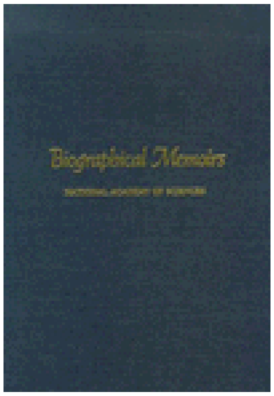

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PREFACE

On March 3, 1863, Abraham Lincoln signed the Act of Incorporation that brought the National Academy of Sciences into being. In accordance with that original charter, the Academy is a private, honorary organization of scientists, elected for outstanding contributions to knowledge, who can be called upon to advise the federal government. As an institution the Academy's goal is to work toward increasing scientific knowledge and to further the use of that knowledge for the general good.

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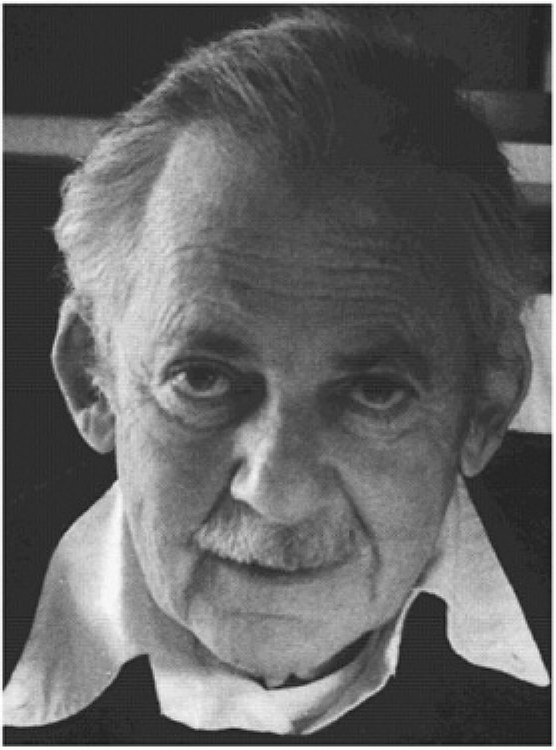

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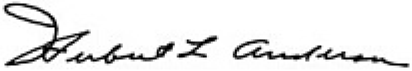

HERBERT L. ANDERSON

May 24, 1914–July 16, 1988

BY HAROLD M. AGNEW

HERBERT L. ANDERSON WAS born in New York City on May 24, 1914, and died in Los Alamos on July 16, 1988, after an almost forty-year battle with berylliosis. Anderson attended New York City public schools and entered Columbia University in 1931. He received an A.B. degree in 1935, a B.S. in electrical engineering in 1936, and a Ph.D. in physics in 1940.

As a high school student Anderson was fascinated with radios and it was this interest in early electronics that led him to electrical engineering and eventually to his distinguished career in physics. He was Enrico Fermi's first collaborator in the United States.

While Anderson was completing his degree in electrical engineering John Dunning, professor of physics at Columbia, decided to build a cyclotron. At the urging of Professor Dana Mitchell, Dunning offered Anderson a job as a teaching assistant while he worked toward his Ph.D., with the proviso that he was also to assist in the design and construction of the cyclotron. As an undergraduate, Anderson made two major contributions to the design and construction of the Columbia cyclotron. The first was the design of a high frequency filament supply to replace the direct current version then in common use. This concept allowed for a much longer filament life in the high magnetic field to which the filament was exposed. However, his most important contribution and the one of which he was most proud was the result of his belief that the high frequency system would be much more efficient if the dees were fed with a pair of concentric lines instead of the usual ordinary induction system. Dunning accepted this innovation, and it became a common feature of all future cyclotron designs. Assisting Anderson in construction of the cyclotron were Eugene Booth, Norris Glascoe, Hugh Glassford, and, of course, John Dunning. In late 1938 in anticipation of doing experiments with the cyclotron Anderson built an ionization chamber and a linear amplifier.

In late 1938 and early 1939 the experiments of Otto Hahn and Fritz Strassmann had been correctly interpreted by Lise Meitner, who, with her nephew Otto Frisch, was at Bohr's institute in Copenhagen, having fled there to escape Nazi Germany. At that time Fermi had been awarded the Nobel Prize (for his work on nuclear processes induced by slow neutrons) and he and his family were in Sweden for the ceremonies. After acceptance of the prize Fermi and family went on to Columbia rather than return to fascism in Italy.

Frisch contacted Bohr who was leaving for America and told him of his and Meitner's concept of the implications of the experiments by Hahn and Strassmann. Consequently, when Bohr arrived in New York he immediately contacted Fermi. Fortunately for Anderson he couldn't find Fermi, but he did find Anderson and proceeded to tell him about nuclear fission of uranium. Bohr left, and Anderson searched for Fermi and found him at work in his office (Bohr hadn't looked there). Anderson proceeded to explain his conversation with Bohr, but Fermi, according to Anderson, immediately took over and explained fission to Anderson. Anderson seized the opportunity to point out to Fermi, who had arrived only ten days earlier, that he, Anderson, had equipment to do experiments ready to go, needed a sponsor for his thesis, and in effect made Fermi "an offer he couldn't refuse." Using his equipment, Anderson on January 25, 1939, became the first person in the United States to demonstrate the large energy release in the fission of uranium. His crucially important Ph.D. thesis, "Resonance capture of neutrons by uranium," finished in 1941, was not published for reasons of national security until ten years later.

Thus began a most rewarding relationship between these two physicists that lasted until Fermi's death in 1954. In anticipation of the importance of the discovery of the fission process Fermi and Anderson conducted a series of experiments at Columbia on the fissioning of uranium, slowing down of neutrons in graphite, absorption and reflection of slow neutrons by numerous relevant materials, and preliminary experiments involving a lattice of uranium in graphite.

When the Metallurgical Laboratory was started at the University of Chicago in February 1942 Anderson and Fermi along with Wally Zinn from Columbia became the leaders in the construction of the first man-made nuclear chain reaction, accomplished in the racquet court under Stagg Field. The experiment, known as CP-1, went critical the afternoon of December 2, 1942. Following CP-1 Anderson led the construction of CP-2 at the Argonne site in 1943 and was a key consultant for Dupont in the construction of the Hanford reactors, which produced the first plutonium for the U.S. nuclear arsenal.

Anderson left Chicago for Los Alamos in 1944 and participated in determining the critical mass of ^{235}U using the Omega reactor. When it was decided to test the first nuclear device on July 16, 1945, Anderson and his radiochemist colleagues devised methods for determining the yield of the device by collecting fission products from the crater under the tower on which the device had been detonated. Subsequently, this technique was perfected and used to analyze air samples containing radioactive debris from U.S. and foreign tests.

After the war Fermi and Anderson returned to the University of Chicago where they established the Institute for Nuclear Studies. Anderson became successively assistant professor of physics (1946–47), associate professor (1947–50), professor (1950–77), and distinguished service professor (1977–82). He served as director of the Enrico Fermi Institute from 1958 to 1962. He was appointed a Guggenheim fellow (1955–57) and a Fulbright lecturer in Italy (1956–57). He was elected to the National Academy of Sciences in 1960, to the American Academy of Arts and Sciences in 1978, and was accorded the Enrico Fermi Award for 1982.

At Chicago in 1946 he and Aaron Novick constructed the first plant for extracting tritium and He^3 from material irradiated at the Hanford reactors. Using a novel form of radio frequency bridge for detecting the nuclear magnetic resonance, he made the first precision measurements of the nuclear magnetic moments of these nuclei. Tritium eventually became a key ingredient in all modern nuclear warheads.

At the University of Chicago, Fermi was approached by Urner Lidell of the Office of Naval Research who said to him, "Look Fermi, isn't there something you would like to do? I'd like to get you the money for it." After discussions between Edward Teller, who urged the construction of a large computer, and Anderson, who volunteered to build whatever was decided, Fermi elected to build a cyclotron. Pi mesons had just been discovered and it was decided to build a 450 MeV synchrocyclotron. Work started in 1947. Anderson with John Marshall successfully completed the construction in 1951. The magnet coils were unique because they were cooled by circulating water through the interior of the actual copper windings. Initially it was believed that their accelerator was the most powerful in the world at that time. However, it was subsequently determined that a somewhat higher energy machine had been completed in Russia in 1949.

Anderson's first research with the cyclotron, in collaboration with Fermi, Nagle, and others, had to do with the scattering of pions and protons. This work established the nature of the pion-proton interaction and made evident the fundamental role played by pions in accounting for the nuclear force. This work was climaxed by the discovery of the N* (1236), which turned out to be the first of a series of excited states of the nucleon.

Anderson's further work at the cyclotron dealt with rare modes of the π and μ decay. This helped establish the form of the weak interaction. His study of μ capture and μ mesic atoms turned out to be a very fruitful field of research, which he pursued with his students and a group of Canadian physicists for more than ten years. These experiments gave highly precise measurements of the size and shape of the distribution of electric charge in nuclei. They also provided a searching experimental test of vacuum polarization and the theory of quantum electrodynamics as it applied to muonic atoms.

When the Argonne 12-GeV ZGS accelerator was completed Anderson turned his attention to physics at higher energy. He developed a system for automatic readout of spark chambers using TV vidicon cameras; this was used on a number of studies of boson production in pp collisions done with students and a group of Canadian collaborators. Then came a study of the reaction $pp \rightarrow d\pi^+$ at the Bevatron, using specially designed decision-making, proportional-wire chambers in the spectrometer arms.

In 1971 Anderson collaborated with physicists from Chicago, Harvard, Illinois, and the University of Oxford to carry out a series of experiments on the deep inelastic scattering of muons in hydrogen and deuterium. The work was done at Fermi's lab where a new accelerator made available muon beams of energy up to 219 GeV. For this experiment the magnet from the recommissioned University of Chicago cyclotron was moved from the University of Chicago campus to Fermi's lab. The experiment showed a breakdown of scaling in the manner predicted by quantum chromodynamics. It gave strong evidence for the model that colored quarks and vector gluons were constituent parts of the nucleon.

In 1978, when the question of lepton conservation was revived as an important issue for elementary particle research, Anderson went to Los Alamos to search for the process with a collaboration from Stanford, Los Alamos, and Chicago. The null result found set a new upper limit in the branching ratio at 2×10^{-10}.

From the early fifties until his death Anderson carried on his strenuous activities while fighting the debilitating effects of berylliosis, incurred while preparing radium beryllium sources. In the spring of 1942 Anderson and I made the first compressed radium beryllium source. The powdered beryllium was prepared by filing on a block of beryllium. The die and press for making the source were designed by Anderson, and we flew to New York City with them. We prepared the source by pouring a solution of radium chloride on the beryllium powder, evaporating the mixture on a hot plate, pouring the dried powder into the die, pressing the mixture to form a pellet which was hand soldered into a brass capsule. The source was then wrapped in tissue, inserted into a small mayonnaise jar, and placed in a briefcase. We then boarded a plane and placed the briefcase under our seat and returned to Chicago. On our return the tissue was taken from the mayonnaise jar and tested with a Geiger counter to check that the source was properly sealed and no radon was leaking from the source. The rationale for making pressed sources was to make them physically smaller and even more important stabilize their neutron output. Previously made, loosely packed sources had the disadvantage that the neutron yield could vary because of variations in the mixing of the powdered beryllium and radium salt.

In addition to his work in Italy and Brazil, Anderson intermittently spent time at Los Alamos, and finally in 1978 he returned to Los Alamos as a fellow and was a senior fellow until his death in 1988, on the same day as the Trinity atomic test in 1945. At Los Alamos he initially concentrated his research at the Los Alamos Meson Facility, where he initiated, in collaboration with Darragh Nagle, a program of research that included studies of rare and normal muon decays. At the end of his career he collaborated with biologist Theodore Puck in developing automatic instrumentation to analyze the proteins made by living cells. The proteins were separated by two-dimensional electrophoresis. For this he designed a protein analyzer to measure the separated proteins by direct β-ray counting. In addition to his research at the University of Chicago he sponsored many graduate students who went on to successful careers in physics.

Those who were privileged to know and work with Anderson remember him as a physicist's physicist. He was an innovator, a tireless searcher for new knowledge, an inspiring teacher, and one to whom his colleagues owe a debt of gratitude.

At the time of his death Anderson's survivors included his wife Mary Elizabeth Anderson of Jacona, New Mexico; Jean Clough Anderson, his first wife, and their sons Dana Zachary Anderson of Boulder, Colorado; Kelly Pierce Anderson of Salt Lake City, Utah; Clifton Leon Anderson of Sunnyvale, California; stepdaughter Faith A. Campbell of Sonoma, California; and one grandchild.

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ARCHIBALD PHILIP BARD

October 25, 1898–April 5, 1977

BY TIMOTHY S. HARRISON

PHILIP BARD WAS born on October 25, 1898, in Hueneme, California, the youngest of seven children. He died in California on April 5, 1977. Although he was a tireless student of central nervous system physiology, much of Bard's genius transcended science into unique intellectual relationships with a wide variety of people, including other nervous system scientists of his era. Many of these individuals came to Johns Hopkins often and to Bard specifically for advice with some of their most perplexing problems.

Two factors contributed to Philip Bard's remarkable, unsought leadership. The first was exposure in depth to Walter Cannon at Harvard. Around Professor Cannon were scientists from all over the world, and there was an exceptional faculty in physiology. Senior staff consisted of Cannon, Alexander Forbes, Alfred Redfield, and Cecil Drinker; junior staff included Hallowell Davis, William B. Castle, David Brunswick, and Harold Himwich. Bard interacted instinctively with them all, initially as a graduate student and later as a junior faculty member for two years.

The second key to Bard's leadership was strength of character. This permeated all of his interpersonal relationships and was clear immediately upon meeting him. His friendship was total and permanent. The only scientist in his family, Philip inherited from his father and other family members selfless respect and a sustained deep interest in others.

In 1741 the Bards emigrated from Ireland's County Antrim, settling into 5,000 acres of farm land, which was later redistricted into Pennsylvania. Many family members lived in or near Chambersburg, where Philip's father, Thomas Robert Bard, was born in 1841. Denied a Princeton education by the premature death of his father in Chambersburg, Thomas instead read law with the local judge.

Through his success with a small local railroad and other activities, Thomas Bard attracted the attention of financier Thomas A. Scott, and was hired by him in 1864 when Bard was twenty-three years old. Scott commissioned Thomas to develop potential land and oil interests in California, and on May 16, 1871, Thomas Bard first appeared in California. He became a permanent resident, raised a family, and built Berylwood, the Bards' first California home, where Philip Bard was born. Thomas also served one term in Washington as a senator from California.

Philip openly admired his father and it was he who sent Philip to the Thacher School in the Ojai. Academically Philip was unmotivated and a mediocre student. His confessed interests in student affairs, horses, and baseball were passions his father understood. Nevertheless, headmaster Sherman Day Thacher recognized unusual talents in his pupil and, along with family friend Walter Alvarez, suggested medicine to Philip.

Philip graduated from the Thacher School in 1916, just after the United States entered World War I. His mother insisted that Philip wait until he was eighteen years old before joining the Stanford unit of the American Army Ambulance Corps. The unit saw heavy action in France. We know from personal letters that in France Philip read Howell's

Physiology (he was struck by Walter Cannon's barium X rays of a dog) and *Principles and Practice of Medicine* (presumably by Osler).

After World War I Philip traveled to California to see Harriet Hunt of Pasadena and Walter Alvarez. Letters between Philip and Harriet indicate they were "promised" to each other for a year before Philip left for France. Alvarez, who had once worked in Walter Cannon's laboratory gave Philip more information about Professor Cannon. Alvarez thought Cannon would be right for Philip.

After World War I Princeton's biology department was busy with high-quality science, including strong basic research in biology. E. Newton Harvey and Edwin Grant Conklin were recognized as leaders in the department. At that time Princeton had no married undergraduates, but Philip was among the first to be granted an exception (he and Harriet had married on June 29, 1922). Philip immersed himself in biology, learned how to study, and worked harder than ever before graduating in 1923 with highest honors. The next year he was a graduate student supervised by Newton Harvey, whose clear thinking and genius for experimental design had already led Philip away from medicine to physiology.

Remembering his discussion with Alvarez, Bard applied to Harvard to work on his Ph.D. under Cannon, and in 1924 he and Harriet moved to Cambridge with Virginia, their first child. Before long the question arose of a suitable project for his thesis. Cannon had determined that cats deprived of cerebral cortex display anger on slight provocation, and he termed this behavior "sham rage." Exploring the possibility of an essential central nervous system center for sham rage seemed to Cannon to be a suitable thesis topic for Bard. The result, "Diencephalic Control of the Sympathetic Nervous System," was published by Harvard in 1927. The copy in the Harvard archives is personally inscribed, "To Dr. W. B. Cannon who suggested this work and whose advice and encouragement carried it along." In the first 157 of 251 pages Bard reviewed the pertinent literature. In this review we find the most arresting feature of the entire thesis, his analysis of rage as a tenable scientific concept. The center for sham rage follows convincingly from Bard's well-planned experiments.

In 1928 Bard accepted an assistant professorship in the biology department at Princeton. While it was satisfying to be invited back to Princeton, he missed the community of scholars at Harvard. He felt wholly on his own with no one to share his interests. Cannon surmised this and kept in close touch. Every week, or more often, there were reprints or notes and letters from Cannon directing Bard to a new thought or article.

In 1929 Bard reviewed the neuro-humoral basis of emotional reactions. First, he defined emotional consciousness and realized the necessity for a connection between brain and viscera. Sympathetic and parasympathetic divisions of visceral nerves were defined and sketched. Last of all, he dealt respectfully with the James-Lange theory and joined others (Sherrington, Cannon, Lewis, and Britton) in putting it to rest.

Chandler McCuskey Brooks, after graduating from Oberlin, came to Princeton and was Bard's first graduate student. After earning his Ph.D. at Princeton, Brooks followed Bard to Harvard and ultimately to Johns Hopkins. For fourteen years Brooks collaborated extensively with Bard in variations of brain reduction experiments. The first reflexes studied in depth were placing and hopping reactions; control of this efferent system was localized by studying carefully animals with cortical ablations (1933).

In 1931 Bard found Princeton's biology department was violating promises made to him and two other assistant professors, and resigned from the Princeton faculty. Apparently conditions necessary for long-term, uninhibited research could not be met. His response was to distance himself quickly and totally from all concerned. Bard accepted right away Cannon's prompt offer of an assistant professorship at Harvard. "We are overjoyed that you are coming back," Cannon told Bard in a letter written when Harvard's approval was underway.

Harvard was happy and productive for Bard. Chandler Brooks was the first to join him there. David Rioch, a neuroanatomist, came from Johns Hopkins in 1929. Bard and Rioch studied all aspects of emotional behavior and any deficiencies in motor coordination and sensory response in four cats from which they had removed the cerebral cortex and differing amounts of forebrain. Cannon cited this report (1937) years later as a model. With Brooks's collaboration there were more studies localizing hopping and placing reactions. Brooks believes these experiments established Bard as one of the first to identify localized cortical control of a behavioral reaction (1933).

In 1933 at age thirty-four Bard was appointed professor and director of the Department of Physiology at the Johns Hopkins Medical School, succeeding William Howell. Bard thought himself too young. He had published five papers of which only three were scientific research. Bard was startled by the appointment. Those who knew him were not surprised, particularly Cannon, who saw him as a future leader. Cannon put it, "I have so much faith … in your ability to exert an important influence in the development of physiological science that I could not help wishing for you a position of great strategic advantage. That you will have at Johns Hopkins." Bard continued as chairman for twenty-eight years (five unwillingly as concomitant dean). Retiring from active teaching in 1961 at age sixty-three, he continued for twelve more years as professor emeritus. He returned to California in 1973.

In 1940 Bard identified the central nervous system structures necessary for individual components of sexual behaviors: arousal, mounting, and copulation. Even with huge parts of the brain removed each of these was preserved. As others were to confirm, each reaction required the presence of ovarian or exogenous estrogens. Dependence of a central nervous system reflex on peripherally produced hormones remains a helpful concept to neuroendocrinologists today.

Locating a hypothalamic center for sexual activity led Bard to his definition of centers: "Patterned responses of the type under consideration have shown certain ones are dependent on the functional integrity of one or another circumscribed part of the brain. The essential neural mechanism thus delineated may be spoken of as the center for that particular behavior pattern." The concept of centers is less important now than during Bard's time; his definition was simpler than many others.

Bard's science was careful, thorough, and honest. As well as any experiments later in his career, the brain-reduced animals reflect these special qualities. During World War II, at the request of the National Academy of Sciences' Committee on Aviation Medicine, Bard pursued a different direction and investigated motion sickness. Removal of part of the cerebellum cured dogs of motion sickness, as did division of a section of the vestibular nerve.

In the 1930s Wade Marshall, a biophysicist trained by Ralph Gerard in Chicago, brought a cathode ray oscilloscope to Johns Hopkins. Marshall's exceptional gifts were appreciated quickly, and Bard's laboratory immediately capitalized on the oscilloscope. They mapped cortical areas of several sensory modalities, using evoked electrical potentials. Elwood

Henneman, Clinton Woolsey (in 1933), and Vernon Mountcastle all left surgical careers to join Bard's young department. Along with Chandler Brooks each was swept up in a whirlwind of electrically recorded cerebral localization experiments. Publications of the Bard department from this era reflect their excitement. Woolsey concentrated on cerebral cortical potentials. Bard supported unequivocally the electrical recording experiments and those doing them, although he did not take direct part in most of this work. In his well known autobiographical article, "The Ontogenesis of One Physiologist" (1973), Bard mentions abstaining from electrical recording experiments. Perhaps he realized these would be done expertly by his younger colleagues, leaving him free to concentrate on other things. These are Henneman's memories of the early electrical potential experiments: "At about that time Clinton (Woolsey) was doing a series of experiments on the cerebral cortex. Mainly he was interested in localizing touch in the … cerebral cortex. And he was doing a really lovely job, showing the beautiful patterns that these representations made in the post central gyrus." Henneman and Mountcastle were inspired to study thalamic patterns of electrical potentials evoked by touch, first in the cat and then in the monkey. Henneman found his way to Harvard's Department of Physiology and was its chairman twice.

In 1948 Jerzy E. Rose and Reginald Bromiley joined Bard, and Woolsey left Hopkins to become research professor of neurophysiology at the University of Wisconsin. (In 1975 the Laboratory of Neurophysiology in Madison became the Department of Neurophysiology.) Chandler Brooks left Bard's department the same year to become professor and director of the Department of Physiology of the State University at New York at the Downstate Medical Center in Long Island. Directly before leaving Hopkins Brooks spent the years

1946–48 in the laboratory of Professor John Eccles at Otago University in Auckland, New Zealand.

In 1949 Sol Erulkar joined Bard as a graduate student. Originally from one of Calcutta's B'nel Israel families, Erulkar was educated first at Oxford and just prior to coming to Johns Hopkins completed an M.A. in biochemistry at the University of Toronto. After his Ph.D. with Bard, Erulkar earned a D. Phil. Oxon., working with Fillenz on integrative mechanisms in the lateral geniculate nucleus. Bard alerted George Koelle, chairman of pharmacology at the University of Pennsylvania, to Erulkar. Starting as an assistant professor of pharmacology Sol Erulkar was a sophisticated neuroscientist at Penn. He and his science were respected internationally and also at Penn. Until his unexpected death in 1995 Erulkar often spoke publicly of his persistent, unique devotion to Philip Bard.

In 1951 Jean Marshall joined Bard's department after finishing her Ph.D. in physiology at Rochester and a postdoctoral fellowship in smooth and cardiac muscle physiology at Oxford. Marshall went on to an investigative and teaching career in the pharmacology department at Harvard and in 1966 moved to Brown University as section chairman of microbiology when the medical program was initiated.

From 1953 to 1957 Bard was dean of the Johns Hopkins Medical School simultaneously with his chairmanship of physiology. It was often too much. Fortunately others helped him handle the Department of Physiology's affairs when it was necessary. Bard retired from his chairmanship of physiology in 1961. Made professor emeritus, he continued actively with research and teaching for twelve more years. His last investigations were with temperature regulation in cats. With Jim Woods's help the indwelling temperature probes were eventually linked into the Bell telephone system; this was the most modern equipment Philip Bard ever used.

For many good reasons David Jackson, Bard's last Ph.D. student, felt Bard was a father to him. A national merit scholar, Jackson intended to enroll in a post-high school M. D. program at Hopkins, but promised money never appeared. Graduate work with Bard was possible. The tuition costs were met, clandestinely, by Bard. Jackson's research localized central nervous system cells taking up bacterial pyrogen. They called this a fever center. Medical school followed, where expenses were met by an anonymous source known only to Bard. Jackson's career bespeaks intellectual versatility and the capacity to grow. Philip Bard understood and encouraged this, just as he had with all the others.

