. My father's maternal forebears were farmers in the plain of Hérault. Of the other two great-grandparents, one built stone houses in Nîmes and the other belonged to a family working a water mill on a coastal river, the Hérault. My great-grandfather operated the mill in Saint-Thibéri, but was deported to Algeria in 1848 with his two elder sons for giving food to republicans. My grandfather, another son, owned and operated the mill with his brother-in-law in the village of Bessan where my grandmother was born. Our family house still stands in Bessan, although the mill burned down after the birth of my father. The mill, constructed between the 13th and 15th centuries, is now almost totally in ruins, and only the dam is still in use.

These families of peasants and artisans wanted to provide a good education for their children, and so my father, Jacques Albe, and his two brothers became a teacher, a lawyer, and an engineer. To undertake the studies leading to these positions, they had to be boarders from the start of primary school in larger towns. They went home for only a month or two each year. My father began his studies in Béziers and finished them in Paris. On graduating from engineering school, he became an artillery officer at Nîmes, where he met my mother. They then settled in the Languedoc where their two families lived. Just before the Great War, my brothers were beginning their secondary studies and my father, who was then working in

Béziers, decided to accept a position in Paris to keep them with him and spare them the hard life in boarding school that he had known. That is why I was born in Paris, the fourth and last child. I was lucky, for at that time it was more acceptable in Paris than in the provinces for girls to have the same education as boys. In middle class families at the end of the 19th century, when my mother was a child, it was exceptional for women to have a career other than mother of a family. It was frequently claimed that women were intellectually inferior.

When she arrived in Paris at about 30 years of age, my mother spoke French with the southern accent that my father had lost during his studies there, and she passed on to all four of her children the singsong speech that the French north of the Loire often associated with lack of culture. I had this southern accent until I was 11; while attending high school, I understood that it had to be lost, and I took on the "pointu" accent of the Parisians.

My mother hoped that her younger daughter might one day pursue the studies that she herself had dreamed of, and she insisted that I be placed in the free state school, not in private school like my older sister. At that time in France, education in state school was solid but nonreligious, which often led it to be condemned by "bourgeois" families. Such education was, however, one of the good achievements of the third republic. We learned arithmetic and French in state school as well as the basic facts, unattractive but solid, of history and geography. Of the people who received this primary education, the best ones most often continued their studies in secondary education, which led them to the normal schools and allowed them in turn to teach in primary school. Only a few pupils from the state school went on to secondary education in a high school, which was not free. At 10 years of age, the most gifted children from the primary school took examinations for scholarships offering free secondary education. In my class of about 35 pupils there were only two of us who sat for this competitive examination. The headmistress prepared us for the exam, and we both succeeded and went to different high schools.

My father asked that I be placed in a class where living languages were taught, not Latin and Greek. He knew that I was particularly gifted for what was then called arithmetic and geometry, but not for languages. Having learned the importance of living languages from personal experience, he thought that they would be more important than the dead languages for a scientific education. So I learned English, and Spanish a little later. Languages were taught in a bookish way that did not assist communication. I learned English mainly from reading Shakespeare, which was of no help on my first trip to England, nor for my first literature searches. I am grateful to my professor of Spanish, who made us read in the language after the first year.

At high school, the history of ancient civilizations, which encompassed our own country in its broader context, was imparted by excellent teachers who knew how to interest us in matters beyond the anecdotal and who also taught us to present a subject and to endeavor to place facts in a general context. I never lost the taste for history awakened by these teachers, whereas I understood only later, after traveling, the importance of geography. Two other subjects were the joy of all my secondary studies—mathematics and drawing. Algebra and geometry were well taught at the time, and learning them was the most satisfying activity for me until I was 18. Drawing was also a pleasure; my siblings and I had practiced drawing from life as our father had done. Like all girls at that time, I learned quite early to play the piano without obvious talent, and it was only later through my father's influence that I learned to love classical music.

I had learned to read between ages five and six before entering primary school, and I think I must have been seven when I could read fluently. Henceforth I devoured all the books I could obtain. At first I was satisfied by the magazine called *L'ouvrier,* which my grandfather subscribed to and which published historical novels. This storybook history nurtured my childhood as it did for my elder brother, who shared my tastes and used to tell me about the history of Greece when he occasionally came to collect me after school when I was eight. My later reading, though always assiduous, was not so well organized, for my father had retained from his southern childhood certain ideas about authors that a young girl must not read. He hid the books of some of our best. I discovered them only when my mother gave them to me in secret, or when one of my friends lent them to me.

During my years of secondary education, I learned little about nature; natural science teaching was not very strong. When I first encountered philosophy in elementary mathematics class it replaced French lessons, which had always been a pleasant subject for me. The teacher in charge was certainly anticlerical. Having received a Catholic education, I did not agree with her way of seeing humanity, and our relations were bad. For a long time, I remained suspicious of everything concerning philosophy. However, I discovered soon after, thanks to a professor of logic in the Collège Chaptal, how interesting the history of scientific thought was.

Until the age of 11, I lived in the Paris apartment in the 17th arrondissement where I was born. Then my parents had a house built at Vanves, an inner suburb served by a convenient rail line. We went to live there, and I entered the Victor Duruy Lycée, which I left only after passing the baccalauréat in elementary mathematics. This move upset all my friendships, and I lost the affection of a boy I had known my whole childhood. He was good and intelligent, more literary than I. We met again in 1938, to be parted once more in 1940; he was among the first war dead, a young lieutenant killed during the French army's advance along the Albert canal after the invasion of Belgium by the Germans.

My mother's three younger brothers were also victims of the wars. One was killed in the Sahara, the second died of illness due to the Great War, leaving two daughters behind. The third, wounded several times, survived four years of trench warfare. My mother was particularly attached to him and he was to be my godfather, so my baptism was delayed six months as my uncle could not leave the battle raging at Verdun. My mother often told me about the piteous state of her brother when he came to spend his leave from the trenches, and of his despair when she accompanied him to the station in 1917 to rejoin the front. From these tales, I retained the conviction that the War of 1914 was the worst trial that men have had to undergo this century. All her life, my mother feared her sons might suffer the terrible conditions that her brothers had known.

After the death of my grandmother, the house at Nîmes, where we used to spend our holidays, was sold, so my parents had a holiday house built near the Atlantic Ocean in the Vendée. We often went to the village near Béziers where my father was born and where his older brother ran the family vineyard. He had no children and divided his property among his nephews and nieces, and I still own a part.

Once I obtained the baccalauréat in elementary mathematics at 17, I had to choose an area for higher studies. I was much influenced by my brothers, both good technicians. The younger, who was seven years my senior, had just finished a chemistry course at the school of physics and chemistry (PC). My brothers advised me not to study medicine because of the difficulties that women were facing at that time in the profession. So I decided to be an engineer like my father and one of my brothers. Several schools had recently begun to take women students, especially the PC directed by Paul Langevin. Entry was by special competition, and mathematics was important. I entered the Collège Chaptal and spent a year in a special preparatory class. The mathematics teacher, whose teaching was pleasure rather than work, was the best I ever knew. At the end of the year, I was accepted into the PC.

At that time studies in the school were spread over three years and were divided into three hours of lectures and five hours in the laboratory each day. At the PC I learned how to organize an experiment and write a report. I was less interested in the mathematics lectures, which were given by big names who did not meet their students; the half-year examinations were severe; one needed an average of 14 to 15 out of 20 to continue. After 18 months, we had to choose a specialty, and although I had intended to become a chemist, the analytical laboratory class cured me of it. On the other hand, I loved the physics courses, especially their practicals in electricity, and thus made a choice that influenced my whole career. In the last year I learned to build balanced amplifiers, studied the construction of generators, and saw the first complete cathode ray oscilloscope (CRO) arrive in the laboratory. I graduated as an engineer physicist in 1937.

It was difficult for women in physics to find work in industry. The leading firms did not employ them in their shops but offered them positions researching the literature. However, female chemists were better accepted in research centers, so I entered Rhône-Poulenc to work in chemistry. I found it so uninteresting that I left after a month. I wanted to prepare for a doctorate and took a job as technical assistant in the Centre National de la Recherche Scientifique (CNRS) with Daniel Auger, who had a small laboratory in the institute of physico-chemical biology. He was a plant electrophysiologist who worked on the seaweed *Nitella*, which has long filaments and is able to transmit action potentials like a nerve fiber but at a much slower speed. To study the slow electrical potentials of *Nitella*, measured in millivolts, a direct current amplifier was necessary. My job was to maintain the amplifier system, which introduced me to the problem I was to encounter from then on—the faithful recording of bioelectric phenomena. But first I had to have clear ideas about them, and I had none. Auger had worked for several years on the problem and did not understand my total ignorance of vital phenomena, whereas I had no idea what studies I needed to do to understand them. Even if I had an engineering degree, a university science degree was necessary to proceed to doctoral studies. I had intended to receive such a degree in physics. I slowly realized that there was also a degree in natural sciences allowing specialization in physiology. It was not until 1943 that I took that course. Meanwhile, I continued amplifying weak currents without understanding their origin. Auger certainly could have helped me, but he had fallen seriously ill. Only on seeing a demonstration of electroencephalography organized by Alfred Fessard at the "Palais de la Découverte" did I realize that weak potentials were also produced by the brain, with the same problems as in *Nitella*, albeit much briefer and more rapid than in excitable algae.

The usual galvanometers accurately followed slow events, but their inertia prevented them from recording the rapid phenomena of nerve and muscle in vertebrates. Happily, the events in *Nitella* were slow enough for ordinary galvanometers. Later, I discovered that electrophysiologists had been building galvanometers with progressively lighter moving elements for 50 years. The appearance of the CRO was the perfect solution, but it was not yet generally used. Even if tubes were available, it was usually necessary to build the time base and amplifier for biological recordings. Alfred Fessard had long collaborated with Daniel Auger and sometimes visited us; he had installed his own laboratory at the Collège de France in Henri Piéron's department. Alfred Fessard was interested in the electroencephalogram (EEG), which is slow enough to be studied with a galvanometer. He also recorded action potentials of nerve and muscle, and from a grant of the Singer-Polignac foundation he had obtained a CRO, a French model in which the vacuum had to be re-established in the tube before each measurement. German tubes without this inconvenience had just appeared on the market, but it was still necessary to build the time base and amplifier.

At the Institute of Physico-Chemical Biology, the small laboratories were isolated and, despite the friendly welcome by Denise Lévy, the administrative secretary, and by Pierre Auger, the brother of my new chief, I had difficulty using the technical facilities. The university degree courses I was enrolled in were also disappointing for me. I was on my own, and the instruction was more theoretical than practical. All in all, these difficulties made me consider changing my profession. My mother died at that time, and life in a country that was just getting over social upsets, linked with the political conflicts of 1936, became more difficult under the threat of war with Germany.

During the War, 1939–1945

When war was declared in 1939, many laboratories were moved to the provinces, especially to the Bordeaux region, which at times had been the temporary capital during the Great War. I was sent as a CNRS technician to the laboratory of Professor Jean Mercier in the science faculty of Bordeaux, to join a team trying to improve the recognition by the human ear of the sounds made by different airplanes. I received a friendly welcome and, with another researcher, organized a laboratory at the air force base in Mérignac. I went there regularly and could see how ill prepared our air force was for the war. The equipment we needed was slow to arrive and I had plenty of free time, allowing me to pass certificates in physics taught by Professors Mercier and Alfred Kastler, and in theoretical mechanics taught by Professor Jean Trousset.

Daniel Auger became too ill to work. Alfred Fessard was mobilized and sent with Professor Piéron to a facility near Bordeaux for selecting aviators. The "funny" war was soon over; Parisians were trying to regroup in the Bordeaux region, and our laboratory at the science faculty even served for a while as headquarters for the war ministry, with General Charles de Gaulle briefly occupying the offices of the dean, Professor Mercier, who later directed the CNRS.

It was in a truck in the center of the recently bombed city of Bordeaux that I heard the announcement of Marshal Philippe Pétain requesting an armistice, and I wept bitterly with my companions. We thought we would be under the German heel for many years, with England alone unable to reverse the situation and Russia in a pact with Germany. The remaining French army had moved toward the Pyrénées. A departing Czech friend, Vladislav Kruta, left me his bicycle. The occupiers did not appear aggressive, and we did not know what to do or what to expect. We lived from day to day at the university, realizing it would be useful to leave but not knowing how. I often visited the family of my friend Denise Lévy, who became refugees in Arcachon, and learned from her niece about de Gaulle's appeal to the nation. Few of us knew of it, and we could not see its significance, nor could we comprehend the opposition between two respected patriots. Those who had survived the Great War had extolled to us the human qualities of Pétain who had cared for soldiers' lives more than other military leaders had, and it was hard for us to believe that he could so mistake the country's interest as to make deals with the enemy. For us, any contradiction between the two men could be only in appearance.

