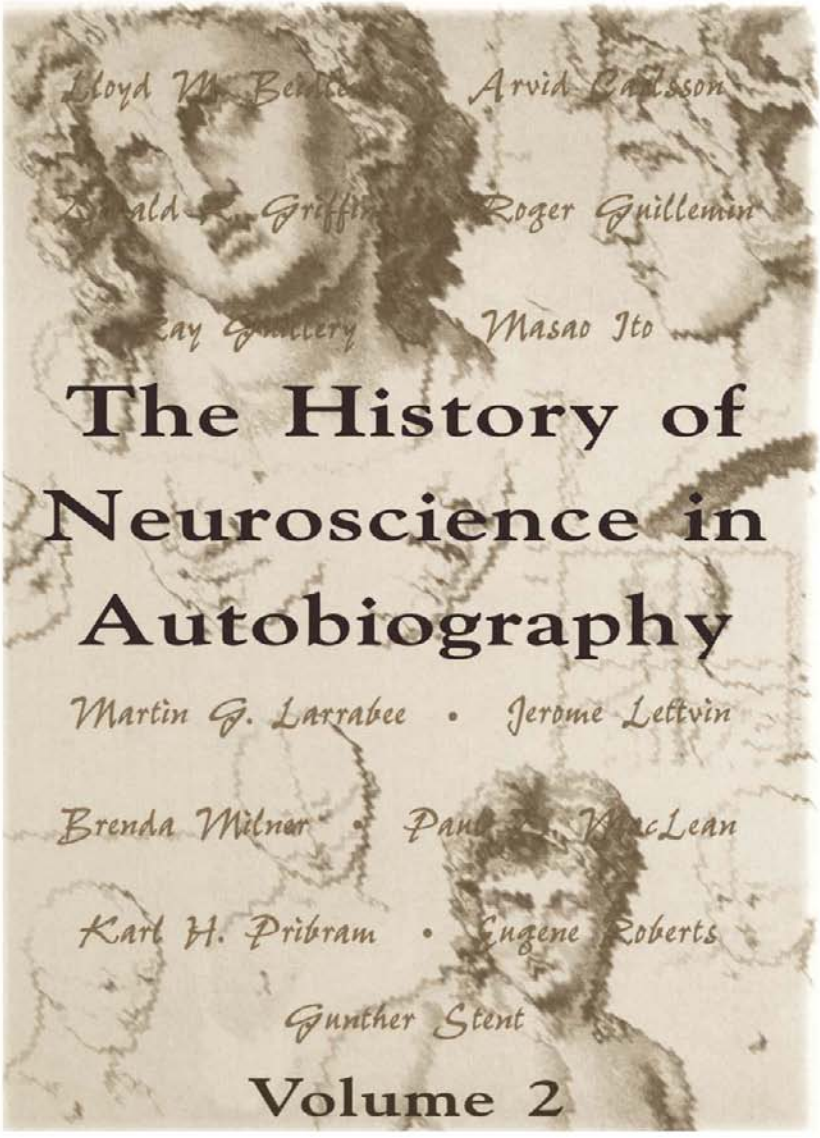


Edited by Larry R. Squire

# The History of Neuroscience in Autobiography

VOLUME 2

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# Preface to Volume 1

Before the Alfred P. Sloan Foundation series of books began to appear in 1979, the scientific autobiography was a largely unfamiliar genre. One recalls Cajal's extraordinary *Recollections of My Life,* translated into English in 1937, and the little gem of autobiography written by Charles Darwin for his grandchildren in 1876. One supposes that this form of scientific writing is scarce because busy scientists would rather continue to work on scientific problems than to indulge in a retrospective exercise using a writing style that is usually outside their scope of experience. Yet, regardless of the nature of one's own investigative work, the scientific enterprise describes a community of activity and thought in which all scientists share. Indeed, an understanding of the scientific enterprise should in the end be accessible to anyone, because it is essentially a human endeavor, full of intensity, purpose, and drama that are universal to human experience.

