A book on the history of neuropsychology should include biographies of luminaries. Deciding which ones to include can be difficult. Roger W. Sperry, however, is an obvious choice, as this chapter will amply illustrate.

Sperry did groundbreaking work in the areas of both neuroscience and neuropsychology and is the only person with a graduate degree in psychology to have received a Nobel Prize. Even the lay public knows about his discovery of left and right brain differences in split-brain patients. He published over 300 articles in the most rigorous scientific journals, often as the sole author. He provided training to nearly 100 visiting scientists, graduate students, and postdoctoral fellows from 12 different countries and several different disciplines, ranging from medicine to philosophy. Finally, historians of psychology consider Sperry one of the major figures in the discipline's century-old pursuit of the scientific understanding of the mind and behavior. His scientific methodology was highly rigorous, on par with the best contemporary research. Yet the questions he asked were the same ones that were asked by William James at the start of the twentieth century: what is consciousness and what is the origin of behavior?

This chapter is divided into three sections. The first section presents an overview of Sperry's personal and professional development, including his relationships with his mentors, students, and collaborators. As will be shown, Sperry's commitment to neuropsychology began as early as his adolescence and spanned 65 years. The second section of the chapter discusses "the four turnarounds," as Sperry called them, of his scientific career. Although the focus of his research shifted during each of the turnarounds, Sperry was unwavering and relentless in his focus on simplicity and elegance in research methodology. Sperry also had a uniquely far-sighted understanding of historical questions in psychology. The final section of the chapter centers on Sperry's concern about society and the
Figure 5.1. Roger W. Sperry
implications of his research and ideas for the solution of societal problems. Sperry was always uncomfortable with the limited role of psychology in solving societal predicaments. Without question, Sperry embodied both a unique personality and a remarkable life.

**Personal and Professional Development**

As a teenager, Sperry was exposed to the ideas of William James when his father brought home James’s (1890) *Principles of Psychology* from the public library. Reading and scholarly achievement clearly were valued in the Sperry home. Roger was one of two boys, both competitive and good students. Sperry was to come in contact again with James’s ideas in his “Introduction to Psychology” course taught by William Stetson at Oberlin College. These proved to be two defining events: Sperry’s entire career was subsequently devoted to answering the questions posed by James and presented by Stetson. Neuropsychology provided Sperry with the methodology to answer these questions.

While he was a student, it would have been almost impossible to predict that Sperry would choose a research career. His interests appeared to be in sports, and he was captain of some of his high school teams. He was a fashionable dresser and apparently quite popular with his peers. His early experiences revolved around the typical things a boy might do in the rural areas of Hartford, Connecticut; however, a sign of his early scientific curiosity can be found in his habit of searching for dead animals to dissect.

The untimely death of his father had an adverse effect on the family’s income, resulting in their move to East Hartford, Connecticut, where Sperry continued his interest in sports. So strong was this interest that Sperry listed athletic coaching as his primary goal in his college applications. As a second choice, he indicated medical research. Sperry and his brother were admitted to Oberlin College in Ohio, a rigorous liberal arts college with a strong tradition in athletics. Again, he continued with his pursuit of sports, lettering in baseball, basketball, and track. While his brother pursued chemistry, Sperry drifted toward English. Although he went on to receive his undergraduate degree in English, near the end of his undergraduate career, his interests began to shift to psychology.

Sperry was particularly impressed with Raymond Stetson and his research. Stetson had gone to Harvard to obtain his Ph.D. with William James. Unfortunately, by the time of Stetson’s arrival, James had changed his focus to religion, so Stetson studied with others, including Hugo Münsterberg. Nevertheless, the indirect impact of James’s ideas prevailed at Harvard, probably because of the use of James’ publications for teaching purposes. After Harvard, Stetson secured a position at Oberlin College, remaining there for the rest of his career.

Stetson was not a particularly prolific researcher, although he was well liked as a professor. Stetson’s field of expertise was speech therapy and his papers, for the most part, were published in European journals. Oberlin College in general, and
Stetson in particular, had a great impact on Sperry during his training. This is evidenced by two facts: (1) Sperry’s notes on the first day of Stetson’s “Introduction to Psychology” class served as the foundation for Sperry’s entire future research program, and (2) Sperry left his papers to Oberlin College upon his death. The admiration appears to have been mutual, as the neuroscience building on campus is named after Sperry, and he received an honorary doctorate degree from Oberlin.

After completing his master’s degree with Stetson, Sperry wanted additional training. He decided to study at the University of Chicago under the tutelage of Paul Weiss, a prominent scientist working in the Department of Zoology. Weiss’s research was prolific, continuing well into the 1960s. Weiss’ basic premise was that the nervous system is plastic. This idea became the focus of Sperry’s first research turnaround. Using nerve regeneration techniques, Sperry set out to prove that his advisor’s idea was incorrect, a pattern that would repeat throughout his entire career. That is, Sperry seemed to enjoy challenging the intellectual status quo, whether it was his own graduate advisor or the intellectual zeitgeist of the day.

After Sperry’s academically prosperous time in Chicago, Weiss suggested that Sperry apply for a postdoctoral fellowship at Harvard with Karl Lashley, another prominent scientist of the middle-1900s. Lashley, like Sperry, originally was trained as a zoologist but had migrated over time to study the brain and its relation to behavior. Lashley was the director of the Yerkes Primate Research Center in 1941. Robert Yerkes had originally established the Center at Harvard but, due to the inclement weather in Boston, it eventually was moved to Orange Park, Florida. There Lashley worked with other prominent (e.g., Donald Hebb, Henry Nissen) as well as promising (e.g., Karl Pribram) scientists of the time. The focus was on behavior, but the conduit to understanding behavior was the nervous system. Again, Sperry went on to challenge his advisor, resulting in his second research turnaround. Both at Yerkes and later back at Chicago, Sperry developed simple experiments that were to challenge Lashley’s equipotentiality theory that proposed that brain lesions have nonspecific effects such that the exact placement of a lesion may be unrelated to the degree and type of resulting behavioral deficit.

After completing his postdoctoral training at Yerkes, Sperry secured a tenure-track position in the Department of Anatomy at the University of Chicago. It was during this time that Sperry became involved with a wide variety of young scientists. Some of them became lifelong colleagues. Robert Doty, now at the University of Rochester, included Sperry as part of his doctoral thesis committee in 1950. Sperry also worked with several other researchers, including Nancy Miner and Ronald Myers. Despite Sperry’s productivity, and for reasons not clearly understood, the University of Chicago decided not to grant him tenure.

Subsequently, Sperry obtained an appointment as section chief in the Division of Neurological Diseases and the Blindness Laboratory, under Seymour Kety, at the then recently formed National Institutes of Health. Sperry, however, never resided in Bethesda, Maryland, the eventual home of this agency. Tuberculosis, probably contracted while working with monkeys at Yerkes, forced him to leave his position. He spent six months in Saranac, New York, recovering his health. Sperry
used this time to contemplate more seriously the role of consciousness in brain function. Indeed, the last section of the article "Neurology and the Mind–Brain Problem" (1952) provides a glimpse of his research focus for the next 40 years. This signaled the beginning of the third turnaround in his thinking.

Sperry continued forging ahead with his theories and writing. After a particularly successful symposium talk, Sperry was invited to the California Institute of Technology (Caltech) by Norman Horowitz to present a lecture. That lecture so impressed the faculty that Sperry was offered a tenured position, and Caltech's Hixson Fund was converted into the Hixson Chair of Psychobiology, a position Sperry held until his retirement in 1984. Sperry and Horowitz developed a lifelong friendship, and Horowitz supported Sperry's behavioral research at Caltech's molecular-based Division of Biology.

Caltech was an odd place for Sperry. There was no question that he felt right at home with the likes of other world-class scientists and students. The Division of Biology not only was extremely well funded and productive, it also was on the cutting edge of a then emerging field—microbiology. Sperry, however, was the only behavioral scientist on the staff. This isolation continued until his death. Thus, he was surrounded by some of the best and brightest scientists of his time but worked in geographical isolation from the colleagues he most wished to impact—psychologists and neuropsychologists. Sperry often shared that he felt that neither Caltech nor the field of psychology truly appreciated his work.

Nonetheless, while at Caltech, Sperry became as productive as at any time in his fifty-year research career. By the peak of his productivity during the 1960s, Sperry was occupying large areas of laboratory space—eventually occupying an entire wing. He had up to 10 different scientists and/or graduate students working on a variety of projects. During this time, he kept a relatively low profile, opting to stay in his office, often with his feet on the desk and "thinking."

Sperry could be a daunting supervisor. Meetings often were prefaced with questions such as, "Is this important? Will your ideas make a difference?" Unprepared contacts with Sperry were highly inadvisable. To some, Sperry came across as cold, distant, and aloof. In reality, however, Sperry simply was quite shy and preferred to avoid public contact. Simultaneously, Sperry was driven. He challenged his students and colleagues, but never more than he challenged himself. For example, his writings were reviewed a number of times, not only by himself but also by colleagues as well as his spouse. He rewrote and edited manuscripts numerous times. Manuscripts with theoretical or philosophical ideas were sent to colleagues outside the laboratory to see whether there were any flaws in his thinking. Sperry often read drafts of his papers at laboratory seminars for criticism. This rigorous approach resulted in extremely high quality manuscripts from Sperry's laboratory and brought constant funding from a variety of sources for the entire length of his career.