Bard's origins were important to him; his own family was more important. Harriet Hunt Bard and Philip were married for forty-two years. Their two children, Virginia Hunt Bard Johnson and Elizabeth Stanton Bard O'Connor, currently live in California and South Carolina, respectively. There are several grandchildren. Harriet Hunt Bard died suddenly from a massive coronary occlusion in 1964. Janet Rioch and Bard were married in 1965. Janet Rioch Bard grew up in India as had David Rioch, her brother. David was an early collaborator with Bard in Cannon's department. Janet Rioch Bard died in 1975. Colleen Gillis, widow of a close friend of Harriet and Philip and of the Bard family, was married to Philip briefly before his death.

Bard permitted himself only a few extra activities. Prominent among these was the editorship of three editions of Macleod's *Physiology*. Soon after Bard contributed a chapter to the book, Macleod died. The publishers quickly sought and were given Bard's support.

Bard's commitment was unstinting once he agreed to do something outside conventional academic work. He was a respected trustee of the Rockefeller Foundation. With Chandler Brooks he was a trustee and important influence in the International Foundation in Princeton, New Jersey. For many years he was an active board member of the Thacher School and, when his term was finished, still supported the school in many special ways. He returned to California happily and often, usually with one or more members of his family.

Bard's unwavering conviction that collaborators and students could reach farther and deeper than he could is more easily understood now than during Bard's time. He cared deeply about the quality of thinking evident in his colleagues and students. His support of them and their ideas was total and permanent.

"He was equally at home with the man who took care of the animals and visiting dignitaries. Each felt he was interested in them as a person; each was treated with equal respect as human beings. None of them achieved as much as Phil Bard did and he did it with a sense of dignity and respect for everybody … This should be the sort of common currency of our interpersonal relationships." One of Philip Bard's legacies is confidence in those who at times lacked it in themselves.

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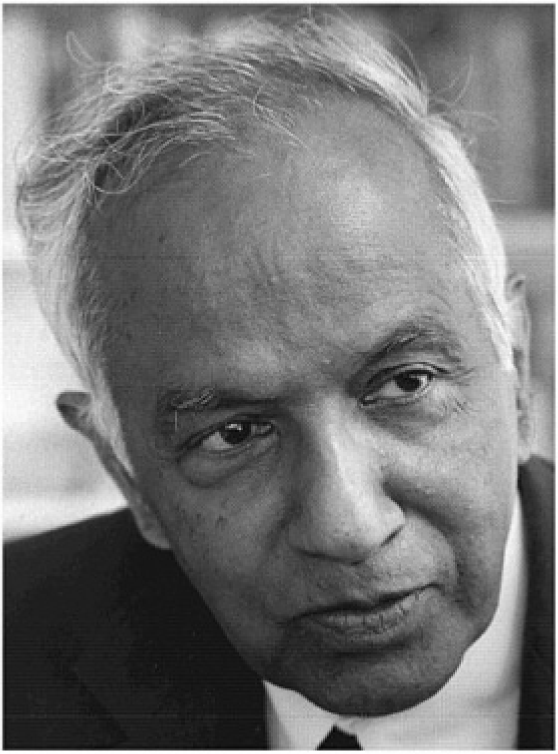

Courtesy of the University of Chicago

S. Chandrasekhar

SUBRAHMANYAN CHANDRASEKHAR

October 13, 1910–August 21, 1995

BY EUGENE N. PARKER

SUBRAHMANYAN CHANDRASEKHAR was born into a free-thinking, Tamil-speaking Brahmin family in Lahore, India. He was preceded into the world by two sisters and followed by three brothers and four sisters. His mother Sitalakshmi had only a few years of formal education, in keeping with tradition, and a measure of her intellectual strength can be appreciated from her successful translation of Ibsen and Tolstoy into Tamil. His father C. S. Ayyar was a dynamic individual who rose to the top of the Indian Civil Service. It is not without interest that his paternal uncle Sir C. V. Raman was awarded a Nobel Prize in 1930 for the discovery of the Raman effect, providing direct demonstration of quantum effects in the scattering of light from molecules.

Education began at home with Sitalakshmi giving instruction in Tamil and English, while C. S. Ayyar taught his children English and arithmetic before departing for work in the morning and upon returning in the evening. The reader is referred to the excellent biography *Chandra, A Biography of S. Chandrasekhar* (University of Chicago Press, 1991) by Prof. Kameshwar C. Wali for an account of this remarkable family and the course of the third child through his distinguished career in science. Chandra is the name by which S. Chandrasekhar is universally known throughout the scientific world. Chandra's life was guided by a dedication to science that carried him out of his native culture to the alien culture of foreign shores. The crosscurrents that he navigated successfully, if not always happily, provide a fascinating tale. He was the foremost theoretical astrophysicist of his time, to paraphrase his own accounting of Sir Arthur Eddington.

The family moved to Madras in 1918 as C. S. Ayyar rose to deputy accountant general. Chandra and his brothers had private tutors then, with Chandra going to a regular school in 1921. His second year in school introduced algebra and geometry, which so attracted him that he worked his way through the textbooks the summer before the start of school.

Chandra entered Presidency College in Madras in 1925, studying physics, mathematics, chemistry, Sanskrit, and English. He found a growing liking for physics and mathematics and an ongoing attraction for English literature. One can assume that his fascination with English literature contributed to his own lucid and impeccable writing style.

Chandra was inspired by the mathematical accomplishments of S. Ramanujan, who had gone to England and distinguished himself among the distinguished Cambridge mathematicians until his early death in 1920. Chandra aspired to take mathematics honors, whereas his father saw the Indian Civil Service as the outstanding opportunity for a bright young man. Mathematics seemed poor preparation for the Civil Service. Sitalakshmi supported Chandra with the philosophy that one does best what one really likes to do. Chandra compromised with physics honors, which placated his father in view of the outstanding success of Sir C. V. Raman.

On his own initiative Chandra read Arnold Sommerfeld's book *Atomic Structures and Spectral Lines* and attended lectures in mathematics. His physics professors noticed that he was learning physics largely through independent reading and provided him with the freedom to attend mathematics lectures. In the autumn of 1928 Sommerfeld lectured at Presidency College. Chandra made it a point to meet Sommerfeld and was taken aback to learn that the old Bohr quantum mechanics, on which Sommerfeld's book was based, was superseded by the wave mechanics of Schroedinger, Heisenberg, Dirac, Pauli, et al., and that the Pauli exclusion principle replaced Boltzmann statistics with Fermi-Dirac statistics. Sommerfeld had already applied the new theory to electrons in metals and kindly provided Chandra with galley proofs of his paper. Chandra launched into an intensive study of the new quantum mechanics and statistics and wrote his first professional research paper "The Compton scattering and the new statistics" (1929). In January 1929 he communicated this work to Prof. R. H. Fowler at Cambridge for publication in the *Proceedings of the Royal Society of London*. The name Fowler suggested itself because Fowler had applied the new statistics to collapsed stars (i. e., white dwarfs). Fowler was an open-minded and generous individual who perceived the merit of Chandra's paper, which he duly communicated to the Royal Society. This contact was to play a crucial role a year later when Chandra arrived in England.

Heisenberg lectured at Presidency College in October 1929 and Chandra had the opportunity to carry on extensive discussion with him at the time. Then Meghnad Saha at Allahabad, known for the statistical mechanics that provided the interpretation of stellar spectra, invited Chandra for discussions of Chandra's paper in the *Proceedings of the Royal Society of London*. Wali, in his biography, contrasts this early appreciation of Chandra's work by the scientific community with the class snobbery of the British Raj on the personal level.

Final examinations at Presidency College came in March 1930 and Chandra established a record score. In February Chandra was informed that a special Government of India scholarship was to be offered to him to pursue study and research in England for three years. When the scholarship was announced publicly, Chandra experienced resentment from fellow Indians who perceived him as abandoning his country and his legacy. Worse, it was becoming clear that Sitalakshmi was terminally ill and, if Chandra went to England, he would not see her again. True to form Sitalakshmi decided the issue by declaring that Chandra was born for the world and not just for her.

Chandra informed the authorities that he wished to use his government scholarship to study and carry on research with R. H. Fowler at Cambridge. The Office of the High Commissioner of India proceeded with the arrangements. Chandra departed Bombay on July 31, 1930, bound for Venice, from where he traveled by rail to London, arriving August 19. He undertook the journey in his personal pursuit of science, and that journey was culturally irreversible, a departure from home from which he never really returned.

It is well known that Chandra spent his time on shipboard working out the statistical mechanics of the degenerate electron gas in white dwarf stars, appreciating, as Fowler had not, that the upper levels of the degenerate electron gas are relativistic. Since it is the upper levels that are affected by changes in density and temperature, it follows that a density change $\Delta\rho$ and pressure change Δp are related by $\rho\Delta p/p\Delta\rho = 4/3$ rather than the nonrelativistic value 5/3 employed earlier by Fowler. The value 4/3 meant that the pressure supporting the star against gravity grows no faster than the increasing gravitational force as the star contracts, with the result that there is a limiting mass above which the internal pressure of the white dwarf cannot support the star against collapse. This is in contrast with the familiar nonrelativistic situation where the pressure increases more rapidly than the gravitational forces so that sufficient contraction must ultimately provide a sufficient pressure to block further contraction. The limiting mass was clearly of the order of the mass $M_{\odot}$ of the Sun (2×10^{33} g). A precise value would require detailed calculations of the interior structure of the star with the precise value of $\rho\Delta p/p\Delta\rho$ for intermediate levels as well as the upper fully relativistic levels at each radius in the star. But the implication was clear. A massive star, of which there are many, cannot fade out as a white dwarf once its internal energy source is exhausted. Instead it shrinks without limit, always too hot to become completely degenerate, and disappears when the gravitational field above its surface becomes so strong that light cannot escape. In modern language, the massive star eventually becomes a black hole. The reasoning was straightforward and the conclusion was startling. The repercussions that ultimately followed his discovery served to push Chandra farther into the obscure and lonely byways of science in a foreign Western society and ever more distant from his cultural origins.

Upon arrival in London Chandra discovered that the Office of the Director of Public Instruction in Madras and the High Commissioner of India in London had thoroughly bungled his admission to Cambridge. What was more, the secretary for the high commissioner's office had not the least interest in correcting the mistake and was openly rude in his assertion of that fact. Chandra was saved only by the eventual firm intervention of Fowler, who was vacationing in Ireland at the time of Chandra's arrival in London. The consequences of Chandra's first research paper were more far reaching than anyone could have imagined.

Chandra took up his studies at Cambridge and spent a lonely but productive year in intensive study and research. Sitalakshmi died on May 21, 1931, adding grief to his loneliness. Chandra was introduced to the monthly meetings of the Royal Astronomical Society and became acquainted with E. A. Milne and P. A. M. Dirac. Chandra devoted his research efforts to calculating opacities and applying his results to the construction of an improved model for the limiting mass of the degenerate star. Milne was enthusiastic about the work, but it turned out later that his enthusiasm was based more on his rivalry with A. S. Eddington than on an appreciation of the scientific merits.

The year of intensive study at Cambridge moved Chandra to look for a change of scenery, and at the invitation of Max Born he spent the summer of 1931 at Born's institute at Gottingen. There he became acquainted with Ludwig Biermann, Edward Teller, Leon Brillouin, and Werner Heisenberg. Back at Cambridge in the autumn Chandra continued his work on atomic absorption coefficients and mean opacities, but with a growing sense of frustration from his feeling that he was abandoning mathematics through his pursuit of physics and abandoning pure physics through his pursuit of astrophysics. Chandra was invited to present his results on model stellar photospheres at the January 1932 meeting of the Royal Astronomical Society (RAS) and was complimented by both Milne and Eddington following the presentation.

Chandra's feeling of frustration with his "peripheral science" led to his spending his third year at Bohr's institute in Copenhagen. He adapted readily to the informal atmosphere and became acquainted with Victor Weisskopf, Leon Rosenfeld, M. Debrueck, H. Kopferman, and others. During the time in Copenhagen Chandra succeeded in convincing himself that his real strength lay in developing and expounding the implications of the basic physical laws of nature as distinct from the pursuit of new laws of nature. He found an interested and appreciative audience in the physics community for his work on degenerate stars. Chandra was invited to the University of Liege to lecture on his work, following which he was presented with a bronze medal. The overall experience of the year was to ease his mind and set him firmly on a path in theoretical astrophysics.

Chandra finished the year with four papers on rotating self-gravitating polytropes, which became his Ph.D. thesis. His government scholarship ran out in August 1933 and the question was what to do next. It was clear that there were no opportunities in India unless he rode on the coattails of his uncle Raman, which he was loathe to do. Fortunately he won one of the highly competitive appointments as a fellow of Trinity College, which ran for four years. Milne nominated Chandra for fellow of the RAS, and the future was clear for the immediate years at Cambridge. At the monthly meetings in Burlington House Chandra and such contemporaries as William McCrea generally sat in the back row, but became acquainted with some of the denizens of the front row (e.g., Sir James Jeans, Sir Arthur Eddington, Sir Frank Dyson, and such international visitors as Henry Norris Russell and Harlow Shapley).

Chandra spent four weeks in the Soviet Union in the summer of 1934 at the invitation of B. P. Gerasimovic, meeting L. D. Landau and V. A. Ambartsumian, along with many other enthusiastic young men. Unhappily only Landau and Ambartsumian survived the massive purges that were soon to follow. Ambartsumian grasped the significance of Chandra's work on dwarf stars and suggested that it was worth working out exactly (i.e., by direct radial integration of the exact equations, using the complete pressure-density relation). This moved Chandra to tackle that immense problem upon his return to Cambridge.

The work was accomplished with the aid of a hand calculator and was completed by the end of 1934. He submitted his results for presentation at the January 1935 meeting of the RAS. Eddington had taken an interest in the work through the autumn, often dropping by Chandra's room to see how things were progressing, but never saying a word to Chandra about his own private thoughts. Eddington suggested to the secretary of the RAS that Chandra's work merited double the usual fifteen minutes for presentation and then set himself up to present a paper with the title "Relativistic degeneracy" immediately following. Eddington refused to divulge the nature of his presentation beforehand. McCrea notes in his obituary for Chandra that Eddington began by pointing out that Chandra's calculations were entirely correct based on the relativistic degenerate electron gas. Eddington then noted that the result predicted that a white dwarf with mass in excess of the critical value ($\approx 1.4\, M_{\odot}$) would continue to radiate and shrink until it disappeared. Then Eddington went on to declare that stars do not behave in that way, and Chandra's calculations showed only that the theory of relativistic degeneracy is incorrect. Later he asserted that the Pauli exclusion principle does not apply to relativistic electrons. One might have asked Eddington how he knew that stars do not behave in that way, but Eddington was so formidable and influential a person that no one did, apparently. Egos were the same then as now, and one has only to read Eddington's remarkable monograph *Fundamental Theory* (Cambridge University Press, 1944) to realize that he was coming around to the idea that he could deduce the physical nature of the universe from his own personal declarations.

The physicists, Chandra's young contemporaries (e.g., Pauli, Rosenfeld, Dirac, and others), considered Eddington's assertions to be nonsense, but Eddington moved in a different world. R. H. Fowler and H. N. Russell did not voice the essential points in opposition to Eddington's assertions, evidently intimidated by Eddington's preeminence. Russell, for instance, refused to allow Chandra to say a few words in response to Eddington's hour long exposition of his personal views at the meeting of the International Astronomical Union (IAU) in Paris in July 1935. Chandra managed a brief comment at the "International Colloquium on Astrophysics: Novae and White Dwarfs" in Paris in July 1939, but Russell quickly closed the session before a discussion could proceed.

The question of returning to India was raised by C. S. Ayyar, but Chandra found himself increasingly out of sympathy with the political nature of academia in India. Then Harlow Shapley invited Chandra to visit the Harvard Observatory. Chandra arrived in Boston on December 8, 1935. He enjoyed the friendly atmosphere but was unhappy with the informality after the tightly structured society at Cambridge. He became acquainted with Fred Whipple, Gerard Kuiper, Jerry Mulders, and others. Shapley liked Chandra's lectures so well that he nominated Chandra for election to the Harvard Society of Fellows. Then Otto Struve invited Chandra to visit the Yerkes Observatory of the University of Chicago, followed by an offer of a position as research associate for a year with the expectation that it would become a tenure track appointment in a year. The formal offer came from the office of Chancellor Robert Maynard Hutchins. By the end of the month Chandra had returned to England.

The Eddington factor had the effect of closing the doors in England, and India offered no acceptable situation. So

Chandra accepted Struve's offer, much to the disgust of his father who saw his son receding farther into the mists of foreign culture.

Since his departure from India in July 1930 Chandra had corresponded occasionally with Lalitha Doraiswamy who had been a fellow student in physics at Presidency College. She was in Bangalore in 1935 working in Raman's laboratory. They were both aware that they did not know each other very well, and Chandra had fretted over whether a marriage relationship might interfere with his pursuit of science. Chandra returned to India for a visit in August 1936 and wrote to Lalitha that he would be at Madras. She took the train to Madras to meet him and his misgivings vanished when they met after six years of geographical separation. They were married September 11, 1936.

Chandra and Lalitha spent a month in Cambridge on their way to Boston and then the Yerkes Observatory. Struve contacted the legal counsel of the University of Chicago to arrange a visa for Chandra as a missionary, for otherwise there was no quota for Indians to enter the United States. They arrived at the Yerkes Observatory on Williams Bay on Lake Geneva in Wisconsin on December 21, 1936. They stayed a few days with the Kuipers until their house was ready, and the cold Wisconsin weather was offset by the friendliness of the atmosphere at the observatory.

Lalitha recognized the importance of Chandra's single-minded pursuit of science, and she supported him at the expense of her own career. She was active in the American Association of University Women and her outgoing sociability complemented Chandra's more austere view of life so that they got on very well in their new surroundings.

The University of Chicago provided Chandra with his scientific home for the next fifty-nine years, but there were difficult moments. Chancellor Hutchins intervened on more than one occasion to smooth the way. For instance, in 1938 Struve organized a course in astronomy on the campus of the university to be taught by members of the Yerkes Observatory. However Henry G. Gale, dean of physical sciences, vetoed Chandra's participation, evidently on grounds of skin color. When the problem was referred to Hutchins he said, "By all means have Mr. Chandrasekhar teach." At that point it became clear why the original offer of a position had come from the chancellor's office rather than through the dean.

In 1946 Princeton honored Chandra by offering him the office and position vacated by the retirement of Henry Norris Russell with a salary approximately double Chandra's salary at Chicago. Chandra was inclined to accept. Hutchins matched the Princeton salary and asked Chandra to come by his office to discuss the matter. In the course of the discussion Hutchins remarked that, if conditions for Chandra's research were better at Princeton, then he would not attempt to dissuade Chandra from leaving. When Chandra responded that he did not think so, Hutchins noted that Chicago could not offer Chandra the honor of succeeding a Henry Norris Russell because Chicago had no Russell. Then he asked Chandra for the name of the person who had succeeded to Kelvin's chair at the University of Glasgow. Chandra replied that he had no idea; to which Hutchins replied, "Well, there you are." Chandra declined the Princeton offer and Hutchins remarked on more than one occasion that acquiring Chandra for the University of Chicago was one of his major accomplishments as chancellor.

The course of Chandra's research is perhaps best summarized by the monographs that he wrote as he completed each phase of his work. *An Introduction to the Study of Stellar Structure* (1939) contains his development of the theory of stellar structure, including his work on degenerate stars and the mass limit for white dwarfs, and still makes an excellent textbook on the subject. *The Principles of Stellar Dynamics* (1943) and "Stochastic problems in physics and astronomy" (1943) outline his development of the theory of the dynamics of the motions of stars in the presence of many other stars, showing the frictional drag exerted by neighboring stars and setting up the basic theory for the evolution of clusters of stars. *Radiative Transfer* (1950) contains his systematic development of the radiative flow of energy in stellar interiors and photospheres including his work on the negative hydrogen ion that dominates the opacity at the surface of a star.

In 1952 the Department of Astronomy revamped its graduate curriculum to keep up with the rapid development in the fields of atomic physics, stellar atmospheres, and stellar evolution. Chandra had been offering a repertoire of basic courses in stellar structure and radiative transfer. These courses, based in large part on his own fundamental work, provided excellent background for the theoretical students, but were heavy going for the observational students and lacked up-to-date information needed by both groups of students. Chandra was alienated by the revision and Enrico Fermi seized the opportunity to invite Chandra to become a member of the Department of Physics and the Institute for Nuclear Studies (now the Enrico Fermi Institute). Chandra accepted the invitation and henceforth confined his teaching principally to the Department of Physics, commuting from Yerkes to Chicago two days a week to teach. In 1964 Chandra moved permanently to the Chicago campus, the transition catalyzed by John Simpson's offer of a spacious corner office in the newly constructed Laboratory for Astrophysics and Space Research.

It is ironic that 1952 was also the year Chandra took up the onerous task of managing editor of the *Astrophysical*

Journal. He carried on the responsibilities in his own style, personally attending to the problems of production, refereeing, and politics within the community. The editing was managed with the help of a secretary and an editorial assistant at the University of Chicago Press. Under Chandra's leadership the journal developed into the leading international journal in astrophysics. The journal was in reality privately owned by the University of Chicago. Chandra was its heart and soul, and Chandra realized the unstable character of the situation. In 1967 he set in motion a reorganization that would transfer the primary responsibility to the American Astronomical Society (AAS), although the actual production was to continue at the University of Chicago Press. The rapid expansion of the journal from six issues a year to two large issues a month made it increasingly difficult for a single editor to handle, particularly with Chandra's establishment of the *Astrophysical Journal Letters* in 1967. So Chandra proposed that there be associate editors to assist the managing editor. To make a long story short, the new order of things was approved by the American Astronomical Society, and Chandra was able to pass on his enormous burden to the new team in 1971. It is remarkable that during his years as editor Chandra carried on his scientific research at a rate not noticeably diminished at the same time that he taught his quota of courses in the Department of Physics. It is an example of the extraordinary feats that can be accomplished through dedication and self-discipline to the exclusion of nearly everything else in one's life. His retirement from the position as editor was a great relief to Chandra. He had never intended that the burden should have continued for so long.

Chandra and Lalitha were faced with the question of U.S. citizenship, and after thinking about it for a time came to the conclusion that it was the only realistic choice. It was a big step away from their origins, but to do otherwise would have ignored the fact of their permanent commitment to a life in the United States. So in 1953 they became naturalized citizens. Lalitha's careful explanation of the evolution of their thinking did little to assuage the bitter feelings of C. S. Ayyar who saw the move only as a betrayal of their cultural origins rather than an inevitable evolution in their circumstances. Following citizenship Chandra was elected to the National Academy of Sciences in 1955.

During Chandra's early years as editor, the field of plasma physics and the confinement of ionized gas in magnetic fields in the laboratory was coming into prominence, with the hope, still unrealized today, of producing available power through the fusion of hydrogen into helium. At the same time it was being appreciated that the physics of fully ionized gases (i.e., plasmas) is the basis for the dynamical behavior of stellar interiors, atmospheres, and the interstellar gas. Plasma conditions range all the way from the tenuous, essentially collisionless gases in space to the incredibly dense plasma in the central regions of a star. Chandra was attracted by the challenge of the unknown. He expounded the existing theory of collisionless plasma in a course on the foundations of plasma physics based on the standard free-particle approach and the collisionless Boltzmann equation. S. K. Trehan put together a book *Plasma Physics* (University of Chicago Press, 1960) based on the notes from that course. In collaboration with A. N. Kaufman and K. M. Watson Chandra carried through the immense calculation of the dynamical stability of the collisionless plasma confined in an axial magnetic field. At the same time Chandra entered into an extensive study of the dynamical stability of fluids in various configurations, including the presence of magnetic fields and rotation of the entire system. His contributions are summarized in his monograph *Hydrodynamic and Hydromagnetic Stability* (1961).

From there Chandra took up the classical and unfinished problem of the dynamics of rotating, self-gravitating spheroids of homogeneous incompressible fluids. The problem had been initiated by Newton in connection with the oblateness of Earth and carried on from there by such great names as Maclaurin, Reimann, Dedekind, Jacobi, Dirichlet, et al. Chandra reopened the unfinished problems with the tensor virial equations whose great power had not been appreciated up to that time. The results if that work appear in his monograph *Ellipsoidal Figures of Equilibrium* (1969).

The work on self-gravitating objects soon brought Chandra to the doorstep of general relativity as the basic theory of gravity. His efforts in that field led to development of the Chandrasekhar-Friedman-Schultz instability, which became a source of gravitational radiation from black holes. Extensive investigation of the Kerr metric and the rotating black hole led to the monograph *The Mathematical Theory of Black Holes* (1983). Chandra also developed the post-Newtonian approximation for treating the field equations of general relativity. It is now the means for calculating the gravitational radiation from multiple star systems, etc. He went on to work out a variety of exact solutions to the equations of general relativity in collaboration with B. C. Xanthopoulos and V. Ferrari, showing some of the remarkable singularities that turn up in the interaction of gravitational waves and at the apex of the conical space solutions. One of the more curious discoveries was that the radial pulsations of a star, which are known from Newtonian gravitation to exhibit overstability in the presence of dissipation (e.g., viscosity) become unstable in general relativity through the energy loss represented by the emission of gravitational waves. Thus the star without internal dissipation is stable according to

Newtonian theory, but unstable in the context of general relativity.