While writing a full autobiographical text is a formidable undertaking, preparing an autobiographical chapter, which could appear with others in a volume, is perhaps less daunting work and is a project that senior scientists might even find tempting. Indeed, a venture of this kind within the discipline of psychology began in 1930 and is now in eight volumes (*A History of Psychology in Autobiography*). So it was that during my term as President of the Society for Neuroscience in 1993 to 1994, I developed the idea of collecting autobiographies from senior neuroscientists, who at this period in the history of our discipline are in fact pioneers of neuroscience. Neuroscience is quintessentially interdisciplinary, and careers in neuroscience come from several different cultures including biology, psychology, and medicine. Accounts of scientific lives in neuroscience hold the promise of being informative and interesting, and they could be a source of inspiration to students. Moreover, personal narratives provide for scientists and nonscientists alike an insight into the nature of scientific work that is simply not available in ordinary scientific writing.

This volume does have a forerunner in neuroscience. In 1975, MIT Press published *The Neurosciences: Paths of Discovery,* a collection of 30 chapters in commemoration of F. O. Schmitt's 70th birthday edited by F. Worden, J. Swazey, and G. Adelman. The contributing neuroscientists, all leaders of their discipline, described the paths of discovery that they had followed in carrying on their work. While writing in the style of the conventional review article, some authors did include a good amount of anecdote,

opinion, and personal reflection. A second, similar volume, *The Neurosciences: Paths of Discovery II,* edited by F. Samson and G. Adelman, appeared in 1992.

In any case, neuroscience writing that is deliberately and primarily autobiographical has not been collected before. This project, *The History of Neuroscience in Autobiography,* is the first major publishing venture of the Society for Neuroscience after *The Journal of Neuroscience.* The book project was prepared with the active cooperation of the Committee on the History of Neuroscience, which serves as an editorial board for the project. The first chairperson of the committee was Edward (Ted) Jones; its members were Albert Aguayo, Ted Melnechuk, Gordon Shepherd, and Ken Tyler. This group compiled the names and carried out the deliberations that led to the first round of invitations. In 1995 Larry Swanson succeeded Ted Jones as chair of the committee, and as we go to press with Volume 1 the committee members are Albert Aguayo, Bernice Grafstein, Ted Melnechuk, Dale Purves, and Gordon Shepherd.

In the inaugural volume of the series, we are delighted to be able to present together 17 personal narratives by some of the true pioneers of modern neuroscience. The group includes four Nobel Laureates and 11 members or foreign associates of the National Academy of Sciences, USA. The contributors did their scientific work in the United States, Canada, England, Australia, France, and Sweden. It is difficult to imagine a finer group of scientists with which to inaugurate our autobiographical series. The autobiographical chapters that appear here are printed essentially as submitted by the authors, with only light technical editing. Accordingly, the chapters are the personal perspectives and viewpoints of the authors and do not reflect material or opinion from the Society for Neuroscience.

Preparation of this volume depended critically on the staff of the book's publisher, the Society for Neuroscience. The correspondence, technical editing, cover design, printing, and marketing have all been coordinated by the Society's Central Office, under the superb direction of Diane M. Sullenberger. I thank her and her assistants, Stacie M. Lemick (publishing manager) and Danielle L. Culp (desktop publisher), for their dedicated and skillful work on this project, which was carried out in the midst of the demands brought by the first in-house years of the Society's *Journal of Neuroscience.* I also thank my dear friend Nancy Beang (executive director of the Society for Neuroscience), who from the beginning gave her full enthusiasm to this project.

*Larry R. Squire*
Del Mar, California
September 1996

# Preface to Volume 2

This second volume of *The History of Neuroscience in Autobiography* presents 13 autobiographical chapters by senior neuroscientists. The authors tell about the experiences that shaped their lives, the teachers, colleagues, and students with whom they worked, and the scientific work that has absorbed them during their careers. As with Volume 1, this volume was prepared with the help of the Committee on the History of Neuroscience at the Society for Neuroscience. This group, which serves as editorial board for the project, compiled the names of those who were invited to contribute to the volume, and the committee's chairperson (Larry Swanson) shared in editing the manuscripts.