As at the University of Chicago, the focus of Sperry's work at Caltech initially was nerve regeneration with students such as Nancy Miner. By the late 1950s, however, he had shifted focus to examining the split–brain phenomena, initially in cats
(1956), later in monkeys (1958), and finally in humans (1962). The research with cats was done in collaboration with Harbans Arora, Nancy Miner, John Stamm, Ronald Myers, A. M. Schier, and Theodore Voneida. Sperry also did work with chicks in conjunction with Larry Benowitz. Sperry’s work with monkeys involved Mitchell Glickstein, A. M. Schier, Colwyn Trevarthen, Richard Mark, Evelyn Lee-Teng, and Charles Hamilton. When his corpus callosum work shifted to humans, another set of young scientists collaborated including Joseph Bogen, Michael Gazaniga, Jerre Levy, Harold Gordon, Robert Nebes, Eran Zaidel, Leah Ellenberg, G. Plourde, and R. Saul. It is important to note that there was extensive collaboration in the laboratory and investigators shared subjects for experiments.

At his peak, Sperry’s lab at Caltech resembled more of a highly organized research factory than a standard research laboratory. For example, in 1976 Sperry’s intellectual assembly line included his research associate Charles R. Hamilton; his visiting associates Evelyn Lee-Teng and Colwyn B. Trevarthen; his research fellows Laura Franco-Testa, Ronald L. Meyer, and Eran Zaidel; and his graduate students Sheila Gillard Crewther, Karen E. Gaston, Karen F. Greif, David S. Isenberg, Larry E. Johnson, Margaret Y. Scott, and Betty A. Vermeire. In addition to these colleagues, there were one student assistant and ten research staff members including Lois E. MacBird, Sperry’s long-time secretary, and Dahlia Zaidel, a long-standing collaborator, technician, and assistant.

Other individuals played a role in Sperry’s professional development. These include Brenda Milner, Professor of Neurology at McGill University; Robert Galambos, Emeritus Professor of Neuroscience at the University of California School of Medicine; William Burbank, Emeritus Professor of Biology at Emory University; Jerry Kollros, Emeritus Professor of Zoology at the University of Iowa; and Kao Liang Chow, Emeritus Professor of Neurology at the Stanford University School of Medicine. There is little question that Sperry interacted both with his students and with colleagues around the country, relying on them for feedback on the development of his concepts.

Sperry’s articles have been published and/or translated into several languages including Russian, Chinese, Japanese, and Spanish. A large percentage of his articles are single-authored although I believe he was generous in many instances by placing himself as second or even last author despite the fact that he generated the research designs. Several of his articles, especially those involving the human split–brain work, have been reprinted in numerous journals and books; however, it is interesting to note that subsequent researchers and reviewers have at times failed to cite Sperry’s original split–brain research.

Toward the 1990s, Sperry stopped collecting data, and almost all his students and collaborators secured positions in other academic and research settings. Sperry contracted a neurological motor disease late in life, and as it increasingly affected him, he began to focus primarily on the fourth turnaround of his career—Sperry’s writings became almost exclusively philosophical in nature. Sperry’s colleagues and students delivered his presentations, as he opted to stay out of the public eye. He ceased giving almost all lectures by 1980 because of slowed and
slurred speech. He chose instead to split his time between his office at Caltech and his home where he furthered his ideas by publishing in a variety of journals both in the United States and abroad.

In contrast to his earlier work, Sperry’s work on consciousness and neurophilosophy was undertaken almost exclusively alone. While he collaborated and sought the advice of fellow colleagues and past-students, including Colwyn Trevarthen, Theodore Voneida, and Joseph Bogen, almost all of his publications in this area are single-authored. In fact, at the time of his death, Sperry was still working on the revision of at least one manuscript.

The Four Turnarounds

As already noted, Sperry’s first psychology class, taught by Raymond Stetson, profoundly affected him. Sperry wrote two questions on the first page of his class notes:

1. Where does behavior come from?
2. What is consciousness?

These two questions, which arose from William James’s view of psychology, provided the foundation for Sperry’s half-century of research. Sperry often shared with me that Stetson had the greatest impact on his intellectual development. It should not be surprising to note, then, that Sperry’s research program followed a systematic road of attempting to answer these two provocative and central questions in the history of psychology. While Sperry divided his research program into four phases, or “turnarounds,” he attempted to address the above two questions in each phase. The four turnarounds were as follows:

1. Nerve regeneration research
2. Studies involving equipotentiality
3. Split–brain studies
4. Consciousness research

Sperry’s research program was systematic, despite the shifts in direction, and each phase evolved into the next one. There are several pieces of evidence supporting this contention. Sperry repeatedly indicated this to me as well as to others. He kept a scientific diary for a number of years that attested to the flow of his research. Finally, Sperry’s most recent writings, especially during the 1990s, allude to this fact although not often in straightforward terms.

There are specific overlapping periods of time for each of the four major phases (see Table 5.1). The first phase began in 1937 and ended in 1975. The second phase was rather short—1952–1955. The third phase began in 1950 and lasted until 1985. The fourth and final phase probably began with a lecture at Caltech in the
spring of 1962 and continued until his death in 1994. What is curious is that each
time a particular line of research evolved, its focus would fade only slowly. For
example, Sperry published his first article on nerve regeneration in 1939 and his
last one in 1975, although his major focus in this area lasted just 20 years.

Table 5.1 also lists the number of publications resulting from each phase of
Sperry's career according to the years in which that particular research was carried
out and the number of articles produced that focused on that particular area. It is
almost impossible to be specific about these estimates because in some review arti-
cles, Sperry addressed research that involved more than one of the turnarounds
and in some rare instances, the empirical research involved two areas simultane-
ously. The estimates provided, however, are good approximations.

Several issues need clarification. First, Sperry always kept a small portion of his
research allocated to areas that were no longer central to his interests. Hence, even
though the peak of his nerve regeneration research occurred during the 1940s and
early-1950s, he and a small number of his students continued doing research in
this area for another 10 to 15 years. Second, it appears that he devoted equal time,
in terms of publications, to the three areas that were most important to him. I
believe he felt that Lashley's theory of equipotentiality was well answered by the
five or so studies devoted to this topic. Indeed, I questioned him on the validity of
including this as a turnaround in his research program. Sperry considered this
phase of his research quite important. It disproved Lashley's electrical field theory
and confirmed principles of vertical organization within the cerebral cortex.

Third, the consciousness research spans the longest time—almost half a cen-
tury. The first article, published in the American Scientist (Sperry, 1952), contains
in the summary section a blueprint of what was to come from his research pro-
gram for the next 30 to 40 years—an unbelievably clear and well-laid-out plan for
attacking the second question from his "Introduction to Psychology" course.
There was little doubt that Sperry was interested in using science to answer age-old
philosophical questions. In fact, I believe that he considered this last phase of his
research to be potentially the most important.

In reviewing his publication record, we observe that Sperry clearly preferred to
publish empirical articles in rigorous scientific journals. In fact, his first review
article was published only after 37 empirical articles had been published. And, it
was not until his 45th article that psychological issues crept into his discussions. By
the 1960s, Sperry's ideas were starting to have an international impact as his

### TABLE 5.1: SPERRY'S FOUR TURNAROUNDS

<table>
<thead>
<tr>
<th>Phase</th>
<th>Years</th>
<th>Articles Published</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nerve Regeneration and Chemoaffinity</td>
<td>1937–75</td>
<td>75</td>
</tr>
<tr>
<td>Equipotentiality</td>
<td>1952–55</td>
<td>5</td>
</tr>
<tr>
<td>Split–brain</td>
<td>1950–85</td>
<td>80</td>
</tr>
<tr>
<td>Consciousness and Values</td>
<td>1962–94</td>
<td>70</td>
</tr>
</tbody>
</table>
research began to be translated and published in other languages. It is worth noting that it was not until 1946 that Sperry published his first article in a psychological journal, the *Journal of Comparative Psychology*. Another article that qualifies as a psychology article was “On the Neural Basis of the Conditioned Response” which appeared in the *British Journal of Animal Behaviour* (1955). This is interesting in light of the fact that, historically and vocationally, Sperry considered himself much more of a psychologist than a zoologist or even a biologist.

Each of Sperry’s turnaround was well funded, mostly by governmental agencies. He received his first research grant in 1940 and his last one in 1980—though he was funded until his death. Further, his research was funded continuously for nearly 50 years. Funding came from federal agencies ranging from the National Institutes of Health to the Public Health Service. In addition, he received support from various other sources including the Penrose Fund (American Philosophical Society), Eli Lilly and Company, and the Caltech Hixson Fund. Further details are presented in Table 5.2.

I will characterize briefly the research done during each of the four turnaround. In the first turnaround, nerve regeneration, Sperry was interested in addressing the nature versus nurture question. He saw a challenge and an opportunity in the work of his new doctoral supervisor, Paul Weiss, who provided both techniques and a theoretical framework from which to attempt to answer this question. Weiss had postulated that the nervous system was plastic and, indirectly, had provided support to the then emerging theory of behaviorism (i.e., nurture).