As a brief aside it is interesting to note that in 1982 Chandra was invited to lecture on Sir Arthur Eddington at the celebration at Cambridge of the hundredth anniversary of his birth. The lectures are published in the small book *Eddington, the Most Distinguished Astrophysicist of His Time* (1983). The lectures emphasize the remarkable insights of Eddington into stellar structure and his early recognition of the implications of Einstein's general relativity. Chandra's reflections on Eddington's assertions on electron degeneracy and the Pauli exclusion principle are of particular interest.

By 1990 Chandra had developed a growing interest and admiration for the work of Sir Isaac Newton, and over the next several years he constructed a detailed and critical review of Newton's *Principia*. The results of this effort are published as *Newton's Principia for the Common Reader* (1995). This was the first time that a world class physicist undertook a thorough reading and critical commentary of the *Principia*, dispelling such perpetuated notions that Newton's theory of the perturbations of the orbit of the Moon is in error, or that some of his diagrams were incorrectly drawn.

Chandra's book *Truth and Beauty* (1987) shows an entirely different side of his thinking. It includes his Ryerson Lecture "Shakespeare, Newton, and Beethoven" in which he explored and compared the motivations and feelings involved in the creation of science and art.

Chandra's scientific papers are collected in seven volumes under the title *Selected Papers, S. Chandrasekhar* (1989–96). They complement the monographs listed above and provide a more detailed historical picture of the day-by-day development of his thinking.

Chandra attached great importance to training Ph.D. students. He saw them clearly as the future of astrophysics when the present generation of working scientists has passed into retirement and beyond. Struve had assigned him the responsibility for the weekly colloquium, held on Monday afternoons, and Chandra saw to it that the graduate students were in regular attendance. The Yerkes faculty, graduate students, and visitors presented their work at appropriate times, and Chandra gave each hundredth colloquium himself, as well as many in between. The count of weekly colloquia passed 500 before Chandra moved to the campus. He also conducted seminars on Monday evenings for the edification of the graduate students, who took turns reporting on interesting papers that had appeared in the literature. Chandra supervised forty-six known Ph.D. research students, many of whom have become prominent in the field of astrophysics, and not a few of whom are members of the National Academy of Sciences. Chandra was a stern taskmaster who insisted on rigorous training and research. The graduate courses in theoretical astrophysics taught at Yerkes by Chandra were the usual preparation, until the early fifties. After that most of Chandra's students came through the Department of Physics. Once a student successfully completed the Ph.D., Chandra gave his full support in getting the student established in the scientific community. In fact Chandra's support was not limited to his students alone. He appeared at critical moments in the career of this writer, as with others as well.

It is no surprise, of course, to learn that Chandra was awarded many honorary degrees and medals. He was elected a fellow of the Royal Society in 1944, which awarded him the Bruce Medal in 1952. The Royal Astronomical Society awarded him its Gold Medal in 1953. He was awarded the National Medal of Science by President Lyndon Johnson in 1967. The fundamental nature of Chandra's mass limit for degenerate stars has come to be appreciated in the astronomy and physics communities, recognizing that it is perhaps the most direct and striking example of the effect of quantum physics on macroscopic bodies. Chandra was awarded a Nobel Prize by King Carl Gustav in 1983 in recognition of his work of fifty years before. On the other hand it must be appreciated that Chandra's work on radiative transfer, stellar dynamics, dynamical stability of fluids, plasmas and self-gravitating bodies, and gravitational theory collectively represent a much larger contribution to physics and astrophysics than the more spectacular mass limit.

Chandra's death in 1995 heralded the end of the era that developed the basic physics of the star. He was the most prolific and wide ranging of those who applied hard physics to astronomical problems.

I EXPRESS MY APPRECIATION to D. E. Osterbrock for his careful reading of the manuscript and several important suggestions and corrections from his own association with Chandra over the years.

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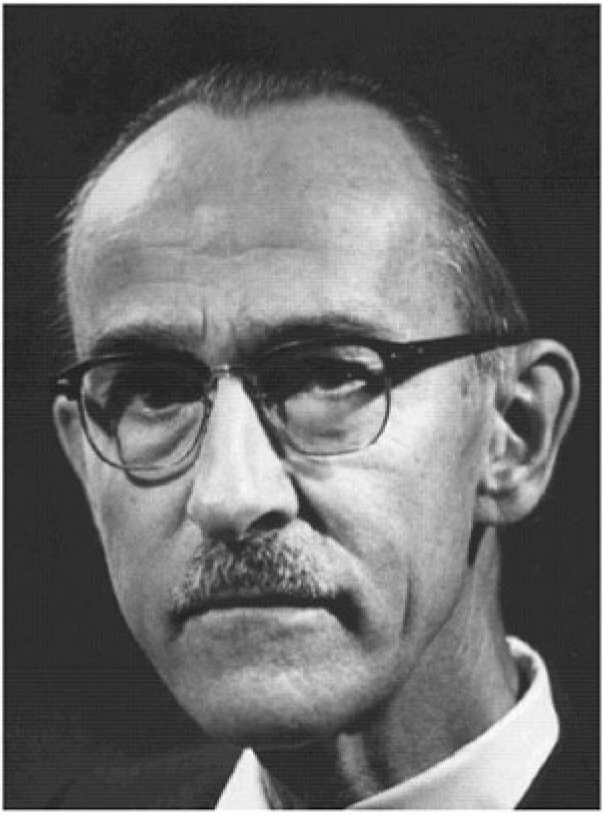

Courtesy of the California Institute of Technology Archives

ROBERT BRAINARD COREY

August 19, 1897–April 23, 1971

BY RICHARD E. MARSH

ROBERT COREY'S SCIENTIFIC career will always be identified with Linus Pauling. He worked closely with Pauling during the exciting years of the 1940s and early 1950s at the California Institute of Technology, where the basic concepts of structural biology, including the α helix and the β sheet, were being formulated. While it was Pauling who had the intuition and imagination that produced these wonderful concepts, it was Corey who was primarily responsible for proving them correct by carrying out the necessary diffraction experiments. A major product of Corey's work was the development of atomic models to study the arrangements of atoms and configurations of amino acid arrangements in proteins of all types; his name survives as the first initial in the naming of the CPK models, which are still in extensive use.[1]

PERSONAL HISTORY

Sometime in his youth—I don't know when or why—he was given the nickname "Jim"; his wife and intimate friends continued to call him Jim throughout his life. Professionally he was Bob, and that is the name I shall use.

Bob Corey was born in Springfield, Massachusetts, the first of two sons of Fred Brainard Corey and Caroline

(Heberd) Corey. Both of Bob's parents could trace their genealogies back to the mid-seventeenth century in America and much further back in England. They both graduated from Cornell University, his father in 1892 and his mother in 1893. Fred Corey was a mechanical and electrical engineer, employed for many years by General Electric in Schenectady as a developer of railway equipment. Bob's early education was at the Brown School, a private elementary school in Schenectady. When his father went to work for Union Switch and Signal Company in Pittsburgh, Bob attended high school in Edgewood, Pennsylvania. From there he went to the University of Pittsburgh, where he graduated in 1919 with a bachelor's degree in chemistry. At some period during his youth—I do not know just when—he was stricken with the scourge of the time, poliomyelitis (infantile paralysis). A partially paralyzed left arm, a pronounced limp, and a frail constitution remained with him throughout his life, and probably contributed to his being somewhat more serious and less active socially than most of his contemporaries.

Not surprisingly, Bob's choice for graduate school was Cornell. There he majored in inorganic chemistry with Professor L. M. Dennis, with minors in spectroscopy and physical chemistry. According to A. W. Laubengayer, who also worked with Professor Dennis at the time,

(Jim) and another graduate student, R. W. Moore (Slippy), were collaborating and constructing what undoubtedly was the first all-glass vacuum line, patterned after that initiated by Stock in Germany, in this country. Considering that only 'soft' glass was then available and interchangeable slip joints and stopcocks were unknown, and diffusion pumps had not yet been invented, this project was indeed heroic. Only one with the determination and ingeniousness of Jim would have mastered it. His partner, Slippy, was a confirmed worry-wart and pessimist, despairing each day, and Jim had to rally Slippy to the cause. They finally succeeded in synthesizing and characterizing GeH_4 and Ge_2H_6 and got a less volatile fraction containing higher hydrides. Their work established that germanium resembles silicon closely in its ability to form an homologous series of hybrid.[2]

After receiving his Ph.D. in 1924, Bob remained at Cornell as an instructor in analytical chemistry. While there he became fascinated with one of the first GE X-ray spectrometers, which had been used a few years earlier by Ralph W. G. Wyckoff. He succeeded in rehabilitating the instrument; more important, he became interested in the technique of X-ray diffraction and decided to join Wyckoff, who was then at the Rockefeller Institute. In 1928 he moved to Rockefeller as an assistant in biophysics and was promoted to associate in 1930—an eventful year, for it was then that he married Dorothy Gertrude Paddon. Although they had no children, their marriage was a joyous success and lasted until his death.

At the time, Wyckoff had become convinced that "x-ray methods were by then sufficiently developed to permit an attack on organic crystals more vigorous than had previously been feasible, and our work had this direction … The ultimate objective was the examination of crystalline proteins but it seemed advisable first to establish the structures of a number of simple compounds possessing the C-C and C-N bonds that are the backbone of protein molecules."[3] During the approximately ten years that Bob Corey was at the Rockefeller, he and Wyckoff were joint authors of eighteen papers describing diffraction studies on compounds ranging from organic chlorostannates to crystalline and fibrous proteins. In 1937 Wyckoff's laboratory at the Rockefeller Institute was dissolved, and Wyckoff moved to the National Institutes of Health in Washington. As a something-less-than-golden parachute, Corey was given a one-year fellowship "to be used in any institution where I could profitably continue my crystal structure studies." He was also offered "an ample allowance for laboratory expenses and the use of much of the equipment with which I have been working in case it would be of assistance to me." Corey immediately wrote to Linus Pauling at Caltech, who was beginning to apply his great knowledge of structural chemistry to the study of biologic systems. Pauling replied by return mail, "I would be very glad indeed to have you spend the year in Pasadena," offering an appointment as research fellow without stipend, but with the caveat that "so far as I can tell, there would be no possibility for you to be added to the staff at the end of the year." Pauling's alacrity to accept the visitor may well have been influenced by the lure of the equipment that Corey might bring with him, for he added, "Apparatus which we do not have and which you might well need for your work would include a Weissenberg camera, a simple spectrometer for the rapid measurement of intensities, special apparatus for taking powder photographs, etc. I would recommend that you bring with you apparatus of this type which you think is needed for your own work." Corey accepted the appointment, also by return mail, on May 8, 1937. He and his equipment arrived in Pasadena in September (that Weissenberg camera remained in service at Caltech for approximately forty years). Despite Pauling's caveat, Bob had no apparent problem in securing his future. He was advanced to senior research fellow in chemistry in 1938, research associate in 1946, and professor of structural chemistry in 1949. He became emeritus in 1968. By then his health was worsening, and his appearances at Caltech were rare. He died in the spring of 1971 of atherosclerosis complicated by hypoglycemia.

Bob was a private person. He seemed to dislike social events of all kinds, preferring to be at home with Dorothy listening to Gilbert and Sullivan or perhaps tending to his lawn. Here he was the direct opposite of Pauling, who enjoyed the limelight and relished both adulation and confrontation. Perhaps this difference in personality is what made the Pauling-Corey duo so effective in advancing—indeed, in establishing—the field of molecular biology. Pauling would give lectures so charming and entertaining that the audience might get a whiff of snake oil; but then a definitive paper would appear, carefully written and with strong supporting evidence supplied by Corey.

Care and attention to details were the essence of Bob Corey. Among the vivid memories I have of my experiences with him was the preparation of our paper on the structure of silk fibroin (1955). After we (or, rather, Bob) had decided on the general layout of the paper, it became my task to prepare each day a single paragraph of material. I would present this paragraph to him at 9:15 in the morning, when he would—in my presence—dissect and usually destroy it, substituting his own words that would say clearly just what I had meant to say all along. It was always my hope that this confrontation would end by 10:00 a.m., for that was the standard coffee hour for the half-dozen or so members of his biological structure group. If we were not finished, I would summarily leave his office. I am sure that my summary exit pained him greatly, but he never complained, nor did he offer to join us; but I can guarantee that every word, every punctuation mark, every nuance of that paper was the result of careful consideration. To the extent that I have any appreciation of the sound and impact of the written word, I owe that appreciation to Bob Corey.

Corey's relationship with Pauling, though scientifically close, was not socially intimate; as far as I am aware, each referred to the other's wife as "Mrs. Pauling" or "Mrs. Corey." He followed similar patterns with, I believe, all of his scientific associates; while he was deeply caring of their welfare and progress, he was not comfortable in social situations. Surely this reluctance to participate in the casual Pasadena life-style was due in part to his traditional childhood; but I believe it was primarily a result of his frail health. It was physically uncomfortable for him to stand for long periods of time at a cocktail party; it was mentally uncomfortable for him to come up with small talk. Nevertheless, as his early collaborator and lifelong friend Ralph Wyckoff wrote, "Corey was truly remarkable for the spirit he maintained and the amount he accomplished in spite of lifelong physical handicaps."[3]

Corey was awarded an honorary doctor of science degree by his alma mater, the University of Pittsburgh, in 1964; he was elected to the National Academy of Sciences in 1970.

PROFESSIONAL HISTORY

Bob Corey's earliest publications, resulting from his work in graduate school, described the isolation and identification of numerous hydrides of germanium. They gave clear indications of the care, thought, and attention to details that were the features of Bob Corey's entire scientific career. They also indicated his fondness for designing and building equipment, the importance he placed on the careful use of that equipment, and the satisfaction he found in definitive results. Among other things, these early papers described, in clear words and with careful drawings, the construction of the vacuum line that he had used to generate and separate GeH_4, Ge_2H_6, and higher hydrides. The vacuum line contained, along with a dozen or so collection tubes, a mercury manometer and ten mercury valves—Y-shaped tubes with mercury reservoirs at the bottom; by admitting (through a stopcock) the outside atmosphere to the reservoir the mercury level could be forced up into the

Y, sealing off the two upper arms. This sort of apparatus was used extensively by Alvin Stock in Germany, and Stock in his later years apparently suffered mental damage from exposure to mercury vapor. There is certainly no indication of any similar damage to Bob Corey's intellect.

Although Bob's initial faculty appointment at Cornell was in analytical chemistry, he quickly became attracted to the field of X-ray diffraction. It is not difficult to see the reason for the attraction, since X-ray diffraction required very careful experimentation (in those days) and offered as a reward the possibility of definitive and unassailable results; the number of measurements available in a crystal-structure analysis of a normal compound greatly exceeded the number of parameters necessary to describe the structure. Moreover, Bob surely realized—far earlier than most—that this relatively new technique might play an important role in uncovering some of the mysteries of biologic molecules. But he could not have known how overwhelmingly important that role would be or that his own participation would have such tremendous influence.

Having been introduced to X-ray diffraction by equipment left at Cornell by Ralph Wyckoff, Corey decided to join Wyckoff at the Rockefeller Institute in order to learn more about the technique. It was here that he carried out the first of his many small-molecule crystal structure analyses. It was also here that he and Wyckoff made some preliminary studies of the possibility of investigating very large molecules (proteins) using X-ray diffraction. (Similar studies were being undertaken by Bragg and others.) Accordingly, when Wyckoff's support at the Rockefeller Institute was discontinued, Bob was quick to apply to Linus Pauling for a position at Caltech (and even quicker to accept a one-year appointment), for Pauling was also interested in applying the concepts of structural chemistry to the study of biological molecules. So the year 1937 was to mark the beginning of a Pauling-Corey collaboration that lasted until Pauling left Caltech in 1964.

Corey's pre-war work at Caltech was on determining the crystal structures of three small biologic molecules—glycine, d,1-alanine, and diketopiperazine (the cyclic anhydride of the dipeptide glycylglycine). These were among the earliest organic molecules to have their complete, three-dimensional structures elucidated; glycine and alanine were the first amino acids, and diketopiperazine the first peptide. The measurements and especially the calculations of the many (approximately 300 in the case of d,1-alanine) diffraction intensities necessary for these studies was a prodigious undertaking, for the only computing aids he had were a slide rule and a mechanical adding machine. The structures of glycine and alanine were derived from Patterson functions, which had been introduced by A. L. Patterson in 1935; that of diketopiperazine was deduced from packing considerations. For diketopiperazine the first model that Corey tested was based on a puckered six-membered ring, as in cyclohexane; only when that model failed was a planar ring tested and found to produce satisfactory agreement between measured and calculated diffraction intensities. While Pauling and perhaps others may already have suspected that the amide grouping in peptides would be planar (because of the double-bond character in the C-N bond), this structure was the first demonstration.[4]

During World War II Pauling became the head of a gunpowder project and was charged with investigating the stabilities and explosive characteristics of various forms of gunpowder. This project involved extensive administrative interactions with the War Department—reports, requisitions, and the like. Pauling apparently realized that Bob Corey had the necessary mental and emotional discipline to cope with these details, and Bob became the administrative coordinator of the project. It was a full-time job, and it was not until the war was over that he returned to scientific research.

Before the war Pauling's interest in structural chemistry had been thoroughly eclectic, embracing thermodynamics, quantum mechanics, gas-phase electron diffraction, and crystal structure studies of all kinds—minerals, intermetallic compounds, and organic and inorganic molecules. After the war he concentrated more heavily on biologic systems and assimilated at Caltech a large number of students and postdoctoral people in a number of areas. For the structural part of the program—obviously his favorite—he put Corey in charge, recognizing not only Bob's extensive background in the field but also his skills as an administrator and a facilitator. Bob's first projects were to assemble all the available knowledge on the detailed structures of amino acids and peptides (most of this was based on his own pre-war work) and to plan and oversee further studies in the area. By 1955 the crystal structures of six amino acids and of three dipeptides had been published by various workers at Caltech. No such studies had yet been carried out anywhere else in the world.

In the late 1940s Pauling had come up with the concept that polypeptide chains in proteins—particularly fibrous proteins, such as hair, muscle, and tendon, which gave relatively good diffraction patterns suggesting extended chains—might form regular helical structures but with a non-integral number of amino-acid residues in each turn of the helix. This was a novel concept, since diffraction from crystalline materials had always indicated discrete unit cells, which would require an integral number of residues per turn. With the help of the structural concepts that had arisen from the crystallographic work of Corey and others as Caltech (planar peptide groups with known interatomic distances, attached to one another through N-H···O hydrogen bonds) Pauling succeeded in constructing a number of helical models, and he and Corey demonstrated that one of these models—the α-helix—was compatible with the diffraction patterns observed for the synthetic polypeptides poly-γ-methyl-L-glutamate and poly-γ-phenyl-L-glutamate. There resulted the watershed group of papers by Pauling and Corey, published in the *Proceedings of the National Academy of Sciences*, describing the α-helix, a second less compact helix (which has not yet been observed in fibrous proteins), and two extended β-sheet structures, one with parallel and the other with antiparallel arrangements of adjacent polypeptide chains. What was perhaps most remarkable about these papers is that they included coordinates for the atoms of the peptide groups, so that the structures could be accurately reproduced in other laboratories and also so that diffraction intensities could be calculated for comparison with observed patterns. From such calculations the α-helix and the β-sheets were soon shown to be major constituents of many fibrous and globular proteins. More important, the realization had arrived that large biological molecules could be discussed and eventually understood in terms of the exact arrangements of their constituent atoms. The age of molecular biology had arrived.

It is surely worth noting in passing that these seminal papers by Pauling and Corey were written in the most conservative of styles and backed up by extensive evidence and calculations. One finds such phrases as, "We think it is likely that ….," "It is our opinion that ….," "We conclude that there is strong evidence for ….," and the like. That such ground-breaking work would be described so modestly is clear evidence of Bob Corey's hand.

As Corey and others in his group at Caltech were working on the structures of small molecules such as amino acids and dipeptides, they were making extensive use of molecular models; these models were normally constructed from "Tinker Toys," wooden balls and sticks representing atoms and bonds. But they were of limited use; the sizes of the balls did not correspond to the actual sizes of the atoms, and it was difficult to keep the wooden bonds from twisting so as to create perhaps a nonplanar amide group. The solution to this problem was to construct space-filling models that could incorporate the known structural features: bond lengths and angles, conformations (especially the planar arrangement of the peptide linkage), hydrogen-bond formation, and van der Waals radii. The first such models, designed by Corey in about 1946 and built in Caltech's instrument shop, featured individual wooden atoms with carefully machined surfaces to represent covalent and van der Waals radii; these atoms could be glued together to form planar groups or machined with metal inserts and links where bond rotation was allowed. But these model atoms were very large—the scale was 1.0 inch/Å—and too heavy to be assembled into lengthy polypeptide chains. Subsequently, smaller versions were molded from plastics of various types with the C, N, and O atoms of the peptide grouping cast as a single planar unit. Eventually, hydrogen bonds were simulated by imbedding magnets in the hydrogen and oxygen atoms. These models were vitally important to Corey and Pauling during the early 1950s, when they were testing (successfully, usually) their helices and pleated sheet structures on all sorts of proteins; they were the prototype of the CPK space-filling models, which have served the last generation of structural biologists so well.

During the years 1950–55 the Pauling-Corey group at Caltech studied the structures of a large number of fibrous proteins: hair, silk, collagen, wool, feather rachis, and others; in most cases they were able to show that the structures they proposed were entirely compatible with X-ray diffraction, infrared dichroism, and other measurements. Along the way they proposed a structure for the nucleic acids. In deriving this structure they assumed a density of 1.62 g cm^{-3}, from which they deduced that the structure should be based on a triple-strand helix. They constructed many models, but could find no satisfactory one in which the purine-pyrimidine groups were at the center of the triple helix. So the model they eventually proposed had the phosphate groups at the center, the three chains attached to one another through O-H···O hydrogen bonds. They were not entirely satisfied with this structure, which they called only promising, and added that "the structure cannot be considered to have been proved to be correct." They were, of course, justified in their doubts; the structure derived almost simultaneously by Watson and Crick was based on a double helix with the phosphate groups on the outside.

During this time Pauling had become increasingly involved in world peace and antinuclear activities; while he maintained keen interest in all areas of structural chemistry and biology and continued to give fascinating lectures on the α-helix and other scientific topics, Corey became the de facto head of the structure program. In addition, Bob found himself in demand as a lecturer—a chore he surely disliked, because of his shy and unassuming personality. In 1955 he even went on tour, giving lectures on "The configuration of polypeptide chains in proteins" throughout the world; he returned exhausted but elated at the warm reception he had received everywhere.

In the late 1950s, with the structural features of fibrous proteins firmly in hand, Corey focused his research in two related areas: more intensive studies on crystalline proteins—lysozyme in particular—and crystal structure studies of nucleosides and nucleotides in an attempt to confirm the base-pairing scheme proposed by Watson and Crick. An important result of this latter project was the discovery by Karst Hoogsteen of the reversed pairing—the Hoogsteen pairing—of adenine and thymine.[5] For the lysozyme project he assembled a large (for the time) research group to prepare crystals of the tetragonal form of the native protein and also crystals in which the novel heavy-atom complexes Ta_6Cl_{14} and Nb_6Cl_{14} were incorporated.[6] Over the next several years a tremendous amount of intensity data was collected from these three types of crystals, and eventually a three-dimensional electron density map was obtained; before it could be interpreted, however, the structure of tetragonal lysozyme was reported by another group. [7] By now Bob's health was failing and he was facing retirement; his research group was disbanded and the lysozyme project was terminated.

The failure of the lysozyme project was a tremendous disappointment to Bob. Throughout his career he had envisioned as an ultimate goal the determination of the complete three-dimensional structure of a crystalline protein and he watched with envy the success of the British groups working on myoglobin and haemoglobin. Possibly the attack on lysozyme came too late in his career, when much of his ebbing strength was needed for raising support money and handling personnel problems; by then he was spending little time in the laboratories. Perhaps, too, the efforts were hampered by his need for perfection. The unit-cell dimensions found for crystals of the niobium and tantalum derivatives of lysozyme were always slightly larger than those for the native protein, and Bob feared that the lack of true isomorphism would make the resultant electron density maps unreliable. It is quite possible that, if he had damned these torpedoes—as other protein crystallographers have now learned to do routinely—and proceeded with confidence and vigor, he would have succeeded in this final project.

Bob Corey's place in scientific history is clear; he was a central figure in the birth of the field of molecular biology. Linus Pauling has often been called the father of this field, but Bob's role was crucial. As Pauling's close associate he carried out many of the key experiments needed to confirm Pauling's theories; and he carried out these experiments with such care and thought that the results could not be doubted. He preferred to remain out of the lime-light, but his presence could always be felt in the precision of the way in which Pauling's ideas were formulated and in the care with which they were presented. The molecular models that he designed are a tangible legacy; his concept of scientific progress—careful experimentation with loving attention to detail—is a less tangible but not less important legacy. He was, as Pauling said, "a good man, a sincere man, a man with a deep interest in the physical and biological world, a man who found happiness in scientific research."