At the Society for Neuroscience, Holly Seltzer (production director) coordinated the early phases of Volume 2. In 1997, Academic Press joined with the Society for Neuroscience as a partner in this project. Although the volumes continue to be official publications of the Society for Neuroscience, Academic Press has coordinated the technical editing, printing, and marketing for Volume 2 under the very capable direction of Jasna Markovac (Editor-in-Chief, Biomedical Sciences). The collaboration between the Society for Neuroscience and Academic Press has proceeded smoothly, and I hope readers will find Volume 2 as informative and enjoyable as Volume 1.

*Larry R. Squire*
Del Mar, California
May 1998

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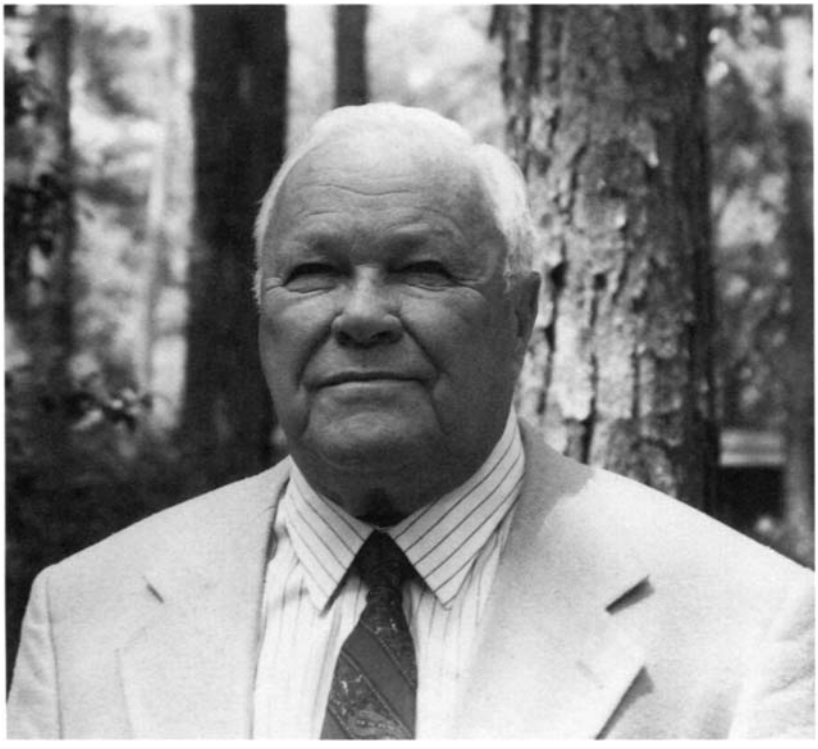
Lloyd M Beidler



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# *Lloyd M. Beidler*

**BORN:**

Allentown, Pennsylvania
January 17, 1922

**EDUCATION:**

Muhlenburg College, B.S. (1943)
Johns Hopkins University, M.S. (1944)
Johns Hopkins University, Ph.D. (1951)

**APPOINTMENTS:**

Florida State University (1950)
Professor Emeritus, Florida State University (1992)

**HONORS AND AWARDS:**

National Academy of Sciences U.S.A. (1974)
American Academy of Arts and Sciences (1975)

*Lloyd Beidler carried out pioneering physiological studies of chemoreception working with a variety of species and sensory systems. He carried out psychophysical studies with sensory nerves, and he demonstrated turnover in sensory cells. He is an authority on the sensory physiology of taste.*

*The History of Neuroscience in Autobiography, Volume 2*

# Lloyd M. Beidler

## My Early Life

My interest in science as a possible career started when I was a senior in college. Most of the attitudes, skills, and philosophy utilized in my scientific career, however, were developed long before. The effects of growing up with family stability, in a semirural environment, and with great personal freedom were reflected throughout my life.

My father's family were Mennonites whose origin can be traced to 1736 in Pennsylvania. My mother's family emigrated from Germany more recently. My father left school after eighth grade and became a railroad clerk in Bethlehem, where my mother also worked after she graduated from high school.