### TABLE 5.2. SAMPLING OF FUNDING ACTIVITY

**BY SOURCE OF FUNDING, DATE, AND TYPE OF RESEARCH**

<table>
<thead>
<tr>
<th>Source</th>
<th>Date</th>
<th>Type of Research</th>
</tr>
</thead>
<tbody>
<tr>
<td>Penrose Fund</td>
<td>1940</td>
<td>Muscle Transposition</td>
</tr>
<tr>
<td>National Institutes of Health</td>
<td>1955</td>
<td>Visual Pattern Perception</td>
</tr>
<tr>
<td>Public Health Service</td>
<td>1955</td>
<td>Visual Pattern Perception</td>
</tr>
<tr>
<td>Southern California Society for Mental Hygiene</td>
<td>1955</td>
<td>Visual Pattern Perception</td>
</tr>
<tr>
<td>National Science Foundation</td>
<td>1956</td>
<td>Perceptual Integration</td>
</tr>
<tr>
<td>Mental Health Foundation</td>
<td>1956</td>
<td>Perceptual Integration</td>
</tr>
<tr>
<td>Eli Lilly &amp; Company</td>
<td>1957</td>
<td>Myotypic Respecification</td>
</tr>
<tr>
<td>National Science Foundation</td>
<td>1960</td>
<td>Split–brain Rhesus Monkeys</td>
</tr>
<tr>
<td>Commonwealth Fund</td>
<td>1961</td>
<td>Central Nervous Pathways</td>
</tr>
<tr>
<td>Hixson Fund</td>
<td>1968</td>
<td>Hemispheric Disconnection</td>
</tr>
<tr>
<td>Public Health Service</td>
<td>1970</td>
<td>Absence of Neocortical Stimulation</td>
</tr>
<tr>
<td>Institutes of Health</td>
<td>1971</td>
<td>Developing Brain</td>
</tr>
<tr>
<td>Public Health Service</td>
<td>1973</td>
<td>Interhemispheric Interaction</td>
</tr>
<tr>
<td>Public Health Service</td>
<td>1977</td>
<td>Hemispheric Lateralization</td>
</tr>
<tr>
<td>Public Health Service</td>
<td>1980</td>
<td>Lateralized Functions</td>
</tr>
</tbody>
</table>
Thus, Sperry’s overall goal was to determine whether this theory was correct. In other words, is the peripheral nervous system sufficiently malleable that nerves can be interchanged while functioning remains intact?

In order to answer this question, Sperry began a series of studies involving muscle transplantation in rats. He then added nerve crossing. After coming to the conclusion that little plasticity was evident with these nerves and muscles, Sperry proceeded to work on sensory functions, including vision and olfaction. He also expanded the research from rats to other species including amphibians, monkeys, and fish. By the early-1950s, Sperry was fairly convinced that plasticity in motor and sensory activities in a wide variety of animals was limited in scope. After conducting a research program lasting two decades, Sperry had successfully challenged the ideas of Weiss, thus supporting the role of nature over nurture.

Sperry’s second turnaround occurred during his postdoctoral years with his next supervisor, Karl Lashley. Sperry developed relatively simple experiments to test and challenge Lashley’s theory of equipotentiality. To briefly restate, equipotentiality theory proposed that brain lesions have nonspecific effects. Lesion location, consequently, is unrelated to the nature of the resulting behavioral deficit. The mechanism by which these nonspecific effects arise is a hypothetical spread of electrical impulses. To test Lashley’s theory, Sperry used dielectric plates initially, and later used subpial slicing and tantalum wire and mica plate implants in the visual cortex. The spread of electrical impulses that Lashley hypothesized did not occur. The experiments were simple, elegant, and methodologically robust. Sperry had been able to express Lashley’s concepts in simple behavioral terms that, in turn, were translated into experimental paradigms. Sperry concluded after three to four years of work in this area that it was probably time to move on to more complex issues.

The work in Lashley’s laboratory left Sperry wanting for more of a challenge. There was little question that he had become very interested in the brain, in contrast to his earlier work that had focused almost exclusively on the peripheral nervous system. Further, I believe he wanted to challenge his own ideas by focusing on both the brain and higher-order mammals. Sperry’s return to the University of Chicago during the mid-1940s signaled the beginning of a shift in his focus toward the brain in monkeys and eventually in humans. This led to the third phase in Sperry’s career—split–brain research.

Sperry is most known for his split–brain research and, unfortunately, this is the area in which his ideas are the most misunderstood. He began by focusing on the transfer of sensory information across the corpus callosum in cats and monkeys, presenting his findings for the first time to a group of psychologists at the 1960 convention of the American Psychological Association. In 1961, Sperry published “Cerebral Organization and Behavior” in Science, noting how the separated hemispheres in many respects behaved like individual brains. For example, Sperry showed that after severing the fibers connecting the cerebral hemispheres (i.e., the massive corpus callosum, the smaller anterior commissure, and the optic chiasm), visual discriminations could be taught to a single hemisphere by closing one of the
animal's eyes. The opposite hemisphere would have no knowledge of these discriminations when it, in turn, was tested.

A surgical procedure for the treatment of epilepsy made it possible to study the split–brain effect in humans. In the 1940s, William Van Wagenen had cut the connections between the cerebral hemispheres in 26 epileptic patients, reasoning that this would prevent the spread of seizure activity from one hemisphere to the other (Gazzaniga, 1985). In direct contradiction to Sperry's animal research, the initial studies of these patients suggested that the surgery left them behaviorally unchanged and with negligible improvement in their seizure control. Michael Gazzaniga, who was beginning his work in Sperry's laboratory, attempted unsuccessfully to gain access to and test Van Wagenen's original cases.

Fortunately, Joseph Bogen, a neurosurgical resident at White Memorial Hospital, and his neurosurgery professor Peter Vogel, were interested in giving the procedure another try for the treatment of intractable epilepsy. Bogen had done a post-doctoral fellowship at Caltech with Anthony Van Harreveld. Harreveld's office was next to Sperry's, a fortuitous configuration as it allowed Bogen to learn of Sperry's split–brain effect (Gazzaniga, 1985). Bogen's review of reports on the original split–brain cases suggested that at least some showed improvement in their seizure control. This motivated him to perform his first commissurotomy on a war veteran with intractable epilepsy. Bogen made the patient available for study, and this patient began Sperry's extension of the split–brain effect to humans.

The original study in humans by Gazzaniga, Bogen, and Sperry (1962) laid out the foundation for the research that was to continue for another 25 years. Starting with the war veteran case, Sperry slowly expanded the program to include a dozen research volunteers with similar conditions. Based on studies of these patients, Sperry and his colleagues eventually concluded that each cerebral hemisphere had quite a different functional specialization. The left hemisphere readily names stimuli presented to it alone, while the right hemisphere struggles and typically cannot do so. When allowed to use the left hand, which the right hemisphere controls, the right hemisphere can point, find, or otherwise indicate stimuli about which it is verbally silent. Further, and possibly more importantly, when the two hemispheres are disconnected, the person functions with an apparently divided consciousness (Sperry, 1966). Each hemisphere seems to possess an independent capacity to perceive, process, react to, and store information (Gazzaniga, 1985).

The notion of right– and left–brain differences has become widespread in western popular culture. Sperry did not believe, however, that the left–brain was dominant, nor did he believe that people could be classified as left– or right–brained. Such ideas have little empirical foundation. What Sperry did believe is borne out by the close to 100 studies that he and his students and colleagues at Caltech performed in cats, monkeys, and eventually humans.

What emerges from these meticulous studies is that the left hemisphere is more analytical than the right and, in contrast, the right is more appreciative of gestalt and emotional behavior. An elegant and simple approach to understanding hemispheric functioning and the function of the corpus callosum across three species
was Sperry's singular most important scientific accomplishment, resulting in his receipt of the Nobel Prize in 1981 (Sperry, 1981).

The fourth and final turnaround of Sperry's research centered on the concept of consciousness. This phase could probably be further subdivided into two parts. The first part involves the idea that consciousness arises from the unified interaction of both hemispheres in the intact normal individual. Each hemisphere produces a type of consciousness in its own right; however, behavior is not as purposeful and goal-directed as when both hemispheres work in unison. Consciousness emerges from brain activity and, in turn, consciousness has a unique downward causation effect, controlling and directing the brain's activity.

The second part of this phase of research involves the consideration of value systems. Sperry asked two obviously value-laden questions: what thoughts ought to arise in consciousness, and which values can be deemed the most useful? Thinking about what ought to arise in consciousness automatically invokes a value system that permits such thoughts to emerge in the first place. Sperry proposed that a system of thinking in which scientific methodology is applied to values might enhance the development of consciousness. He also thought that nature and time would ultimately determine the values that are most useful to the human species.

**Future Directions: Neuropsychology and Society**

There is little question in my mind that Sperry believed that his work was misunderstood. For example, historically he believed that Caltech focused too much on molecular biology and that his work, at the level of behavior and mind, was considered too "soft," especially for the "hard" science focus of Caltech. He believed that the split-brain work was over-interpreted, especially by non-scientists. In addition, he thought that his work on nerve regeneration was best appreciated by neuroscientists but largely ignored by psychologists, the group with whom he most strongly identified. Sperry's ideas on consciousness were considered by some to be misguided efforts at philosophy. Finally, I believe Sperry felt very strongly that a consciousness revolution began in the 1960s—a revolution he thought could be as significant as the Copernican revolution. Sperry believed that psychology could contribute to this revolution. Again, these ideas have yet to be fully appreciated by the scientific community.

Sperry's work nonetheless has had a significant impact on both psychology and society. Continuing surveys about major historical figures in psychology typically include Sperry. Sperry's work has produced ripple effects such as the development of the Declaration of Human Responsibilities by a group of distinguished scientists from around the world. The document is being considered by the United Nations as the next step to follow the 50th anniversary of the Declaration of Human Rights.

Sperry was interested in the brain from the very beginning but needed to know how it functioned before he could draw any conclusions about higher-order activities. Behavior is dependent to a large degree on the structure and the physiology
of the nervous system. Sperry believed that a more accurate understanding of this brain–behavior relationship was seminal to the development of a consciousness revolution. Such an understanding could serve as the foundation for the solution of modern-day problems. The integration of science and mind, combined with a focus on consciousness and brain function, will serve as the legacy of Roger W. Sperry.