At the science faculty we had been engaged in holding special baccalauréat classes and examinations. We had received three months' salary in advance from the CNRS and our contract was terminated. We had to find new work, which was difficult under the circumstances. As my family had returned to Paris, I too had to go back. Fessard was demobilized, had started to set up a small electrophysiology laboratory at the Institut Marey in Paris, and suggested I ask for a position as a CNRS technical officer attached to the laboratory. So I returned to Paris in October 1940, after painful farewells to the friends left in Bordeaux. None of us imagined the restrictions we were to suffer. The house in Vanves where I lived with my father and sister had central heating, but we did not have enough coal to fuel it. The little coal we had allowed us to heat the smallest room, where the three of us lived. The bedrooms were icy. Moreover, we had no stocks of food, and food distribution was poorly organized. A black market network was in place, but the prices were too high for our salaries, and the assistance we later got from the country was not yet available. I have never been as cold and hungry as during that first winter of the occupation. After first trying to get us on their side, the occupying forces began to be aggressive, and I remember how the sudden application of an early curfew crammed the Métro cars with French people.

The laboratory at the Institut Marey was organized quite slowly. We had three rooms, and were very cold, with a stove in which we often had only old papers to burn. The equipment often broke down and it was impossible to find spare parts. So passed the next three years without leaving me much to remember but hunger and cold. However, I was able to finish my university physics degree, pass the examination in general chemistry in 1942, and enroll for the general physiology certificate, which I obtained in 1943. I married Alfred Fessard in 1942 and we lived in an apartment near the Institut Marey. We could heat only one room, often only in the evening during the severe war winters. My remaining memories of that period are above all linked to the search for food, with intellectual concerns taking second place, though I have noticed a significant memory loss for that epoch. We survived on stews of carrots and turnips, and the rare rabbit sent by a friend in the country. Thanks to my brothers and sister, to my sister-in-law whose husband was a prisoner of war, and to the family of my husband's first wife, we managed to have some good days, the families closing ranks against adversity. For several months, I continued to see my Jewish friends whose lives were much harder than ours because they had to stay in hiding or try to reach the unoccupied zone. Denise Lévy's family left slowly for the Massif Central. The Salomon family, whose daughter had stayed with me in Bordeaux, led a difficult life, and it was hard to assist them. A friend of my husband also went to the unoccupied zone, leaving us her radio set.

In the book shop near our apartment, a "collaborator" issued inflammatory talk every day, until one night a bomb put an end to his activities but nearly caused the arrest of innocent curious bystanders like me, who just had time to escape before a German patrol arrived. We lived near the Molitor swimming pool and used to hear the German soldiers go there in the morning singing their marching songs, which were characteristically fine, but beginning to annoy us a lot. I believe that this was the only contact most Parisians had with the occupiers over those months. I often saw French women move their children away when a German soldier, deprived of his family, would try to give them candy.

Our only relations with the Germans were at the laboratory, and in peculiar circumstances. One day a Cuban, who had worked part-time at the Institut Marey before the war, brought us a German civilian who offered to subsidize our research. We were able to get rid of him by showing our poverty in equipment and installations. We had another visit, this one in 1943: we saw a civilian standing at attention before the tomb of Etienne-Jules Marey, below the laboratory windows. It was a German who asked to speak to the directors, who were at the time my husband and Lucien Bull, an Englishman who had come to work with Marey about 1900 and who never left France. Bull had dual nationality but was a director at the École Pratique des Hautes Études, and hence a French official, which had spared him the trouble his nationality of origin could have given. However, he still had a slight English accent that was obvious to a good ear. The visitor told us he was in charge of a medical laboratory of the Kriegsmarine, and wanted to set up EEG examinations of submarine personnel. He was a Viennese psychophysicist named Robert Stigler (who had demonstrated the phenomenon of metacontrast) and, knowing that my husband had been one of the first to work on the EEG, he came seeking collaboration. To avoid his asking to use the laboratory, my husband told him of a demonstration of EEG techniques at the Palais de la Découverte and offered to show it to him. We all met by appointment at the Grand Palais, where Stigler arrived in a high-ranking marine officer's uniform with some collaborators. He appraised the technique, was happy to see that metacontrast was also demonstrated at the Palais, and never insisted on returning to the laboratory to obtain our assistance. Even though he almost certainly understood Bull's origins, the issue never came up. After the war, he came back to visit Bull at the laboratory. Stigler's life had since been hard, his sons had been killed, and life was not easy in Vienna, and Lucien Bull received him as a friend.

With the Allied invasion imminent, my sister-in-law took my husband's daughter, who lived with us, to her in-laws near Vercors, where we thought there would be more food and safety from the war. My husband, members of my family, and I stayed in Paris, where food supplies became even more scarce, electricity was cut off, and the Métro ran only a few hours a day. Luckily we still had bicycles to get about in Paris. I remember one day being on the only moving vehicle on the Champs Elysées. A German order came to hand over the bicycles, which was almost immediately countermanded by the prefecture. Barricades had been built at our door, the high school nearby was full of ferocious Tatars recruited by the Germans in Russia, and some men in a neighboring house were arrested one night and shot in the Bois de Boulogne. We shifted to Alexandre Monnier's place at the Parc Moutsouris, which was less exposed to danger, returned to the rue Molitor by bicycle, then left again for avenue Mozart to stay with friends of my husband. There, near midnight, we heard the church bells sounding the arrival of the advance guard of the Leclerc column. The next day, trying to return home, we encountered the first jeep with two Americans followed by the Leclerc tanks, which unleashed the joy of the Parisians and the activity of snipers.

Although the liberation was far from solving the food problem, we were relieved of the great load of the occupation. We had no news of our family in Vercors. A few days later, we had the pleasure of receiving a telephone call at the laboratory (the Paris telephones had never stopped working) from Professor Bryan Matthews, whom my husband had worked with in Cambridge. He was on the Champs Elysées and was leaving the next day on a mission. To see us, he came all the way on foot, as the Métro was not yet working. This first contact with an Englishman is one of my greatest memories of the liberation. I remember him explaining the difference between V1 and V2 rockets, which the Germans were then using against England. A little later, we were also visited by some American colleagues, and I worked wonders to find something to offer them to eat. The Bastogne offensive terrified the Parisians, with bombing expected, and this time the Monniers came to our place as we had deeper cellars for shelter.

Professor Henri Laugier, whom I had heard a lot about, arrived from Algiers. He had led the CNRS before the war but the position was conferred on Frederic Joliot in 1944. Laugier wanted to resume his teaching at the Sorbonne and then be replaced by Alfred Fessard, but this proposal was opposed by Alexandre Monnier, already a professor at the faculty of science, who refused to have a competitor. These arguments helped to separate us forever from the Sorbonne group. We continued to keep the Institut Marey functioning modestly. The first years after the liberation were difficult, with laboratory supplies almost impossible to obtain.

From Electric Fish to Mammals, 1945–1955

In 1945 or 1946, Professor Edgar Adrian, with whom my husband had worked, invited us to Cambridge, where we stayed several days with Wilhelm Feldberg. We met the laboratory investigators William A.H. Rushton, Alan L. Hodgkin, and Andrew F. Huxley, but it now seems to me that Bryan Matthews was not yet demobilized from the forces. We attended a meeting of the Physiological Society at Oxford, where I met Eduardo Liddel and Charles Phillips, and lunched next to David Whitteridge who "for my own good" made me speak English, though I later realized he spoke perfect French, which he had learned from his French mother. His wife Gwenneth was a historian specializing in medieval French. These contacts gave rise to a long friendship. We had told the Whitteridges about the difficulties of our laboratory and left England with a bagful of parts from David Whitteridge. When we arrived at the Cambridge laboratory, we were questioned in a friendly way by Professor Sir Joseph Barcroft, who was still working, and he took us to see his sheep experiments. That trip leaves me with the memory of pleasant contacts somewhat spoiled by the mental confusion caused by the mixture of languages.

Back in France, my husband was next involved in organizing the selection of officers for the army, which brought us to know many British, French, and Allied psychologists and neurologists. To regain contact with American research, my husband left for the United States with Dr. Auguste Tournay, aboard a liberty ship, where they encountered Louis Bugnard, professor at the faculty of Toulouse, who became director of the institute for medical research (INSERM) and one of our best friends.

I was still at the CNRS, where a research grant had replaced my salary as technical officer, but I found it difficult to interface my training in physics with physiological research. A doctoral thesis seemed to demand a great deal of time, so I was pleased to accept a post as physics assistant in the one-year course of physics, chemistry, and biology (PCB) that medical students had to take. The post was suggested by my friend Georges Destriau, whom I had known in Bordeaux. I kept this position until 1950, and in this service made devoted friends who helped me when preparing my thesis took up a large part of my time.

The subject I then worked on did not inspire enthusiasm—it was whether the passage through spinal ganglia slowed down the messages in sensory fibers. The only merit of this research was that it required bipolar recordings of independent, closely neighboring electrical phenomena, and therefore the construction of balanced amplifiers. At this time we were visited by Professor Carlos Chagas of Rio de Janeiro, who had worked in Paris before the war. My husband had spent some time in Rio before the war, and Chagas suggested he return to work on a local electric fish, the Gymnote (*Electrophorus electricus*). In 1947, we set off in a ship of the Chargeurs Réunis line. In Rio we found other French people, Professor Henri Piéron and his wife Mathilde; Yves Legrand and his wife Françoise; Mme Gabrielle Mineur, who had been appointed cultural attaché at the embassy; André and Sabine Wurmser, who had spent part of the war in Brazil; Brazilian friends; the Chagas family; and members of the Ozorio de Almeida family, especially Miguel and his sister Branca.

There was also Professor George Brown of University College of London, and several Brazilian researchers, such as Aristides Leão.

We wanted to understand how the Gymnote could develop such a high electromotive force; measurement showed that its principal electric organ produced short trains of brief impulses (2–3 msec) able to develop a potential of 300V out of water and over 100V when functioning in water. How the thousands of elementary electric plates, only tens of microns thick, arrayed in series in an organ nearly one meter long, managed to discharge almost simultaneously (one impulse of the organ lasting only a few milliseconds) was the topic of our first visit and part of the following visit. On our return to France, I pursued this study on another electric fish that produced sufficiently strong potentials, the Torpedo, on which my husband had already worked with Wilhelm Feldberg and David Nachmanson. These flat fish produce short impulse trains with a potential of 40V in open circuit, and they also have a mechanism for synchronizing the elementary electric plates. I devoted my summers to studying the function of these electric organs, when Torpedo could be caught in the Arcachon Basin, and when I had the chance to go to Brazil.

I returned to the Institute of Biophysics in Rio in 1950 with my husband, then alone for many summers between 1953 and 1958. These visits allowed me, with Hiss Martins Ferreira and Antonio Couceiro, to advance our knowledge of the electric organ. My first investigations on electric fish—Gymnote, Torpedo, Ray—allowed me to pass a science doctoral thesis in 1950. I later added microphysiologic studies to this first analysis, published mainly in Portuguese and French. The study of electric organs allowed me to apply my knowledge of electrical phenomena to a physiological problem and gave me the opportunity to better understand the function of the cells in the bulbar nuclei controlling electric organs. In Torpedo, the cell bodies of axons commanding the discharge are grouped in the electric lobe, whereas in the Gymnote and the Ray the cell bodies of the motor nerves for discharge are spread along the spinal cord. In all these fish, the firing of these cells is triggered by signals from bulbar nuclei are easily visible in histological sections, as demonstrated by Fessard and Antonio Couceiro in Gymnotes and by Fessard and Thomas Szabo in Torpedo. The cells of this bulbar center receive peripheral stimuli and send out trains of rhythmic commands for repetitive discharge of the electric organ. The cells in both the motor nuclei and the command center are large, so it was possible to study with microelectrodes the bulbar reflex arc; provoking the discharge, which we did.

After our first trip to Brazil, the Institut Marey laboratory expanded progressively into rooms that had been empty. Thanks to Mr. Georges Jamati, and to Professor Emile Terroine, the CNRS had established the Centre d'Études de Physiologie Nerveuse. The grants received added to those from the École Pratique des Hautes Études, where my husband was a laboratory director, and gave us the means to install new experimental rigs. We were joined by Pierre Buser, a young assistant at the École Normale Supérieure; Ladislas Tauc, a Czech investigator; Jacques Paillard; and Jean Scherrer, a neurologist who was returning from Chicago. Dr. Auguste Tournay, a neurologist who collaborated with my husband throughout the war, continued to come to the laboratory to study the electromyography of movement using himself as subject.