NOTES

This biographical memoir was originally commissioned to Linus Pauling and E. W. Hughes, but was not completed. I am indebted to Ruth Hughes, Eddie's widow, for material he had collected; to the Caltech archives for letters, references, and the photograph of Corey; and to Ramesh Krishnamurthy, project director for the Ava Helen and Linus Pauling papers at Oregon State University, for early correspondence between Corey and Pauling. I am also indebted to Verner Schomaker for many helpful comments, ideas, and remembrances.

1. In naming these CPK models Corey's initial obviously came first. The "P" is for Pauling and the "K" for Walter Koltun, who oversaw the design and construction of the models.

2. Letter from A. W. Laubengayer to E. W. Hughes, Mar. 4, 1975.

3. Letter from R. W. G. Wyckoff to E. W. Hughes, undated, probably 1975.

4. Corey's description of the structure of diketopiperazine was published in 1938, and Pauling refers to it several times in the second edition of *The Nature of the Chemical Bond*, published in 1948. However, Pauling's book also contains the curious statement, "There exist no data regarding the configuration and dimensions of the amide group." Probably Pauling believed that the constraints imposed by the cyclic nature of diketopiperazine ruled out its consideration as a legitimate amide.

5. K. Hoogsteen. The structure of crystals containing a hydrogen-bonded complex of 1-methylthymine and 9-methyladenine. *Acta Crystallogr.* 12(1959):822–23. Although Corey initiated and supervised this work, it was his policy to include his name as co-author only if he had taken active part in the experimentation.

6. These complexes had originally been synthesized by Herbert Harned in 1913. *J. Am. Chem. Soc.* 35:1078. In 1956, over forty years later, Corey asked Harned to spend a few months at Caltech to reproduce the syntheses. Harned accepted the invitation and was the source of most of the material.

7. D. C. Philips. The hen egg-white lysozyme molecule. *Proc. Natl. Acad. Sci. U. S. A.* 57(1967):484–95.

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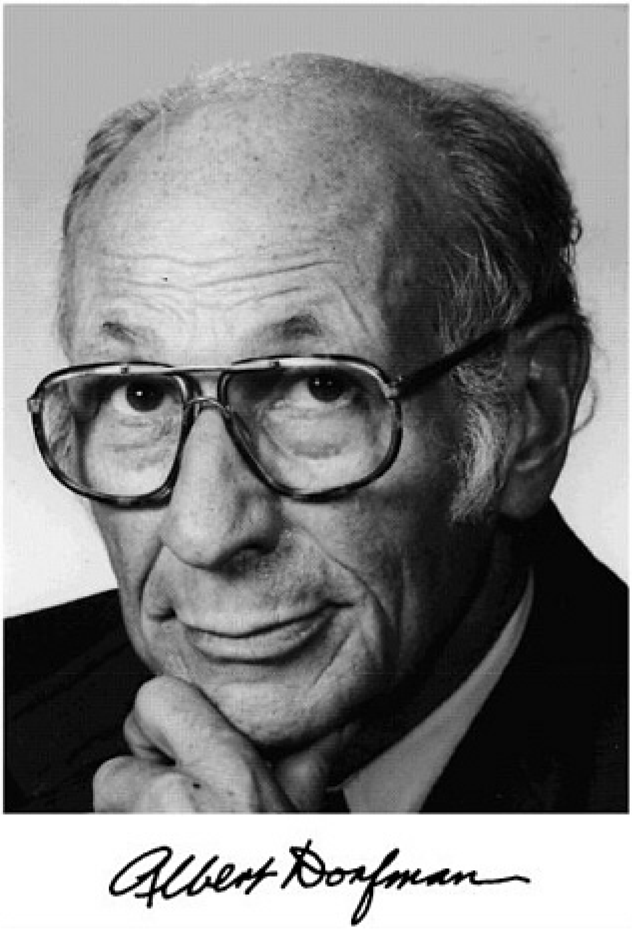
Albert Dorfman

ALBERT DORFMAN

July 6, 1916–July 27, 1982

BY NANCY B. SCHWARTZ AND LENNART RODÉN

ALBERT DORFMAN'S RESEARCH for more than thirty-five years on the biosynthesis and chemistry of bacterial and connective tissue polysaccharides provided the basis for many medical advances in human biochemical genetics, as well as in prenatal diagnosis of genetic diseases that cause mental retardation. One of his many scientific accomplishments was discovering the cause of Hurler's syndrome, a genetic disease that affects the bones and cartilage and results in mental retardation.

Albert Dorfman was born and raised in Chicago, the third child of Russian Jewish immigrant parents. His father was manager of a metalware factory and his mother was a seamstress. Although his parents had received no formal education, they placed great emphasis on scholarship and instilled a love for learning in their children. Al's older sister was a pre-law and accounting student and an accomplished singer, which fostered a lifelong interest in music in her younger brother. His older brother Ralph I. Dorfman, who was also a member of the Academy, became interested in mathematics and science early in life and had a great influence on Al by emphasizing high academic achievement and kindling an interest in chemistry. His younger sister, Florence

Jacobson, became a mathematician and married Professor Nathan Jacobson, also a member of the Academy. In 1940 Al married Ethel Steinman, and they had two daughters, Abby and Julie.

Early schooling was a pleasant experience that Al pursued with an all consuming energy. While Al was in high school, his interest in science gradually matured as a result of the stimulation received from his brother, who was then majoring in chemistry at the University of Illinois. Since his high school years were during the depths of the Great Depression, higher education seemed impossible. This was all changed by a high school teacher who convinced Al to take the competitive scholarship examinations at the University of Chicago. Success in obtaining a scholarship opened a vista of higher education. When Al arrived at the University of Chicago, with the limited background of public schooling, the new college system was just getting underway. The intellectual stimulation was intoxicating, and during the first year, almost weekly, he was ready to change his career to such diverse fields as sociology, economics, and history. However, stimulation in the sciences was greatest largely as a result of the teaching by many great scientists in the introductory courses at the college.

Gradually, it became clear that Dorfman wished to pursue a career in medicine or chemistry; the conflict between the two was never to be completely resolved. Accordingly, after first pursuing a curriculum in chemistry, he switched to biochemistry and also entered the University of Chicago School of Medicine during his senior year in college. However, he found biochemistry so interesting that he began graduate work in this discipline and dropped out of medical school after two years. Early in his graduate work he came in contact with Felix Saunders, who was then interested in bacterial metabolism, a field yet in its infancy. This talented but unrecognized scientist played a most important role in Dorfman's scientific and personal development. Saunders foresaw the great advantages microorganisms offered for the study of metabolism and was also an accomplished carbohydrate chemist. This early training was to be of great advantage when Dorfman subsequently became interested in carbohydrate-containing macromolecules. His Ph.D. thesis research was concerned with the identification of nicotinamide as a growth requirement for *Shigella dysenteriae* and the synthesis of various nicotinic acid derivatives to correlate structure with biological activity (1939). A microbiological method for nicotinamide was also devised based on these studies (1940).

After receiving the Ph.D. degree from the University of Chicago in 1939, Dorfman tried in vain to obtain a position or a postdoctoral fellowship to study elsewhere. During this period, he became intrigued with the beginning expansion of enzymology, resulting particularly from the studies of Warburg and von Euler. With stimulation and help from R. W. Gerard and E. Guzman-Barron, he learned some of the early techniques of enzymology and remained at the University of Chicago as a research associate, initiating studies on the role of bacterial growth factors in metabolism (by then known to be vitamins). These studies led to development of the technique of growing deficient cells to be used to determine the role of growth factors in metabolism. This short period was extremely productive and led to the discovery of the role of pantothenic acid in pyruvate metabolism (1942), the role of biotin in aspartic acid biosynthesis (1942), and one of the earliest suggestions that drugs may be competitive inhibitors in enzyme reactions (1942).

With the advent of World War II and lack of an academic position, Dorfman returned to medical school, graduating in 1944. Contrary to his expectations, exposure to clinical studies immediately rekindled an interest in medicine, in particular pediatrics. An early encounter with a child with rheumatic fever stimulated an interest in the mechanism of action of aspirin and profoundly affected Dorfman's subsequent career. Following completion of medical school, an internship at Beth Israel Hospital in internal medicine, and a residency in pediatrics at the University of Chicago, Dorfman served two years in the U.S. Army. He was assigned to the Army Medical School and was able to take up a career in biochemistry. Because of a publication at this time by Guerra claiming that aspirin exerted its antirheumatic effect by inhibition of hyaluronidase, he initiated studies on connective tissue polysaccharides, an area of research which he pursued for the next thirty years. In particular, his earlier experiences in bacterial metabolism and carbohydrate chemistry served as an excellent background to pursue the biosynthesis of hyaluronic acid in Group A streptococci. These studies led to the development of quantitative methods for assays of hyaluronidase (1948), discovery that chondroitin sulfate was a substrate for testicular hyaluronidase (1951), and recognition that hyaluronidase was unusually stable to heat and acid pH (1954), special properties that were later recognized as those of lysosomal enzymes.

Upon returning to the University of Chicago, Dorfman initiated studies on the synthesis of hyaluronic acid with the goal of determining the origins of the fourteen unique carbon atoms of the polysaccharide, using specifically labeled precursors. At that time the biosynthetic reactions leading to formation of hexosamines and glucuronic acid were unknown. Together with Saul Roseman, [1-^{14}C]-glucose, [6-^{14}C]-glucose, and [1-^{14}C]-acetic acid were synthesized. It was then established that glucose was converted to the glucosamine and glucuronic acid portions of the molecule without scission of the carbon chain, that acetate was a precursor of the acetyl group of N-acetylglucosamine, and that glucosamine but not N-acetylglucosamine served as a precursor of the N-acetylglucosamine residue in hyaluronic acid. Neither glucuronic acid nor glucuronolactone was found to be direct precursors of the glucuronic acid residues in hyaluronic acid (1953,1954,1955). Besides Dorfman and Roseman, the participants in these investigations included Julio Ludowieg and his wife Frances Moses, whose grandmother on her father's side, Anna Mary Robertson Moses, is better known as Grandma Moses (1860–1961).

In parallel with this work similar studies were carried out on mammalian polysaccharides—hyaluronic acid and dermatan sulfate of rat and rabbit skin—with the added dimension that the turnover rates of these polysaccharides in vivo were also determined. In these studies, now part of the classical accomplishments in the field, it was established that the two polysaccharides have a surprisingly rapid turnover with half-lives of only a few days. Following the same pattern of experimentation, Sara Schiller and Dorfman subsequently carried out a number of studies on the effects of various hormones on the metabolism of the glycosaminoglycans.

The discovery of uridine nucleotide sugars by Luis Leloir suggested that these compounds may be intermediates in polysaccharide synthesis. The identification of certain uridine nucleotide sugars and the appreciation of their role in monosaccharide interconversions and as glycosyl donors then occurred in rapid succession. Together with J. A. Cifonelli, Dorfman established that streptococci contained the two uridine nucleotide sugars, UDP-N-acetylglucosamine and UDP-glucuronic acid, requisite for the biosynthesis of hyaluronic acid (1957). The chance observation of large amounts of UDP-glucuronic acid in one batch of streptococci made it possible to prepare substrate amounts of labeled nucleotide sugar by the Wilzbach procedure. With the labeled nucleotide, synthesis of hyaluronic acid in a cell-free preparation of streptococci was then quickly demonstrated together with Alvin Markovitz and J. A. Cifonelli (1959). This work followed earlier studies by Glaser and Brown, who had obtained evidence for the formation of small hyaluronic acid oligosaccharides in a cell-free preparation of the Rous sarcoma, but the investigation by Dorfman and coworkers represented the first conclusive demonstration of the formation of macromolecular hyaluronic acid. Together, these investigations were the first to show the cell-free synthesis of a heterologous polysaccharide and established a basis for understanding the mechanism of synthesis of many other complex carbohydrates. In a farsighted prediction it was proposed that a single bifunctional enzyme catalyzes both glucuronyl and N-acetyl-glucosaminyl transfer, which we now know to be true. Attempts to solubilize the enzyme failed, but led to the important conclusion that the enzyme responsible for glycosyl transfer—in contrast to those required for nucleotide synthesis—was localized on the protoplast membrane (1962). This was one of the first observations relating macromolecular synthesis to membrane-associated enzymes. More than twenty years later Nancy B. Schwartz and Louis Philipson localized the mammalian hyaluronic acid synthetase to the inner side of the plasma membrane and proposed a mechanism for membrane bound synthesis of the HA polymer.

Dorfman also contributed significantly to the understanding of the biosynthesis of other glycosaminoglycans. The biosynthesis of sulfated glycosaminoglycans by eucaryotic cells required the study of additional reactions not necessary for hyaluronic acid formation because of the sulfate content and covalent linkage to protein. Together with Frank

K. Thorp, Robert L. Perlman, Alvin Telser, and H. C. Robinson, cell-free preparations of embryonic chick cartilage were developed that synthesized chondroitin sulfate, and glycosyl transfer from UDP-N-acetylgalactosamine and UDP-glucuronic acid to small acceptor oligosaccharides was demonstrated (1964,1966).

In addition to utility for subsequent enzyme purification, Dorfman's studies offered more conclusive evidence concerning the mechanism by which genetic information is translated to a specific monosaccharide sequence in carbohydrate-containing macromolecules. The individual glycosyltransferases were found to be specific for the transferred glycosyl group as well as for the nonreducing terminus of the acceptor molecule. Variation of structure of the penultimate monosaccharide of the acceptor did not abolish enzyme activity. The structural regularity of complex polysaccharides is accordingly determined by the specificity of the glycosyltransferases, which in turn are specified by the appropriate structural genes.

After the cell-free synthesis of the repeating disaccharide portion of the chondroitin sulfate chains had been accomplished, Dorfman turned his attention to the carbohydrate-protein linkage region of the chondroitin sulfate proteoglycans, the structure of which had been determined by Lindahl and Rodén in Dorfman's laboratory. The presence of a galactosylgalactosylxylosyl group, linked to serine hydroxyls in the core protein of the proteoglycan, suggested the participation of specific glycosyltransferases in the biosynthetic process. Initiation of biosynthetic studies on the linkage region was greatly aided when David S. Feingold and his coworkers synthesized UDP-[^{14}C] xylose. Robinson and Telser showed the transfer of xylose and galactose from the respective UDP-sugars to endogenous acceptors in the particulate preparations obtained from embryonic chick cartilage (1966). The formation of a xylosylserine linkage was established. These findings opened the way for more detailed studies of the biosynthesis of the linkage region, which were subsequently pursued by Torsten Helting and Lennart Rodén in Dorfman's laboratory. Purification of the individual enzymes was initiated by Allen L. Horwitz and Allen C. Stoolmiller (1972), with ultimate purification of the xylosyltransferase, as well as solubilization and partial purification of the rest of the glycosyltransferases accomplished by Nancy B. Schwartz and Lennart Rodén.

Recognizing the need of more knowledge about fundamental aspects of the chemistry of the glycosaminoglycans, Dorfman devoted a substantial part of his research program to studies of the structure of these polysaccharides. Not only did the pursuit of such investigations lead to important new basic knowledge, but on more than one occasion was there a stimulating, synergistic interaction between the structural chemists and other members of the team, which enhanced the productivity of the group as a whole and raised the quality of the individual contributions from the laboratory. An early example of the synergism in the Dorfman laboratory was the merger of the pioneering investigations of Martin B. Mathews on cartilage proteoglycans—the native form of the chondroitin sulfates in the tissues—with Dorfman's research on metabolism, resulting in early studies of the turnover of the protein and carbohydrate moieties of these complex carbohydrates.

While a medical student in the 1940s, Dorfman encountered a patient with the Hurler syndrome and, upon surveying the literature, found inconsistencies in the earlier data which had indicated that this condition was a lipid storage disease. Interest in Hurler's syndrome was rekindled by the publication of an intriguing abstract by Brante in 1952, which reported the isolation of chondroitin sulfate from the urine of patients with this disease. However, no tissues from Hurler patients were available for study at that time. Several years later, a former resident, Eugene Diamond, provided a urine sample from a patient with Hurler's syndrome, and in a few hours Dorfman established that the concentration of mucopolysaccharides (glycosaminoglycans) was much higher than in a control sample from one of his daughters, who was of the same age as the patient. In collaboration with A. E. Lorincz this work was quickly extended to other patients, and more detailed analyses showed that the elevation was due to an increase in both dermatan sulfate and heparan sulfate (1957). The selective increase in the excretion of these two polysaccharides, as compared to chondroitin 4- and 6-sulfate, for example, could not be rationalized at the time, especially since dermatan sulfate and heparan sulfate were considered to be distinct entities without any common structural features. These studies, however, did lead eventually to the identification and delineation of a number of genetically distinct types of mucopolysaccharidoses.

After the discovery of glycosaminoglycans in the urine of Hurler patients, many years passed before the nature of the defect in Hurler's syndrome was finally elucidated by the study of cultured fibroblasts from afflicted individuals. The tissue culture technique had been used previously for the investigation of other diseases and was now applied to the study of the mucopolysaccharidoses by Reuben Matalon in Dorfman's laboratory. Danes and Bearn had demonstrated metachromasia in Hurler fibroblasts, and Matalon and Dorfman independently made the same observation and by quantitative analyses showed that the fibroblasts contained elevated amounts of dermatan sulfate (1966). Based on the finding that an increased incorporation of radioactivity into glycosaminoglycans occurred in Hurler fibroblasts cultured in the presence of labeled polysaccharide precursors, Matalon and Dorfman first assumed that there was an overproduction in the cells from mucopolysaccharidosis patients. However, studies by Elizabeth Neufeld and her coworkers at the National Institutes of Health subsequently proved that the Hurler syndrome was due to a defect in the degradation of the glycosaminoglycans, which led to excessive accumulation of labeled polysaccharides in the afflicted cells.

By now, it was known that heparan sulfate, like heparin (1962), contains L-iduronic acid, which is also the major uronic acid constituent of dermatan sulfate. Dorfman therefore postulated that an α-L-iduronidase was required for the normal catabolism of the two polysaccharides and that the increased accumulation and excretion observed in Hurler patients was due to a deficiency in this enzyme. By the end of 1970 Matalon, Cifonelli, and Dorfman (1971) had established the existence of a α-L-iduronidase by demonstrating release of iduronic acid from desulfated dermatan sulfate upon incubation with an extract of normal fibroblasts, and preliminary experiments also showed a deficiency of the enzyme in Hurler cells. Conclusive evidence for the deficiency (1972) was subsequently obtained by use of a more specific iduronidase substrate, phenyl α-L-iduronide, which had been synthesized by B. Weissman in the meantime. This was the first enzyme defect established in the mucopolysaccharidoses. In a relatively short time the enzyme defects in the other mucopolysaccharidoses were identified largely as a result of the work in the Dorfman and Neufeld laboratories. Studies on the mucopolysaccharidoses highlighted the importance of the degradative enzymes and helped develop the concept of lysosomes, which led to a better understanding of the relationship of human genetic diseases to the dynamics of cell structure.

The finding by Cifonelli and Dorfman (1962) that heparin contains L-iduronic acid is perhaps the most significant contribution from Dorfman's laboratory to our knowledge of the basic structure of the glycosaminoglycans. This discovery, which has had a profound impact in many areas of research, was initially met with skepticism or outright disbelief. The works of Maurice Maeterlinck come to mind: "At every crossway on the road that leads to the future, each progressive spirit is opposed by a thousand men appointed to guard the past." Cifonelli and Dorfman never published a full paper on the subject, a circumstance that may have contributed to the widespread skepticism facing their discovery. But why say in many words what can be said in few?

Increasing knowledge of the structure, biosynthesis, and degradation of matrix components led naturally to an inquiry into more biological aspects of complex carbohydrates. Dorfman's biological studies were concerned primarily with the mechanisms that control the quantity and quality of glycosaminoglycans synthesized intracellularly but destined for export to the cell surface or extracellular matrix, and considered aspects of functional cyto-architecture as well as the mechanism of differentiation of eucaryotic cells (1972, 1974, 1975). Studies on growth of cartilage were stimulated during a sabbatical in the laboratory of Leo Sachs, at the Department of Genetics at the Weizmann Institute, which permitted an intensive firsthand experience with tissue culture methods. In addition to determining that chondrocytes multiply in soft agar, thereby providing a selective method for propagation of cartilage cells (1973), Dorfman established environmental conditions for promoting differentiation of mesenchyme to chondrocytes (1972). These kinds of studies, which originally appeared to be out of the mainstream of the revolutionary progress of biochemistry and cell physiology, illustrated the unappreciated importance of complex carbohydrates in a large number of vital functions of eucaryotic cells; they also were the forerunners of understanding many aspects of the behavior of eucaryotic cells that are clearly governed by the interaction of cell surface glycoconjugates with substances that impinge on the cell surface.

Dorfman was always aware of the latest developments in the broadest spheres of biology. Thus, he was among the first to develop monoclonal antibody reagents and introduce immunohistochemistry (with B. Vertel) and quantitative RIA to the proteoglycan field (1978, 1979, 1980). He was also at the forefront of undertaking a new program on the molecular biology of connective tissue macromolecules and succeeded in developing a cell-free system for synthesis of proteoglycan core protein and type II collagen, and, finally, using recombinant DNA technology to isolate cDNA clones for type I and II collagen with William Upholt (1979).

During this latter period of scientific achievement, Dorfman was director of the Kennedy Mental Retardation Center, chairman of pediatrics and Richard T. Crane distinguished service professor at the University of Chicago, and director of La Rabida, a hospital and research center for children with chronic diseases. Thus, he was able to significantly influence clinical medicine, genetics, and developmental biology at the University of Chicago. He was instrumental in the construction of Wyler Children's Hospital and the establishment of the Joseph P. Kennedy Mental Retardation Research Center, one of the charter mental retardation research centers from the National Institute of Child Health and Human Development. After giving up most major administrative responsibilities in the late 1970s, he indulged more in his scientific efforts where his enjoyment and enthusiasm for research were infectious. This was also the period when he was most adventurous. Although never afraid to go beyond his own sphere of expertise to borrow and apply new ideas and methodology to his own field, he was even more ready to speculate on mechanisms of differentiation and molecular genetics.

Dorfman cared deeply about the scientific enterprise and was very concerned about changes that had taken place and other pending changes that he considered detrimental to the future of the biomedical research endeavor. In his last years Dorfman always took the opportunity to complain about the constriction of research funding and was among the first to discuss the scientific, ethical, and social implications of genetic engineering and screening. Notably, the Ryerson Lectureship at the University of Chicago in 1978 and the address he presented when inducted as president of the Pediatric Society in 1979 contained pleas concerning the demise of basic research and controversies in these areas. The final paragraph from the Ryerson Lecture is particularly poignant:

It is possible that the technology that stems from curiosity will destroy mankind. Perhaps the mutation that produced intelligence is indeed lethal. If so, there are more likely vehicles for man's demise than research on human genetics. I would prefer to believe that the mutation which produced intelligence will lead to a continuing increase of wisdom and that the technology that results from curiosity will continue to enhance the quality of life.

WE APPRECIATE THE ADVICE and support of Albert Dorfman's wife Ethel Dorfman. We have also benefited from personal recollections cited in A. Dorfman's "Adventures in viscous solutions," *Mol. Cell Biol.* 4(1974):45–64 and "Answers without questions and questions without answers," Ryerson Lecture, University of Chicago Press, 1978.

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LEE ALVIN DUBRIDGE

September 21, 1901–January 23, 1994

BY JESSE L. GREENSTEIN

LEE A. DUBRIDGE WAS born in Terre Haute, Indiana, on September 21, 1901; he died of pneumonia at age ninety-two in a retirement home in Duarte, California, on January 23, 1994. To quote the memoir by John D. Roberts and Harold Brown, DuBridge was "one of the most influential American scientists of the 20th century. He was a first-rate physicist, a leader in research of immense importance to the Allied victory in World War II, an exemplary research university president in a time of enormous scientific, societal, and educational change, as well as an influential statesman for science in the postwar era."[1]

Lee directed the MIT radar lab (1940–45), was president of Caltech (1946–69), and advised the government and military throughout his long career. He received the King's Medal for Service in the Cause of Freedom, the U.S. Medal for Merit, and the Vannevar Bush Award of the National Science Foundation. The Caltech trustees established the Lee Alvin DuBridge professorship.[2]

I will not attempt to describe his full career. He was a modest, eminently likable man, small in stature, but with strong presence. His conversation ranged from reminiscences of great world scientific events and personal friends to the finances of KCET, the Los Angeles PBS station, which he helped found and served as president of its board. He loved opera and made and listened to shelves-full of video recordings of nearly all broadcast performances. I will emphasize his life and personality at the expense of his scientific accomplishments, which came early.

YOUTH

For his family Lee wrote both professional and personal autobiographies of great interest. He deposited one copy, which I have used, in the Caltech archives. It reflects a difficult, poor childhood with many moves and no permanent home. His father, who changed jobs frequently, was a YMCA secretary, football coach, and physical education director at YMCA camps in Iowa, California, Montana, and Michigan, as well as in the Army. Later his circumstances worsened, and he taught, ran a filling station, sold insurance, and after the Depression returned to YMCA work in Chicago. Lee's mother was a poet and writer, who during the Depression wrote poems for greeting cards, some of which are still in use. The Caltech archives has a slender book of her work. Perhaps it was from her that he inherited his fondness for music.