REFERENCES


Hans-Lukas Teuber posed a question, laid out a research program and, according to one historian (Benton, 1994), gave an emerging field its name in a paper he gave at the 1948 American Psychological Association (APA) meeting in Boston (Teuber, 1948). He asked, “What is the psychologist’s role in the neurological laboratory?” The vision he articulated was of “the coalescence of experimental psychology and experimental neurology.” Contributions would come from clinicians and experimenters, “the most advantageous constitution being that of the centaur, who combines the human head of the clinician with the horse trunk of the brass instrument psychologist. The brass instrument portion is required because of the need for specialized tests and procedures in the border region of psychology and neurology” (Teuber, 1948, pp. 1–2).

In Teuber’s (1948) vision of neuropsychology, the specialized tests would be used not simply to identify “the” brain injured patient as such, but also would be used by the psychologist “with the eyes of a theorist” who asks “the fundamental question: how does living structure, especially neural structure, ‘mediate’ psychological functions?” (p. 1). Using detailed experimental results from his World War II research with Morris Bender on brain-injured soldiers, and under the guise of a “methodological” critique that in fact advanced important theoretical points, Teuber concluded that an answer could not be found until some difficulties of testing and of interpreting test results were resolved. “The coalescence of psychology and neurology is not an achievement, but a goal,” he concluded (p. 13).

It was a goal Teuber would pursue all his life, and in retrospect it is surprising to realize how early in his scientific life he committed himself to it. He was 31 years

1. I was a graduate student in the Massachusetts Institute of Technology Psychology Department from 1965 to 1969, although I did not do my dissertation research with Teuber. Like other students, I took courses with him, went to the weekly colloquia followed by dinner at the Teubers’ home in Arlington, and during one summer was a part-time research assistant to Teuber and Suzanne Corkin. As I have become immersed in archival material that provokes memories of the department Teuber created, I realize that his interest in the history of science influenced me and perhaps other students from that period (e.g., Squire, 1996).

I want to thank Stephen Chorover, Suzanne Corkin, Charles Gross, and Richard Held for their thoughtful and informative comments on an earlier draft of this chapter. Responsibility for the final version is my own.
old when he gave the APA paper in Boston. He had a newly awarded Ph.D., was newly discharged from the U.S. Navy, had a job in a new city, and had a new (his second) son. His U.S. citizenship and his third language (English) were of only slightly less recent vintage.

After Hans-Lukas Teuber’s untimely death on January 4, 1977, memorials of his life and career appeared in scientific journals and in the National Academy of Science’s Biographical Memoirs (Benton, 1994; Gross, 1994, 1999; Hécaen, 1979; Held, 1979; Hurvich, Jameson, & Rosenblith, 1987; Pribram, 1977; Richards, 1978; Weinstein, 1985; Weiskrantz, 1977). He is described as “one of the most influential neuropsychologists of his generation” (Gross, 1994, p. 451), someone who made “an enormously significant contribution to the form and content of the field of neuropsychology as we know it today” (Benton, 1994, p. 31). He was the founder of the influential Psychophysiological Laboratory at New York University (NYU)–Bellevue Medical Center and later of the Massachusetts Institute of Technology (MIT) Psychology Department, which at the time of his death “had grown into a center of psychology and the brain sciences that came to be known and admired the world over” (Hurvich et al., 1987, p. 461).

These obituaries and memorial tributes outline, with more or less detail depending on the forum, Teuber’s family and educational background in Germany and Switzerland in the 1920s and 1930s, his graduate work in psychology at Harvard in the 1940s, and his research on effects of brain injuries, first while serving in the U.S. Navy and then in his Psychophysiological Laboratory at NYU in the 1950s. They recount his 1960 move to MIT, where he founded a highly unorthodox psychology department, and his teaching and scientific work in the 1970s as he became even more active nationally and internationally as “a consummate organizer, synthesizer, and sponsor of research on the brain, as well as the mentor of many of today’s leading brain researchers” (Gross, 1994, p. 451). The tributes refer to and in some cases list Teuber’s many publications, memberships in professional associations, awards, and honors. And they try to convey—each from the perspective of an author who knew Teuber in different settings and roles—the enthusiasm, intellectual style and vigor, and personal qualities of the man.

The personal reminiscences and anecdotes in the memorial essays are varied and often vividly detailed, and the authors highlight different aspects of Teuber’s


The most detailed published account of Teuber’s family background and early education is the essay by Leo Hurvich, Dorothea Jameson (both colleagues of Teuber at NYU), and Walter Rosenblith (a colleague and then provost at MIT when Teuber headed the Psychology Department), prepared with assistance from Marianne Teuber (Hurvich et al., 1987). This essay seems to follow the general outlines of a narrative vita Teuber wrote in the late 1950s that is now in the MIT Archives.

I too follow this outline, drawing as well on other material in the MIT Archives, from Teuber (1994), and from other details provided by Marianne Teuber. I want to express my deep appreciation to Marianne Teuber for her generosity, kindness, and valuable insights; the photograph included in this chapter also is courtesy of Marianne Teuber.
scientific contributions to neuropsychology, psychology, and neuroscience. But there is, inevitably, a certain sameness about these brief appreciations. The general outlines of Teuber’s education, scientific life, and career do not vary much from account to account—both for the obvious reason that the basic biographical facts do not change with the teller and probably also because the genre (obituaries written by colleagues and published in scientific journals) is limited in length and viewpoint and, by convention, does not explicitly discuss historical context. At the suggestion of the editors of this volume, and with the luxury of greater length, this essay will try to fill in the general outline by considering some of the people, institutions, ideas, and scientific developments that shaped and were influenced by Teuber’s scientific life and work.

**Background and Education**

Hans-Lukas Teuber was born in Berlin on August 7, 1916, oldest son of Eugen Teuber and Rose (Knopf) Teuber. A second son, Ulrich, was born in 1920. Both parents were musical (they both played the piano well and enjoyed dancing); Ulrich went on to become an organist and historian of music. Early years were spent in Doberan, west of Rostock near the Baltic Sea.

Teuber was educated first at a private preparatory school in Berlin and then at the Collège Français, a Huguenot school founded in 1689, where he studied Latin, Greek, Ancient History, and the Natural Sciences—all taught in French. He received the baccalaureate degree in 1934. When he was in his 40s Teuber wrote that the greatest single influence during his childhood and early years had been his father, who had shared with young Hans-Lukas his seemingly disparate interests in literature, animal behavior, and mathematics, and stimulated in the son a search for integrative principles that continued throughout his student and later years.3

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3. Eugen Teuber (1889–1958), also educated at the Collège Français, had been a young student of philosophy and psychology at the University of Berlin when he was sent by the Prussian Academy of Sciences in 1913 to Tenerife (in the Spanish Canary Islands) to install a Primate Station for behavioral observations of chimpanzees. As its first director he secured a suitable site and buildings for research, hired staff, and with Rose Teuber began systematic observations of chimp social behavior and physical development.

When Wolfgang Kohler (1887–1967) came to Tenerife to direct the Primate Station in late-1914, Eugen and Rose Teuber participated as observers in the fruit basket experiments, the first of Kohler’s famous experiments on intelligence in anthropoid apes. Some of the Tenerife chimpanzees were housed in the Berlin Zoo after Kohler returned to Berlin in 1920. When Eugen Teuber took young Hans-Lukas to visit the animals, Marianne Teuber (1994) later recounted, they still evidently remembered their scientist–friend from Tenerife: as father and son approached, “the animals would come to the fence, grab it and shake it vigorously, uttering the staccato ‘o, o, o’ of joy and greeting on spotting Teuber in the crowd” (Teuber, 1994, p. 574).

After leaving Tenerife, Eugen Teuber served in the German army as a communications officer on the Eastern front during World War I and then returned to doctoral studies at the University of Rostock, where he earned a Ph.D. degree in 1921 (with a dissertation on the philosophy of art
Another important influence was the neurologist Kurt Goldstein, a family friend whom Teuber first met in Berlin in the early 1930s. Goldstein was at that time well known for his extensive investigations of brain injured World War I soldiers whose altered perceptual and intellectual functions he interpreted holistically in his “organismic theory” (Teuber, 1966).

After traveling in Italy, France, and Switzerland, Teuber enrolled in 1935 at the University of Basle in Switzerland. There he concentrated in philosophy with an emphasis on philosophy of science and took courses and laboratory training in biology, comparative anatomy, and embryology. Teuber (1959a) dated the beginnings of his interest in the physiology of the nervous system to these university days, being particularly influenced by the work of embryologist Hans Spemann, which suggested that “problems and methods of study of CNS [Central Nervous System] functions might be similar to those found in experimental embryology (equipotentiality, vicarious functioning, organizers)” (Teuber, p. 2). Teuber also in later years mentioned the importance—in this case personal as well as intellectual—of his participation in “a small interdisciplinary group in Basle, composed of young instructors and students, who explored the methodology of various sciences and attempted to bridge the gap between biological and social science” (p. 2).

One of the fellow students in this interdisciplinary group was Marianne Liepe, who was studying art history at Basle—and who was strikingly beautiful and intelligent, widely read, and from an academic family. Liepe left for the United States in 1939 to continue her study of art history at Vassar College, and in that year Teuber...
ber was awarded the Holtzer Fellowship at Harvard and planned to leave for the United States as well.\(^6\)

With the outbreak of World War II, Teuber’s entry to the United States was delayed until 1941. Immediately upon his arrival, Liepe and Teuber were married, and he enrolled as a graduate student in Harvard University’s Psychology Department. Teuber was a student at Harvard from 1941 to 1944 and from 1946 to 1947. From 1944 (when he and Marianne Teuber became naturalized American citizens) through 1946, he served in the U.S. Naval Reserve, stationed, after basic training in Geneva, New York, at the U.S. Navy Hospital in San Diego.