My husband was soon appointed to the Collège de France position vacated by Henri Piéron's retirement, so the buildings of the Institut Marey were, by its reattachment to the Collège de France, progressively modernized because it was part of national building stock. My husband regularly attended meetings of the Physiological Society and urged me to try in electric fish the intracellular microelectrode technique that John C. Eccles and his colleagues had just used on spinal cord cells. I had the disinterested help of Tauc, who was already using microelectrodes for measuring the membrane potential of slime molds. He had perfected the technique for making microelectrodes and constructed the indispensable impedance-matching amplifier. Helped by his advice, I quickly learned to make glass electrodes using a Fontbrune microforge and built a vacuum-tube head-stage amplifier that we used for several years with electric fish and then with mammals. We spent the summer of 1952 at Arcachon doing intracellular recording in the electric lobe of Torpedo. We easily impaled the large cells of the lobe and observed intracellular phenomena like those already described by Eccles and colleagues in the cat spinal cord. This work was carried out with Buser, who had joined us in Arcachon. Microphysiologic recordings were later made in the bulbar command nucleus with Szabo, and at the electroplaque level in Rio in 1953 and 1954, where I was helped by the young researcher Carlos Eduardo Rocha-Miranda and a skillful technician, Raimundo Bernardes, who, using the microforge, made the best microelectrodes I have used.

Because intracellular microelectrode recordings had proved easy in fish, with Buser we tried to apply this technique to the large cells of the cat somatomotor cortex. But this procedure required respiratory and fixation procedures. Stereotaxic methods for placing electrodes in desired regions of the brain required a special apparatus perfected in the United States by Horace W. Magoun in Stephen W. Ranson's laboratory. Jean Scherrer had learned the technique in Chicago, and he helped us with equipment that was built in France from plans brought back by Paul Dell. The first recordings in cells of the cat's motor cortex showed us that a prolonged hyperpolarization followed the initial phase of excitation in response to messages from the periphery. For this work, we used chloralose anesthesia, most commonly employed by European physiologists.

Before World War II, my husband had been the first in France to practice EEG, so we had steady contact with those applying the technique clinically. The French EEG Society was founded, and Professor Frederic

Bremer came to Paris for the occasion, as well as an English investigator, Grey Walter. At a later joint meeting with the English EEG Society, I met Henri Gastaut, then working with Grey Walter, whose work on EEG localization of cerebral tumors was well known, and many other French neurologists of the period. Meetings of the French EEG Society were organized in the old Charcot theater, a sort of narrow tunnel with an abrupt slope, now replaced by a modern structure, where we gave our first communications on cortical activities. The sessions of the EEG Society had a fruitful effect on the advancement of mammalian research in France. At that time, we were interested in problems of localizing epileptic foci and tumors, for which the noninvasive EEG technique was of great service at a time when modern imaging methods did not exist.

My first contacts with the Russian researchers Georgyi D. Smirnov and Vladimir S. Rusinov were made about 1954 at a conference organized by Henri Gastaut in Marseille. They had a great sense of humor and a good understanding of neurophysiology. We hit it off immediately and made plans for reciprocal visits, which political conditions did not always allow.

Invited by Belgian neurologists, my husband and I spent several weeks in Brussels, then in Antwerp, where we visited the clinic directed by Professor Ludo van Bogaert, and could admire the Bruegels.

To boost our activity, the CNRS organized a colloquium at the Institut Marey in 1949, gathering the big names in neurophysiology of the time, Alan L. Hodgkin and Rafael Lorente de Nó in particular. Their data on nerve fiber activity seemed to put them in opposition, whereas each held a part of the whole truth. Along with Stephen Kuffler, there was Frederic Bremer, who had managed to work right through the war, with his rapid grasp of problems and always a penetrating question. It was also a pleasure to meet Fernando de Castro, one of Santiago Ramón y Cajal's last pupils, whose results with anastomosing sympathetic efferents and heterologous nerves were wrongly neglected in this period of difficulty for the non-Francoist Spanish. I saw him again in Madrid in 1966.

The neurology congress of 1951 in Paris gave us the opportunity to meet many well-known researchers such as Wilder Penfield and Warren McCulloch, but I remember above all the friendly attitude of John Fulton whose book was the neurophysiologists' bible.

At the second CNRS colloquium at Gif in 1955, we presented our microphysiological results in electric fish and in cats. The latter were considered artifactual by some, but were supported by Professor Richard Jung of Freiburg, who like us had moved into microelectrode work. He had done work on the visual system, still not sufficiently recognized for its originality.

My memories of this time are mixed with the euphoria of obtaining new results on brain function and the difficulty of having to present them. Because my spoken English was far from fluent, I had to present my data in French, even to an Anglo-Saxon audience. My research was thus known only to a restricted circle, and most often it was only the French men who went abroad. My husband was punctilious in attributing my work when he presented it, but it is still true that the intellectual activity of women makes men suspicious, and they attribute it to a masculine influence—I did not escape this.

At that time my work was split into two annual periods. In winter I worked on cats and rabbits and started on frequency analysis of the EEG using English equipment bought by Dr. Herman Fischgold. Summers I usually spent in Rio on the microphysiology of the Gymnote, and during those visits I met several impressive personalities. Professor Bernardo Houssay, who used to go to Rio to forget for a few weeks his difficult conditions in Buenos Aires. He spoke fluent French with a trace of Pyrénées accent acquired from his grandmother. There was also Professor Celestino da Costa from Lisbon, several big names in European and American research, often among them Professors Edgar Adrian, Eleonor Zaimis, Wade Marshall, the charming Robert Oppenheimer, Corneille Heymans, Andre Cournand, and Robert Stampfli. My work on the electroplaque put me in competition with Harry Grundfest; and I met his wife, a painter, whose open mind I admired. I received great assistance from the French cultural attaché in Brazil, Mme Mineur, with whom I often stayed. Thanks to her, I was able to obtain grants permitting Carlos Eduardo Rocha-Miranda and Eduardo Oswaldo-Cruz to come to the Institut Marey for training, and Raimundo Bernardes was able to stay for several months with us. I also had the pleasure of meeting French visitors—Professor Paul Rivet, Professor Henri Laugier and his friend, who had a great aesthetic sense, and the Jean Vilar theater company.

One of my last studies on the Gymnote was on the action of curariform drugs on the electric organ. The work was initiated by Carlos Chagas, who was curious about all pharmacological developments and taught me much about different curares. With Antonio Couceiro, we also studied the distribution of cholinesterase in the electric organ. A meeting on curare was the finale of these investigations for me, but with my Brazilian pupils I soon began to do research on the cat and then the monkey. Conditions for working on mammals were not always good because of shortages of imported laboratory supplies, but the personal conditions were perfect with the understanding I enjoyed from Chagas, the institute director, and from all my laboratory friends, researchers, and technicians.

I was made a corresponding member of the Academy of Sciences of Brazil and received the Officer's Cross of the Cruzeiro do Sul. I also enjoyed a family atmosphere in Brazil thanks to my friend Annah Chagas, her sister, and above all her four daughters who for several years took the place of the children I did not have. Through the Chagases I also met the great Brazilian painter Candido Portinari. His son came to study engineering in Paris, which brought us closer. During one of my Brazilian sojourns, in 1957, I went to a colloquium organized by the Mexican Raúl Hernández Peón in the southern Chilean city of Concepción, where he was teaching. He had worked in Magoun's laboratory in Los Angeles and was full of original ideas. I thus was able to meet other Chilean brain researchers and visited Santiago and Valparaiso.

My repeated stays in Latin America ended only because the birth of my son made the trips difficult, and I returned to Rio only for short conference visits in 1966, 1970, and 1995. But I have maintained lasting contact with my Brazilian friends, who visit me in Paris. Annah and Carlos Chagas and my friend Aristides Leão never fail to come and discuss work with me. Antonio Couceiro and Hiss Martins Ferreira also made visits to Paris, and more recently two Chilean researchers made long stays in my laboratory.

After having recorded the activity of cerebral cortical neurons, I went on to look at cerebellar activities. To find out how best to activate the Purkinje cells, Thomas Szabo and I studied the spinal and bulbar pathways from the periphery to the cerebellum in the cat. These investigations were published only as short notes in French. Szabo left to train with Alfred Brodal and then devoted himself to studying, with my husband, signals emitted by electric fish for localization. This short excursion into cerebellar physiology had two advantages. It led me to study Brodal's publications of admirable clarity. It also allowed me to meet Fernando Morin, an Italian working in the United States who came to visit me after an exchange of reprints. Thereafter he visited each year when passing through Paris.

From Cats to Primates, 1955–1968

The second phase of my research in mammals really began in 1955. I was trying to map in the chloralose-anesthetized cat the cortical region of potentials evoked by stimulating the anterior limb. With this anesthetic, multiple cortical recordings showed responses over relatively wide zones of the anterior cortex. In the course of rearranging my apparatus I accidentally stimulated the ipsilateral instead of the contralateral limb as normal. Ipsilateral stimuli evoked potentials with the same localization, but with longer latency. Responses of shorter latency were of course observed in the classical "primary" regions (SI and SII) as already described by Edgar Adrian, Clinton Woolsey, and Bard's school. But we were seeing additional bilateral activities of latency, longer by several milliseconds. The same bilateral projections had been described a little earlier by Vahe Amassian. The signals producing these responses did not arrive by cortico-cortical pathways. The regions where these responses were seen were then called "associative," and my existing notions of thalamic anatomy led me to seek their afferent relay in the dorsomedial nucleus. A systematic study showed that bilateral responses were not observed in this nucleus, but lower down in the region called *centre médian* (CM) in the cat brain atlas made by Herbert Jasper and Cosimo Ajmone-Marsan. Bilateral inflow also arrived in some other medial nuclei. By microphysiology we established that these convergent afferent responses could be recorded over the whole of a structure as well as in each of its cells. This work was done with Arlette Rougeul, a young postdoc who had just joined INSERM as a researcher. The work was published in French in the *EEG Journal* in 1958. The article, according to "Current Contents," has been one of the most cited classics.

The results reported in the article were greeted in various ways and gave rise to interminable argument. An American team, led by Vernon Mountcastle, was at that time recording thalamic activities in barbiturate-anesthetized animals but did not find the responses we called convergent, in either thalamus or cortex. Results similar to ours were, however, obtained by teams working in Seattle (Vahe Amassian, and several others), using chloralose. This difference in effect of different anesthetics deserved to be investigated, not to be dismissed in one or the other condition as erroneous. An anesthetic substance cannot create a pathway, but can only modify its use. Another criticism came from William Mehler, who challenged our nomenclature. For him the CM was present only in primates, and the zones where we found convergent activity in the cat corresponded to another thalamic nucleus, centralis lateralis. In any event, under certain anesthetic conditions, the part of the region later referred to as the medial thalamus receives, as does the ventral posterior thalamic nucleus, signals derived from the periphery. But the cells of the region are activated from less restricted peripheral regions than those of the lateral thalamus and are not spatially organized as a function of the peripheral region that emits the signals.

The region where convergent signals are received is close to the medial part of the ventral posterior nucleus. The anatomist Jerzy Rose thought we might by error have poorly defined the nuclear boundaries. His pupil Lawrence Kruger visited me, assured himself it was not so, participated in recordings, and left convinced. I found friendly understanding also from Clinton Woolsey and some of his pupils. David Bowsher at Liverpool had, like W. Mehler in the United States, studied the course of the spinothalamic tract in primates (then considered the only conductor of thermal and painful signals) and came to work with me over several periods, when together we studied this medial spinothalamic projection in monkeys. This work was possible because an anatomic laboratory had been organized at the Institut Marey. Thanks to Mme Suzanne Laplante, a CNRS technician who was attached quite early to the Centre d'Études de Physiologie Nerveuse, this laboratory was well equipped and active. Classical staining methods for verifying electrode positions were used, and other techniques using degeneration and transport of markers were developed that allowed us to correlate macroscopic anatomy with electrophysiological research. In this we were influenced by the ideas of Carl Vogt, for whom the techniques of recording and of anatomy had to be used in parallel. My husband, Pierre Buser, and I visited Cecile and Oscar Vogt early in the 1950s in the laboratory installed for them in Neustadt, which held only a fraction of the anatomical material they had once assembled in Berlin. Oscar Vogt explained some of his ideas on neurological diseases and recounted his relations with the socialists at the turn of the century. I was impressed by the intelligence and vast culture of the Vogts, who continued to work despite their age and the difficulties they had known during the Hitler period. The German researchers I later knew best, Richard Jung and Rudolf Hassler, were their pupils, whereas Jerzy Rose, Lorente de Nó, and Jerzy Olszewski had worked in their laboratory. Later I was to know their daughter Marthe Vogt, who had begun her career in Berlin. She showed me her mother's thesis, which at the start of the century had used a modification of the Flechsig method to describe the primary sensory projection zones on the cat cortex, the same regions that were rediscovered much later by electrophysiology.