I was born in 1922 in Allentown, Pennsylvania. My two sisters Rose and Doris were born a few years earlier. When I was about 6 months old, my father purchased half of a two-home dwelling in a newly formed rural development north of Allentown. The house was 20 feet wide and two and half stories high. We were on the edge of the development and faced a large farm. About 300 feet from our house was a large abandoned iron ore pit which was used as a trash dump by the city. The church was our only social center. It contained a large worship area, a small, one-room library on the first floor, and a basement where the Boy Scouts and other social groups met. The village had only one store, located in the large front room of a house. We children had no playground or any other gathering place. As a result, my chief entertainment as a boy was at home and at the trash pit, which became the source of parts for any toy I desired, including my first and only bicycle. Later the pit became the source of parts for electrical devices and components for radios. Thus the pit contributed greatly to my interest in the physical sciences.

My father worked hard and followed a rigorous schedule from the time I was born until after I graduated from college. He assigned jobs of cutting the grass, weeding the garden, and taking the coal ashes outside, and they had to be accomplished on time. Serious offenses were followed by spankings but I soon learned to avoid them. When my chores were finished I had almost complete freedom but I had to be home when my father returned from work. Home rules were strict and clear, but because my sisters and I obeyed them, we had an unusual amount of independence.

My father was an avid reader and we three children again followed his example.

The depression greatly impacted my life as it did most people of the time. We collected rainwater from the roof of the house and stored it in an underground cistern. A hand pump in the kitchen brought the water up from the tank. My mother heated water on a wood stove for our baths every Saturday night. Our toilet was an outhouse about 20 feet from our dwelling. We converted the house to indoor plumbing when I was in high school. Today this type of living seems primitive but then it was the norm and most families were considered neither poor nor wealthy. My life, and those of my friends, was greatly influenced by the immense social and economic upheaval produced by the great depression and World War II. Simultaneously, though, the rewards of science were noticeable. Electrification was spreading rapidly to rural areas. Electric-powered delivery trucks were being replaced by noisier but faster gasoline-powered trucks and roads were being constructed. As a boy, I ran outside if I heard a small plane. Radio was introduced and television came later. The practice of medicine was still primitive and we waited until shortly before the war before antibiotics were available. These were tough but exciting times for a future scientist.

My formal education started in a two-room, six-grade schoolhouse. Mrs. Kistler, a motherly teacher with 2 years of normal school education, taught the first three grades. I then moved across the hall to Mr. Kistler's class for the next three grades. I still remember doing my arithmetic assignment in Mrs. Kistler's class while others were either singing or reading in the same room. In winter the classes would be interrupted by students who had to get dressed to go to the outhouse. The mental discipline needed to study at school was also useful at home because neither of my parents could help with homework. My father, however, carefully scrutinized our monthly school grades.

Middle school and high school were quite different. We were bussed several miles to the middle school, but walked only about a mile to the high school. The buildings were modern and the teachers quite accomplished and well educated. The high school curriculum was broad and included arts, music, and industrial training every year. My experience in wood working and metal lathe operation was helpful years later when I had to make much of my own research equipment. The most interesting courses for me were chemistry and physics. I obtained permission to do experiments in both laboratories instead of attending a study period. My experiment in the physics lab was to build a Tesla spark producer. Another student and I used a high-voltage transformer from an old neon sign. We fed the high voltage into a coil made from ¼-inch-diameter copper tubing which served as the primary winding of the Tesla device. The secondary winding consisted of No. 22 wire wound on a 6-inch-diameter and 30-inch-high cardboard cylinder that we obtained in a dress shop. Our Tesla device produced a very

high-voltage but low-current spark about 10 inches long. Our teacher never came into that part of the laboratory while we were experimenting! We demonstrated the coil to the class with dramatic results. During my senior year I did a series of chemistry experiments on making dyes and applying them to different types of cloth. The principal often came into the lab and showed his interest by asking questions.

My father and I never discussed future plans and I had little idea of what I might do after graduation. However, when I was a senior, my high school principal took me to see the dean of a local liberal arts college, Muhlenberg College. The meeting resulted in a scholarship and a change in my life.