While at Harvard, Teuber supported himself and his growing family (Andreas Wolfgang was born in 1942, Christopher Lawrence in 1946) by working as a research assistant on the staff of the Ella Lyman Cabot Foundation. The Foundation was then engaged in a ten-year experiment to see whether (in the language of the time) delinquency could be prevented among underprivileged boys by interventions consisting of guidance, counseling, and psychotherapy. Teuber—learning English at breakneck speed through total immersion\(^7\)—conducted interviews, trained interviewers, constructed questionnaires, and evaluated interactions between counselors and boys.

This work, he later said, “impressed me with the need for quantitative indices in the evaluation of behavior, and with the necessity of obtaining adequate control groups in assessing behavior change” (Teuber, 1959a, p. 3). His doctoral dissertation, “Dyadic Groups: A Study in Counseling Relationships,” grew out of this work. Gordon Allport was his supervisor; Jerome S. Bruner and Robert F. Bales completed his committee.\(^8\) It is possible to see some influences of this “excursion into social field work” (as he later described it) in Teuber’s later research and writing, but by far the more decisive influences on his developing commitment to understanding brain structure and function came from other sources. Fortunately, these are described in Teuber’s own words (Teuber 1959a; also quoted by Hurvich et al., 1987, pp. 466–467):

My original biological interests had been fostered at Harvard through contacts with Lashley, through avid reading of the work of J. W. Gibbs, L. J. Henderson, and W. B. Cannon. The possibility that the logic of Gibbsian systems

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6. In 1929, Charles W. Holtzer established the Holtzer Fellowship in Harvard’s Graduate School of Arts and Sciences “for students of German birth who have received their preliminary education in German institutions of learning. Open to students in any department of the University.”

7. Teuber failed the required German language examination at Harvard the first time he took it because he did not know enough English to translate the German texts.

8. In the preface to his dissertation (already showing indications of the vigorous English prose style he would develop later), Teuber said: “Our indebtedness is greatest to our teacher, Professor Gordon W. Allport, of Harvard University, who has always wanted a monograph to be written on the dyadic group, and for some reason felt that the author should make this first attempt. From the very conception of the problem down to the cleansing of the writer’s English, he has helped to see this thorny matter through.”
(set up for physical chemistry) might be equally applicable to biological and social systems was considered more and more seriously.

A more direct influence was that of Kurt Goldstein's, who at that time (1941) was Visiting Professor and William James Lecturer at Harvard. Frequent personal contacts made me aware of the strategic role of experimental neurology within the framework of general biological science, and suggested a reconsideration of the earlier German work (Bethe, Uexkull, Weiss) in comparative physiology of nervous systems and problems of sensorimotor integration.

The final and decisive "push" in the direction of my chosen field was provided almost fortuitously by a two-year period in the U.S. Navy. In 1944, I arrived at the San Diego Naval Hospital where Dr. M. B. Bender was in charge of the neurology wards. He was interested in studying peripheral nerve injuries, causalgia, and sensory disturbances after cerebral injury. Hearing of my acquaintance with Goldstein's work, he suggested that I stay with him at the Naval Hospital. An improvised laboratory was set up early in 1945, and men with acute battle injuries [from the Southwest Pacific] of the nervous system were studied by us for nearly two years. The unique opportunity of observing effects of acute brain injuries resulted in a number of joint papers. . . . In these papers, we tried to continue the tradition of Goldstein and Gelb, of Poppelreuter, of Head and Holmes, considering the injuries as experiments of nature and studying the disturbances of brain function as a clue to normal modes of central nervous system functioning.

Following discharge from the Navy, and after completing my work at Harvard, I came to New York University College of Medicine to build up there, under the original sponsorship of M. B. Bender and S. B. Wortis, a small laboratory for the study of effects of brain injuries. (pp. 4–5)

Morris Bender gave a more vivid, backstage view of their joint research in San Diego and relocation to New York University:

When I first met Luke . . . it was in the Personnel Office at the San Diego Naval Hospital during World War II . . . Luke was well versed in experimental psychology, even though he did his work in social psychology, and [was] especially knowledgeable of the German literature on battle injuries of the brain sustained by victims of World War I.

Once connected, we studied our patients with great enthusiasm, often

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9. Bender was speaking at the January 19, 1977, gathering to remember Hans-Lukas Teuber held at MIT after his sudden death on January 4. On January 19, Teuber was to have delivered his James R. Killian Faculty Award Lecture—"Mood, Motives, Memory, and Values"—in the same auditorium.
working long hours and well into the night. I particularly enjoyed his imaginative and insightful interpretations of our data and the lively discussions we had in writing our papers. Since no equipment was made available to us, Luke would improvise by using discarded motors and whatever else he could liberate from the Navy junk pile. We continued in this manner until the end of the war.

In April 1949, when we were separated from the Service, we took along one of these motors and drove, still in our uniforms, across the United States. It was less of a drive and more of a harrowing flight, because during the trip Luke has the constant feeling that we might be stopped by the Military Police, who would catch us red-handed with the Navy goods. We finally reached New York, and Luke immediately set up the motor for experiments on flicker fusion in a small office of the Bellevue Psychiatric Hospital. Shortly afterwards, he was appointed to the Department of Psychology at New York University . . . [where] he became an extremely popular and influential teacher, attracting many gifted students into psychology and medicine.

**Psychophysiological Laboratory, New York University**

As Bender’s remarks suggest, the research with brain-injured World War II veterans begun in San Diego was continued and expanded in the Psychophysiological Laboratory at NYU, directed first by Bender, until he left to head the Neurology Department at Mount Sinai College of Medicine, and then by Teuber. In 1948 Teuber became area consultant (for the greater New York area) to the U.S. Veterans Administration, and the laboratory received support from the Committee on Veterans Medical Affairs of the National Research Council (NRC) to recruit and test men with battle injuries of the brain due to penetrating missiles. Although the NRC would not fund research on a control group of veterans, Teuber and Bender recruited a control group (veterans who had sustained peripheral nerve injuries from penetrating missiles, with no signs of CNS involvement) through collaboration with Harry Grundfest at the Neurological Institute in New York.10

With the patient recruitment procedures in place (186 active cases and 101 control subjects were being tested by the summer of 1953), the Psychophysiological Laboratory was up and running, and Teuber embarked on a productive decade of research, teaching, lecturing, and writing. His first doctoral student was Stan Batterby, his second Sidney Weinstein, and he soon attracted the nucleus of a

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research group—augmented by visiting students and researchers—that included Lila Braine (then Lila Ghent) and Josephine Semmes as well as Sidney Weinstein. During the 1950s, the laboratory produced a steady stream of papers and two classic monographs: *Somatosensory Changes after Penetrating Brain Wounds in Man* (Semmes, Weinstein, Ghent, & Teuber, 1960) and *Visual Defects after Penetrating Missile Wounds of the Brain* (Teuber, Battersby, & Bender, 1960).

Both the biographical essay by Hurvich et al. (1987) and Weinstein’s reminiscences (Weinstein, 1985) highlight some of the methodological innovations developed by Teuber and his group at NYU: rigorous experimental testing of the performance of large groups of patients; use of control groups; and development of specific tests for assessing performance in somatosensory, visual, and auditory domains. During this period, Teuber also developed more general methodological and theoretical principles for testing strategies and interpreting performance deficits: double dissociation of symptoms, cerebral differences and hemisphere interactions, and corollary discharge (Teuber, 1959b, 1960). He also wrote two major papers, which proved influential, both of them theoretically rich and comprehensive in their coverage. One was on physiological psychology, in the *Annual Review of Psychology* (Teuber, 1955) and one on perception, in the *Handbook of Physiology* (Teuber, 1960).

The influence of these and other contributions to neuropsychology is discussed in the memorial tributes to Teuber by colleagues cited at the beginning of this essay and may be well known to some readers of this volume. A few examples will be briefly described, however, to give a flavor of the work for those who may be less familiar with it.

11. The core people in the small laboratory at NYU represented a remarkable convergence of significant trends—personal influences, ideas, and techniques—in neuropsychological research at that time. Lila Ghent was newly arrived in New York City from Montreal, where Donald Hebb was a major figure in physiological psychology. Josephine Semmes had been a student of Lashley, had worked with him at the Yerkes Laboratory of Primate Biology in Orange Park, Florida, and had earlier worked in the research group of Warren McCollough and Walter Pitts in Chicago.

12. According to Charles Gross, neuroscientist, member of the National Academy of Sciences, and historian of neuroscience, the *Annual Review of Psychology* paper on physiological psychology “set the program for the field for the next decade” (Gross, 1994, p. 452). Teuber was active in the APA throughout the late 1940s, 1950s, and 1960s, and was much involved (and Sidney Weinstein played a key role) in the reestablishment of APA’s Division of Comparative and Physiological Psychology in 1962 (Dewsbury, 1996). Teuber served as president of the division from 1965 to 1966.