Several events in the years 1956 to 1958 changed my way of life and reduced the time I could allot to research. In 1956, the French physiological society, in which Professors Robert Courrier, Henri Hermann, Georges Morin, and Daniel Cordier played an important role, asked Pierre Buser and me to present a report on central nervous system (CNS) activities. Buser chose to deal with associative activities, so I undertook the primary projection of somatic, visual, and auditory afferents. In so doing, I drew up an extensive bibliography and received unpublished articles from numerous authors. Thus I made contact with Professor Yasuji Katzuki in Tokyo, with Archie Tunturi, and with Vernon Mountcastle who had just published important articles with Jerzy Rose on the microelectrode study of primary somatic thalamic relay activities. I presented the report in Geneva. My text had been checked by my friend Valentine Bonnet, who was working with Bremer but had come to Paris to learn about microelectrodes. The oral presentation was prepared with my friend Serge Tsoulatse, a Georgian who was working part time in the laboratory. Bonnet correctly advised me to remove the section I had devoted to the projection of pain afferents, as she judged it to be incomplete.

This first contact with this difficult pain problem left its mark on me, and it later became one of the main lines of my research. When working on the CM of the cat, Lawrence Kruger and I had observed a double response, the second with a latency attributable to the arrival of C fiber input. This finding fitted with the spinothalamic projections observed by the anatomists at the medial thalamic level. But the second response coincided with the end of a prolonged inhibition that followed excitatory responses of this nucleus in chloralose-anesthetized animals. As the first interpretation was probably not sustainable, we investigated the types of fiber delivering somatic messages to the CM. Alberto Mallart, a trainee from Barcelona, showed in my laboratory that large-diameter fibers conducted somatic inputs to the CM. If the medial thalamus was involved in the reception of pain signals, its role had to be complex and warranted further study. Mallart also drew my attention to the importance of collaterals from the posterior columns, described by Ramón y Cajal, in the function of the spinothalamic pathways.

Since presenting my thesis in 1950, I had been appointed assistant director of the laboratory directed by my husband at the École Pratique des Hautes Études, thanks to Henri Piéron who at that time was president of the natural science section of the school. I had therefore given up my teaching in first year medicine at the Institute of Physico-Chemical Biology and devoted myself entirely to research, with some administrative duties imposed by the laboratory of nervous physiology, which was expanding. I had asked to be listed as having aptitude for advanced teaching but had not achieved this until 1955. Professor Pierre Grasset, who had important influence in biology and psychophysiology teaching, suggested that I apply for the second position of lecturer in psychophysiology that had just been created at the Sorbonne. Professor Laugier strongly supported my candidacy. I was appointed maître de conferences in 1957 after visiting most of the professors then teaching at the Sorbonne. I remember some interesting visits, particularly with mathematicians; and the visit when I met the professor of biochemistry, Claude Fromageot, who proposed a collaboration—soon interrupted by his untimely death—for which I began to prepare an atlas of the rat thalamus.

Once appointed, I had to prepare my lectures, and I had never taught physiology. I gave my first lecture in the physiology theater of the old Sorbonne. I was petrified with fear and hence no doubt uninteresting to the audience. With time, I overcame the stress of teaching in large theaters with large audiences. In the first semester, Professor André Soulairac, coordinator of psychophysiology teaching, let me teach the basics of neurophysiology, my specialty. But in the second semester he asked me to deal with animal behavior from the viewpoint developed by two American authors who had worked on invertebrate behavior and whose book was unobtainable. I had absolute need of it, as I had never before studied these questions. My friend Thèrèse Kleindienst, then at the Bibliothèque Nationale, was most efficient, and I soon had a copy of the book. Although to justify my appointment I strove to approach problems of behavioral research, the animal psychologists did not admit me to their company for a long time. So I do not regret the efforts I put in that gave me a fuller knowledge of animal research. Anyway, the leadership of the CNRS was soon to appoint me to membership on the psychophysiology commission, and there I met the psychoanalysts Daniel Lagache and Juliette Boutonnier, as well as specialists in human and animal behavior with whom I established good relations.

Nicole, my husband's daughter from his first marriage, lived with us, and we got on well. In 1952, after passing her university exam (agrégation) in natural sciences, she worked in a laboratory dealing with paleobotany. She married Louis Grambast, a researcher in this specialty, and they had a daughter in 1956. In 1957 I lost my father, and in 1959 I brought into the world my son Jean, who greatly resembles my father. I had long wanted a child, and the risk of women over 40 producing Down's syndrome children was only known publicly a few weeks before Jean's birth. Yves Galifret, a pupil of Piéron's, who was then at the Institut Marey, helped out with my teaching. When I wrote my report for the association of physiologists, I had appraised the work of Mountcastle, and at my request my husband had invited him to give a lecture at the Collège de France. Mountcastle came to Paris to do this in April 1959, but unfortunately he arrived while I was still in the hospital with Jean. He came to visit me, but the environment was not conducive to discussing the discrepancies between my thalamic recordings and his. Because we got off to a bad start, contacts between us were never amicable.

After my recovery, I arranged things so that Jean's presence did not reduce my research activities too much. Trips abroad were abandoned for some years and were replaced by frequent sojourns to a house we had bought in 1959, to take Jean out of the polluted air of Paris. The property was an old run-down farm from before the revolution, which we gradually made habitable room by room, thanks to a prize from the French Academy of Sciences and to the aid of a technician working part-time at the Institut Marey, who helped me in his free time. Jean found in this village of Seine et Marne those country roots that far-off Languedoc could no longer provide.

During my times in Brazil, I had met Eleonor Zaimis, professor of pharmacology at the Royal Free Hospital medical school in London. I often visited her in London. She organized lectures for me and introduced me to Charles Downman who taught in the same school, often had me rejoin Marthe Vogt, who was then working at Cambridge, and introduced me to Robert Lim who was studying the transmission of pain signals. Eleonor left London to return to Greece, but when I visited her in Athens in 1982 she was nearly blind and died soon after.

In 1958 a Belgian researcher, Jean Massion, came to work with me. He was a pupil of Professor Jean Cole of Louvain, whom we met regularly at the French physiological society. Cole had trained three pupils in research—Jan Gybels, who was doing further training with Herbert Jasper in Canada; Michel Meulders, who was with Giuseppe Moruzzi; and Massion. So Massion could have his own research topic, we began a microelectrode investigation of the red nucleus, which in the cat gives rise to the rubrospinal pathways, partly duplicating the pyramidal tracts. In particular, we looked at relations of the red nucleus with the cerebellum through which a pathway significantly inhibits some rubral cells. Massion undertook further study of this nucleus, and he was able to pass his thesis of agrégation and to obtain a CNRS post when he chose to settle in France. Until then, I had collaborated mainly with Brazilians, and later with Lawrence Kruger. Michel Dussardier, who had done interesting work on rumination at the Institut Marey, and later in the INRA (Institut national de la recherche agronomique) station at Jouy en Josas, chose me as director of the thesis he had to lodge quickly in order to apply for the position of professor of physiology of Marseille. It was the first thesis I had supervised since I was appointed professor, and it was the start of Dussardier's career there, where he established an important school investigating visceral systems. It was also the start of a long friendship; his frequent critiques have always been useful. At that stage, I had never had lasting direct collaboration with German trainees, but I remember well the ones who visited Buser, and those who visited the laboratory—Jung and some of his pupils, particularly Otto Creutzfeld, and researchers I met on visits to Freiburg. At that time French and German people felt united and European. My relations with Rudolf Hassler, after a poor start at the Brussels physiology congress, became amicable. At Brussels, too, I first met Professor Hans Schaefer of Heidelberg, whose book on electrophysiology and work on neuromuscular transmission I knew. Around 1965, he invited me to Heidelberg where, among other researchers, I met Robert Schmidt, who had returned from training with John C. Eccles. Again at Brussels, around 1955, I met the two Czechs Jan Bureš and Olga Burešovà who were using spreading depression described by my friend Aristides Leão, and who asked me to send him their publications.

Contacts with Soviet researchers initiated at the Marseille congress organized by Gastaut were followed by an invitation to Moscow for those then working on the brain. So to Moscow went Fessard, Henri Gastaut, Herbert Jasper, Horace Magoun, Clinton Woolsey, Hsiang-Tung Chang, Mary Brazier, Raúl Hernandez Peón, Robert Naquet, and many others. Our friends Georgyi D. Smirnov and Vladimir S. Rusinov were present, as well as Ezrad A. Astratyan and Piotr K. Anokhin, whom we were later to see often in Paris. I was invited to the congress, but I could not go because I was awaiting the birth of my son. After that meeting, the International Brain Research Organization (IBRO) was created, with my husband actively involved in its development. The general secretary of IBRO was then in Paris, whose presence allowed us to receive visits from many foreign researchers attending meetings of the organization. Thus I established contacts with Alfred Brodal, then with Professors Henrich Waelsch and Klaus Una, who served terms as general secretary, and later with Herbert Jasper, when I became a friend of his wife, Margaret. When Mary Brazier agreed at a difficult time to become the IBRO secretary, I had just been elected a member of the general assembly. Mary asked me to take over the grants program common to UNESCO and IBRO, which I did until her departure. I resigned because of feeling, at a later meeting, that my work was not appreciated by some of the French. I have been sorry to see changes in the IBRO institution, the only one that allowed scientific relations between the East and West during the era of the Iron Curtain. During that period I received many Russian, Czech, Hungarian, and Romanian trainees, usually for brief stays. Thus I met Endre Grastyan and Niklos Rethelyi, who I later saw again in Hungary. I also remember a telephone call from Georgyi Smirnov to congratulate me on Jean's birth. Our relations with the Russian scientists remained friendly even when government politics hardened.

A few months after Jean's birth, the International Physiology Congress was held in Buenos Aires, but I had to stay in Paris. One day I had a call from Professor Jerzy Konorski in Warsaw. He was going to Buenos Aires and had to get a visa in Paris, and asked for my help. I went to fetch him from the airport, and he soon managed to leave for Argentina. We made excellent contact, both speaking in what Konorski referred to as continental English. Afterward he sent me his pupil Elizabeth Jankovska, who left to work with Anders Lundberg in Sweden. Konorski also invited me to spend two weeks in Warsaw, where I first met Mircea Steriade from Romania. I returned to Warsaw for a symposium just before Konorski died. He saw difficult political times ahead and told me that visits to Poland were going to be impossible. At the same meeting I saw Professor Adrian for the last time, as well as Donald Lindsley and several scientists from Leningrad.

In 1962 Professor Cyril A. Keele of London organized a symposium on pain in humans and animals; Bowsher and I were invited, and we grouped our contributions together. There I first met Ainsley Iggo, Ingve Zotterman, and M.J. McComas. My results in the cortex and the CM were also presented at a Pisa symposium organized by Giuseppe Moruzzi in honor of Frederic Bremer, with Horace Magoun, Mary Brazier, Ragnar Granit, and Cosmo Ajmone-Marsan present. The Magoun school had already found in the brainstem of the awake animal responses similar to those I had observed in the thalamus, and these responses were suppressed by certain anesthetics. On this occasion, I first had the courage to make a presentation in English. My friend Suzanne Tyc-Dumont urged me and helped me to do it, and ever since I have given my results in English in Anglophone countries and during visits to Germany, Japan, and Russia. But even after improving my English thanks to American collaborators, I have always had some difficulty of expression in that language, above all in replying quickly to questions. It is always difficult to be subtle in a foreign language, and the necessary simplicity of my oral expression has often led me to be accused of aggressiveness. I think that those who have so misjudged me ought to have had to present their own work in a language not their own—they would have understood me better.

I frequently visited Czechoslovakia, invited first by Jan Bureš. On the eve of my departure for Prague in 1962, President Kennedy gave his speech on the Cuban missile crisis, and I wondered whether I should cancel my trip. Massion, who was taking me to the airport, remarked that an atomic bomb would not have different effects on Paris versus Prague, so I went. In Prague I could not learn how events were developing, as foreign broadcasts were jammed, and it was reminiscent of Paris during the occupation. I asked at the hotel for permission to telephone Paris, on the pretext that my little boy was ill. My husband, not realizing the sort of atmosphere in Prague, replied that the child had never been ill, and asked if I was having problems there; terrified, I hung up. The next day I visited Bureš' laboratory in the Academy of Sciences for which comfortable premises would later be built. Then they had only a single large room where experiments were organized in different corners, manifesting the qualities of the experimenters.