Lee earned whatever money a young boy could; at age sixteen he worked in a Union Carbide laboratory for fifty cents an hour. Nevertheless, he had a good high school education in Sault Ste. Marie. He was somewhat interested in chemistry, but found it monotonous as taught. With small savings and a minuscule scholarship he started at Cornell College in Iowa in 1918. He was a good student but showed little early concentrated interest in science. He lived on a scholarship plus the thirty dollars a month he earned as a waiter in a girls' dormitory. There he met his wife-to-be, Doris Koht. Doris followed him through his career and proved a capable president's wife and hostess at Caltech. Doris died in 1973.

At Cornell he attracted the interest of the physicist O. H. Smith, to whom Lee was long grateful. Lee took advanced work in physics and served as laboratory assistant. Cornell was a small school but had devoted teachers. Smith later received the Oersted Medal for teaching from the American Association of Physics Teachers. Lee had a few years of study and normal student activities (debate, Milton, and singing in oratorios and church choirs), and was president of the student YMCA. He graduated in 1922 with a Phi Beta Kappa and went to the University of Wisconsin for graduate study. At the University of Wisconsin he was awarded a Ph.D. in 1926 (and later received one of his twenty-eight honorary degrees). The physics department was small and congenial and strongly emphasized laboratory work, use of vacuum, and measurement of small currents.

Lee had a summer job with the Bell Telephone Lab with future Nobelist Clinton Davisson. Supported by fellowships at Madison, Lee studied the photoelectric effect from platinum surfaces in which light (photons) causes the emission of charge (electrons) from metallic surfaces. Some proton energy must be expended to remove the electrons from the solid, depending on the material and its surface finish. His first assigned text was Arnold Sommerfeld's new text *Atombau und Spektrallinien* in German, taught by Charles E. Mendenhall. Sommerfeld himself visited two years later. Other instructors included Warren Weaver and Max Mason (with whom Lee had many later contacts).

Lee's first important experiment (published in *Physical Review*) proved that positive ions were definitely not emitted during the photoelectric process. Lee spent fifteen years on the photoelectric effect, building all apparatus required to perform experiments of increasing delicacy and precision. His thesis provided values of the photoelectric thresholds. With his Ph.D. and appointment as instructor (at $1,800 per year) he could at last afford marriage to Doris Koht, on September 1, 1925. Their two children were Barbara Lee (born 1931, and who married David MacLeod in 1955) and Richard Alvin (born 1933). Lee received an offer of a National Research Council fellowship for 1926–28 at $2,000 per year to work at Caltech under Robert A. Millikan. That great figure in American physics repeatedly became Lee's sponsor during his career.

The family bought a car, without a trunk ($400) and drove it across country in 1926, camping for six weeks as they explored the scenery, campgrounds, and treacherous roads of the Mid- and Far West. For thirty-five dollars a month they rented a bungalow in Pasadena. Such details of Lee's early career show how much has changed from the way physics was done in pre-war America. Yet in the next ten years things really did not change much. With my middle-class background, I received an NRC fellowship at $2,200 per year in 1937 and drove with my wife from Cambridge, Massachusetts, to Williams Bay, Wisconsin, in my first car (a Ford that cost $400). My house rented for forty dollars a month. Yet in Madison, 60 miles distant, photoelectric astronomy was being created by astronomer Joel Stebbins and physicist Albert Whitford. Science in the United States was only a small community and had a quite penurious support system.

At Caltech Lee continued to work on the photoelectric effect with Millikan, who was first to verify Einstein's relation between the photon energy and the maximum energy of the ejected electrons. Millikan befriended all young faculty, which he viewed as a big, happy family with himself as genial, strict father. They had many talks about the short history of Caltech, the philosophy of leadership in science, and the overriding necessity to attract the best people in every field of teaching and research. Caltech had such distinguished physicists as Richard C. Tolman, Paul S. Epstein, and students like Clark Millikan, Charles Richter, William V. Houston, and the humanist Clinton Judy. In a far better laboratory with an ultraviolet spectrometer, better vacuum, measuring instruments, and electronics, Lee studied the parallelism of the thermionic and photoelectric emission processes.

In 1928 Lee was invited to Washington University in St. Louis as assistant professor. Chairman Arthur Hughes welcomed him and soon proposed collaboration on a book. Much early work is consequently discussed in two books, *Photoelectric Phenomena* with Arthur L. Hughes (1932), which became a bible for experimenters, and *New Theories of the Photoelectric Effect* (1935). On the theoretical front, R. H. Fowler had just found that free electrons in solids obeyed Fermi-Dirac statistics; Lee's experiments in St. Louis provided many tests, measuring the photon frequency and the temperature dependence of the ejected electrons. Finally, nearing the end of his photoelectric work, he developed a Brown-DuBridge amplifier that proved useful for many years. But changes were coming even at the bottom of the Great Depression.

LEADERSHIP

In 1934 an important change occurred in his career. Lee DuBridge was invited to the University of Rochester, which recently had been heavily endowed by George Eastman. At age thirty-three he had not only become a full professor but department head as well. His salary reached $5,500 as his work and influence changed. He brought Fred Seitz and Milton Plesset to Rochester; he helped rescue Victor Weisskopf from Hitler's Europe.

An abrupt shift to the new subject of nuclear physics was stimulated by the success of Ernest O. Lawrence's cyclotron. The change marked the relatively inexpensive beginning of "big science" in which many people collaborated. They had invaluable advice from Lawrence and Cooksey of Berkeley. DuBridge led a group of physicists and a strong group of electrical engineers at Rochester, who borrowed metal for the magnets, got electrical generating equipment and high-power vacuum tube oscillators free, and raised $4,000 from local philanthropists for cash outlay. By 1936 DuBridge and S. W. Barnes had in operation an 18-inch cyclotron that reached 5 million electron volts and eventually nearly 8 million electron volts. It was still working in 1954.

The targets could be whatever was desired; the bombarding particles were protons from hydrogen. Isotopes of charge (Z) and atomic mass (A) were accessible targets if Z were not too large. A student and collaborator, Joseph B. Platt, remembers a table of isotopes on the control room wall. As each new unstable isotope was produced, its entry square (Z,A) was filled in and colored yellow to denote Rochester's priority. Platt claims that the wall turned mostly yellow. Oddities were detected (i.e., alpha particles); positrons were produced and then decayed. Lee (working with Barnes) found neutrons (n) from their newly discovered (p,n) reactions. Nuclear transmutation, at least from elements of moderate charge Z, became an established procedure. Almost any value of (Z,A) was accessible as the cyclotrons increased in energy and number.

Lee became dean of the Faculty of Arts and Sciences, bought a house, and received an honorary degree from his alma mater, Cornell College of Iowa.

By 1940 when it was clear that this country could be involved in the war in Europe the National Defense Research Committee was organized by President Franklin Roosevelt with members Bush, Compton, Conant, Jewett, and others. Lawrence held a commanding position in physics and in advice to the government and had become Lee's good friend. DuBridge was too successful and well known to be allowed to stay at peace in Rochester. Alfred Loomis (wealthy banker and amateur electronics wizard) and Lawrence recognized the reliance that the British were being forced to place on the United States for help with radar. Britain developed ground and airborne radar with the meter-wavelength magnetron tube at the Telecommunications Research Establishment. It helped win the Battle of Britain against German bombers. But Britain lacked the industrial and manpower base required to exploit microwave radar fully. Loomis initiated the U.S. production of magnetrons working at the higher frequencies and power.

The United States was clearly involved in preparing for a highly technological war. In November 1940, under pressure from Lawrence and Loomis, Lee became the founding director of the new Radiation Laboratory (RadLab) centered at the Massachusetts Institute of Technology. It did not disband until January 1946. His first helpers, recruited on a crash basis, included a dozen from the nuclear physics community, Alex Allen, Ken Bainbridge, Ed McMillan, I. I. Rabi, Norman Ramsey, Stan Van Voorhis, and Milton White; some of the staff later won Nobel Prizes. By 1945 the lab employed 4,000 scientists and engineers. Lee's style, one that he retained all his life, was one of showing leadership rather than exerting authority. He listened and understood the problems well, but he could be finally decisive. Their first project was to design and build a radar for air interception, which took three months plus a year to mount on the Northrup Black Widow airplanes.

Also designed and built were radar to detect ships and submarines at sea, for night bombing, and to point guns. Over 100 types of microwave radar were created. For each there were training programs, service instructions, and manuals for field maintenance. Many prospective users were trained at MIT. RadLab personnel fanned out over the world to train users and improve field operations. The annual budget reached $50 million. The lab and its products (the theory of high-frequency circuits and the many uses of microwaves) were described in an unclassified twenty-seven-volume series published at the end of the war. DuBridge lists in his unpublished "Memories" a few RadLab products he viewed as particularly significant:

LORAN, invented by Loomis, was the universal navigation aid and depended on timing long-wave pulses from three or more transmitters. Essentially these established lines of position, replacing the stars by a worldwide timekeeping radio net. It is the grandfather of the global positioning satellite network.

H2S AND H2X were airborne radar at 10 cm and 3 cm wavelengths and presented maps of the surroundings at sea and land. The radar was widely used for bombing in Europe.

EAGLE, invented by Luis Alvarez, provided even higher resolution for bombardment in Japan. The plane's long cylindrical antenna oscillated as it scanned the forward area.

MEW, used for high-power search to detect aircraft at a distance of 100 miles.

SCR 584 was a system of ground-based antennae that fed range and direction data into computers to help bring down German V-1 missiles over England.

Lee visited Europe. RadLab established overseas radar advisory centers for U.S. forces, one at the Telecommunications Research Establishment in England, one at Air Force headquarters, and one in Paris. Scientists in uniform followed troops in the field, installing new equipment in forward centers. Lee even visited Buchenwald, the recently liberated concentration camp. Radar centers were being established in the Pacific theater and the nuclear bombing of Hiroshima and Nagasaki depended on airborne RadLab radar sights. Lee also saw the first bombs at Los Alamos. He had close friends there, some of whom he had released from RadLab as its mission wound down. Los Alamos and RadLab, each with different styles of management, employed essentially every active physicist of the time and were the sources of physicists of the future.

RadLab was dissolved after the war's end, and in an orderly manner its responsibilities were transferred to industry. Spin-offs followed, among them air traffic control, microwave communications, timing circuits for electronics (including television and computers), the maser, the laser, nuclear magnetic resonance, and even the household microwave oven. Lee summarized his work at the RadLab as largely administrative and claims not to have understood the complex electronic circuitry involved. But his enthusiasm and clarity of expression were outstanding, his personal influence was enormous and he left hundreds of newly educated friends to populate the postwar world of electronics.[3]

Lee's family moved near to Cambridge, where they enjoyed New England vacations and helped create a center for the social life of RadLab. There were parties and picnics, insider jokes, and songs. The latter are epitomized by what Art Roberts wrote for a party honoring Rabi's 1944 Nobel Prize. "It ain't the money that makes the nucleus go 'round. It's the philosophical, ethical principle of the thing."

Lee commuted to Washington almost weekly, flew to Europe in 1943, 1944, and 1945, while Doris took care of the children and prepared for RadLab parties.

Among the important personal relations created between men working at the two laboratories was that of close cooperation between Lee and Robert F. Bacher, who had worked at RadLab, and who at Alamogordo had assembled the charge for the Trinity test weapon. Eventually Bacher joined DuBridge at Caltech. His thoughtful advice and cooperation proved invaluable to Lee and to the Caltech faculty.

Los Alamos continued as a nuclear weapons source and remains an active scientific center, especially now in large-scale computing. Both RadLab and Los Alamos were successful and expensive with unlimited budgets under the military necessity. Security was tight and generally successful at both laboratories.

Scientists and engineers learned a great deal from wartime research, and it changed their lives. Radar was a feature story in *Time*. In 1955 Lee's portrait was on the cover of *Time* where he was called "senior statesman of science." Lee had already been honored by election to the American Philosophical Society and the National Academy of Sciences. His career as a laboratory scientist had ended. He and the world changed. Closing down RadLab and finding jobs for its staff of 4,000, in fact, went particularly well because of openings in universities, which were expanding, and industry, which was converting to peacetime work. The important question was what came next for Lee. Because he wanted to become a university professor again, the letdown he felt in his attempted return to nuclear physics at Rochester must have been severe.

THE CALIFORNIA INSTITUTE OF TECHNOLOGY

Within six months an old friend, Max Mason, phoned.

He had known Lee at Wisconsin, was a former foundation head, and was then a Caltech trustee. He also headed the project to complete the 200-inch telescope. Max said Lee had to come to Caltech to be its first president (Millikan had been chair of the Executive Council of the Board of Trustees, but was never the president). The prospect was intrinsically attractive; the Pasadena climate was far better; the trustees and faculty were cordial; and housing was found. From a life of secrecy that limited friendship to wartime colleagues the entire DuBridge family found themselves in the social and public eye. They were important guests at public and private functions. It was a heady change to go from the relative poverty of his childhood to leadership of a rapidly growing institution in an expanding community. While both Lee and Doris were simple mid-westerners by background, they adapted well to this sophisticated world. Lee had an enthusiastic way of talking about Caltech and about science and engineering. He was almost never without the large, quick smile that helped make him an irresistible public figure. Their daughter Barbara attended a private school (Westridge); son Dick went through the then excellent Pasadena school system and was eventually accepted at Harvard.

Doris was closely involved with the Caltech Women's Club, so much so that it eventually established an annual "Doris DuBridge Day," which is still being celebrated. Social contact with prosperous friends from the community came often through the Caltech Associates, a group of non-academic people who contributed from $10,000 to $25,000 annually. Associates became members of the Athenaeum (the luxurious faculty club) and were able to attend many lectures by distinguished visitors and faculty. The 1,300 Associates are now an important source of Caltech's unrestricted income.

Lee and Doris traveled extensively, partly for Caltech, but often because Lee was a member of a board or committee. Fortunately, in 1949 they found an excellent old (1915) house with large grounds. Caltech purchased and modernized it to become its president's home. The DuBridge children were married in its garden. New and old faculty, families of graduating students, and Associates were entertained at large garden parties.

During the rapid growth of federal support of the sciences a struggle went on to establish the nature of the management of federal agencies, their independence from political pressures, and the size of their budgets. In these struggles Lee was an active participant, especially those involving the National Science Foundation. Caltech received excellent support from the Office of Naval Research, the Air Force Office of Scientific Research, the National Science Foundation, the National Aeronautics and Space Administration, and the National Institutes of Health. Lee also proved skillful at fund raising from private sources, the aerospace industry and foundations. During his presidency[1] the Caltech endowment grew from $17 million to $140 million; thirty new buildings were constructed; the 200-inch Hale telescope was put into service; and the Jet Propulsion Laboratory, originally a military production center, was made into a focal point for unmanned space exploration.

He was in many ways an ideal college president for the twenty-three years he held that office.[4] He was soft-spoken, responsive, and persuasive. He somehow knew how to say "no" and still retain the friendship of a faculty member. He visited faculty offices to ask what was new in a member's field. As senior professor of astronomy I enjoyed Lee's questions as to what had been found recently at Palomar. His broad physical insight gave him quick understanding and he savored what remained puzzling. A further extraordinary ability was to repeat a story, with background and speculation about its future, to a meeting of the Board of Trustees or to the Associates. Sometimes the solution was only money, which he provided at once in small amounts and in large amounts after some effort. Lee spoke very well in public and was under constant pressure to explain Caltech science and education to organizations and the public. In the Caltech archives there is a list of nearly 400 typed manuscripts of the speeches he gave in twenty years. The 347th listed is a "Farewell to Caltech," given on December 20, 1968, before he left for Washington. The faculty grew from 260 to 550 members (if we include postdoctoral fellows); the student body grew slowly from 1,200 to 1,550 in twenty-three years. Women students were admitted.

Among the most actively supportive faculty I must mention Earnest Watson (his early dean of the faculty) and Robert F. Bacher, whom Lee met in 1929 and who served after 1949 as chair of the Caltech Division of Physics, Mathematics, and Astronomy, provost, and vice-president. The history of Caltech's growth under Lee included low-energy nuclear physics, astrophysics,[5] aeronautical science, and all branches of engineering. It also included the 200-inch telescope, which came into general use in 1952, cosmology, and the enormous growth in molecular biology (Nobelist George Beadle) and chemistry (Nobelist Linus Pauling).

Lee was personally most interested in the low-energy physics studied with Van de Graaffs in the Kellogg Laboratory under the leadership of W. A. Fowler and C. C. Lauritsen. They studied both the reactions that produced the energy of stars and the reactions between light elements in the early universe (involving H, He, Li, Be, and B). A billion-volt electron synchrotron was built under Bob Bacher and was successful. But its small size indicated that Caltech could never join the race for the megamachines built, for example, at Berkeley. The local faculty, like most others, participate by bringing experiments and students to the internationally operated giant accelerators.

Lee's influence in the inner circles of the interlocking foundation boards' the "old boys clubs" dates back to 1940, when Lawrence and Loomis first suggested Lee as director of the MIT RadLab. Lee had faculty friends in aeronautics (through Clark Millikan and Theodor von Kármán), biology (Nobelist George Beadle), nuclear physics (Nobelist William A. Fowler), neurobiology (Nobelist Roger Sperry), and chemistry (Nobelist Linus Pauling). He was personally accessible and had strong links everywhere. The system may have invited misuse, but soon after World War II it was working at its best. Another DuBridge link to the inner circle of power was his 1948 election to the Bohemian Club in San Francisco. He attended essentially every summer encampment. He participated and enjoyed the sight of the leaders of California strenuously relaxing. There were symphonies, band concerts, plays, musical comedies, campfires, and lectures. It was also a time of family feeling at Caltech.

One important aspect of his leadership was his personal courage in relations[6] with Washington. The era of Senator McCarthy arrived in the early 1950s, and the chemist Linus Pauling, a fighting liberal, became McCarthy's target. Pauling was accused of membership in the Communist Party because he publicly opposed the continuation of nuclear weapons testing. A few leading Caltech trustees joined the McCarthy witch hunt, demanding that Lee fire Pauling in spite of Pauling's academic tenure and standing as a chemist. Pauling assured Lee in writing that he had never been a member of the Communist Party. Robert Bacher, chair of the Committee on Academic Freedom and Tenure, and Lee disagreed with Pauling's wish to stop nuclear testing, but they stood their ground against the trustees, and Lee offered to resign rather than exert pressure on Pauling. Instead, the trustees resigned. Pauling stayed on at Caltech until he won his second Nobel Prize, the Nobel Peace Prize, and then resigned in 1963.

When Lee retired Harold Brown, secretary of the Air Force, was selected to succeed him as president. Ruben F. Mettler, later chair of the Board of Trustees, says, "He was honored with the title of president emeritus and served as a lifetime trustee … Lee A. DuBridge was a towering figure in Caltech's history and in the world of science and engineering. He was also a kind and compassionate man, with a strong love of family and friends … His devotion to Caltech was complete. He often said he thought Caltech was the most wonderful place in the world."[4] On a lighter side, literary historian and faculty member Kent Clark wrote several amusing celebratory musicals; the chorus of his "Lee and Sympathy" has the refrain, "Give me a view from DuBridge; give me a ray along the way that lies before us."

Lee was involved in the formulation of national science policy and the creation of the National Science Foundation. In 1951 President Truman had appointed him a member of his Science Advisory Committee. President Eisenhower made him chairman (until 1958). On retirement from Caltech in 1969 he became science advisor to President Nixon. Lee met with the National Science Board and the Science Advisory Committee and became a member of the President's entourage at embassy parties and the summer White House. This was the time of the Apollo program and lunar landing. Lee led a small group of U.S. scientists on a successful mission to England, France, Belgium, the Netherlands, Romania, and Yugoslavia. But, after conflicts with the administration, and well before Watergate, Lee resigned after eighteen months. Back in Pasadena he spoke little of that period, even to old friends. But he did find the Nixon administration lacking in interest in science and technology because of their "only slight political importance."

Lee continued to serve on many boards (e.g., the Rockefeller Foundation) and the General Motors Science Advisory Committee. He and Doris returned to California and in 1970 bought a home in the retirement community of Leisure World in Laguna Hills, where they could participate in Caltech life. There, Doris was found to have spinal cancer; she died in November 1973 after forty-eight years of happy marriage to Lee. In 1974 Lee married the extraordinarily lively Arrola Bush Cole, widow of a college classmate who had been president of Cornell College for seventeen years.

LATER YEARS

Lee and Arrola lived a quiet life less than half a mile from Caltech's Athenaeum. They were well taken care of by a maid who prepared some meals and a driver who took Lee to the Athenaeum or Arrola to a meeting. Arrola had been on the board of and now became active in the ARCS, an organization that raised money for college scholarships. Many of these were awarded to Caltech students at lunches and elegant dinners, where the recipient squirmed before a formally dressed audience of Los Angeles donors. Because Arrola was so interested in students, two foundations have endowed student scholarships in her honor, a step that gave her the utmost delight.

In 1970 the trustees, led by industrial designer Henry Dreyfuss, raised a million dollars to create the Lee A. DuBridge professorship. Lee's interests remained centered on the discoveries and novelties at Caltech; he kept in touch with his old friends and met and talked with new colleagues. The conversation often took place around a large round table at the Athenaeum, where Lee's chair was kept available.

That round table is still active. The conversations concerned both national science politics and local academic problems. Lee's personal contacts with Washington were open and he often had news to bring.

Naturally a conservative and a patriot, Lee was quite open to new ideas. he was especially interested in changes in the budgets of the National Science Foundation and the National Aeronautics and Space Administration. He always wanted to know the results of the latest Jet Propulsion Lab experiment and the progress being made on the DNA code. He would, all too rarely, reminisce about what had happened at the RadLab, on problems of Anglo-American relations, and the background of the Cold War, but he never was the hero of his own stories.

Lee was enormously pleased to receive the Vannevar Bush Award of the National Science Foundation in 1982; the fact that he was old and his work had not been forgotten made it a double pleasure. Lee's career as a scientist had ended long before, but his strengths as an advisor and leader marked him as an outstanding human being and stayed with him until the end.

As they approached ninety Lee and Arrola gave up their home and moved to a retirement community, the Royal Oaks Manor (where we lived). Their apartment was carefully designed to house the mementos of a crowded and successful life: awards, opera tapes, autographed photos of presidents, family photos, the symbols of a full, happy life. Lee had to pick his way through the construction that surrounded them and climb the stairs when the elevator was under repair. But in the public dining room, Lee and Arrola spoke to everyone, told stories of their youth, and of Arrola's years as director of the floating World College and as a counselor living inside the Framingham (Mass.) Correctional Facility for Women. In the short time for such exchanges it became even clearer why Lee A. DuBridge was so respected and revered. He died peacefully of pneumonia in our nursing home in 1994; to be near her children, Arrola moved to the Boston area, where she died later the same year.

IN PREPARING THIS MEMOIR I relied heavily on material in the archives of the California Institute of Technology, to whose staff I am deeply grateful. Central was the forty-two-page autobiographical essay titled "Memories" (labeled "Professional") that terminated in 1969 and the sixty-five-page "Memories (labeled "Personal"). They were dictated in 1979 from memory, apparently without reference to his papers in the archives. Four copies of "Memories" were made for his family and one for the archives.

DuBridge's written record in the fully catalogued archives is very large and well indexed; I have consulted only a few sections. There exist further written autobiographical records from 1954, 1969, and 1978, including data from the National Academy of Sciences, to whose staff I am obligated. He kept a very systematic record, but apparently did not plan to write an autobiography. There is a list of sixty-nine national, professional, and civic activities, part of which are in "Selected Honors and Distinctions" below. The material deserves further study, which I could not do because of my age. I am personally indebted to Caltech professors emeriti John D. Roberts (chemistry) and Robert P. Sharp (geology) for critical readings.

NOTES

1. J. D. Roberts and H. Brown. Lee Alvin DuBridge. *Trans. Am. Philos. Soc.* 140 (1996):231–38.

2. I had the honor of retiring from Caltech as the first DuBridge Professor Emeritus. The current appointment is held by the distinguished planetary scientist Peter Goldreich.

3. I should mention one of his personal associates at RadLab, Charles Newton, a former newspaper and radio writer, who headed special publications and photography there. Newton came to Caltech with Lee, supervised Caltech publications, lectured in the humanities, helped raise money, and created the Industrial Associates. He died only a few months after Lee.

4. The Caltech alumni magazine *Engineering and Science* [57 (1994):14–21] contains personal recollections by Joseph B. Platt, William A. Fowler, Harry B. Gray, Ruben F. Mettler, and Thomas E. Everhart.