Teuber’s much cited “Perception” chapter in the authoritative *Handbook of Physiology* was written in part while Teuber was in hospital, recovering from injuries suffered in an automobile accident caused when the driver of an oncoming truck lost control and veered into Teuber’s lane. Though badly injured in the accident, Teuber remained conscious at the scene and insisted that no one move him until trained medical help arrived, thereby sparing himself possible damage to the nervous system. Hospital visitors recall Teuber, bandaged and in traction, with books and papers piled high on the bed, and a drawing of a skeleton mounted inside the closet door of his room marked with 15 or so red lines showing where his bones had been broken.
Arthur Benton, a leading neuropsychologist and contemporary of Teuber, has provided a concise description and evaluation of the research on somatosensory function reported in the 1960 monograph by Semmes et al.

This study showed that bilateral and ipsilateral sensory defects were a fairly frequent consequence of unilateral brain disease; that these defects occurred more frequently after left hemisphere injury; that the cerebral representation of somatosensory function was more diffuse in the right hemisphere than in the left; and finally, that the patterns or combinations of sensory defect were different in the two hands. Thus the study modified traditional concepts of contralateral innervation, and at the same time, demonstrated hemispheric asymmetry in the mediation of somatosensory performance. These findings, which were in the main confirmed by later investigators, had a profound impact on thinking in this field. (Benton, 1994, p. 38)

Other work from the Teuber group bore on the question of recovery of function following brain injuries and led to modification of the widely-held belief (the "Kennard" principle) that brain injuries incurred early in life nearly always cause less impairment than similar lesions incurred at maturity (Teuber, 1975). When the researchers compared performance on visual, motor, and somatosensory tests, administered to soldiers 10 or more years after they had sustained their injuries, with records of initial examinations conducted immediately following their wounding, Teuber and his colleagues found support for the Kennard principle (despite the restricted age range of the men at the time of injury and the greater sensitivity of the laboratory assessments). The picture grew more complex, however, when children with brain injuries early in life were also tested on a wider variety of tasks:

By following cohorts of 25 children with right and 25 children with left hemiplegias of early onset, we observed . . . that language does indeed tend to be "spared" after sufficiently early lesions of the left cerebral hemisphere; but this escape is not complete—there are subtle changes on tests of syntactic competence—and the escape is not without its price. The child often develops speech under these conditions by sacrificing some of those complex nonverbal capacities that would normally depend on the right hemisphere. . . . It is as if language development had precedence and as if reliance on the atypical (right) side for linguistic tasks exerted a crowding effect on that side, compromising its normal function. (Teuber, 1975, p. 475)

Teuber's methodological principle of "double dissociation of symptoms" (a strategy for inferring an underlying neural mechanism from behavioral symptoms) is described in his own words in the context of the group's research using a visuo–spatial task:
In cases of double dissociation, one symptom is found with one particular lesion but not with a contrasting lesion, and conversely, thereby suggesting separable mechanisms . . . [For example,] “double dissociation” can be seen in the test of setting a luminous line to the visual vertical, where it turned out that lesions of the anterior brain regions produced difficulties in setting the line when the patient’s head and body were tilted; those with parietooccipital lesions had much less difficulty in this respect. Conversely, patients with such parietooccipital penetrations performed much more poorly than those with frontal ones on a somewhat different task: that of setting a line to the vertical while their own body was upright, but the line had to be adjusted against an interfering (obliquely striped) background. By searching for such patterns of symptoms and by systematically varying the tasks employed we can hope to get at root changes in perception or in other aspects of behavior. (Milner & Teuber, 1968, p. 274)

Finally, Teuber’s empirical efforts over many years to solve the “riddle of frontal lobe function” (e.g., Teuber, 1964; Teuber, Battersby, & Bender, 1952) and his wide-ranging knowledge of the scientific literature led him to formulate the concept of “corollary discharge” (Teuber, 1960), which he incorporated into his thinking for the rest of his life (e.g., Teuber, 1964, 1975, 1978). “Corollary discharge” is part of a system in which

. . . self-induced motion of the eye (efferent pattern) causes peripheral motor effects and, concurrently, a central discharge back into the appropriate sensory system (corollary discharge) which normally matches (i.e., cancels) the sensations produced by the active movement (re-afferent pattern). Under normal conditions, corollary discharges and re-afferent patterns balance, so that signals from the environment are perceived (e.g., motion of external objects) and distinguished from relative motions due to the perceiver’s own movements. . . . Although entirely conjectural at this point the hypothesis [a corollary discharge from motor to sensory structures which prepares the latter for anticipated change] is attractive, since it can subsume normal and abnormal phenomena, and can perhaps be elaborated into a more general theory of constancies and illusions, and of perceptual identification. (Teuber, 1960, p. 1648)

Teuber goes on to say, “We have come to the conclusion that disturbances . . . on the level of the corollary or anticipatory discharge, represent an important common denominator in many forms of frontal lobe pathology in man” (Teuber, 1964, p. 419).

In summarizing the impact of these and other contributions, Henri Hécaen described Teuber as “la fondateur et l’animateur de Neuropsychologie contemporaine” [the founder and guiding spirit of contemporary neuropsychology] (Hécaen, 1979, p. 122). The overall scientific significance of the work is signaled by
awards and honors Teuber received during his lifetime, including the Karl Spencer Lashley Award for Research in Neurobiology from the American Philosophical Society (1966), election to the American Academy of Arts and Sciences (1962), election to the National Academy of Sciences (1972), the Eastman Professorship at Oxford (1971–1972), and honorary degrees from the Universite Claude Bernard, Lyons, France (1975) and the Universite de Geneve, Switzerland (1975).

Lest we lose sight of the concrete realities behind Teuber’s influential contributions during the NYU years, however, glimpses of his work “backstage” remind us that neuropsychology depends not only on the scientist and his or her ideas, but also on instruments and research sites, the human participants called patients or subjects, and networks of professional colleagues extending beyond the laboratory. Sidney Weinstein’s reminiscences of the early days of the Psychophysiological Laboratory provide engaging detail about some of these components:

[W]e started our lab on a virtual shoestring. Using penknives and cardboard we created testing boxes. From scraps of material we created tests of texture discrimination. Cardboard and children’s construction paper and library paste provided the ingredients for many of our tests of problem solving and I still recall that the first Semmes–Weinstein Pressure Aesthesiometer was housed in a small cigar box labeled “El Paso Cigars, The Cowboy’s Payday Smoke.” (Weinstein, 1985, p. 282)

Richard Held’s reminiscences tell how Teuber transmuted these seemingly unpromising materials into scientific substance:

I first met Lukas Teuber in the late 1940s on a visit to his Psychophysiological Laboratory at Bellevue Hospital in New York City. The quarters were old and dingy. Debilitated neurological patients and harried physicians filled the halls. During my visit, however, this initial unsettling impression quickly gave way to the sense of excitement generated by the activity going on in the laboratory. Lukas characteristically imbued his surroundings with a particular tension. His presence made a great difference, not only in the general ambience, but also in the style and substance of what was communicated in the informal discussions that were an inevitable part of a visit to his quarters. By his very manner of raising questions and pointing at the crucial issues, he breathed life into what might have been simply recitation. (Held, 1979, p. 117)

Weinstein (1985) described how this intellectual atmosphere was translated into networks of people and ideas:

[A]t the Psychophysiological Lab, with Luke, Joji [Josephine Semmes], and Lila [Braine nee Ghent] we . . . [had] informal luncheon meetings every Thursday. These weekly talks by many eminent, visiting scientists were con-
ducted while we sat around a rather decrepit table and munched on rather large hero sandwiches. It was these Thursday lunches that provided much of the interaction between the NYU group and the outside world of neuropsychology. (p. 280)

Recalling some of these visitors, Weinstein provided a long list of names that would figure prominently in any history of the field. Teuber's location in New York City, his broad range of knowledge, and his connections with European scientists (he read their publications in German and French and undertook an extensive trip to visit laboratories in England, France, and Germany in the summer of 1957)—all contributed to the networks of professional—cum—personal friendships Teuber formed during the NYU years. One gathers from the correspondence and reminiscences, furthermore, that these ties were strengthened through hospitality he and Marianne Teuber offered visiting scientists and their families in their home. 

A further revealing glimpse into another side of Teuber's work during the 1950s


Some of the names Weinstein correspondence also figure prominently in Teuber's correspondence from the NYU years (including especially Karl Pribram), as do others not on the list. These include (in no particular order) Herschel Leibowitz, Donald M. MacKay, Walter Rosenblith, Richard Solomon, Richard Held, George Miller, Thorne Shipley, Alberta Gilinsky, Ulrich Neisser (then a Harvard undergraduate), Margaret Mead, Edwin Newman, Martin Scheerer, Paul Schiller, Wale Nauta, Joseph Brady, and many others.