I next visited our friend in Brno, Professor Vladislav Kruta, who had come to France in the 1930s to work in Louis Lapicque's laboratory before the second world war, where he met my husband. He had married a French woman and returned to Czechoslovakia as professor in the faculty of medicine. At the time of the German occupation of his country, the Krutas were in France, where his wife and children spent the whole war with her family. In 1940, Kruta himself left Bordeaux for England. He was with the Allied armies through the war and returned to France just after the Normandy landing. He had brought all sorts of little things he rightly thought we would be lacking, and I have never forgotten the distribution of all those presents. He then returned to Czechoslovakia. He would have left when the Communist regime was installed but thought it important that free minds should not abandon the place, and he stayed in Brno. He was still a professor in this faculty on my first visit, but he was soon sidelined from teaching and the laboratory, and then forced to retire. Curiously, it was then that he was able to come to France easily. It seems that the Communist government was happy when a retiree did not return so they need not pay a pension any longer. We found ourselves sympathetic from his first visit, and even though I could not return to see him in Brno on later visits, Kruta always arranged to go to Prague for a couple of days to see me during my frequent visits to the researchers in the academy laboratory—the Bureš', Pavel Hnik, Ladislav Viclický, and others.

My first time in Brno it was cold, the Krutas could heat only their kitchen, food supplies were scarce, and we still had no information about Russo-American relations. As I was about to leave, the détente occurred. I then went to the Plzen physiological laboratory where I met Yamila Hassmanova and Richard Rokyta, who both later worked with me at the Institut Marey. On my return to Prague, I saw Kruta again and during a stroll with Bureš we saw the demolition of a large statue of Stalin. I returned to Paris with good memories and an assortment of Czech marionettes to earn my son's pardon for my absence.

The first American to come to work in the Institut Marey was Robert Livingston in about 1950. In 1958, Lawrence Kruger stopped over in Paris on his way to spend a year in London in the anatomical laboratory of W.E. Le Gros Clark. Kruger paid us many visits from England thereafter, in the course of which we began to write two articles. He returned to Los Angeles shortly after Jean's birth. There he advised a young postdoctoral researcher, Richard Wendt, to go and work for a few months in Paris. This was a happy event for me. I greatly admired Dick, as we called him. He was a skillful researcher already experienced in single-cell studies, with a balanced, pleasant character, and we had a period of productive collaboration. He did some work on the amygdala, then on the orbital cortex, and he was the first to use the method of local cooling by butane expansion using the deep probe just put into use by Max Dondey for my friend, the neurosurgeon Jacques Le Beau. That technique was later neglected and abandoned in France; the required improvements aroused disputes about priority. These localized cooling probes were used in the Institut Marey by Robert Naquet and Monique Denavit in the mesencephalic reticular formation of "chronic" cats. Naquet was at the time director of a laboratory in Marseille, but he came to Paris regularly to work at Marey.

In the 1950s, brain activities were, in the majority, recorded in anesthetized animals. Barbiturates or chloralose were the most frequently used anesthetics, however, these substances were not only modifying the animal awakeness but also the cells' activities. To avoid this last effect, recording without anesthesia was a necessity. Different solutions were found by different working teams. One solution was to implant, in aseptic conditions, recording and stimulating electrodes in anesthetised animals and to wait for the disappearance of the anesthetic effect during a few days before recording. The animals were prepared in such a way that they were free to move and did not feel pain from fixation techniques. The electrodes were said to be chronically implanted. Such animals were rapidly called "chronic" animals. They were used to study the behavioral effects of blockade that were produced by the cooling of localized brain structure.

The Institut Marey progressively lost some of its older researchers. Jean Scherrer, after passing the physiology agrégation, rejoined the Salpêtrière, where he organized several conferences between neurologists and physiologists. Pierre Buser had been appointed maître de conferences about 1955 in the physiology department of the Sorbonne, and in 1960 he set up his laboratory on the new premises at the former Halle aux Vins on the quai St. Bernard. Our collaboration had ended several years earlier, and he was working on the motor cortex of cats with Michel Imbert. I was teaching the psychophysiology certificate, which was an option for the degree in physiology, and many science students who wanted to get a doctorate chose it. So I had an audience of psychologists and scientists, and the examinations included an oral exam through which it was possible to get to know the candidates better. In this way, I oriented various psychology students toward physiology—Jean Delacour and Michel Imbert, as well as science students—Monique Denavit, Elizabeth Trouche, Jacques Glowinski, and many others, not all of whom stayed in research.

When I met Glowinski, he had just finished his studies in pharmacy. After a brilliant oral exam, I suggested that he go into neurochemistry, which was just beginning to develop, and I thought of finding him a training post at the Pasteur Institute. That attempt met with some difficulties, so I looked for ways to place him in an American laboratory. He was accepted by Julius Axelrod, who was starting to shine in neurochemistry. Because the available position had to be filled quickly, I saw our friend Louis Bugnard, who in a few weeks obtained a grant for Glowinski to leave for the National Institutes of Health (NIH). Before going, he learned rat stereotaxy at the Institut Marey. I had steered Glowinski toward neurochemistry in agreement with Professor Guillaume Valette, dean of pharmacy, who was going to find him a permanent post on his return. Unfortunately, after his long stay at NIH, the situation had changed in the faculty of pharmacy, and Glowinski rejoined us, setting up a laboratory on the premises my husband had prudently reserved for him in the Collège de France. Rhône-Poulenc and INSERM contributed generously to his set-up.

The Institut Marey had several departments; mine was on the top floor. Its equipment and personnel were of different origins—CNRS, Collège de France, university, and grants over several years from the United States Air Force and NIH. Professor David McK. Rioch of the American naval laboratory had visited our laboratory and offered aid, but he had to withdraw it as the Navy could not be in competition with the Air Force. I always remember his friendly attitude and visited him my first time in the United States when Nauta and William Mehler were working in his laboratory.

In 1962 or 1963, while Dick Wendt was working on amygdaloid responses, I learned that a Russian trainee, Mme Olga Merkulova, a pupil of Vladimir N. Chernigovsky, was arriving earlier than expected at the Institut Marey. Only Dick's research related to Chernigovsky's on visceral projections. Dick was kindness personified, so I asked him to collaborate with Merkulova. At that time of cold war, a Russo-American team was not necessarily viable, and for a while there were a few snags, but progressively our Russian and American colleagues developed a sound friendship. One day Merkulova, who had a son in Russia, told me that Dick Wendt was like another son to her, and she wished he could work with her one day in Leningrad. She went back after six months, and I have not seen her since. Although we exchanged letters, I never had the courage to tell her that Dick Wendt died in sad circumstances soon after his return to the United States. He had stayed several years with me, then obtained a post at the Massachusetts Institute of Technology (MIT) in the department of Walter Rosenblith, but Dick was ill and could not bear the pressure of his illness. Before leaving Paris, he promised me he would return from his "training course" in the United States. In turn, he entrusted me with another investigator, John Liebeskind, who played an important role in my research. I received a letter in almost perfect French from Liebeskind, a young American postdoc, asking to come and work in France for a limited time. I replied in excellent English, offering a place for the following years. When Liebeskind arrived, he spoke no French, and my English was still poor. His letter had been written by Dick, mine by George Krauthamer, an American who had just arrived in Paris and who was perfectly bilingual.

Krauthamer was married to a black American, Eleanor, who had trained in sociology. She came to work in the laboratory with Mme Laplante and quickly learned the anatomical techniques. George Krauthamer was a skillful researcher who had spent several years in France as a schoolboy before emigrating with his parents to the United States. He served in the American army and later worked with Hans Teuber and was familiar with behavioral methods. With us he soon assimilated the techniques of neurophysiology. We had intended to work with Krauthamer on the behavioral role of the projections to the caudate nucleus demonstrated with the Brazilians. After some fruitless tests, we noticed that stimulus trains to the caudate nucleus suppressed all activities arriving in the medial thalamus and associative cortex, without affecting primary responses. This became George Krauthamer's personal topic, which he pursued with several collaborators; American, Japanese, and French. He remained at the Institut Marey for several years on NIH contracts and returned to the United States in 1966 after a period as a part-time assistant secretary of IBRO.

With John Liebeskind I returned to recording unitary activity in the somatomotor cortex, the problem that had initiated my research on mammals. This time we recorded in monkeys, in which cortical mapping had been started with my Brazilian friends. We placed microelectrodes in the pre-Rolandic cortical region where Clinton Woolsey and Hsiang-Tung Chang, as well as Karl H. Pribram and Lawrence Kruger, had demonstrated evoked potentials on stimulating dorsal roots or motor nerves. Microelectrode recording showed that cells there were activated by movement but also by muscle stimulation. Those experiments were always long, and I recall once leaving the laboratory toward midnight, exhausted, after we had begun to record from a pyramidal cell that responded tonically to movement, and to flexion or extension of the hind limb with prolonged excitation or inhibition. At about 7 a.m. John came looking for me; the cell was still active. Frank Echlin, a New York neurosurgeon who had formerly worked with my husband, participated in these experiments during visits of several weeks; his wife came also, and we are friends to this day.

Professor Adrian was to retire, and his pupils organized a symposium. I was invited at Bryan Matthews' initiative, and I presented our first results on the representation of muscle afferents in the motor cortex. There I again encountered many English friends and American acquaintances. I dined for the first time in the hall of Kings College. Women had not been admitted to these dinners before, and a female student of Adrian and I found ourselves in evening gowns at the foot of a table full of men, in an icy hall. On my previous visit, Lady E. Adrian had looked after me while my husband was invited to the high table. Only 10 years later, at a meeting organized by the psychophysiologists, were the rules moderated, and I dined at the high table.

Dr. William D. Neff, who had heard me present an overview of our work at an American meeting, asked me to summarize it for the neurobiology review he edited. For the first time, I wrote the text in English; it was corrected by American and English friends visiting the laboratory.

The responses we had obtained in cat and monkey with chloralose anesthesia had always been received with reservations, the more so because the responses to muscle input we found in the motor cortex of monkeys had not been observed in the cat by Mountcastle's team, who thought such signals only reached the cerebellum. The actions of different anesthetics then had to be explained. With Pierre Aléonard we decided to look for responses in the unanesthetized animal. Aléonard was the technician who had helped me in many of the experiments I have described. He had fashioned a sealed chamber maintaining liquid over the cortex during microelectrode recording and had made bipolar recording electrodes inspired by those of Magoun.

To avoid using anesthesia during recording, in a preliminary stage we placed bipolar concentric recording electrodes in anesthetized cats and fixed them at the upper limit of the structures to be recorded, with indwelling stimulation electrodes on a cutaneous nerve of each anterior paw. The assembly led to a connector fixed to the skull. Operated in aseptic conditions, the animals supported these implants well; they remained friendly and allowed recording of responses to moderate stimulation without need for restraint. The recording electrodes had a central part that could be lowered by fractions of a millimeter, with the main part fixed. We thus observed bilateral responses to stimulation of cutaneous nerves—similar to the responses described with chloralose—several days after the animals had eliminated all trace of anesthetic.

To our astonishment, these responses were not consistently present, appearing only when the animal took no notice of what we were doing, and disappearing when it looked at us. These observations, made with Aléonard and Mallart, showed us that responses in the medial thalamus were of large amplitude only when the animal was drowsy or in slow-wave sleep and were absent or of feeble amplitude in the awake animal or one in paradoxical sleep. Thenceforth most of my recordings were done in "chronic" cats and monkeys, and we practically gave up using chloralose anesthesia. The responses of the medial thalamus were almost completely forgotten for a time, but recent research on thalamic activity in chronic pain has recalled that this part of the thalamus certainly plays a role in pain sensation.

This work was done in the department I directed at the Institut Marey, and it was matched by the department directed by Ladislas Tauc, dealing essentially with more elementary phenomena. Attached to Tauc's department was a pupil of Buser, who had not followed him to the university and who worked with the former Argentine researcher Hersch Gerschenfeld, who with his wife had obtained CNRS positions. Mme Dora Gerschenfeld had left for the university with Buser, along with Michel Imbert and Gesira Battini. At that time several new workers, French and foreign, joined us—Philippe Richard of INRA and Henri Korn, a neurologist who worked for a while with Dick Wendt then did related research with Pierre Auffray from INRA. I also had Jim O'Brien and Angharad Hews-Pimpaneau, who was English but was married to a Frenchman; Ilan Spector from Israel; Yamila Hassmanova, a Czech; and later Yeheskel Ben-Ari, another Israeli. I had long wanted to average evoked potentials from "chronic" animals but the methods were not easily available. The apparatus built by George D. Dawson in London used capacitive memory. In Paris, Scherrer at the Salpêtrière was the first to have an averager, thanks to his pupils' technical prowess.