5. One example is DuBridge's organizational effort on behalf of astronomy. The Owens Valley Radio Observatory (OVRO) epitomizes his style. I had strongly pressed for our own radio observatory. Members of the Mount Wilson and Palomar staff had helped make exciting identifications of a few strange radio sources. We needed both the equipment and a leader. DuBridge knew E. G. (Taffy) Bowen and R. Hanbury Brown, who had brought the first magnetron from Britain to the embryonic RadLab. Taffy was postwar head of the Australian Commonwealth Scientific and Industrial Research Organization (CSIRO), which had an experienced scientific staff, but wanted to build a 210-foot parabolic, steerable radio antenna. But, it would cost $10 million, which they did not have. DuBridge and Bacher discussed our needs with Taffy, who loaned John Bolton, an Englishman and a CSIRO radio astronomer, to us for six years. However, the finite term of the loan was never discussed with me. John came to our department in 1955, and designed and built the OVRO interferometer, which had two 85-foot antennae moveable on long tracks. He built the receivers and delay lines and personally trained the first generation of American radio astronomers, many of whom have since become world leaders. John never warned me. He returned to Australia in 1960 to supervise the construction of the 210-foot parabolic dish. But how was that financed? Half the money was provided by U.S. foundations after Lee assured Alfred Loomis that it was a worthwhile project.

6. There were several less pleasant and less well known interludes. Lee seldom described participation in postwar classified projects, but the Korean War brought us an important one, Project Vista, which Lee headed and which involved much of the senior faculty. Dean Earnest Watson had pressed for Caltech's participation in this study for the military as a public service. The name contains no secret acronyms since the locale was the grounds of an old luxury hotel, Vista del Arroya. Managed by William A. Fowler and Charles C. Lauritsen, some of vista was concerned with the Korean conflict. My group was devoted to combat intelligence. But there was a highly classified part that studied the use of battlefield tactical nuclear weapons. The Strategic Air Command (SAC) at that time monopolized then scarce nuclear material, which it planned to use for strategic

bombing of large targets. Vista brought together many hundreds of experts from much of the U.S. physical science community. It lasted only from November 1951 to March 1952, cost $600,000, and took 7,800 man days. It ended with three debriefings; I participated in two. J. Robert Oppenheimer and Edward Teller were strong-minded visiting participants and Vista may have been one seed in their later mortal conflict. Perhaps the idea of limited nuclear warfare was premature in 1952. It was certainly resisted by SAC. By July 1952 selective leaks, none of Caltech's doing, brought bitter editorial criticism in *Aviation Week*, alleging that the scientists involved were wrong as well as probably disloyal. A full criticism came from military expert Hanson Baldwin in the *New York Times* of June 5, 1952. Baldwin's article is thoughtful, but echoes the strong sentiment of SAC that it should continue to control both the nuclear explosives and the planes on close-support missions. DuBridge's personal folder about Vista in the Caltech archives is nearly empty. All copies were removed and most were destroyed. The report was not generally distributed to the services. Lee's archive folder contains the final financial report, thank-you letters from a few generals, and a final thank-you letter from Frank Pace, Jr., secretary of defense. What were the actual consequences of Vista? One fact was that battlefield tactical nuclear weapons were manufactured and distributed sometime afterward. Yet among the first agreements between the United States and the Soviet Union, reached after détente, was removal and destruction of the numerous tactical nuclear weapons under local command that had spread over most of the European theater.

SELECTED HONORS AND DISTINCTIONS

1934–46	Professor of physics and chairman, Washington University
1938–42	Dean, Faculty of Arts and Sciences, University of Rochester
1940–45	Director of the Radiation Laboratory, MIT
1942	Member, American Philosophical Society
1943	Member, National Academy of Sciences
1945–49	Scientific Advisory Board, U.S. Air Force
1945–51	Naval Research Advisory Committee, U.S. Navy
1946	King's Medal for Service in the Cause of Freedom

1946–69	President, California Institute of Technology
1946–48	Research Advisory Panel, U.S. Army
1946–68	Board of Trustees, Southwest Museum
1947	President, American Physical Society
1947	Research Corporation Award
1948	United States Medal for Merit
1948–61	Board of Trustees, RAND Corporation
1950–54	Member, National Science Board
1950–51	President, Western College Association
1951–56	Science Advisory Committee, Office of Defense Mobilization (chair, 1952–56)
1951–57	Board of Trustees, Carnegie Endowment for International Peace
1953–61	Board of Trustees, Air Pollution Foundation (chair, 1956)
1956–60	Board of Trustees, Institute for Defense Analyses
1956–67	Board of Trustees, Rockefeller Foundation
1958–64	Member, National Science Board (vice-chair, 1962–64
1959–63	Board of Governors, Los Angeles Town Hall
1960–68	Board of Trustees, Edison Foundation
1962–68	Chair, Board of Directors, Community Television of Los Angeles (KCET)
1962–68	Board of Trustees, Huntington Library and Art Gallery
1964–68	Board of Directors, National Educational Television
1966–67	President's Task Force on Education
1967	Governor's Award, National Academy of Television Arts and Sciences
1968	Sesquicentennial Award, University of Michigan
1968	President's Air Quality Advisory Board
1969–70	Science Advisor to President Richard M. Nixon
1969	Lehman Award, New York Academy of Sciences
1970	Robert Andrews Millikan Award, California Institute of Technology
1971–75	Science Advisory Committee, General Motors
1973	Golden Plate Award, American Academy of Achievement
1977	Advisory Council, Jet Propulsion Laboratory
1982	Vannevar Bush Award, National Science Foundation

HONORARY DEGREES

1940	Sc.D., Cornell College, Iowa
1946	Sc.D., Polytechnic Institute, Brooklyn
	Sc.D., Wesleyan University, Connecticut
1947	Sc.D., University of British Columbia
1948	Sc.D., Washington University, St. Louis
	LL.D., University of California, Los Angeles
1952	Sc.D., Occidental College, Los Angeles
1953	LL.D., University of Rochester
1955	Sc.D., University of Maryland
	Sc.D., Columbia University
	Sc.D., Indiana University
	LL.D., University of Southern California
	Sc.D., University of Wisconsin
1958	L.H.D., University of Redlands
	LL.D., Northwestern University
	L.H.D., University of Judaism, Los Angeles
1961	D.C.L., Union College, Schenectady
1962	Sc.D., Pennsylvania Military College
	Sc.D., DePauw University
1963	LL.D., Loyola University, Los Angeles
1965	Sc.D., Pomona College
	D.Sc., Rockefeller University
	Sc.D., Carnegie Institute of Technology
1967	LL.D., University of Notre Dame
1968	LL.D., Illinois Institute of Technology
1969	Sc.D., Tufts University
	Sc.D., Syracuse University
1970	Sc.D., Rensselaer Polytechnic Institute

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Courtesy of the University of Michigan News Service, Ann Arbor

LEO GOLDBERG

January 26, 1913–November 1, 1987

BY LAWRENCE H. ALLER

LEO GOLDBERG WAS ONE of the most distinguished leaders of the astronomical community in this century. He achieved outstanding success in the application of atomic physics to astrophysical problems, and is best known for pioneering efforts in the study of the sun from space. He was director of three important observatories: University of Michigan (1946–60), Harvard (1960–71), and Kitt Peak National Observatory (1971–77). He played an important role in founding the Association of Universities for Research in Astronomy, Kitt Peak National Observatory, and the National Radio Astronomy Observatory. He contributed real leadership as president of the American Astronomical Society (1964–66) and the International Astronomical Union (1971–76).

Goldberg came from a family of very limited resources, emerging as a Horatio Alger-type hero in the academic world. His experience is an irrefutable answer to those who would deny a young person a chance in life because he happens to be poor. Goldberg's parents emigrated from East Poland, then part of the Russian Empire, before World War I. His father worked in the needle trades: men's caps and for a time in ladies' millinery. In 1922, when Leo was nine, a disastrous fire destroyed their Brooklyn tenement home and killed his mother and younger brother. Leo and his older brother were hospitalized for months. In 1924 his father remarried and in 1925 the family moved to New Bedford, Massachusetts.

His father had little formal education but great appreciation of its importance. The family was poor. After school and on weekends Leo worked in the family store. Young Goldberg did well in school; he especially liked science and mathematics. He won a scholarship to Harvard where he enrolled in the engineering program in 1930. The scholarship covered only tuition, not living expenses. A member of the scholarship committee, recognizing Leo's remarkable abilities, offered a loan of money, which, combined with his earnings during the summer and at various odd jobs, permitted him to attend Harvard.

Goldberg started out in engineering, but he was not very excited by it. By a lucky accident, he enrolled in Astronomy 1 with Bart J. Bok, who introduced him to Donald H. Menzel, a theoretical astrophysicist who had come to Harvard in 1932 from Lick Observatory. Leo quickly switched to astronomy, which was not difficult because he had the necessary courses in mathematics and physics. He worked on a problem of relative multiple strengths in atomic spectra, a serious topic in the interpretation of stellar spectra. His interest in the field of atomic spectra continued throughout his career.

Harvard summer school attracted distinguished astronomers, and Pol Swings and Anton Pannekoek from Europe and Paul W. Merrill, Henry Norris Russell, and Otto Struve from the United States proved a great inspiration to young Goldberg. Leo won a prize for a paper on He I lines based on his 1938 thesis. He was able to attend a meeting of the International Astronomical Union in Stockholm. In 1940 he visited the Mount Wilson and Lick Observatories, where he saw a variety of high quality spectroscopic equipment (then utterly lacking at Harvard). This experience proved very enlightening. Shortly before Pearl Harbor, Leo was appointed to the staff of the McMath-Hulbert Observatory, directed by Robert McMath, a Detroit businessman who had built it with the help of his father and an acclaimed jurist, Judge Ferdinand Hulbert. The observatory was well known for its pioneering photography of transient solar phenomena, primarily prominence and chromospheric changes. During the war, Goldberg worked successfully on an antisubmarine project at McMath-Hulbert Observatory.

In 1946 offers of tenure positions at Yale and Michigan came almost simultaneously. Goldberg chose the job of department chairman and director of the observatory at Michigan, where for the first time, at the age of thirty-three, he displayed his remarkable skills as organizer and administrator. Retirements, deaths, and resignations had decimated the staff in Ann Arbor, so Leo set out to build up a new group of young, energetic, and forward-looking scientists. One of his goals was to build a department of capable people attuned to the needs of the mid-twentieth century, somewhat like the group he had known at Harvard but with an important difference. David Layzer, who was a Michigan postdoc in 1951, commented as follows:

In one way Michigan's astronomy department was like Harvard's, where I had done my graduate work. It was full of strong-minded and rather self-centered people with widely different scientific interests. But in another way it was strikingly different. Everyone seemed to like everyone else, and everyone seemed happy and productive in his or her work. This was something my Harvard experience had not prepared me for. It became clear that this harmonious, stimulating, and mutually supportive atmosphere had been created and was being maintained by one person, Leo Goldberg.

Thus, Goldberg was the right man in the right place at the right time. Besides the McMath-Hulbert Observatory at

Lake Angelus, near Pontiac, Michigan's observatories included the original one in Ann Arbor with its small refractor of 1850 vintage and a homemade 37-inch reflector built in the early twentieth century and devoted exclusively to stellar spectroscopic work. There was also the Lamont-Hussey Observatory at Bloomfontein, South Africa. This observatory was devoted entirely to the measurement of optical double stars; it played no role in the teaching or research of the postwar students and staff.

Before World War II, interest had been focused on a large modern reflector, and then director Heber D. Curtis had acquired a 97-inch disk intended for the primary telescopic mirror. First the Great Depression and then the war scuttled the project. Goldberg decided that Michigan with its poor meteorological conditions and "bad seeing" was no place for a large reflector. He opted instead for a Schmidt camera similar to the one at the Case Institute of Technology in Cleveland. After a discouraging start with initially bad optics, a splendid telescope was finally produced. This instrument was used successfully for direct photography, objective prism surveys, and photoelectric photometry. When Cerro Tololo was established, the Michigan Schmidt was sent there to do excellent work on the southern skies, especially objective prism spectroscopy with "good seeing." The foresight of Goldberg and his associates paid off handsomely.

Under Goldberg's leadership, teaching and research advanced impressively. Many capable graduate students were engaged in research projects in solar and stellar spectroscopy, galactic structure, photoelectric photometry, and theoretical astrophysics. At Lake Angelus, McMath and associates had been quick to exploit the advantages of the Cashman lead-sulfide cell for near-infrared high-dispersion solar spectroscopy. Later Goldberg and his staff constructed a high-dispersion, high-resolution spectrograph. The interior was evacuated to prevent air currents, which have blurred spectral lines, from producing bad seeing, thereby ruining the critical definition required in the best solar spectroscopy.

The rise of radio astronomy as a productive area of research led Goldberg to pursue possibilities in this field. Starting with a 20-foot dish and devices to monitor solar activity associated with solar flares, he was able to entice Fred Haddock of the Naval Research Laboratory in Washington, D.C., to take charge of the erection of an 85-foot dish, used in recent years for investigation of variable radio galaxies.

During the 1950s Goldberg became increasingly involved in problems of national astronomical interest. As noted below he played an important role in the foundation of the national optical observatories and later in the establishment of a national radio observatory. Many astronomers and groups had expressed concern about the role of radio astronomy in the United States and conferences were called in 1954 and 1956 to address this issue. In 1956 the National Science Foundation decided to have Associated University, Inc., undertake the establishment of the National Radio Astronomy Observatory. Goldberg was offered the directorship, but he declined.

Goldberg also played an active role in the American Astronomical Society, as well as the International Astronomical Union, of which he later became president. Goldberg was one of the first to appreciate the importance of observations of celestial objects, particularly the sun, from above the earth's atmosphere. The first observations of the ultraviolet solar spectrum were the rocket measurements of Tousey in 1946, but it was the possibility of data secured from satellites following Sputnik in 1958 that really fired his enthusiasm. Then at Michigan, he proposed a large project for satellite instrumentation. This enterprise involved ambitious developments that would require not only experts who would be paid by government contracts, but also a considerable investment in buildings, which would have to be underwritten to some extent locally.

The administration at Michigan failed to grasp the significance of the scientific breakthrough offered by space astronomy. At the same time, Harvard's department made an offer so attractive to Goldberg as to be irresistible. The ineptness of the administration at the University of Michigan just made the decision that much easier.

When Goldberg came to Harvard he undertook an ambitious three-pronged enterprise: the building of orbiting observatories, establishment of an astrophysical laboratory, and the erection of a new building to house these activities and provide new quarters for the astronomy department. The orbiting solar observatories carried telescopes that could get monochromatic images of the solar corona at various wavelengths that were inaccessible to ground-based telescopes and radio these data back to earth. The astrophysics laboratory was equipped with devices such as shock tubes, which would produce plasmas (of short duration) but whose temperatures would mimic those of the corona, chromosphere, and the corona-chromosphere interface.

A striking demonstration of Goldberg's administrative skills was his ability to accomplish all of this and at the same time carry on the inspired research programs described below. The task was far from easy. Under previous director Harlow Shapley the observatory's reputation for instrumentation was undistinguished. Far more serious, however, was the intrinsic difficulty of these horrendously complicated experiments. Goldberg's team worked diligently from 1960 to 1967 before they launched a successful satellite, the OSO IV. The next, OSO V, gave faster scans of the sun and was even more successful.

When Goldberg came to Harvard in 1960 he found himself in a complex administrative situation. Menzel was still director of the Harvard College Observatory (HCO); the newcomer was paid 50-50 by Harvard and the Smithsonian Astrophysical Observatory (SAO). SAO had come to Harvard in 1955. There were in effect two separate observatories with no joint planning. Fred Whipple, director of SAO and a professor at Harvard, sat on the observatory council and participated in affairs of the HCO. But the converse was not true; the department was not consulted on SAO appointments. On the other hand, the SAO contributed a great deal to the HCO, astronomy department, library, and shop and provided part-time jobs for graduate students. Still, this was a radically new experience for Goldberg. In 1966 he became director of the HCO and chairman of the Harvard astronomy department. His philosophy on how the observatories ought to be run differed from Whipple's and contributed to his decision to leave Harvard and accept the directorship of Kitt Peak National Observatory.

Under the leadership of Leo Goldberg the Harvard astronomy department achieved new levels of distinction both in teaching and research. In this period of sustained growth and steadily rising standards many students who are now leaders in astronomy were attracted to Harvard. The door of his office was always open to students. Goldberg combined a passion for astronomical research with a deep interest in people of similar bent. His students admired him. Andrea Dupree, a graduate student and later a research associate in the Solar Satellite Program, wrote:

Leo had true concern for the Harvard students. He worked diligently to ensure that financial support, fellowships, scholarships, and assistantships would be available to enable us to continue in school. For students, this interest and care often provided the encouragement necessary to pull through the degree requirements.

Although Goldberg had zero tolerance for second-class work or thinking, he always felt that criticism should be constructive, a point of view that is often woefully lacking in many research proposal evaluations or refereeing of papers at the present time. In the winter of 1966 officials at the National Aeronautics and Space Administration approached Goldberg to accept the directorship of the Goddard Space Flight Center. This was an immense enterprise with a budget of $500–$600 million a year, mostly spent on tracking stations and engineering. At nearly the same time, Menzel decided to step down as director of the HCO, and the position was offered to Goldberg. Thus, there was no lack of job opportunities, as at the same time he could have taken charge of the Yale astronomy effort. Goldberg decided to assume the directorship of the HCO. He was now working full-time for Harvard, having dropped the SAO half position. He also assumed the chairmanship of the department, previously held by William Liller, and was now able to integrate better the teaching and research capabilities at Harvard, expand and improve the curriculum, and direct the students to more exciting and relevant research problems. He strengthened the research and teaching staff at the HCO with appointments of distinguished scientists such as the spectroscopist Alex Dalgarno.

In addition to his academic duties and government advisory activities, Goldberg undertook an intensive effort to obtain substantial financial support from Harvard alumni and well-wishers. Particularly noteworthy was the gift of some millions of dollars from the Perkin family (of Perkin-Elmer Optical Company fame), which, combined with smaller grants from other friends of Harvard and a grant from the National Science Foundation, financed the construction of the Perkin Building for the HCO. This building provided office, library, and laboratory space for the rapidly developing programs in experimental and theoretical astrophysics.

By 1970 all of these endeavors had either paid off or were well on their way to fruition. The analysis of the solar data laboriously obtained with the OSO satellites had become increasingly the responsibility of Goldberg's younger, gifted collaborators Andrea Dupree and George Withbroe. The astrophysical laboratory with its shock tube and high-temperature experiments under E. M. Reeves and William H. Parkinson was producing valuable data. But relations between the HCO and SAO were not quite as harmonious as Goldberg had wished. His days of untrammeled, novel, and trail-blazing research were over. He was tempted by a new challenge not long in coming.

In the summer of 1952 a small group convened in Washington to consider the needs of astronomy for the newly founded National Science Foundation. Except for the fortunate staff of places like Mount Wilson-Palomar or McDonald Observatories, the outlook was often bleak. Most astronomers were associated with colleges in the east or mid-west, where the climate was deplorable, particularly for stellar photometry. John Irwin of Indiana University urged the establishment of an observatory in the southwest, where the skies are clear and the visibility often good. A group of interested astronomers met in Flagstaff, Arizona, in 1953 to discuss the establishment of an observatory devoted to photoelectric photometry and open to qualified observers from all over the country. Goldberg was among those who attended this conference. He rephrased the proposition somewhat as follows: "This endeavor is so important that we must not restrict such an observatory solely to photometry. It should embrace all the needs of ground-based astronomy, including spectroscopy."

Thus was the fundamental concept formulated for what was to become the Association of Universities for Research in Astronomy (AURA). AURA developed the Kitt Peak National Observatory (KPNO) in Arizona and the Cerro Tololo Interamerican Observatory (CTIO) in the foothills of the Andes near La Serene, Chile. Goldberg formulated the practical plan of how the observatory was to be operated. The governing board was to consist of representatives (a scientist and an administrator) from each of the interested universities. The original AURA board included Caltech and the Universities of Arizona, California, Wisconsin, Indiana, and Chicago and Harvard University; other academic institutions were added later. AURA was incorporated in Arizona. Goldberg was not a member of the first board. McMath insisted on representing Michigan, and Leo did not become a member officially until he became the Harvard director, although he exerted much influence.

Upon the retirement of KPNO director Nicholas U. Mayall in 1971, Goldberg was appointed to replace him. The organization of the observatory was thoroughly overhauled in a period of decreasing budgets. Fortunately, Goldberg was able to do this so that the operation of the observatory was not impaired for the first three or four years. Thereafter, the budget cuts seriously affected activities. The observatory had to maintain the general-purpose equipment in tiptop condition for the users and at the same time develop new instrumentation as research progress demanded it. Problems of organization and administration were tremendous. Guest astronomers, many from small colleges and not too familiar with astronomical instrumentation, had to be taken care of and kept happy, requiring a large, capable, and diplomatic support staff. In addition, CTIO had special problems arising from the operation of a modern observatory at a remote site in a distant country. Victor Blanco, the CTIO director, handled this very capably. In addition, KPNO had the responsibility for developing new instruments and putting them into operation. These included the 4-meter Mayall telescope at Kitt Peak and its counterpart in Chile for which Goldberg had been helpful in raising a substantial contribution from the Ford Foundation. Numerous auxiliary instruments such as image-dissector scanners were designed and built and the first attempts were made to study a "supertelescope" (15-to 25-meter aperture).

During much of his administration at KPNO Goldberg was ably assisted by Beverly Lynds whom he married a year before his death in 1987. Goldberg's problems arose not only from having to do more with a smaller budget but also by the increasing complexity of management. He reached the compulsory retirement age of sixty-five in 1978, but in 1977 the AURA board gave him a three-year appointment as distinguished research scientist at KPNO, where he remained as a research scientist until 1983. In 1984, after serving as a visiting professor in France (1980) and in London (1984), he became an associate in research at the HCO. In 1985 Goldberg became the first appointee to the Martin-Marietta chair of space history at the Smithsonian Institution's National Air and Space Museum, a fitting recognition of someone who had so markedly contributed to the success of the U.S. space science program.

RESEARCH CONTRIBUTIONS

Goldberg's research contributions began as an undergraduate student in 1935, when, under the inspiration of Menzel, he undertook the calculation of relative multiplet strengths in a transition array. How to calculate relative strengths of lines in a multiplet was already known. Soon thereafter he developed the concept of fractional parentage, later adopted by Racah as a basic element of his algebra, making it possible to calculate relative line strengths for all the lines in the most common transition arrays encountered in astrophysics in line-strength coupling. With such data it was possible to interpret the intensities of the spectral lines (e.g., of Ti II) in the solar spectrum and derive the excitation temperature of the solar atmosphere. To obtain the actual abundance of the relevant element, one had to determine (by calculation or by experiment) the absolute strength (or f-value) of the transition. Goldberg explored calculating f-values theoretically using quantum mechanics. He succeeded in calculating good f-values for neutral helium and applied them to the interpretation of gaseous nebulae and hot stars. Computational means did not then exist to tackle many more complex atoms.

Determination of elemental abundances in the solar atmosphere by Goldberg, Edith A. Müller, and Lawrence H. Aller constituted the first thorough investigation since the work of Henry Norris Russell, thirty years earlier. The observational data consisted of total intensities or equivalent widths of the Fraunhofer lines of the solar spectrum from published sources or from data obtained with the vacuum spectrograph at the McMath-Hulbert Observatory. Most workers had assumed that the solar atmosphere could be represented as an isothermal stratum at a fixed temperature and pressure. Goldberg et al. took into account the actual variation of temperature and pressure through the radiating layers by the method of weighting functions. The poor quality of then available experimental f-values, especially for the important element iron, seriously affected the quality of the results. Reliable theoretical f-values were few. More recent investigators, using trustworthy f-values but essentially the same methods as applied by Goldberg's team, have found the chemical composition of the solar atmosphere to match that of carbonaceous chondritic meteorites. Incidentally, with the Pb S detectors at the McMath-Hulbert

Observatory, Goldberg, McMath, Orren Mohler, and Keith Pierce investigated for the first time carbon monoxide and certain atomic lines in the infrared spectrum of the sun. The high-resolution vacuum spectrograph permitted a study of the small-scale motions in the photosphere, which later observers identified with the five-minute solar oscillations.

In 1966 Goldberg showed that slight departures in hydrogen from Boltzmann's law would produce population inversions and give a maser-like action that would act to intensify certain hydrogen recombination lines. Consequently, one must use caution in interpreting the intensities of nebular radio-frequency hydrogen lines. Another important factor emerging for the interpretation of certain lines in the spectra of gaseous nebulae and for understanding observations of the solar corona is dielectronic recombination, which enhances the rate at which ions and electrons recombine. For example, in neutral carbon (C I) Goldberg and Dupree showed that levels of high total quantum number would be overpopulated by dielectronic recombination, producing anomalous intensities for radio-frequency lines.

It was in the satellite observations of the outer envelopes of the sun that Goldberg did his most spectacular and important work. Early in 1958, with the collaboration of Haddock, Liller, and Aller, he prepared a report for McDonnell Aircraft Company entitled "Astronomy from Space Vehicles." Shortly thereafter the National Aeronautics and Space Administration was established, and began to make serious plans for space research.