14. When networks like these condense at a particular time and place—say at the 1948 APA symposium, or the 1950 Pennsylvania State University meeting of APA (which Pribram [1977] named as the beginning of an era in physiological psychology), or the 1955 Eastern Psychological Association meeting (where George Miller gave the influential "Magic Number Seven" invited address and Karl Pribram, with Teuber as Chair, gave a talk on "Neocortical Functions in Behavior")—then, sometimes, scientific fields are changed or new ones formed. It may be a decade or more before most researchers feel the effects. Teuber did not attend the now famous "Moscow Colloquium" of 1958, which one historian suggests marked a turning point in the emergence of the neurosciences as a new field (Marshall, Rosenblith, Gloor, Krauthamer, Blakemore, & Cozzenz, 1996), but he was a participant, and usually an active one, at a surprising number of events of this kind. An attempt to document this persuasively would require more space than is available here.
(but one completely intertwined with the activities described above) comes from Daniel Robins, in words read at a gathering to remember Hans-Lukas Teuber held at MIT after his death in January 1977 (Pfeiffer, 1977):

I shall remember Dr. Teuber—remember him from a different perspective perhaps than most of you gathered here today. I was one of the first of the group of World War II veterans who volunteered for Dr. Teuber’s investigations into head injuries almost 25 years ago. My remembrances of him are from glimpses, glimpses from time to time over the years. He was always “Dr. Teuber.” I was always “Mr. Robins.” But that doesn’t express our relationship. It does not express his warmth at every meeting we had, his concern for my well-being, his interest in what I was doing as a person. I think back to those early days at Bellevue Hospital, the battery of tests—”Don’t let us tire you, Mr. Robins. We can go on next week. Call me whenever you feel the need to.” And last year, when on a business trip to Boston, I stopped by at MIT. Dr. Teuber was never too busy to see me. We talked and the years rolled back: the same warmth, the same personal interest and concern were there in that quiet voice. I remember him as a modest man, a concerned man, an interesting man, a gentle man, a gentleman. (Daniel Robins, quoted by Stephen Chorover, pp. 15–16 of Pfeiffer, 1977)

Remarks by Rita Rudel on the same occasion provide a view of Teuber’s work from yet another perspective:

I was riding home on the bus one day from Columbia Presbyterian [Hospital] where I work now and idly looking through a paper by someone in Montreal on the subject of “effects of brain damage.” And a man sitting on the bus next to me peered over my shoulder and said to me, “Do you know something about this field?” And I said, “Yes, a little.” [Rudel was a researcher in the NYU Psychophysiology Laboratory during the 1950s.] And he said, “You know, I went to City College, the city university. And one time they had an invited lecture by someone named Hans-Lukas Teuber. It had to be the greatest lecture I ever heard. I can’t remember what he said, but this had to be the most exciting thing I ever heard. If I’d had my life to live over again, I would have tried to get into that field.” I never found out what his name was, but I knew exactly what he meant. (Unknown man, quoted by Rita G. Rudel, p. 17 of Pfeiffer, 1977)

As these glimpses behind the stage of public honors and published papers suggest, Teuber’s appointment diaries from the NYU years show a dense intermingling of activities involving different people, knowledge, skills, and goals. They included testing patients; teaching; traveling to local, national, and international meetings; participating in APA organizational affairs; hosting visiting scientists from the United States, Europe, and the Soviet Union; attending operas and
The "Teuber group" at NYU was becoming increasingly prominent nationally and internationally, and Hans-Lukas and Marianne Teuber enjoyed living in their house in Dobbs Ferry, which was both lovely and within commuting distance of the art, music, and plays of Manhattan. Then in early-1960, newly in receipt of a seven-year National Institutes of Health program grant for research and graduate training in neuropsychology at NYU, Teuber received a call from MIT asking if he would consider moving his laboratory to Cambridge and heading the institute's psychology section.

Psychology Department, Massachusetts Institute of Technology

More than ten years of in-house discussion and dissention over "the problem of psychology" at MIT lay behind the call to Teuber. The problem from the

15. In reading appointment books and correspondence from the 1950s, I was struck by how active Teuber was as a speaker/presenter of papers and by the very wide range of groups and organizations he addressed (neurologists, psychologists of every stripe, psychiatrists, undergraduates; to university-wide, regional, state, and national organizations) and by the interest and warm responses he evoked, well beyond the conventional level of thanks and commendation. I also was struck by the extremely detailed discussions of methods and data in his correspondence with Karl Pribram, Heinrich Kluver, and other scientists in the late 1940s and early 1950s. Teuber maintained an active schedule as lecturer and conference participant after his 1960 move to MIT; as his department-building administrative activities increased, his engagement with day-to-day work in the laboratory decreased.

16. Given his background and experiences, Teuber certainly knew that the interdisciplinary, scientific–clinical nature of neuropsychology (a "border region" field) meant that it did not have a natural home in academic institutions organized, as most are, along disciplinary and departmental lines. MIT, on the other hand, had (and has) a well-deserved reputation as a place where problem-oriented rather than discipline-oriented research is the norm. Even in such an environment, however, psychology and psychologists at MIT posed an administrative challenge during the 1950s that seems to have had little to do with the particular personalities involved.

Unlike most major research universities in the United States, MIT did not have a psychology department until the 1960s. MIT was predominantly an engineering school until the 1930s when, under Karl Compton's presidency, faculty research strength and course offerings in the sciences were substantially expanded. In 1949 a post-World-War-II survey led a faculty committee to recommend that MIT strengthen its curriculum in the humanities and social sciences, and in 1950 a School of Humanities and Social Studies was added to the existing schools of Engineering, Science, and Architecture and Planning.

Psychology does not seem to have been an issue for MIT administrators until 1947, when Kurt Lewin died. Lewin, a distinguished German-born psychologist, had built the MIT Research Center for Group Dynamics into a thriving, nationally visible center for "action research" and had developed a graduate program leading to a Ph.D. in Group Dynamics. But the Center was only loosely affiliated with the Department of Economics and Social Science, and MIT made little effort to keep the Center or its students from moving to the University of Michigan after Lewin's death. Subsequently, references to the "problem of psychology" began to creep into MIT administrators' correspondence.
administration's point of view was that, in the absence of a separate psychology department, psychologists had been added to the MIT faculty and staff during the 1940s and 1950s by departments and interdepartmental laboratories of the Institute because psychological expertise was needed in support of ongoing, externally funded research projects. By 1959 there were four main groups of psychologists at MIT, with little in common except the name "psychology" on their degrees and no coordination or consultation among the groups regarding new appointments.\textsuperscript{17}

Undergraduate students continued to ask for courses in psychology and alumni testified to its potential importance in their jobs as engineers and businessmen. And MIT was committed—in principle at least—to providing its undergraduates with a coherent curriculum in the humanities and social sciences, including psychology. What administrative mechanism could be used to develop a psychology curriculum taught by psychologists who were outstanding researchers?

Repeated attempts were made to resolve the problem throughout the 1950s (external and internal committees formed, reports written, meetings with administrators, followed by more of the same) until finally MIT president Julius Stratton decided that enough was enough. There was a psychology section within the Department of Economics and Social Science, which was in the School of Humanities and Social Science. The hope was that, with a clear commitment of support for psychology from the president, this section could provide an administrative umbrella under which all of the psychologists at the institute would gather (regardless of the school or department or laboratory of their research group and/or appointment) to plan a Ph.D. program in psychology and coordinate the undergraduate psychology curriculum. An outside advisory committee was formed to advise the MIT administration in selecting the right man (in the language of the time) to head the section. Hans-Lukas Teuber's name came up almost immediately, and negotiations proceeded swiftly.

Teuber's letter of appointment from MIT's President Stratton (May 4, 1960) indicated that he was to head the psychology section in the Economics Department, but the understanding was that he would work with the other psychologists to propose the formation of a Ph.D.–granting Department of Psychology. It did not work out quite that way.

Teuber had a clear vision of the kind of psychology department he wanted to build at MIT. He was a skilled grant-writer, in the early 1960s federal funding for research (including support for faculty, student, and administrative staff; equipment; and building renovations) was flowing more freely than ever before or since, and procedures for recruiting and hiring faculty were much less formal than they are today. Within three years, most of the psychologists who had been affiliated

\textsuperscript{17} The two main groupings included psychologists interested in organizational behavior, associated with the School of Industrial Relations, and a larger group interested in communications sciences, associated with the Research Laboratory of Electronics and the Lincoln Laboratories (where they worked mostly on the "human factor" in "man–machine" systems).
with the psychology section when Teuber arrived had left MIT or withdrawn from activities of the section. New psychology faculty had been hired, Teuber’s proposal that an independent Psychology Department be established had been approved, and the first graduate students had been admitted.

As Teuber himself said three years later, in a letter to John Burchard, Dean of the School of Humanities and Social Science, “These were rough years, from 1961 on, when I came onto the campus. . . . You called me to the Institute at a time when you had every right to distrust psychology and psychologists. There had been more than a decade of bickering and in-fighting, of half-hearted starts and sudden defections. I came and started out by letting practically everybody go.”

To build the kind of psychology department he envisioned, Teuber raised the outside money necessary to hire new faculty and staff, to support visiting scientists and graduate students, and to move the new department into its own, newly renovated building. This was a remarkable achievement, made possible both by Teuber’s hard work and acumen and by MIT administrators’ willingness to allow him, if he could (with their help) attract the necessary funding from external sources, to implement his vision of what a psychology department at MIT should look like. Equally remarkable was the vision itself.

In the early 1960s, the central core of most psychology departments was learning, and faculty and curriculum reflected (with different emphases) the scientific sub-fields of the discipline (experimental, physiological, comparative, social, personality, developmental, abnormal). Teuber created a very different kind of psychology department, one that neuroscientist and historian Charles Gross said “helped change the concept of a psychology department and led to the founding of neuroscience departments across the country (Gross, 1994, p. 453). By the mid-1960s, the MIT Psychology Department faculty consisted at the senior level of a world-famous neuroanatomist (Walle Nauta), an experimental psychologist (Richard Held), and Teuber; and at the junior level a philosopher (Jerry Fodor) and a psycholinguist (Merrill Garret), as well as physiological, comparative, and experimental psychologists (including Stephen Chorover, Peter Schiller, Alan Hein, Joseph Altman, Charles Gross, Wayne Wickelgren, and Herbert Saltzstein).

18. The quotation is from a letter from Teuber to Burchard, June 8, 1964 (MC417, MIT/IASC). Teuber also moved quickly on another front shortly after arriving at MIT. He discovered that MIT undergraduates were being used without their knowledge as subjects in an experiment on posthypnotic suggestion (by a part-time, visiting faculty member). He put a stop to this practice and helped MIT form a Review Committee on Human Subjects to safeguard human participants in research projects in the future—several years before such review committees were required by NIH.