Computer systems were developing, and Walter Rosenblith at MIT had equipment that was relatively easy to use. Assisted by the research department of the American Air Force, we set up a collaboration. We implanted cats in Paris and shipped them to Boston, where evoked potentials could be studied during the sleep-waking cycle. This procedure allowed us to quantify the amplitude variations over relatively stable states of vigilance, monitored by simultaneous records of cortical and muscular activity. These experiments were performed around 1963 with Jean Massion, who accompanied me to Boston. They were pursued further, always with Rosenblith's assistance, by one of my researchers, Gisèle Guilbaud, who thus began a doctoral thesis which she completed in Paris with the averager we finally obtained. While in Boston, I gave a seminar in my imperfect English on the responses observed in the medial thalamus. I was surprised to see in the audience an English friend, the psychologist Richard Oldfield, who was visiting a neighboring laboratory. I always spoke to him in French, which he used perfectly, and I was ashamed to reveal my poor English. I then decided to improve my vocabulary by reading the simple English books recommended by my "English teacher," John Liebeskind, who spoke excellent French.

From 1961 on, much of my time was devoted to a new theme, recording thalamic activity in humans. The neurosurgeon Jacques Le Beau had for some years paid friendly visits to my laboratory. One day he invited me to a lecture by a colleague, Gerard Guiot, who had for several years been trying to alleviate Parkinsonian rigidity and, above all, tremor, by localized brain lesions. After trying pallidal lesions, he began making them in a thalamic region anterior and superior to the ventral posterior (VP) nucleus. The lesion site was close to the internal capsule, which he was careful not to damage. To localize his electrode, he was using the threshold stimulus through the electrode to provoke a motor response—the farther away from the capsule the higher the threshold—but this evaluation lacked precision. During the discussion, I suggested that the coordinates could be corrected by seeking the thalamic zone showing evoked potentials, thus demarcating the VP just next to the zone to be lesioned. Michel Jouvet and Raúl Hernandez Peón had already recorded responses in the human VP. The next day Guiot, neurosurgeon at the Hôpital Foch in Suresnes, near the Institut Marey, came to the laboratory to persuade me to set up the technique at Foch. I hesitated in view of the difficulties to be met in working on humans, but Pierre Aléonard, who had an enterprising spirit, insisted that I accept.

We quickly organized exploration of the human brain with deep electrodes. Luckily, by grounding the patient's chair, we were able to eliminate artifacts due to mains interference. Aléonard made bipolar concentric electrodes that were similar to those used in animals and large enough to reach the anterior thalamus from the occipital cortex. These recording electrodes passed easily through the tubes for admitting the coagulation electrodes. With little spare recording equipment, we had to take amplifiers, cameras, and stimulators to the hospital for each intervention. The parasagittal trajectories used by Guiot went from the posterior cortex through the pulvinar before reaching the VP. Cellular structures were easily distinguished by their spiking activity from fiber regions, which were practically silent with our electrodes. As in animals, natural stimulation of the periphery gave us evoked potentials in VP and thus allowed us to verify the lateral and anterior positions required for the coagulation electrode. We used this technique for several years and with its success were able to obtain the funds to buy recording equipment for the hospital.

That was the last study I did with Aléonard's assistance. He was intelligent and technically proficient but, having been orphaned when young, he had been unable to pursue his studies and suffered from it. He did not see that technique was not everything; it had to be complemented by knowledge of the literature. I offered to lighten his duties so he could pass the examinations that would allow him to do independent research, but he refused and attached himself to Jean-Marie Besson's team. He died rather early from a heart attack, leaving behind young children and a seriously ill wife.

These first recordings in humans were done with a team including a radiologist, Etienne Herzog, who was easy to work with. The team also included Geneviève Arfel, an electroencephalographer; Guy Vourc'h, an anesthetist; and Serge Brion, a neuroanatomist. We quickly became friends. Many foreign trainees were present in Guiot's department. A Canadian, Jules Hardy, was there when recording began, and he went to Spain with Guiot to present the findings to an international congress. I thus had the occasion to work with trainees from Spain and Latin

America, whom I remember with pleasure. We often had visits from neurosurgeons or neurologists during the operations, so I met specialists I would not have known otherwise. I think particularly of a Barcelona neurologist, of Spanish neurosurgeons S. Obrador and G. Dierssen, Antonio Subirana, Antony M. Halliday and Valentine Logue, John Bates from London, Claude Bertrand from Montréal, F. John Guillingham from Edinburgh, and Rudolf Hassler, Wilhelm Umbach, Hirotaro Narabayashi, and Albrecht Struppler, who would become firm friends. Young French neurosurgeons also did their internships at Foch, among them was Patrick Derome, with whom I worked longest.

Using somewhat finer electrodes, we were then able to observe bursts from thalamic neuron units at the tremor frequency. Some were nothing but evoked activities, but others seemed to precede the tremor. Herbert Jasper, IBRO secretary, back in Montréal also began recording with Gilles Bertrand, but using fine tungsten electrodes, as I learned during my visit to the French University of Montréal and the English language Institute of Neurology, with Guiot in 1963 or 1964. I had prepared two complementary lectures, one for the University of Montréal and one for the Neurological Institute. Alas, the Anglophones did not attend the first, and only a few Francophones the second.

We also presented our results on rhythmic thalamic activity at the New York congress of 1966, organized by Melvin D. Yahr and Dominick Purpura, where I again encountered Pierre Cordeau. This French Canadian had received part of his education in English, and he helped to link the two communities. With Jan Gybels he had observed activity preceding trembling in the cortex of a macaque with tremor from an operation done by Louis Poirier of Quebec. Like me, Cordeau was an engineer who had converted to physiology, and we understood each other. We maintained our friendship until his premature death. He had sent me his pupil, Yves Lamarre, who worked on the rhythmic activities in monkeys and who later completed his training with Ragnar Granit and then Vernon Mountcastle.

Our work on humans had some repercussions, and in 1964 the Foreign Affairs ministry sent Guiot and me to present our results in Japan; I also spoke globally of my neurophysiological work, Guiot of his neurosurgical results. Our visit was organized by Yasuji Katsuki, the dean of medicine in Tokyo who specialized in audition, and by Hirotaro Narabayashi, one of the first neurosurgeons to make thalamic lesions in Parkinsonians.

My trip began with a short stay in Los Angeles to visit the Brain Research Institute set up by Horace Magoun, where Lawrence Kruger and Madge and Arnold Scheibel worked. I had met the Scheibels in Paris when they worked with Moruzzi. Susumu Hagiwara was there also. He was a colleague of Katsuki and was the first Japanese to contact me after World War II. Like me, Hagiwara had worked on an electric fish, the narcine, and we had exchanged letters and publications. He had visited France after my husband's trip to Japan in 1961, as had Katzuki and his wife. We met with Guiot in San Francisco, and our results were presented in the neurosurgery department, where I met Benjamin Libet and his wife, who have remained friends of ours. Mme Guiot joined us, and we left for Japan. We went to Tokyo, Osaka, Nagoya, and Kyoto, then back to Tokyo. We often saw the anatomist Hajime Mannen, who spoke perfect French after working in Paris; Toshihiko Tokizane; T. Tomita, who was making superb ultrafine microelectrodes; and many others. Narabayashi took us for a weekend to Hakone, accompanied by his assistant, Chihiro Ohye.

We decided that Ohye would come to our laboratory and the hospital for a few months. This was made possible by a grant provided by my old friend Dr. Pinchas Borenstein. Ohye completed this tour with Louis Poirier in Canada. Ohye often came to work at the Institut Marey with me, Massion, or Jean Féger. He is one of my oldest collaborators and one of the most faithful. For his part, Narabayashi was always an attentive friend, as were Katzuki and his wife, and Hagiwara. In this sense, my visit to Japan was a great success, and it must be said that my Japanese friends were ahead of their time, for without them I would not, as a woman involved in research, have been well accepted there.

On our return to Paris in 1964, we obtained support from the CNRS for a technician to follow up the patients operated on at Foch. Foreign teams began to use our technique. I would have liked computer methods to determine in which structures the abnormal activities of Parkinsonians originated. But the number of operations diminished with the appearance of drugs that reduced the dopamine deficit of the corpus striatum in Parkinson's disease. Professor Bugnard, director of INSERM, had set up a unit at Foch to allow us to promote research on Parkinson's disease, as well as on other CNS diseases. We had foreseen a program on pain, with the neurosurgeon Dr. Jacques Rougerie. I also pursued some investigations with Dondey and Le Beau on the use of cooling probes in neurosurgery, but the great initial enthusiasm for collaboration between scientist and neurosurgeon was over, and each side resumed the course of its own work.

Grey Walter invited me to present the results obtained in Parkinsonians at the EEG congress of 1965 in Vienna. There I met Russian researchers who were dealing with similar problems—Mme Natalia Bechtereva and Mme Svetlana Raeva. The latter obtained a grant for six months to work with me in Paris. With sound training in electrophysiology, she made in Moscow the sort of recordings we were terminating in Paris. She, Ohye, and Narabayashi were for years almost the only ones to perform lesions on VIM (the anterior part of the VP thalamic nucleus) and to continue research on the human thalamus. About 1985, there was a renewal in the study of thalamic structures in humans, thanks to Ronald Tasker.

For several years, I divided my time between the Hôpital Foch and the Institut Marey. My research in animal physiology had been reduced, but thanks to Liebeskind, Lamarre, Krauthamer, and Massion, work continued with the monkey motor cortex, the caudate nucleus, and the red nucleus. Then, with new researchers, we studied the facial motor cortex, the claustrum, and the role of the amygdala in learning.

I met Professor Wade Marshall, director of the neurophysiology laboratory of NIH in Bethesda, while he was working in Brazil with Leão on spreading depression. After Jean's birth, we met again at the congress in Montpellier, where Wendt presented his work on the amygdala. I visited him in Bethesda and we decided Wade would go to Paris with his wife Louise for a six month sabbatical. Wade Marshall was one of the earlier investigators of the cortex using the CRO. He was at that time looking at the effects of respiratory gas composition on cortical activities, reflected in variations of cortical direct current. We continued this work together in Paris, using an apparatus—a capnograph—to measure expired CO_2 levels in animals. The methods were already available for humans, Vourc'h had told me of them, and a colleague of his lent us the equipment needed for the investigations. This work led to the systematic use of the capnograph in animal physiology, and it was carried on further during visits to NIH. One of the recent arrivals at the Institut Marey, Jean-Marie Besson, joined in, and we worked with Wade's collaborator, Dr. C.D. Woody. So my sojourns to the United States began with Wade's laboratory, and I have kept lasting contact with Louise, who now lives in Los Angeles. Approaching retirement, Wade endured the effects of loss of power, his publications were attacked more freely and sharply. He suffered from such bitterness and died not long after retiring.

In 1964 to 1965, we were visited by a Russian professor, Arpashev I. Karamian, for several weeks, and despite the absence of a common language and difficulties with scientific discussion through an interpreter, we got on well. I met him again later in Moscow.

The American Air Force had developed a chimpanzee breeding-station at the Holloman Air Base in New Mexico. It was tempting to study in a related brain the activities of the thalamic structure we knew so well in Parkinsonian humans. So in 1967 a research program was organized with the Air Force team, and I went to Holloman for two months, accompanied by Patrick Derome from Guiot's team, whose thesis was on recordings in the somesthetic thalamus of Parkinsonians. Derome stayed a month and established the stereotaxy for chimpanzees, based on what had been done in humans. He implanted sterile cortical perforated plates, allowing us to work on the somatomotor cortex of the unanesthetized chimpanzee. John Liebeskind, now in Los Angeles, joined us at Holloman, with two technicians from the Institut Marey. I took Jean with me, hoping that at eight-years-old the experience would help his study of English later. Colonel

Clyde Kratochvil, who was in charge of the laboratory, was most welcoming. The physiologist Jack Rhodes and others there worked with us. The research facilities were guaranteed by a contract with the Air Force. Our technicians got on well with their American colleagues. We installed a laboratory for glass microelectrode recording, and with Liebeskind we examined in the cortex how motor effects of stimulation were linked to afferent signals received by cells recorded in the same sites, depending on the cortical zone. My husband came for several days, then left for Paris with Jean. I returned to Paris a little later after a detour to Montréal for a symposium on Parkinsonism. I had the unpleasant experience the day before my presentation of finding that all my slides had been left at Holloman, and I had to give my talk with chalk and blackboard. I returned to Holloman again for several weeks to try to finish some chronic experiments with Liebeskind, knowing that further visits would be needed for these experiments to bear fruit. But this was at the end of 1967, and after the chaos in Paris in May 1968 it was not possible to get the necessary funds and favorable conditions to work at Holloman. Finally the chimpanzee station, created mainly for sending a primate into space, was disbanded.