## Muhlenberg College in Allentown

The depression was still being felt when I entered college in 1939. Fortunately the cost of a college education was not high. I bicycled to college and carried a packed lunch. I dusted library books 10 hours a week and was paid 40 cents an hour. That federal program helped the college and allowed me to graduate without owing a debt to the government. I also worked in a grocery store every Saturday and as a laborer in the Bethlehem steel plant during the summers. Thus, my father had only to contribute 100 dollars for each year of my undergraduate experience. This seemingly small amount, however, was approximately what my mother was given to feed the family for 5 months.

As a boy I built a radio with a triode vacuum tube, a variable capacitor, a resistor, and a self-wound inductance, all from components found at the trash pit. To this day I still remember the thrill of hearing my first successful radio receptions. Such experiences influenced my decision to major in physics in college.

The chairman of the physics department, Dr. Ira Zartman, received his doctorate from the University of California and was one of the first researchers to measure molecular velocities. He left Muhlenberg when I was a sophomore and went to Johns Hopkins University to organize a research and development group for the Navy. This left a rather young faculty to teach physics to about six physics majors as well as general physics to the science majors. We physics majors had a close relationship with the professors; one of them took us on a hiking trip through the mountains of Vermont during our junior year. The attack on Pearl Harbor was announced on radio while I was a junior studying for an exam in atomic physics. The government responded quickly by organizing new research groups to design and build weapons. Our physics curriculum at Muhlenberg changed rapidly as a result. We physics majors wanted to respond to the need of the military for scientists in electronics and for physicists both to work in the new government laboratories and to help train newly inducted men.

## Johns Hopkins in Baltimore

A few months before I graduated from Muhlenberg College in December 1942, I received a phone call from Dr. Zartman. He had learned that I had received offers to join a research group in Oak Ridge, Tennessee, and to teach at a University in New York. He asked me to consider the physics department at Johns Hopkins University in Baltimore. Two months later I accepted a junior instructorship as a graduate student in physics at Johns Hopkins.

I received an M.S. in physics after a year of graduate work and I was again approached by Dr. Zartman. He was desperately searching for more researchers for the naval laboratory located just one floor beneath the physics department! The mission of this group was to develop proximity fuses that could not be jammed. The antiaircraft shells had small radio transmitters which were energized when the shells were shot toward a flying aircraft and the transmitted beam was reflected by the aircraft back to the shell. When the signal was large enough, the shell exploded. These new antiaircraft shells were very effective; more than 90% of the Japanese attack planes were being destroyed in the Pacific in a single encounter. However, the shells could be harmlessly exploded on the way to their target if a radio transmitter at the same frequency as the shell was operating in the vicinity of the planes. The new research mission was to design electronic fuses that would not respond to the enemy's attempt at jamming. I joined this group and developed new electronic designs. The fuses were then tested for their radiation patterns by four women in the laboratory. One of these women, Mary Lou, became my wife. She had recently graduated from Goucher College in Baltimore, majoring in physics. Her interest in science had led her to join the Civilian Pilot Training program, which was supported by the government to form a group of women that might be called on later to learn to fly larger planes that could be shuttled to England. It was in this program that she obtained her pilot license.

## Johnson Foundation for Medical Physics, University of Pennsylvania

I believed that most biological processes could be explained by a series of physical and chemical events and that someone with my training could contribute to advances in biology and medicine. I discussed my interests with Dr. Samuel Talbot of the Wilmer Institute of Ophthalmology and this led to the opportunity to work in his laboratories in the evenings and hear his views of the future of biophysics. He encouraged me to enter the new field of biophysics when the war was over in 1945 and I applied to the Johnson Foundation for Medical Biophysics at the University Pennsylvania.

The director, Professor Detlev Bronk, had performed the first recording from a single nerve fiber in Lord Adrian's laboratory in Cambridge, England, while he was a postdoctoral fellow. (This research led to the establishment of the famous sensory law relating the frequency of nerve firing and the number of nerve fibers responding to the intensity of sensory response, an important contribution to the total research for which Adrian received the Nobel Prize.) The Johnson Foundation had previously trained many postdoctoral fellows but few predoctoral students.

The faculty included Drs. Frank Brink, Martin Larrabee, and Phil Davies in neurophysiology, Keffer Hartline in vision, and Tom Anderson in electron microscopy. There was no defined curriculum except that everyone had to take human physiology in the medical school. I also studied general physiology with Lewis V. Heilbrunn, chemical embryology with Jean Brachet, and x-ray diffraction with A. L. Patterson. We were required to take three of the five exams required of the doctoral students in the department of physics and show competence in German and French. The program was more like a research apprenticeship. All of the students were very close to the faculty.