In 1960 Leo moved to Harvard, where despite heart-breaking problems with the first orbiting solar observatories, he achieved spectacular success with OSO IV, OSO VI, and Skylab 1973. The object of these experiments was to make monochromatic solar images at any chosen wavelength. This quick look and control capability enabled astronomers at the ground station to monitor and respond immediately to phenomena such as solar flares. One could command the satellite to produce an image in Mg X, N V, or C IV, thereby looking effectively to different levels in the chromosphere, the corona, or transition regions. Thus, one obtained the plasma diagnostics of these regions. Certain strategic lines (e.g., Mg X) occasionally appeared very weak in some areas, which Goldberg identified as "holes" in the corona! They were of special interest to X-ray investigations, being areas from which originates a good part of the solar wind. The shock tube laboratory, operated by Parkinson and Reeves, produced small volumes of gas momentarily at high temperatures and densities appropriate to astrophysical plasmas. This device makes it possible to measure f-values and identify molecular and atomic lines that can be compared with solar and stellar spectra. The vibration-rotation bands of CO had been found in the infrared as noted above; the electronic bands of CO were identified in the ultraviolet. The satellite solar work and its supporting laboratory measurements have proved to be of crucial importance to our understanding of the sun.

Goldberg was also interested in seeing that scientific information was made readily available; hence, he played a leading role in the establishment of *Annual Review of Astronomy and Astrophysics*. An earlier phase of this interest was shown in a volume in Harvard's Books on Astronomy entitled "Atoms, Stars and Nebulae" written in collaboration with Aller.

SCIENTIFIC DIPLOMACY

The decisive role that Goldberg played in the International Astronomical Union (IAU) may not be well known. Soon after receiving his Ph.D. he attended the IAU meeting in 1938. He was chairman of the U.S. delegation to the tenth general assembly in 1958 and eleventh assembly in 1961. He was a vice-president (1958–64) and president (1973–76).

The cold-war rivalries in the United States and the Soviet Union flourished in the 1950s. The Soviet Union invited the IAU to convene in Moscow in 1958 and proclaimed that astronomers from all member countries would be welcome. To save face, the United States would have to host the meetings in 1961 under the same guarantees that the Soviet Union had given. Astronomers from all member countries would be welcome. In particular, since the People's Republic of China (PRC) was an IAU member, its astronomers would be allowed to attend, but Taiwan was not a member at that time. PRC athletes were to attend the 1960 winter Olympic games in the United States. The Department of State headed by Secretary Dulles observed that this action was ad hoc and did not imply that mere scientists could expect such favors. Goldberg contacted his representative in the U.S. Congress, George Meader, a conservative and fair-minded Republican, who presented the case to Dulles, who referred it to his science advisor Wallace Brode. Brode promptly demanded that Taiwan be invited to the IAU.

The fact that Taiwan then had no astronomers and would have to qualify for IAU membership in the approved way meant nothing to the militant anti-Communist Brode. Brode wanted Goldberg to go to the 1958 Moscow meeting and submit the 1961 invitation but with the condition that Taiwan be admitted at once. Such a demand could well wreck the IAU. From Brode's point of view, if the astronomers would not go along with his orders, so much the worse for them.

Goldberg was unable to submit to these demands and offered to resign, but the National Academy of Sciences supported Goldberg's position that the IAU should act on the application of Taiwan in an orderly way. The invitation for the IAU to meet in the United States was issued and accepted. Taiwan was admitted to membership in 1959; the PRC withdrew in 1960, but returned later. Today astronomy is flourishing in Taiwan. Goldberg's quiet diplomacy saved the IAU from disintegration and the world astronomical community from a severe blow.

Leo Goldberg was married to Charlotte Wyman in July 1943. They had a daughter, Suzanne, born in 1944, and two boys, David and Edward, born in 1946 and 1951, respectively. Leo and Charlotte were divorced, and Leo married Beverly T. Lynds in January 1987.

Goldberg received many honors and was a member of a number of learned societies. In addition to the National Academy of Sciences (1958), he was a member of the American Academy of Arts and Sciences (council, 1979–82) and the American Philosophical Society (1958). He belonged to the American Geophysical Union, American Astronomical Society, American Physical Society, Astronomical Society of the Pacific, Optical Society of America, and the Royal Astronomical Society, and was a foreign member of the Royal Society of Liege. Goldberg served on more than a hundred committees covering a wide range of interests from cultural affairs to science. In 1973 Goldberg received the Distinguished Service Medal from the National Aeronautics and Space Administration and gave the Russell Lecture of the American Astronomical Society. He gave the George Darwin Lecture of the Royal Astronomical Society in London in 1984, and received the George Ellery Hale Prize of the American Astronomical Society in 1984. He received an honorary degree from the University of Massachusetts (1970), University of Michigan (1974), and University of Arizona (1977).

Leo Goldberg was a great human being, generous and helpful, as well as a great scientist and superb organizer. When he believed he was right, he could be very obstinate, but always in a logical sort of way. He looked at a problem from a very different point of view or he adopted different priorities. My first contact with Leo Goldberg was while I was a student in Berkeley (1934) wanting to go to Harvard to work with Menzel. I was very discouraged about financial prospects, but Leo gave me a realistic appraisal in such a friendly letter that I was encouraged to take the chance.

My last contact with Goldberg was at a National Research Council meeting in February 1987. He seemed cheerful and optimistic, looking forward to his new assignment with the National Aeronautics and Space Administration. He announced he had had lung cancer, caused by years of heavy cigarette smoking, but that he had licked the affliction. My immediate reaction was one of worry and deep apprehension. Tragically my concerns were justified. Leo Goldberg was my friend who helped me attain goals of scientific achievement that otherwise would have been beyond my reach.

I AM GRATEFUL FOR valuable information and advice supplied by a number of Goldberg's associates, particularly Frank Edmondson, Jesse Greenstein, and Donald Osterbrock.

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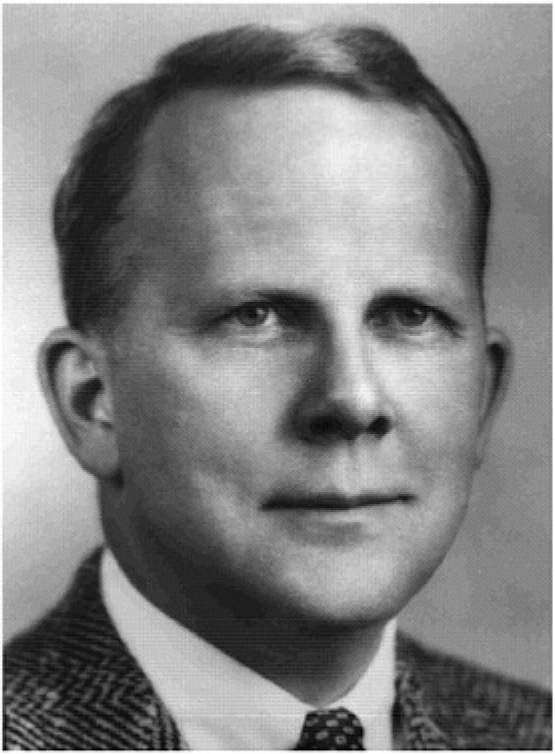

LOUIS PLACK HAMMETT

April 7, 1894—February 9, 1987

BY F. H. WESTHEIMER

LOUIS P. HAMMETT was one of the founders of physical-organic chemistry, and a major contributor to it. Together with Arthur Lapworth and Christopher Ingold he created a new branch of chemistry, a new discipline. The ideas and principles of physical-organic chemistry changed the world's teaching and practice of chemistry and, in particular, changed the way synthetic organic chemistry is performed, with enormous practical consequences.

All important practitioners of synthesis today make extensive use of mechanisms of reactions and the stereochemistry of reactions. The understanding of mechanism and stereochemistry was strongly advanced not only by Hammett's research but especially by his famous textbook *Physical-Organic Chemistry.*[1] The concepts on which chemists today depend include an acidity function that Hammett invented and a famous equation named for him that allows a quantitative understanding of chemical reactivity.

PERSONAL

Louis Hammett was born in 1894 while his parents were visiting Wilmington, Delaware, so that this most quintessential New Englander managed to begin his life south of the Mason-Dixon line. He grew up, however, in Portland, Maine.

His father, a native New Englander whom Hammett described as "intellectually brilliant," was educated as an engineer and had an outstanding record at both Harvard and MIT. From his father and a maternal uncle, Hammett learned to use and love tools and drawing instruments. The Portland schools were hardly superior, as illustrated by the fact that the primary function of the chemistry instructor was that of a basketball coach.[2] Hammett had no talent for sports, but, perhaps despite the teacher, he fell in love with laboratory work, and the school did drill him well in German, English composition, and elementary math.

Following his father to Harvard, Hammett studied analytical chemistry under Gregory Baxter and organic chemistry under E. P. Kohler, whose "intense enthusiasm for unraveling the mysterious principles which controlled the … practical operations of the synthetic organic chemist" [2] put Hammett on the path he followed for the rest of his life. At Harvard he met James Bryant Conant, who was a graduate student and teaching assistant while Hammett was an undergraduate and who also contributed to physical-organic chemistry. Hammett graduated summa cum laude, won a Sheldon Traveling Fellowship, and in 1916 in the middle of World War I went to Zurich to work with Hermann Staudinger.

On his return to the United States in 1917 he expected to be inducted into the Army; he found instead that he was assigned to laboratory work and at the age of twenty-three was put in charge of a group investigating paints and varnishes and especially "dopes" for the fabrics of airplane wings. Hammett wrote, "We did some respectable developmental research … none of which ever got published." Research that Hammett would describe as "respectable" must have been very good indeed.

Shortly after the end of World War I, Louis Hammett had the remarkable good judgment and good fortune to marry Janet Marriner, also from Portland. They had a long and happy married life, raised two successful children, and were blessed with five grandchildren. Janet also entertained Louis's graduate students and postdoctorals, and kept him from becoming too intense. Louis listed "good company" as his leisure interest, and so it was. Janet reported on one occasion that Louis had been born two drinks under par, and although sometimes he did seem a bit formal, a bit stiff, he was always good company, with or without a drink, and Janet was too.

SCIENCE

Hammett did not begin his spectacular career immediately after the war, but started with a mundane industrial job. In the spring of 1920, however, he accepted an instructorship at Columbia, where he taught and simultaneously carried out research on the hydrogen electrode under the supervision of Hal Beans. He wrote that he could not have managed financially without a subsidy from his father-in-law, a situation that anticipated that of some graduate students today. When he received his Ph.D. in 1922 he was not "besieged with offers of highly remunerative positions." On the other hand, his work at Columbia had progressed nicely. He was especially influenced by four men, two he never met and two he met only much later: A. Hantzsch, A. Werner, G. N. Lewis, and J. N. Bronsted. Appropriately, he noted too that you did not have to work as a postdoctoral in someone's lab to be influenced by that person; thousands have been strongly influenced by Hammett, usually because of *Physical-Organic Chemistry*, even though they never met the man himself.

THE ACIDITY FUNCTION

Hammett's first great contribution to chemistry was the concept of superacidity and his acidity function. In 1928 he suggested that HCl is a stronger acid in solution in benzen,[3] where it cannot ionize, than in water, where it is fully ionized. Today we accept as obvious the leveling effect of water and similar basic solvents on acidity, but when Hammett advanced this idea it was considered paradoxical at best. Incidentally, James Bryant Conant was also interested in superacid solutions, however he was less successful than Hammett in developing the concept.

The chemistry of concentrated sulfuric acid is startlingly different from that of more dilute acid. Hammett grasped the essential fact that concentrated sulfuric acid and similar acids have acidities out of all proportion to their concentrations. With A. H. Deyrup he set up an acidity scale[4] based on the indicator properties of aromatic amines, a scale that measures 100% sulfuric acid as 10^{10} times as strong (that is, ten billion times as strong) as 10% acid, and then showed that this acidity scale was relevant to chemistry.

The measurement scheme of Hammett and Deyrup was as follows. They found an indicator (p-nitroaniline) that functioned in dilute aqueous acid, where the ordinary pH scale is valid, and determined its pK. They then defined an acidity function, H_o, as $H_o = pK + \log (B)/(BH^+)$, where (B) and ($BH^+$) are the concentrations, not the activities, of the basic indicator (here p-nitroaniline) and its conjugate acid. In solutions where the acidity was too great to allow a meaningful determination of pH, the ratio of (B) to (BH^+) could still be determined colorimetrically, and so define H_o.

Of course, this scheme could not be carried very far, because when the ratio of (B) to (BH^+) becomes especially small, it becomes experimentally impossible to measure it accurately. But if the ratio of (B)/(BH^+) is first obtained when it is, say, 10 to 1, and then obtained again in a more acidic solution where it is, say 1 to 10, an investigator will have a solution 100 times, or 2 log units, more acidic, as defined by H_o. One can then try another, less basic colorometric indicator, one that is perhaps only 1/10 protonated in this second, more acidic solution, and from this fact and the H_o of the solution determine its pK. Now in still stronger acid, one can determine H_o and then determine the pK of a third indicator and so on, leapfrogging across the acidity scale. It was in this way that Hammett showed that his new acidity function measured 100% sulfuric acid as stronger than 10% acid by a factor of 10^{10}.

It behaves that way in chemical synthesis. With Martin Paul, Hammett pointed out the relationship between his acidity function and the rates of acid-catalyzed reactions.[5] Similar functions based on Hammett's principles even provided other acidity scales, including one that changes 3000-fold between 80% and 90% sulfuric acid, corresponding to the chemical fact that the nitration of nitrobenzene proceeds 3000 times as fast in 90% as in 80% sulfuric acid.[6] One needs such acidity scales to understand the enormous change in rate of reaction that occurs with relatively small changes in the concentration of sulfuric acid. Similar scales have been extended beyond 100% sulfuric acid into true superacidic media, such as fuming sulfuric acid, or solutions of HF in BF_3. George Olah won the 1995 Nobel Prize in chemistry for his demonstration of the catalytic effects of such superacid solutions.[7]

Finally, R. P. Bell showed that Hammett's acidity function really measures the activity of water in superacid solutions. Water binds strongly to protons and reduces their activity. With this work the concept of superacidity and the measurement of superacidity become readily comprehensible.[8]

Hammett and others also expanded the concept of basicity to superbasic solutions and invented an H_ function to describe them. [9]

PROTONATION OF CARBONYL GROUPS

The protonation of a carbonyl group has been postulated as the first step in the mechanisms of many acid-catalyzed reactions. Such mechanisms can be supported by measuring the extent of protonation of aldehydes, ketones, acids, esters, and amides. The ultraviolet spectrum of a carbonyl group markedly changes when it is protonated, and the extent of protonation can be determined from the ultraviolet spectra of solutions of carbonyl compounds in strong sulfuric acid.[10] In fact, carbonyl compounds can be used as a set of Hammett indicators, different from but parallel to the aromatic amines used to set up the H_o function. This work is experimentally easy today with electronic spectrophotometers, but Hammett and Flexser determined the pK of acetophenone and benzoic acid as bases before the Beckman DU spectrophotometer or any such instrument had been invented. Flexser did his ultraviolet spectroscopy photographically, using a grating spectrophotometer. The amount of work involved was considerable, but an important concept was established.

THE HAMMETT EQUATION

Hammett's second major contribution to physical-organic chemistry concerned his σρ equation, commonly called the Hammett equation. This concerns the correlation of equilibria and rates for reactions of substituted aromatic compounds. Chemists had long realized that reactions with large negative free energies may nevertheless be extremely slow. For example, the equilibrium position for Egyptian sarcophagi and air is practically completely to the side of carbon dioxide and water, yet for millennia no detectable reaction has occurred with the samples now in our museums. Nevertheless, Hammett showed how, within certain limited sets of reactions, rates do follow equilibria. The standard to which he referred other reactions is the ionization of substituted benzoic acids, and he defined substituent constants, σ, by the equation:

$$\sigma = \log K_s/K_o$$

where σ is the constant for the substituent s, K_s is the ionization constant for the substituted benzoic acid, and K_o is the ionization constant for benzoic acid itself. He then demonstrated that the equation:

$$\log k_s/k_o = \sigma\rho$$

holds with remarkable fidelity for the rate of a wide selection of reactions of meta- and para-substituted aromatic compounds, where a single constant ρ is adequate to define each reaction and the substituent constants are those defined above. The fact that the equation does not hold for ortho-substituted aromatic compounds presumably means that here steric effects complicate what are otherwise resonance and electrostatic ones.

The Hammett equation is an expansion—a large expansion—of the BrØsted equation. The latter relates acid and base catalysis to acid-ionization constants; the Hammett equation interrelates all sorts of reactions—saponification of esters, nucleophilic displacement reactions, bromination of substituted acetophenones, solvolysis of benzoyl chlorides, and many more.

PHYSICAL-ORGANIC CHEMISTRY

In 1940 Hammett published his revolutionary text *Physical-Organic Chemistry*. The book established the field and proved much more important than Christopher Ingold's

Structure and Mechanism in Organic Chemistry.[11] The experimental underpinnings of those reaction mechanisms that were then known were clearly set out and the principles for the determination of mechanism were established. The large community of physical-organic chemists throughout the world—especially in the United States—that thrived over the next decade or two based its work largely on Hammett's *Physical-Organic Chemistry*. In 1970 he published a second, enlarged edition. But, necessarily, the second edition points out the difficulties as well as the triumphs of the acidity function and exceptions to general rules, and somehow had much less influence than the first. The first edition of Hammett's book stands as one of the great textbooks in chemistry, at least comparable in its impact to *Thermodynamics* by Lewis and Randall[12] or the first edition of *Biochemistry* by Fruton and Simmonds.[13]

In the second edition of his famous book Hammett recognized that "It would be … hypocritical humility for me to pretend that I do not know that [the σρ equation] is commonly called the Hammett equation, or that I am not grateful to those who have honored me in this way …."

THE EXPLOSIVES RESEARCH LABORATORY

The Second World War had started by 1940, and Hammett's efforts were diverted from pure chemistry to national service. The National Defense Research Committee established an Explosives Research Laboratory outside Pittsburgh at Bruceton, Pennsylvania, on the site of the Bureau of Mines with George Kistiakowsky as director and Hammett as associate director. Later in the war when Kistiakowsky moved to Los Alamos, Hammett took on the job of director. The Bruceton lab was remarkably successful even though one industrial chemist predicted that a group of college professors would blow their own heads off and one admiral announced that he already knew what there was to know about explosives. The lab made several important inventions that were useful in the war and after, and it did not sustain a serious accident.

In particular, Kistiakowsky and his lieutenants developed the explosive lenses that effected the implosion needed for the plutonium bomb. Hammett, Frank Long, and their coworkers invented and developed a jet-assisted takeoff system based on a new ammonium perchlorate propellant that was used to help overloaded planes to get off the runway with their bombs and fuel. John Kincaid invented a method of making large "grains" of rocket propellant—grains of thousands of pounds—that permitted the manufacture of the solid rockets that propelled some of the ICBMs, and others at the lab made several minor inventions as well.

The laboratory under Hammett was friendly and productive, perhaps friendly because it was productive and vice-versa. In any event, under Hammett's management it functioned more than efficiently; it functioned imaginatively. The civil service employees of the Bureau of Mines were left behind on the property when the lab was turned over to Kistiakowsky, Hammett, and their new colleagues; and the civil service was responsible for the management of the property. In some instances they didn't seem fully aware that the lab was instituted to help with the progress of World War II. They certainly taught many of the academics some extraordinary and unwelcome lessons about the civil service. Nevertheless, the laboratory was unquestionably a huge success and that success was due in considerable part to Hammett's effective management.

THE POSTWAR YEARS

At the conclusion of the war Hammett returned to Columbia. He wrote [14] "Emotionally exhausted by my wartime experiences, I returned to a university which welcomed me with less than open arms." Joseph Mayer and Harold Urey, two of the superstars of the chemistry faculty at Columbia, had been lured away to the University of Chicago, and Hammett was not appointed chairman until 1951. He felt hurt and said so in a handwritten note filed in the archives of the National Academy of Sciences. In 1951, when he was finally appointed chairman, he supervised the buildup of the Chemistry Department at Columbia and in particular oversaw the professorial appointments of Gilbert Stork, Ronald Breslow, and Cheves Walling. Hammett took emeritus status in 1961 and in 1973 he retired to the Quaker community of Medford Leas. He died there in 1986 at the age of ninety-two.

Although Hammett's postwar years were not entirely happy ones, he received a number of signal honors during that period. He was of course elected to the National Academy of Sciences. He refused to stand for election for president of the American Chemical Society, but served as its chairman of the board. He received the National Medal of Science from President Johnson and a number of other significant honors, including the Chandler Medal, the James Flack Norris Award (twice), the Willard Gibbs Medal, and the Lewis Medal. He was elected an honorary member of the Chemical Society (British) and in 1962 received an honorary degree from Columbia. He served as visiting professor at several universities. These are numerous and splendid distinctions, but his real honor is the esteem in which he is held by his colleagues throughout the world, who rightly regard him as a true pioneer in physical-organic chemistry. Perhaps he sensed at least part of that great esteem.

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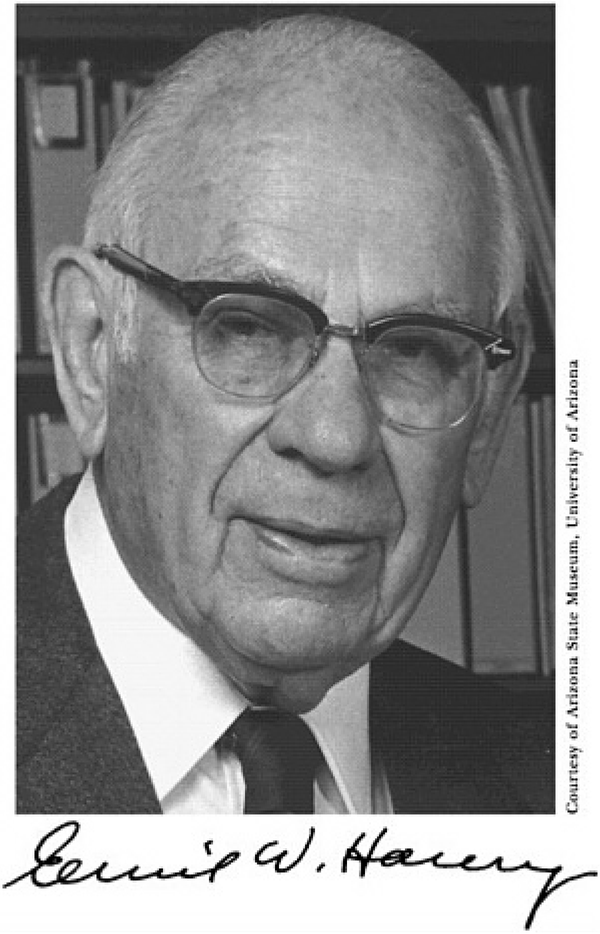

Courtesy of Arizona State Museum, University of Arizona

EMIL WALTER HAURY

May 2, 1904–December 5, 1992

BY RAYMOND HARRIS THOMPSON, CALEB VANCE HAYNES, JR., AND JAMES JEFFERSON REID

AMERICAN ARCHAEOLOGY HAD difficulty overcoming its antiquarian origins until the first decades of this century when Alfred Vincent Kidder (1885–1963, NAS 1936) shifted the emphasis in the southwestern region of the country from whole pots and cliff dwellings to potsherds and culture history. His *Introduction to the Study of Southwestern Archaeology* , published in 1924, was the first synthesis of the prehistory of any North American region based on professionally recovered empirical data. A handful of pioneering archaeologists in several regions of the country completed the transformation initiated by Kidder.

Emil Haury was preeminent among these regional archaeologists. Influenced and inspired by Kidder, Haury kept the Southwest in the forefront of these early paradigm shifts in American archaeology. He was responsible for accumulating much of the evidence that gives the Southwest the most complete culture history of any region of North America. The Southwest, with its spectacular landscapes, well-preserved ruins, and surviving Indian communities had long fascinated eastern and midwestern Americans, including young Emil Haury. He went to Arizona in 1925 to study with Byron Cummings (1860–1954), who had been exploring cliff dwellings in southern Utah and northern Arizona since before the turn of the century. During the following four decades, Haury, building on the humanistic and antiquarian foundations laid by Cummings and others and following the scientific leads of Kidder, developed an understanding of southwestern prehistory that continues to be the basis of our perception of the region today. He devoted his long and productive career to development of field procedures for recovery of empirical data, establishment of chronological controls, setting of high performance standards, scientific training of students, creation of enduring educational and research institutions, and definition of a rational, evidence-based, environmentally sound, anthropological approach to the study of the past. He campaigned successfully for the formulation of a national policy for the protection of archaeological resources and for passage of laws to carry out that policy. He had a powerful influence on the shape and character of archaeology in American society. An internationally respected anthropologist and a member of the National Academy of Sciences since 1956, Haury died at home in Tucson, Arizona, on December 5, 1992.