On returning to Paris, I learned that Jean Massion had agreed to join the Institute of Psychophysiology at Marseille directed by Jacques Paillard, a former researcher at the Institut Marey who had specialized, with my husband and Dr. Tournay, in the electromyography of human movement. Svetlana Raeva arrived from Russia, and with her I studied relations between the substantia nigra (SN) and caudate nucleus in cat and rat. This work had been started with Marthe Vogt, who wanted to look at dopamine liberation in the striatum after nigral stimulation. Marthe had spent two weeks in Paris to establish the stereotaxic bases for stimulating the SN. The nigro-caudate pathway demonstrated by a Swedish team using fluorescence methods was thus studied by electrophysiology, together with a caudato-nigral pathway. Our preliminary publication followed on the heels of a paper by Tomas L. Frigyesi and Dominick Purpura, which showed similar results. The SN-caudate nucleus relations remained a topic of interest for some of my researchers for a long time.

An Australian university researcher, John McKenzie, arrived from Melbourne in 1968 with his family for a sabbatical year in France, and with Paul Feltz he studied the effects of repetitive nigral stimulation on the caudate nucleus. McKenzie often returned to Paris and later worked with Jean Féger. The visits of others were not so happy. A Brazilian who had worked in Russia asked to spend a year in my laboratory while awaiting authorization to return to his country. He arrived while Raeva was here and pretended to work with her, but he certainly engaged in other activities and disappeared in May 1968 after being seen in many political demonstrations. An American, Rosalie Futnick, who had strongly insisted on coming here, also disappeared after some political demonstrations.

The Institut Marey laboratory had become too big and was overpopulated. Difficulties arose between researchers, mainly because of rivalries. The people working with Tauc on molluscan neurons were devoted to the elementary cellular phenomena of synaptic excitation and inhibition. They found their space and funds insufficient. They also believed that an understanding of the nervous system could be gained only by their approach, and their remarks stole all enthusiasm from those investigating the CNS of vertebrates, some of whom abandoned their former research program for more elementary problems. And the French trainees arriving from the diploma of higher studies (DEA) who I was teaching were not always up to standard.

In 1967, I made my first visit to Russia. I was invited, together with Pierre Buser, W.H. Nauta, and Marthe Vogt, to a symposium organized at the Moscow Brain Institute by Semjon A. Sarkisov, successor to my friend Smirnov, who died young. Arpashev I. Karamian was also at the symposium, accompanied by his pupil, Nicolas P. Vesselkin, who spoke perfect French. I met other workers, Vladimir Skribilsky and Leonid L. Voronin, who despite material difficulties had developed intracellular brain microelectrode recording. I also met several female professors or researchers and got on particularly well with them. I again met up with Svetlana Raeva and her husband, an enthusiastic and obliging Georgian. I have ever since maintained good relations with the brain institute and its director Oleg Adrianov, an anatomist who replaced Sarkisov until he died recently. Adrianov often came to see us in Paris, and I returned to Moscow in 1980 at his invitation.

The CNRS had decided to move the Centre d'Études de Physiologie Nerveuse to Gif sur Yvette; the site was selected, and plans were drawn up. Separate departments were envisaged, and everyone wanted theirs to be the most important. My husband was still to be director of the center, but he was two years from retirement and many saw themselves as potential director.

Animal Models of Parkinsonian and Pain Syndromes, 1968–1984

I was still teaching in psychophysiology, but I had enough independence and my colleagues quickly had me promoted to a full personal chair. I continued to group the courses into one day per week, but the neurophysiology teaching in which I was also involved took another morning. I was elected president of the commission for animal biology of the faculty of science, which often occupied one day per week. And for several years, I was an expert for the army department that funded physiological research, an obligation I remember with pleasure. All these duties reduced my time at the Institut Marey, and we did not really recognize the growing disquiet.

The first difficulties arose in the faculty of sciences where students were more and more numerous. To satisfy their demands, our dean obtained a significant number of lecturing positions that had to be filled quickly, although good candidates were rare. The students grew more discontented and, although we had received the means to improve their conditions of study, it could not be done fast enough to satisfy them. For awhile the unrest was confined to the Sorbonne and the laboratory was fairly calm.

In May 1968, I had been invited for some months by Brodal to give a lecture in Oslo, and I went. The cancellation of an Air France flight landed me a day late in Norway, but I was welcomed and was happy to meet researchers who had previously been only names to me, including F. Walberg, E. Rinvik, and Per Andersen. I left for Paris after watching a sunset over Oslo fjord with Brodal and his wife. When it was announced on the plane that we were landing in Brussels, my first reaction was that I had caught the wrong flight; I had once made such a mistake in the United States and found myself in Houston instead of Boston. But this time it was nothing of the kind—the Paris airports were closed by a general strike. Driving back to Paris by an indirect route to avoid customs, I found the city totally disorganized. I was able to get to my apartment but understood how serious things were only upon going to the laboratory.

A general assembly was meeting that included researchers, technicians, and cleaning staff, presided over by a young researcher. I heard criticism of the bosses who were opposed to the employees, researchers, students, and technicians. I started to say that in our profession of research we were all employees of the state and this opposition did not exist, but the president called me to order and told me to speak only when I was recognized. Thunderstruck, I left, and only on exceptional occasions returned to that type of general assembly. However, I had to attend similar sessions at the faculty of sciences, where agitators wearing Mao jackets came to announce student deaths. There was not one student death in 1968, but there was much destruction of material. I also encountered material problems because the centers for postal cheques, which looked after our salaries, were on strike. Luckily a grocer friend gave us credit, and the faculty paid us an advance. We were, however, able to go on May 20 with Jean-François Dormont, a pupil of Massion's, to the symposium on Parkinson's disease in Edinburgh.

When I got back to Paris, the children were not going to school and the laboratory was unbearable. I had enough petrol left to go to our house in Touquin and try to live out this difficult time in a calmer country environment. I had not foreseen that problems would arise in domestic life also. The daughter of my son's baby-sitter used to live with us during vacation, but at 15 years of age she was in complete revolt. When calm returned, I refused to have her at my place, so her mother, who had looked after Jean from birth, left us, obliging me to find a new solution. De Gaulle eventually put an end to the disorder created by the absence of government, and we were able to return to Paris. However, many of the things we were attached to were destroyed and the return to work was difficult.

At the Institut Marey, the agitators did not forgive me for not bowing to their arguments and for refusing the system they tried to establish. Some of my students, impressed by the agitators' speeches, had been led on, others kept quiet out of fear and pretended not to know me. To finish with these absurdities, the "laboratory collective" conducted a sort of trial of my husband because he intended to accept in his group one researcher who was not liked by another. My husband, who had not properly realized what was happening, was greatly affected by this episode. A friend suggested I leave for Canada. I simply decided to separate myself from the Centre d'Études I had helped to create, but which was now in the hands of sectarians, who in any case were soon to abandon the laboratory whose atmosphere they had destroyed. When order returned to the Paris region, the baccalauréat examinations had to be held, but only orals could be organized. Usually, tertiary teachers had little input into this exam, but this time they were called on to organize the boards of examiners. When summer holidays arrived, all the general assemblies dispersed to go camping. I went to Brittany with Jean, but it was hard to forget the weeks we had just lived through. People in the provinces had no idea of the stresses we had borne; it merely seemed to them that Parisians had aged.

The absurdities of 1968 greatly upset the work of French researchers, who had taken many pains after World War II to catch up with other countries, and never fully recovered from this trial. The organization of the new universities from elements of the old was made out of political considerations, without regard for the needs of students, teachers, or research.

For my part, I had decided to join the INSERM laboratory created by Bugnard for Guiot and me. Guiot was in accord, as was Mme Arfel. However, the premises had to be reorganized to install experimental laboratories. The plans for the change were well advanced, but when it came to fixing dates, Guiot told me he no longer agreed to my joining his laboratory. My husband advised me to stay at the Institut Marey, which would be evacuated by the CNRS personnel but would remain the property of the Collège de France. So I reorganized a smaller laboratory, with sadly reduced funds.

For several years an Algerian student, Mohamed Abdelmoumène, had been with me. I had met him when I went to Algiers after the independence war to visit Annette Roger, whom I knew well in Gastaut's department. Abdelmoumène had arrived at the Institut Marey when his government was changing direction. He was cultured, worked and wrote well, and obtained a CNRS post. I advised him to study the inhibitory effects of higher centers on spinal levels. Abdelmoumène chose to look at such inhibition using dorsal root potentials, and for this he collaborated with Jean-Marie Besson. Later, on my advice, they and their collaborators studied these controls with microelectrodes. Then Abdelmoumène passed the physiology agrégation in the faculty of medicine and was appointed professor of physiology in Algiers, choosing Algerian nationality. Besson stayed with me at the Institut Marey, forming a team with Gisèle Guilbaud, who had just passed her thesis on evoked potentials in "chronic" animals. Besson had himself changed his research theme. After working with Wade Marshall, Woody, and me, he passed his thesis on problems associated with the action of different respiratory gases, and then worked with Abdelmoumène on the control of spinal afferent signals.

Besson's wife, Marie-Josèphe, had obtained her secondary agrégation, and after a year teaching in the country had taken the post of assistant in my department at the faculty of sciences. She was trained as a biochemist, and I thought she would do better at research in the laboratory that Glowinski was developing at the Collège de France. She did her doctoral thesis there and continued her research while she served as senior tutor in psychophysiology at the faculty. Shortly before my retirement she became a professor in the same faculty. We maintained good relations, and she took over some of my former students.

At the faculty, a unit for teaching and research (UER) had been created, grouping researchers in physiology and embryology. The first director was a biophysicist, but the major power was in the hands of Professors Alexandre Monnier, André Thomas, André Soulairac, and Louis Gallien, with whom I never got on too well. My position was thus precarious. I was astonished several years later (about 1972) to be called on to direct the UER of physiology. I accepted and was reappointed to these duties until my retirement in 1985. During that time I had the pleasure of seeing my friends Alfred Brodal and, a little later, Stephen Kuffler and Susumu Hagiwara, receive an honorary doctorate from our university.

With the return of calm, we could work. I was first visited by Ian Donaldson, a neurophysiologist who was working on humans at Edinburgh with the neurosurgeon Guillingham. Donaldson and I continued the study of monkeys begun with Liebeskind on the chimpanzee cortex. Donaldson's wife, Patricia, studied histological techniques with Mme Laplante. The Donaldsons returned to work in England, first at Oxford with Whitteridge and then in Edinburgh. We are still in contact. About the same time I received an honorary doctorate from the free University of Brussels, presented by Professor Pierre Rijlant.

During this period Ainsley Iggo, who was editing the volume on somatic sensation in the series published by Springer with Richard Jung as general editor, asked me to write an article on nonspecific projections. I associated Besson with it, and he helped with the bibliography on the spinal relays.

My friend Guy Vourc'h who came to see me, although Guiot had excluded me from the Foch laboratory, entrusted me with his assistant, Alexandre Levante, who worked in the laboratory as well as in Vourc'h's department at Foch. Levante stayed with me for many years.

We worked with the antidromic technique for determining direct connections, with the Czech visitor Rokyta. We looked in the medial thalamus of the cat and monkey for cells projecting to the cortex. Some of the work was done with a Russian visitor, Karine Vetchinkina, who spoke French fluently, as her father had been in the Normandy-Niemen division during World War II. I believe he ran it and, being a widower, raised his little girl himself, among French aviators. Her knowledge of French made her choose to teach at the Patrice Lumumba University, which trained cadets for service in Africa. She had not lost her Francophilia, and we stayed great friends until her recent death. She was in Paris during the period after the Prague Spring, so the discussions with Rokyta were vehement, but of good will. Levante, of Russian origin, spoke the language too, and the atmosphere was pleasant and relaxed.

Through a trip to Sweden at Zotterman's invitation about 1972 I was convinced that I should investigate electrophysiologically the location of the cells of origin of the spinothalamic pathway. Besson's group did not want to undertake the task, so I decided to do it with Levante. It was an interesting experience. First of all, in many cats we failed to find cells projecting directly to VP thalamus. This type of cell was, however, found in significant numbers in the first two monkeys tried, and we verified that these cells were activated by nociceptive afferents. To do these experiments, we corrected the stereotaxic coordinates by the method used in humans—radiography of the ventricles with a contrast medium.

I presented the results at a symposium on pain organized in 1973 by John Bonica in Seattle. I had already presented them in France and in Moruzzi's laboratory in Pisa, and in the laboratory of my friend Edward Perl at Chapel Hill on the way to Seattle. A pupil of William D. Willis, who was working on the same problem in the monkey, though unknown to me, was at the lecture and quickly published their results. At that symposium I met two Italian researchers, Paolo Procacci and Carlo Pagni, with whom I was to maintain a long relationship; and once again, Patrick D. Wall, Jörgen Liebeskind, and several Americans. The creation of the International Association for the Study of Pain (IASP) was initiated at that symposium. On the return trip, I stopped in Toronto to see Ronald Tasker, whom I knew mainly by correspondence. Back in Paris, I undertook with Gunnar Grant and Jörgen Boivie of Stockholm a study of spinothalamic cells by retrograde marking with horseradish peroxidase.