We arrived in the laboratory at about 9:00 AM, ate lunch together in the library room, and left the laboratory at about 10:00 P.M. Each student was assigned to a professor and worked in his office–laboratory. I worked with Frank Brink but he never assigned me a research project. Thus, I made my own decision and chose to study frog embryonic development. After my first year at the Johnson Foundation I married Mary Lou who then became a graduate student and assistant in physics at Bryn Mawr College. Her major professor was A. L. Patterson, who had developed a mathematical method for analyzing x-ray diffraction patterns of crystals and determining their three-dimensional atomic structure. This he did while also serving as a lecturer for several years at the Johnson Foundation where he was attempting to determine the atomic structure of proteins. My marriage caused a stir among the graduate students because, unknown to me, Dr. Bronk had an unwritten rule that Johnson Foundation students should not marry until after they had obtained their Ph.D.

Professor Bronk was very active in the National Research Council of the National Academy of Sciences as well as many other scientific organizations, and seldom was in his office in the daytime. However, he was often at the Foundation in the evening. One evening he asked me about my dissertation plans. As a graduate student I had been independent and had chosen my own dissertation topic without first talking with Dr. Bronk. I had taken graduate courses in quantum mechanics, theory of molecular structure, valence theory of organic chemistry, and statistical mechanics, and thought I would study the interaction of ions and molecules in a biological system; specifically the interactions of ions and molecules with taste and olfactory receptors. Dr. Bronk was not pleased with my choice of research

and suggested instead that I study under Adrian in Cambridge, England, who already had written several papers on olfaction. This suggestion came as a shock to me because Mary Lou, who was now working in x-ray diffraction at the Franklin Institute, and I had no resources to move to England.

Within a few weeks my problem was unexpectedly solved. Dr. Hartline announced that Dr. Bronk was to become president of Johns Hopkins University and that he was to become chairman of a new department of biophysics that was to be located next to a building that housed the departments of biology and psychology. He said that he would like to form good relationships with both departments and that I could help him if I would consider selecting a dissertation in the field of either taste or olfaction because both Vincent Dethier of biology and Elliot Stellar of psychology were interested in the chemical senses! I was ecstatic. Both Mary Lou and I loved Johns Hopkins and we still had friends there. It became apparent to me later that Keffer Hartline knew of my previous conversation with Dr. Bronk about my interest in the chemical senses, but neither Keffer Hartline nor I knew that I would form a lifetime relationship with both Vince Dethier and Elliot Stellar and that Curt Richter would later ask me to do research in his laboratory after I received my Ph.D.

## Back to Johns Hopkins

Several months later, all the students and the neuroscience component of the faculty moved to Johns Hopkins. I started my research with the blowfly by investigating the neural response of the peripheral nerve to chemical stimulation of the leg. After several months of failures I switched to studying the taste responses of the rat by recording from the chorda tympani nerve. The magnitudes of the responses were disappointing and after several months of failures, Hartline suggested I contact Professor Carl Pfaffmann at Brown University. Pfaffmann was most helpful and suggested I visit his laboratory. Several weeks later Mary Lou and I journeyed by bus to Providence, Rhode Island. This proved to be an excellent and helpful trip. I soon learned that Carl had been a postdoctoral fellow with Bronk at the Johnson Foundation for a year after Carl received his Ph.D. in physiology with Adrian in Cambridge! Thus, we had many common friends. Carl spent an entire day showing me the tricks of taste nerve recording. Not only did we become lifetime friends, but he taught me the importance of behavioral science in the study of the senses. Although I was a student and he a professor, he always treated me as an equal. At no time did this relationship ever change. In later years he sent me many of his newly granted Ph.D. students for additional postdoctoral experience.

After returning from Dr. Pfaffmann's laboratory, I used his methods to record from the chorda tympani of the rat. My first recording equipment was primitive. I pushed a 6-foot-long table against another one and suspended