19. Chorover and Altman had come with Teuber from NYU, as did Louise Pfeiffer who was secretary and then administrative assistant to Teuber from 1953 to 1965. After Pfeiffer retired and until Teuber’s death, his secretary/administrative assistant was Eva Ritter (later Ritter-Walker). Like Teuber, Ritter-Walker was fluent in German and French as well as English; before working in the Psychology Department she had been secretary to Norbert Weiner and to Noam Chomsky.
Many of the faculty had undergraduate degrees in the biological or natural sciences or in engineering, as did most of the graduate students admitted into the program in its early years.

Research groups and teaching in the department were organized into three “prongs,” an image derived from the Greek letter psi, the symbol of psychology: physiological psychology (later called brain and behavior), general experimental psychology, and social and developmental psychology. The social-developmental prong was relatively downplayed, however, as it was never possible to find the “right person” for a senior appointment in social psychology.

Within a few years and at the urging of his faculty, Teuber petitioned the MIT administration (unsuccessfully) to change the department’s name to “Psychology and Brain Sciences” and move it into the School of Science. In 1970 the Alfred P. Sloan Foundation designated the MIT Psychology Department a “center of excellence” in the emerging new field of “the neurosciences” (or of “behavior in relation to the neural sciences”), and for the next five years Sloan provided general support to the department’s activities. The Sloan grant, together with National Institute of Mental Health training grants and large research grants from other sources to Teuber and other faculty, constituted the majority of the department’s funding under Teuber’s chairmanship during the 1970s. MIT administrators helped smooth the way for Teuber at some foundations when he sought outside funding, but MIT itself provided a relatively small proportion of the Psychology Department’s total budget.

20. Teuber inherited the elements of this tripartite structure, a legacy from earlier efforts by psychologists at MIT (including, at various times, Donald Marquis, George Miller, J. C. R. Licklider, Roger Brown, Ronald Melzack, and Michael Wallach) to develop a coherent undergraduate psychology curriculum. He stamped it with his own vision, however, selectively limiting the range of existing courses to be included, adding other disciplines, and presenting the whole as an integrated approach to a set of fundamental, clearly-delimited scientific questions about the relation between behavior and brain function.

21. Teuber renewed this request for change of departmental name and school affiliation many times, but it was never granted. The School of Humanities and Social Sciences would agree to the name change, but—even without a change of school—was increasingly uneasy about allowing natural-science-oriented psychology courses to count as part of the undergraduate humanities and social sciences requirement. The School of Science would agree to the name change if the department were moved under its aegis (where Biology Department faculty could scrutinize its appointments more closely). After Teuber’s death in 1977, Richard Held succeeded him as head of the Psychology Department. The department’s name was subsequently changed to Department of Brain and Cognitive Sciences, and it is now located in the School of Science. An undergraduate, interdepartmental “Concentration in Psychology” has evolved in the School of Humanities and Social Sciences to meet the needs of undergraduates interested in a broader and more traditional range of psychology courses than was offered by the Department of Brain and Cognitive Sciences. In many ways it seems that the “problem of psychology” at MIT remains unresolved.

22. In the late-1960s, less than 20% of the Psychology Department’s total budget came from MIT. MIT is an institution where faculty research and graduate education have high priority, but it seems likely that the institutional strength of the Psychology Department Teuber created rested in part on the popularity of its undergraduate classes. Enrollment in these classes climbed
While at MIT, Teuber continued to be active in international, interdisciplinary groups of scientists and clinicians interested in behavior and brain function, including the International Neuropsychological Symposium, the International Brain Research Organization, the European Brain and Behavior Society, and the French Psychological Society. His preface to the English translation of Aleksandr R. Luria’s *Higher Cortical Functions* shows his considerable interest in differences among national traditions in neuropsychological research (Teuber 1966/1980).

And even though the American Psychological Association was increasingly being abandoned by physiological psychologists during the late 1960s and 1970s (Davis, Rosenzweig, Becker, & Sather, 1988), Teuber continued to participate in North American psychological organizations, including not only APA and the more experimentally oriented Eastern Psychological Association, but also the Psychonomic Society and the elite Society of Experimental Psychologists. He was also a member of the Society for Neuroscience, whose president, when the “younger generation” of neuroscientists began to assume leadership, was Larry Squire, an early graduate of the MIT Psychology Department (Squire, 1998).

Teuber’s many connections with European and North American brain scientists enabled him to mount a colloquium series every year that showcased his vision of psychology as he thought it should be—multidisciplinary in method, focusing on the behavior of organisms. He was famous for his elegantly witty, scholarly—

steadily from 1962 on, the most popular class being Teuber’s famous “Introduction to Psychology,” which was so large that for a few years he had to give each lecture two times in the same day. (Telling students they could attend either lecture, he used to say, teasingly, “The facts may change, but not the jokes.”)

23. On one of his European trips, he and Marianne Teuber visited Aleksandr Luria’s country retreat, where Teuber jokingly teased Luria about their meal being served to them at table—in a supposedly egalitarian country.

24. Squire, now Professor of Psychiatry, Neurosciences, and Psychology at the University of California, San Diego, is a member of the National Academy of Sciences, as is another early graduate of the MIT Psychology Department, neuroanatomist Ann Graybiel. In the early-1970s, when the Psychology Department was still relatively small, Teuber used to be modestly proud of the fact that three of its senior faculty (Nauta, Held, and Teuber) were members of the National Academy of Sciences and of the American Academy of Arts and Sciences.

25. Teuber used many different terms to refer to the “border region” field he envisioned, including “neuropsychology,” “physiological psychology,” “biological psychology,” “brain and behavior,” and “behavioral biology.” The latter was the term he urged the Sloan Foundation to adopt in 1969 when the Foundation was planning its new interdisciplinary program focusing on behavior in relation to the neural sciences, but “neuroscience,” already beginning to gain currency by the late-1960s, was chosen instead.

Correspondence with MIT administrators about changing the name of the department suggests something of the way he viewed the term “neuropsychology” in 1972. According to Teuber, “Psychology and Neuropsychology” was considered [by the faculty as a new name for the department] and rejected because it seemed redundant to some people, and because the word ‘neuropsychology’ has acquired an unfortunate meaning of a limited clinical specialty involving the psychometric testing of patients with neurologic illness” (Teuber, 1972, p. 1).
and lengthy—introductions of the speakers, and the colloquia were attended by scientists from throughout the Boston–Cambridge scientific community (including, on occasion, Warren McCullough, Jerome Lettvin, Marvin Minsky, Seymour Papert, Patrick Wall from MIT, David Hubel, Torsten Wiesel, and Norman Geshwind from Harvard). As had been the case when he was in New York, Teuber made his department a stopping-off place for national and international visitors, and the hospitality offered by the Teubers in their Arlington home further strengthened scientific networks and friendships.

In addition, Teuber worked tirelessly to promote research done by faculty and students in the new and unorthodox department he created, guiding visitors through the laboratories (preceded by “Hurricane alert!” memos from his office if they were site visitors from a funding agency) and sending copies of his elegantly written and eye-catchingly illustrated annual report to hundreds of psychologists, brain scientists, and psychology departments. Gerald Schneider, one of the first students to graduate from the MIT Psychology Department (and later to join its faculty), spoke of this side of Teuber’s work at the January 19, 1977, gathering held at MIT after Teuber’s death:

He so much loved to talk about the research in the Department… Thinking about that quality reminded me of when I was a graduate student at MIT, sitting around with a bunch of students talking about a new finding in the Department, you know, where should this be published. “Of course, you should send it to Nature.” Another one said, “No, we should send this to Science, of course.” Then someone said, “Why not just tell it to Lukas?” You see, he was one of our means of publication. And I found out later as I went to give talks at many places that many people I met had heard of my work first, or the work of other people in the Department first, by listening to Lukas Teuber. (p. 12 of Pfeiffer, 1977)

Although Teuber worked tirelessly and with infectious enthusiasm to promote his vision of psychology and research in the department he created, so much activity must have been tiring. He spent a year at Oxford University in 1971 to 1972 as the Eastman Professor—a period characteristically filled with several return trips to the MIT department, grant proposals, and manuscripts, as well as his duties in the Eastman professorship—and returned looking thinner than before. He was taking a well-deserved vacation with Marianne in the British Virgin Islands when he died while swimming, possibly from a heart attack, on January 4, 1977; his body was lost at sea.

One of the speakers at the January 19, 1977, gathering to remember Hans-Lukas Teuber was Jerome Wiesner, who had been, among many other distinguished positions and accomplishments, provost and then president of MIT during part of the time Teuber was there. He reflected the following:

[T]he MIT Psychology Department, with all its diversity and quality and rigor, is indeed Luke’s creation… In the 15 years that Luke was at MIT, he
created a great Department and hundreds of friends—a department poised, as he believed the entire field to be, on the edge of deep understandings of the human mind, how it functions and how it learns. His influence will remain here forever, but we, his close friends, will miss him deeply. (p. 3 of Pfeiffer, 1977)

The “entire field” to which Wiesner referred was the emerging new field Teuber had spoken of 30 years earlier at APA as “the coalescence of experimental psychology and experimental neurology” and had called “neuropsychology” (Teuber, 1948, p. 2). He envisioned the field then as a goal, not an achievement. That goal is closer three decades later thanks to Hans-Lukas Teuber and his work—work that his zest, wit, and enthusiasm always made seem like fun to those whose lives and minds he touched.

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