Around that time I wrote a chapter on somatic sensations in Kayser's *Physiology* (Flammarion) in collaboration with Suzanne Tyc-Dumont with whom I had maintained amicable relations since her stay at the Institut Marey in the 1960s. I also received the Cross of Chevalier of the Légion d'Honneur, conferred by Professor Courrier, life secretary of the French Academy of Sciences, who always gave me solid support. With the support of our vice-dean, Robert Courrier nominated me for the prize of the city of

Paris. This prize allowed me to buy a plot in Brittany, where I had a small house built in the village where Vourc'h was born, where I had spent some weeks each year with my son.

Blaine Nashold came to Paris on sabbatical, and we studied the problem of pain after loss of afferents in humans. We tried to develop a rat model of events after deafferentation. We began the work with a technician from the École Pratique des Hautes Études, Marie-Christine Lombard, who had already received a diploma and could now do a third cycle thesis. Dentists came to the faculty as pupils in 1975 after a change in their course. Knowing this I gave a lecture on facial sensation, which I had not dealt with previously. With two dental students, Alain Woda and Jean Azerad, we studied the location in the spinal trigeminal nucleus of cells connected to VP thalamus.

My husband had been retired from the Collège de France for several years, succeeded by our friend Yves Laporte, who was thus responsible for the Institut Marey. The general secretary of the Collège de France at the time was not pleased to see funds leaving to maintain a laboratory dependent on the university, and he defended us poorly against territorial claims by the tennis club at the Roland Garros Stadium next door. The Institut Marey was condemned, and we had to find another site for my research laboratory. I obtained premises at the faculty quai St. Bernard, where I was teaching, which had been made available by the death of our colleague, Gallien.

Besson's group at the Institut Marey had progressively separated from my team. Their methods differed from mine, and I was not keen on remaining responsible for their work. In any case I did not have the space for them at the university and was happy when the Hôpital Foch offered them the laboratory that Guiot had not been able to get going. Besson soon exchanged these premises for a laboratory located at Saint Anne hospital.

Thanks to Professor Pierre Dejours and also to Robert Naquet, Paul Dell, and others, on leaving the CNRS Centre d'Études I was able to obtain funding for an associated research team, which was renewed until my retirement. This allowed me to retain Mme Laplante, and my laboratory thus kept up histology of good quality. During this period I also received useful support from the Assistance for Medical Research. Jean Féger, who worked with me at Marey, came to the university with me. He later set up his own laboratory in another Parisian university. Paul Feltz worked with us for some time, then took a position as professor at Strasbourg. During those years, I was appointed several times to the consultative committee on universities, which chose candidates for professorial positions. I also returned to Moscow at Adrianov's invitation, meeting up again with Skribilsky, Voronin, Raeva, and Vetchinkina. I also went to Leningrad to meet Alexandre S. Batuev and visited Platon G. Kostyuk's laboratory in Kiev.

Toward 1976, with Professor Courrier's accord, I presented myself as a candidate for the French Academy of Sciences. Professor Maurice Fontaine was a fine referee for me, but I was not the only one to have friends. My opposing candidate was Professor Jacques Benoit, a friend of Courrier. Benoit received more votes than I, but I was not bitter. However, some had said I took credit for work I had not done. I was not given the opportunity to establish the truth, and I never again presented myself as a candidate.

About that time I made contact with Hsiang-Tung Chang, a professor in Shanghai. I knew his work well but had never met him. I received several letters from him and sent documents he asked for. I had the pleasure of his visit around 1978, and he came to dinner with several of his colleagues after a lecture I had organized for him at the university. We worked on related topics and understood one another well. Through him I was invited to Shanghai, but at the time a surgical intervention prevented me from leaving Paris. A second invitation came at the unhappy time of my husband's terminal illness, so I was never able to go to China.

My husband died at 80 years of age. He had suffered from having to leave his office at the Institut Marey, and he never got used to the offices installed for him at the university and the Collège de France. He devoted his last years to assembling and distributing to the museum the remaining vestiges of Etienne-Jules Marey's work that we kept after the Institut Marey was destroyed. Thanks to my husband those materials are now mainly in the Museum of Beaune, Marey's native city.

In 1980, I went at Iggo's invitation to a conference in Berlin on pain and society. There I met Hans W. Kosterlitz, Peter Nathan, Fernando Cervero, Huda Akil, and others. In my last active years, I had some brilliant pupils—Jean-Michel Deniau and Gilles Chevalier continued work on the SN; Pierre Cesaro, a neurologist, worked with me on the relations between corpus striatum and medial thalamus in rats; Jean-Claude Willer, a pupil of Andre Hugelin, did experiments I was involved with on sensory fibers in humans (Peter Nathan came to Paris for his thesis); and finally, my old friend Ed Perl often came to work in my laboratory and give lectures, with visits from his wife and two daughters. A Mexican, Miguel Condès-Lara, a Hindu, Saraj Keisar, and an Australian scholarship-holder, Pamela Sanderson, worked with me on the remote effects of spreading depression propagating at cortical or striatal levels. In 1982, Karen Berkley invited me to speak in Los Angeles at a symposium of the neuroscience congress, on central projection of pain signals in humans.

I received the French Order of Merit, proposed by the president of our university whose efforts to put the university's work in order have been estimable. Thanks to the International Union of Physiological Sciences, I was able to go to the congresses in New Delhi, Budapest, Sydney, and Vancouver. In 1983, I went to a symposium on the basal ganglia organized near Melbourne by McKenzie, where the International Basal Ganglia Society (IBAGS) was created. This society has met several times in different countries, and I was the président d'honneur for the fourth triennial meeting held on the Giens peninsula near Hyères in 1992.

Robert Naquet, Jean Scherrer, Pierre Dejours, and Yves Laporte have remained devoted friends. Another friend, Daniel Bargeton, who took great pains to defend me at the time of my academy candidature, unfortunately died soon after. I have always maintained good relations with my foreign collaborators. Tauc, Glowinski, Massion, Denavit, and Trouche have never neglected me. My friend Professor C. Lucking nominated me as an honorary member of the German EEG Society and invited me to Freiburg for the ceremony. There I once more met Richard Jung, whom Lucking succeeded, and my old friend Creutzfeld. I have received the medal of the city of Grenoble, and more recently of the City of Paris, the Spiegel and Wycis silver medal. Narabayashi, with the aid of Jean-Baptiste Thiébaut and a Swedish friend, Christian Soop, organized a congress at Evian on microelectrode recording in humans, where I was the guest of honor.

Back to Work with Neurosurgeons, 1984–1996

Soon after my retirement, Ronald Tasker invited me to come to his department in Toronto for a few months in 1985 as an exchange professor. Together we revived the recordings permitting demarcation of thalamic structures in humans. Our collaboration has been most pleasant, each respecting the other's work. I have enjoyed the efficient help of Jonathan Dostrovsky and the fine team we formed with a Japanese trainee, Katsumi Yamashiro, and an American of Polish-Mexican origin, Jacob Chodakiewitz. This work was continued by a stay with the neurosurgeon Ronald Young in Los Angeles, where I again encountered Chodakiewitz and met the efficient Patricia Rinaldi, who was easy to work with, and a German neurosurgeon, Wolker Tronnier.

I was invited back to Japan in 1984 by Ohye and Narabayashi, and was welcomed by many friends—Yasuji Katsuki, Yotaka Oomura, Hiroshi Mannen, Toshikatsu Yokota, Katsumi Sasaki, Masao Ito, several neurosurgeons, and others, as well as Professor C. Brooks and his pupil Kiyomi Koisumi. I found that the material situation had greatly improved for scientists, but the situation of Japanese women in research still seemed to be difficult.

On returning to Moscow in 1990 at Raeva's invitation, I met Chihiro Ohye once again. Raeva's work is excellent, and I have been happy to help her publish in the *EEG Journal*. Vladimir Skribilsky, who I had encountered only in Eastern Europe, was at last able to visit Paris. We went to Chartres, which this admirer of old churches had long wanted to see.

When I had to leave my university laboratory in 1985, I was able to set up a research post in Professor Alain Rérat's INRA laboratory with some equipment American friends had given and some the CNRS had loaned. Bernadette Felix was there finishing a thesis on the goose brain. Pamela

Sanderson came with me to Jouy en Josas, with one of my last students, Olivier Rampin, and a trainee from Gabon, Roger Mavoungou. Mavoungou had worked several years at the university and done his thesis while at Jouy en Josas on the activities produced in the pars reticulata of the SN by destruction of the pars compacta, a study still in progress.

The period surrounding my retirement should be called the Italian period, for I spent many months in Bologna and then Chieti. My contact with Italian research began late, though I had known Moruzzi and his pupils. I did not know P. Procacci well until 1976, when he organized the first IASP congress in Florence. We prepared the program with Carlo Pagni, and John Bonica was not entirely satisfied with it. Nevertheless, Bonica asked me, and I was astonished at this, to be the first president of the society. I did my best to fulfill that task, which I was to pass on to Bonica at the following congress in Montreal. In 1982, I attended a symposium on thalamo-cortical relations, organized by Giorgio Macchi in Milan.

Previously I had been visited by Professor Antonio Urbano, a Sicilian working on the claustrum. He invited me to Sicily, where I met his deputy, Salvatore Sapienza, who came to work with me at the university for several years. Sapienza was careful and competent, and I happily received one of his pupils, Rosario Giuffrida. At Sapienza's suggestion, I was invited to give a lecture on pain to the Italian Physiological Society. The professor of pharmacology at Bologna, Carmela Rapisarda, was interested in my report describing the use of spreading depression and invited me to initiate a study with this technique and to give some lectures on pain. In this way, I spent several months after my retirement in the Institute of Physiology of Bologna directed by Professor Pierluigi Parmeggiani. With Rosario Giuffrida and Georgio Aicardi we used spreading depression to study the control of the red nucleus by localized cortical regions. Unfortunately, the reviewers for the American journal to which we sent an article for publication had no idea of spreading depression, and the article was rejected. It was subsequently published by the *Archives Italiennes*, thanks to Ottavio Pompeiano. We should have put up a fight, but Mme Rapisarda was ill and our collaboration ended.

At a symposium on headache organized by Leonardo Vecchiet and Federigo Sicuteri, I met Marie-Adele Giamberardino. She later came to work with me at Jouy en Josas and established a technique to model the pain of renal colic. Our collaboration has continued. I returned several times to Vecchiet's department in Chieti, where with Marie-Adele we set up a laboratory, the first results of which were presented at an international symposium. There I met Professor Renato Galetti, who had a deep understanding of referred pain and whose influence on the Italian school is doubtless underrated. From his pupils, I discovered an interest in research on visceral and referred pain.

During this Italian period, I gave several lectures on pain, which impelled me to write a didactic book, which is now published.

In 1989, I was nominated to the French committee for evaluating universities, on the recommendation of my colleague, Alfred Jost, then permanent secretary of the French Academy of Sciences. I just finished this mandate of four years, which involved visiting universities and drawing up reports. This activity allowed me to meet and evaluate colleagues in other disciplines and to assess the progress accomplished by provincial universities.

At INRA we established stereotaxic methods with radiological intracerebral reference points for the pig, which were to serve as the main model for nutritional research. An atlas of the pig brain was constructed and awaits publication. Our technique is in use by the Japanese.

I was invited to meetings to mark the retirement of my foreign friends Janos Szentagothai, Albrecht Struppler, and Ainsley Iggo. I celebrated the honorary doctorate of my friend Manfred Zimmerman in Siena. In 1989, Otto Creutzfeld and I were invited by the chair of physiology to visit the East Berlin university, where my former Chilean pupil, Guy Santibañez, was teaching. My Montréal friends invited me in 1987 to give the J. Barbeau Lecture. In 1995, my friend Richard Keynes and I were invited to go to Brazil for the 50th anniversary of the research institute.

The INRA laboratory where I used to work disappeared, after a change of direction. With no place to continue my research, I thought I would stop laboratory work completely. But with pleasure I joined the laboratory originally created by my friend Borenstein at the Villejuif Hospital, where he ended his career and where I am now working with his former pupil, Mme Françoise Gekiere and with Guy Allègre, who was my technician 30 years ago at the Institut Marey.

I will soon be 80, and with this autobiography I have reviewed the work accomplished in 50 years of research. I have realized that collaboration is easier and more lasting when done with foreigners, no doubt because power struggles are avoided. I have also realized that fashions in science are a dangerous impediment to progress, and it is well to resist yielding to them.

In ending, I want to thank all who have helped me in my research, and to excuse myself if space limitations have not allowed me to mention them all. I also want to thank warmly Dr. McKenzie, who has written the English version of this text, and Miss C.A. Stewart, who kindly prepared the manuscript.

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