PERSONAL HISTORY

Haury was born on May 2, 1904, in Newton, Kansas. His Mennonite grandparents had left Germany in the 1850s, seeking land, religious tolerance, and freedom from military service. His paternal grandparents left Bavaria and settled in Iowa, where Emil's father, Gustav Adolf Haury (1863–1926) was born. His mother, Clara Katharina Ruth (1865–1935) was born in Illinois, where her Bavarian parents had settled. Both the Haury and Ruth families moved to east-central Kansas, the main region of late nineteenth-century Mennonite settlement in this country. Gustav Haury and Clara Ruth were married on June 11, 1891, and raised a family of four sons: Irwin, Gustav, Alfred, and Emil. The elder Gustav was one of the five founding faculty members of Bethel College in Newton, the nation's oldest Mennonite institution of higher learning. A professor of English and Latin, Emil's father was a highly respected faculty member who served Bethel College in many capacities for thirty-three years. Emil grew up in the modest, comfortable, and orderly family environment of the liberal Mennonite community of Newton, a small midwestern town that produced three professional archaeologists, all of whom studied at the University of Arizona: Haury, Waldo Rudolph Wedel (1908–96), and Roland Richert. Wedel, also the son of a Bethel founding faculty member and Emil's close boyhood friend, had a long and distinguished career at the Smithsonian Institution as an authority on Plains archaeology. Richert, whose father was a mathematics professor at Bethel College, served many years as an archaeological specialist in the southwest region of the National Park Service.

Young Emil, fascinated by American Indians, read many adventure stories about Indians and developed an interest in archaeology. His desire to become an archaeologist was fueled by a black-on-white potsherd that his parents picked up at the Walnut Canyon cliff dwellings near Flagstaff on a trip to Arizona in 1908. Emil pondered over that sherd and about Arizona Indians and years later remembered the sherd well enough to identify it in the taxonomic system for prehistoric southwestern pottery. He learned something about Arizona Indians as a youngster when his parents provided lodging for a young Hopi woman, Polingaysi Qoyawayma (Elizabeth Q. White, 1892–1990), who had been sent to Bethel College by Mennonite missionaries to the Hopi. A school teacher and a potter, she was an effective culture broker in a period when the Hopi people suffered many indignities while being forced to adapt to American ways. Emil and she maintained contact with one another throughout her lifetime.

Haury attended public elementary school in Newton from 1910 to 1919 and high school at the Bethel Academy from 1919 to 1923. He went on to Bethel College for two years and might have become a high school teacher, as did many Bethel students, had Byron Cummings not visited Newton in 1924 to lecture on archaeology. Cummings had been persuaded by his friend Emil Richert Riesen (1884–1956), a former Bethel faculty member teaching philosophy at the University of Arizona, to stop in Newton on a trip east to seek funding for his excavations at Cuicuilco in the Valley of Mexico. Haury met Cummings at that lecture and asked to be included in the exploration of ruins in northern Arizona. This contact resulted a year later in an invitation from Cummings to participate in the third season at Cuicuilco, after which Emil accompanied Cummings to Tucson to complete his undergraduate education at the University of Arizona. He also studied with geomorphologist William Morris Davis (1850–1934) and paleontologist Alexander Stoyanow (1879–1974). He and two fellow students, Clara Lee (Fraps) Tanner and Florence May (Hawley) Ellis (1906–91), earned bachelor's degrees in archaeology in 1927.

Cummings was a warm and generous man who took a deep interest in his students. Haury continued that tradition with his own students, as those of us who have benefited from the warmth and support of the Haury family can attest. Emil's relation with Cummings went beyond the role of student; he was research assistant, right-hand man, and chauffeur for Cummings, especially in 1927 when Cummings was the ninth president of the University of Arizona. Cummings invited his three new graduates to attend the nation's first regional archaeological conference that

Kidder held in August 1927 at his excavation in Pecos, New Mexico. Florence Hawley was unable to go because her mother did not think that the group had an adequate chaperon, but Emil and Clara Lee benefited greatly from the opportunity to be student observers as the leading southwesternists debated the issues identified by Kidder. Haury twice hosted the Pecos Conference (in 1948 and 1951) at his excavations at Point of Pines in east-central Arizona and strongly supported it as an important forum for the exchange of field research results.

Cummings encouraged the three new graduates to continue their studies and they became the first recipients of master's degrees in archaeology at the University of Arizona. Cummings made them instructors for the 1928–29 academic year, thus launching them on their lengthy teaching careers, Emil and Clara Lee at the University of Arizona and Florence at the University of New Mexico. Employment made it possible for Emil to get married. He and Hulda Esther Penner (1904–87) were married in Newton on June 7, 1928, by her father Heinrich Daniel Penner (1862–1933), a distinguished minister and also one of the five founding faculty members of Bethel College. Hulda, the second youngest in a family of twelve, was born on February 27, 1904, in Hillsboro, Kansas. Her oldest sister, Rachel Rebecca Penner (1884–1956), was the wife of Professor Riesen on the faculty at Arizona. After graduating from Newton High School in 1921 and completing two years at Bethel College, Hulda taught elementary school in Brewster and Newton. She completed a bachelor's degree in music and German at the University of Arizona in 1961. Emil and Hulda had two sons: Allan Gene, an engineer born in 1934, and Loren Richard, a biological oceanographer born in 1939. Hulda died February 20, 1987.

Emil and Hulda came from a liberal Mennonite world that placed high value on both education and progress, he from a German, she from a Russian homeland. Heinrich Penner and his wife, Katharina Dalke (1864–1944), were born in the large Molotschna colony established in southern Russia when in 1786 Catherine the Great exempted Prussian Mennonites from military service if they would settle on undeveloped land near the Black Sea. When the policy on military service changed, large numbers of Mennonites migrated in 1874 to Kansas, where they improved the growing of wheat, as well as flour milling and bread making, by introducing hard rust-resistant winter wheat.

Although the liberal Kansas Mennonites had no religious restrictions on transportation, electricity, and style of clothing, they shared many of the stern attitudes toward social behavior and biblical interpretation that characterized most Protestant groups in this country in the early years of this century. Hulda's father presented liberal ideas about the Bible for which he was both criticized and admired. Although Emil and Hulda abandoned many Mennonite doctrines, they never lost the traditional Mennonite values of hard work and industry, honesty and integrity, cleanliness, simplicity of living, personal loyalty, cooperation, and help for others.

DENDROCHRONOLOGY

In 1929 Haury became a research assistant to Andrew Ellicott Douglass (1867–1962), who instilled an appreciation in him for the power of the scientific method. As a result of his association with Douglass, Haury became a key figure in the development of tree-ring dating. An astronomer, Douglass had come to Arizona in 1894 to locate a site for the observatory Percival Lowell (1855–1916) would use in his study of Mars and its so-called canals. Douglass criticized Lowell's methods and interpretations, lost his position in 1901, and five years later joined the faculty of the University of Arizona. Douglass was interested in long-term climatic change and hoped to find evidence of past sun spot activity in the growth record of coniferous trees. The primary product of that research was the development of dendrochronology or tree-ring dating, the most accurate method of dating archaeological events in the absence of a written historical record. Douglass had assembled a chronology extending from the present back to about A.D. 1260 using wood from living trees, historic buildings, Hopi pueblos, and prehistoric sites. He also had an earlier "floating" chronology of 585 years based on the cross-dating of archaeological timbers. He employed Haury and Lyndon Lane Hargrave (1896–1978), also a student of Cummings, to search for archaeological tree-ring specimens in sites with styles of pottery characteristic of the time gap between the two chronologies. On June 22, 1929, they found a charred beam fragment at the Show Low ruin that enabled Douglass to close the gap and date most of the well-known sites in the Southwest. Emil often spoke of that discovery as the most memorable experience of his career.

Haury, the first person to learn the Douglass method of dating, spent the following year processing the huge backlog of specimens Douglass had accumulated. In the spring of 1930 he also assisted Douglass in teaching the first course on tree-ring dating at the University of Arizona. Haury played a critical role in the subsequent development of dendrochronology. He set up a tree-ring laboratory at Gila Pueblo and in his landmark excavation of the Canyon Creek ruin provided the first significant contribution to the theory of archaeological tree-ring dating theory, demonstrated the importance of sampling beams from all parts of a site, and pointed out the value of beams as artifacts for inferring past behavior. In 1937 Douglass, astronomer Edwin Francis

Carpenter (1898–1963), and Haury were co-founders of the Laboratory of Tree-Ring Research at the University of Arizona, with Douglass as its first director. Haury trained and mentored three of the directors who guided the laboratory through the postwar years of growth and stabilization. Although Douglass is the father of tree-ring dating, it was Haury who provided the critical long-term, moral, intellectual, and administrative support. The Laboratory of Tree-Ring Research has expanded beyond the dating of archaeological sites to become an international center of biological, hydrological, and climatic research that is addressing the very problems that stimulated Douglass to begin his studies of tree growth almost a hundred years ago.

GILA PUEBLO

In 1930 Haury became assistant director of the Gila Pueblo Archaeological Foundation that Harold Sterling Gladwin (1883–1983) had established two years earlier in Globe, Arizona. Gladwin sold his seat on the New York Stock Exchange in 1922 and moved to Santa Barbara, California, where he met two people who changed his life. Fellow New Yorker Winifred Jones MacCurdy (1889–1965) helped found Gila Pueblo and in 1933 became Mrs. Gladwin. William North Duane (1869–1944) arranged a camping trip to northern Arizona with his cousin A. V. Kidder. Gladwin was enchanted both by the region and its prehistory and spent several seasons (1925–27) with Kidder at Pecos. Kidder became a member of the Gila Pueblo board and provided encouragement and advice to Gladwin for many years. Gladwin became totally fascinated by the archaeology of the Southwest and devoted his wealth and intellect to an almost feverish effort to survey the entire region, formulate new problems, challenge established positions, excavate key sites, and publish results in a timely manner. He was well into the first phase of this herculean effort in 1930 and needed a professionally trained archaeologist to help him carry it out. Haury was a likely candidate not only because of his extensive field experience but also because of his knowledge of dendrochronology, for Gladwin, who had already begun to challenge Douglass, wanted to have his own tree-ring laboratory.

Haury's move to Gila Pueblo was perhaps his most important career decision, for it provided him with an unparalleled opportunity to do field research without the distractions of the academic and museum worlds. He chose Gila Pueblo rather than continuing with Douglass, teaching at the University of Arizona, or accepting a position at the U.S. National Museum. Gladwin's intellectual charisma and the prospect of extensive field work with prompt publication were important considerations. Another was Gladwin's willingness to provide half pay for two years of doctoral study if Emil would work for three years without a salary increase after earning the degree. Emil spent the 1931–32 and 1932–33 academic years at the Department of Anthropology at Harvard University, where he studied with archaeologist Alfred Marston Tozzer (1877–1954, NAS 1942), ethnographer Roland Burrage Dixon (1875–1934), and physical anthropologist Earnest Albert Hooton (1887–1954, NAS 1935). He had hoped to write a dissertation on the application of tree-ring dating in Egypt, but took Tozzer's advice and analyzed a large collection from southern Arizona excavated by Frank Hamilton Cushing (1857–1900) and the Hemenway Expedition in 1887–88. The resulting dissertation on the classic period of the Hohokam culture, written under Dixon's supervision, earned Haury a Ph.D. in anthropology in 1934. Published in 1945, his dissertation remains a basic reference for late Hohokam prehistory.

The years at Gila Pueblo gave Haury a breadth and depth of field experience throughout the Southwest that is unique in the history of southwestern archaeology. He carried out extensive archaeological surveys in the Grand Canyon, the Sierra Ancha, the White Mountains, the Mimbres region, and southeastern Arizona. The Haurys accompanied the Gladwins on a wide-ranging survey to identify the western limits of the distribution of red-on-buff pottery. Emil excavated key sites in the plateau and canyon country of northern Arizona, the mountains of central Arizona and west-central New Mexico, and the southern Arizona deserts. Emil's stature, strength, and health enabled him to endure the rigors of travel and work in the sparsely populated, largely unmapped, and mostly roadless Depression-era Southwest. Because Gladwin insisted on prompt publication, Haury had many opportunities to present his conception of southwestern prehistory. He defined the Mogollon culture of the mountain zone of the Southwest and gave substance to Gladwin's Hohokam culture of the southern Arizona desert in the pages of the *Medallion Papers* published by Gila Pueblo.

By 1936 difficulties began to develop in the Haury-Gladwin relationship. Gladwin's tremendous creative energy, his insatiable desire to solve all the problems in southwestern prehistory simultaneously, his persistent challenge of established views, and his propensity (even though often tongue in cheek) to espouse unconventional, even outlandish ideas meant that though life at Gila Pueblo was stimulating, it was also tense. A. E. Douglass, who had his own problems working with a creative and wealthy employer (Percival Lowell), had warned Emil that it was often difficult in small, private organizations to deal with differences in an impersonal and objective manner. It was fortuitous, therefore, that Cummings came to Globe in the fall of 1936 to announce the start of his retirement at the end of that academic year and to ask if Emil would be interested in becoming head of the Department of Archaeology at the University of Arizona. Although the Gladwins at first approved of the proposed move, misunderstandings over the offer from Arizona added stress to the Haury-Gladwin relationship. Haury, enriched by seven years of intensive research in all regions of the Southwest, left Gila Pueblo and began his long and productive career at the University of Arizona in July 1937 as assistant professor and head of the Department of Archaeology at age thirty-three. After the full retirement of Cummings the following year, Haury was promoted to professor and appointed director of the Arizona State Museum.

UNIVERSITY OF ARIZONA

When Emil assumed the leadership role at Arizona, the university was a small land-grant institution, the state was a cattle-raising and mining frontier, and the nation was in the grip of the Great Depression. The legislature had drastically reduced the university budget and the faculty had been forced to take a series of salary cuts. The regents, governor, legislators, and various newspaper editors were busy trying to manage the affairs of the university, and the strong president, who had guided the institution through the early years of the Depression, had resigned in 1936. Cummings urged Haury not to make changes during this critical period, but his brother-in-law, Professor Riesen, now dean of the College of Liberal Arts, advised him not to waste the advantage of being a newcomer. Emil took his brother-in-law's advice and began a vigorous campaign to increase the budget, size of the faculty, library holdings, and student support. He changed the name of the Department of Archaeology to Department of Anthropology at the beginning of the 1937–38 academic year, recognizing and continuing the four-field anthropological breadth in the curriculum that Cummings had been developing since 1915.

One of Haury's most important goals at the University of Arizona was the creation of a nationally competitive doctoral program. He mentioned this goal in his very first annual report, but recognized the need to expand the faculty, curriculum, and library holdings before proposing a new degree program. He continued to press for a doctoral program and his persistence was rewarded in 1948 when the University of Arizona was authorized by the Board of Regents to offer a Ph.D. in anthropology. The first graduates, Charles Corradino Di Peso (1920–82) and Joe Ben Wheat received their degrees in 1953. By the time of Emil's retirement in 1980, the Arizona Department of Anthropology had awarded 175 doctoral degrees, thirty of them under his direction. Although many other universities developed doctoral programs in subsequent years, only Arizona and UCLA (which also began its program soon after World War II) have entered the ranks of the top ten graduate programs in anthropology. That Emil achieved his goal of creating a nationally competitive program is symbolized by the fact that Arizona has ranked fifth in the last two rankings of graduate programs in anthropology by the National Research Council of the National Academy of Sciences.

The close relationship between experience in the field and learning in the classroom was at the very core of Emil's educational philosophy. He was a consummate field archaeologist. His many years of varied field experience coupled with his superb observational skills gave him a unique ability to extract fascinating bits of information from the most recalcitrant of archaeological contexts. He had a legendary reputation for identification and interpretation of small and unprepossessing potsherds. He believed that there should be ongoing opportunities for student field experience, but found it difficult to create such opportunities during the academic year. He had great success in the development of a summer archaeological field school, an approach Cummings had begun in 1919 with "A Summer Course Among the Cliff Dwellers" and was continuing at Kinishba on the Fort Apache Indian Reservation at the time of his retirement. Haury built on the Cummings tradition with a field school at Forestdale, also on White Mountain Apache land in east-central Arizona (1939–41), and later at Point of Pines to the south on the reservation of the San Carlos Apache (1946–60). He believed that a field school had to offer students more than just the thrill of digging. The experience gained from the physical participation in excavation had to be supplemented by experience in field laboratory procedures and by lectures and discussions, even though such educational activities slowed down the research itself. The standards for field school training that he set at Point of Pines at the beginning of the postwar period have served field training programs throughout the country.

THE ARIZONA STATE MUSEUM

As director of the Arizona State Museum, Haury faced a different set of challenges. Cummings had been progressive in his conception of anthropology as a teaching department, but with respect to his concept of a museum, he was very much a product of the turn of the century. He was an indefatigable but indiscriminate collector who believed that everything possible should be exhibited. His exhibits were a kind of antiquarian hodgepodge with little interpretation and he paid only limited attention to basic museum concerns such as record keeping, storage, and conservation. In all fairness, however, Cummings had no staff and practically no budget. Haury was faced with the same problems and although he changed policies, it took years to build the staff and resources to implement them. In 1944 Haury was able to invite an old Gila Pueblo colleague, Edwin Booth (Ted) Sayles (1892–1977) to be curator. When Emil retired from administration in 1964 after twenty-six years as director, he had increased the staff to thirteen and the budget from less than $7,000 to almost $110,000. Emil's policies brought order and professional standards to the museum. He reduced the clutter in the exhibit hall, produced explanatory labels, developed collection guidelines, introduced a modern catalogue system, and generated a concern for conservation.

Haury also gave a great deal of attention to the statewide responsibilities of the state museum for the protection of archaeological resources. In 1938 he established the Arizona State Museum site survey by expanding the system the Gladwins had established for the Gila Pueblo surveys. In 1950 he negotiated an agreement with the state land commissioner requiring a museum permit for archaeological work on state land and in 1959 convinced the Arizona Highway Department to establish the Arizona Highway Salvage Program. In 1960 he orchestrated the passage of a new Arizona Antiquities Act that corrected flaws in the law Cummings had obtained in 1927. He played an influential role in the creation of the Arizona Parks Board in 1957 and the subsequent designation of the state parks director as liaison officer (now state historic preservation officer) for the preservation programs of the National Park Service.

Two important gifts greatly increased the museum's holdings during Emil's directorship. Gladwin gradually lost interest in maintaining Gila Pueblo as an active research institution and in 1950 gave its collections and assets to the Arizona State Museum, more than doubling the museum's holdings of prehistoric southwestern ceramics. This gift guaranteed that the materials accumulated as a result of Gladwin's massive and seminal impact on southwestern archaeology would continue to play an important role in research on the prehistory of the region. In 1957 the Museum received as a bequest from Victor Rose Stoner (1893–1957), a Catholic priest in the Diocese of Tucson who had earned an M.A. under Cummings, a large library of rare and valuable materials on southwestern archaeology and ethnohistory. This bequest enabled Haury to establish the Arizona State Museum Library, which has benefited from many other gifts to become one of the nation's best anthropological research libraries.

REGIONAL AND NATIONAL INFLUENCE

Haury's institution-building activities at the University of Arizona were not limited to the Department of Anthropology and the Arizona State Museum. In addition to playing a key role in the development of the Laboratory of Tree-Ring Research, he helped establish the Geochronology Laboratories and the Office of Arid Land Studies, the Radiocarbon Age Determination Laboratory (now part of the Laboratory of Isotope Geochemistry), the Bureau of Ethnic Research (now the Bureau of Applied Research in Anthropology), and the University of Arizona Press. It was quite a shock for Haury to move from Gila Pueblo where Gladwin required publication as soon as possible after field work to the University of Arizona where there were very limited opportunities for scholarly publication. The University of Arizona Press was founded in 1959, along with the *Anthropological Papers*, as a direct result of Emil's persistence.

Haury was also active in professional affairs both regionally and nationally. In 1938 he and Frederic Huntington Douglas (1897–1956) of the Denver Art Museum, founded the Clearinghouse for Southwestern Museums, today the

Western Museums Association. He served on the boards of most of the anthropological organizations in the Southwest: Southwest Parks and Monuments Association, which established an annual award in his name (1938–83); Laboratory of Anthropology in Santa Fe (1938–60); Museum of Northern Arizona in Flagstaff (1938–82); Heard Museum in Phoenix (1940–54); and Amerind Foundation in Dragoon (1982–92). He played an important role in the development of the social sciences within the National Science Foundation; promoted federal action on threatened archaeological sites as the representative of the American Anthropological Association to the Committee for the Recovery of Archaeological Remains; provided leadership in the early sixties for many of the activities of the National Academy of Sciences; influenced federal conservation policy as a member of Interior Secretary Stuart Udall's National Park Service Advisory Board; and served on the National Council on the Humanities. He was an active president of the American Anthropological Association in 1955, although his presidency of the Society for American Archaeology in 1943–44 was just the opposite because of the second World War.

Haury was widely recognized for his achievements: Viking Medal for Archaeology in 1950; National Academy of Sciences in 1956, the first member of the Arizona faculty to be so honored; honorary LL.D. from the University of New Mexico in 1959; American Academy of Arts and Sciences in 1960; Salgo-Noren Foundation Award for Excellence in Teaching in 1967; American Philosophical Society in 1969; Fred A. Riecker Distinguished Professor of Anthropology in 1970, the first holder of the University of Arizona's first endowed chair; Conservation Service Award of the Department of the Interior in 1976; and the Alfred Vincent Kidder Award for Eminence in the Field of American Archaeology in 1977. Two funds in the Department of Anthropology at the University of Arizona, the Education (now Haury) Fund for Archaeology (1980) and the Emil W. Haury Graduate Fellowships (1990) honor Emil's long-standing desire to provide greater support to students.

RESEARCH ACCOMPLISHMENTS

One of the preeminent archaeologists of the twentieth century, Emil Haury was a perceptive researcher and a master teacher, a skilled administrator, and a dedicated institution builder. The uniqueness of his contributions, however, derives primarily from the breadth and depth of his archeological research in the greater Southwest. He surveyed more of that territory, excavated more sites in it, observed more details of its prehistory, and gained a more sensitive perspective of its problems than any of his contemporaries. He possessed an enormous store of regional knowledge, witnessed first hand, that enabled him to identify key problems, select analytically appropriate sites for investigation, and make interpretations of lasting value because they were consistent with the quality and character of empirically recovered data. When Haury entered the field, little was known of the earliest periods of the occupation of the New World. The Anasazi culture found in the cliff dwellings and pueblos of the northern periphery of the region was thought to be characteristic of the entire region, possibly diluted or elaborated in varying degrees. Kidder recognized the inadequacy of this situation and recommended more research in the less known parts of the region. Haury was a key player in carrying out that recommendation, first under Gladwin's energetic and creative aegis and later on his own.

Emil first became interested in Paleoindians, the earliest inhabitants of the New World, during his senior year in college when he assisted Cummings in the excavation of a nearly complete mammoth skull overlying artifacts of the

Cochise or Archaic culture at Double Adobe in southeastern Arizona. He subsequently excavated two mammoth kill sites in the same region. The Naco site was the first occurrence of Clovis fluted points west of the continental divide. The Lehner site was the first site to yield secure dating by the radiocarbon method and the first Clovis site with elements of extinct fauna other than mammoth. His excavations at Ventana Cave southwest of Tucson revealed a 12,000-year stratigraphic sequence from Paleoindian times to the present, confirming a relative sequence of cultures in southern Arizona that had been pieced together from many sites with only limited stratigraphic information. To this day, Ventana Cave contains the most complete stratified Archaic sequence in the Southwest.

Haury collaborated with Sayles and geologist Ernst Valdemar Antevs (1888–1974) in the definition of the Cochise culture, the first evidence for the Archaic cultures that followed the Paleoindian big game hunters. His excavation of the Cienega Creek site at Point of Pines demonstrated the presence of Archaic peoples in the mountain zones possibly somewhat later than in the desert valleys. The site produced the earliest cremations and earliest evidence of tobacco smoking in the Southwest. He was deeply interested in the transition from the hunting and gathering cultures of the Archaic to the pottery-making farmers of later times. His work at the Matty Canyon sites suggested that late Archaic people had a maize farming economy derived from Mexico for as much as a thousand years before the introduction of pottery. Recent work by Bruce Benjamin Huckell, one of Haury's last students, and others has fully demonstrated the existence of large prepottery Archaic farming villages. At the other end of the time scale, Haury had a keen interest in the historic period, especially the trail of Francisco Vásquez de Coronado's futile search for the Seven

Cities of Cábola. Haury believed that Coronado must have had a major base camp somewhere in southeastern Arizona, probably in the San Pedro valley. His interest included research on the Spanish presidio of Santa Cruz de Terrenate and the search for Quiburi by one of his first Ph.D. students, Charles Di Peso.

Gladwin, following Kidder's advice, focused his attention first on the archaeology of the southern Arizona desert. He proposed that a desert Hohokam culture be distinguished from the Anasazi culture of the northern plateau region. Haury joined Gila Pueblo just as these ideas were being converted into research problems. He became the chief field worker for the research on the Hohokam and ultimately coauthored with Gladwin the definition of this now basic culture in southwestern prehistory. Haury excavated a key Hohokam site, Roosevelt 9:6, which had been exposed by the receding water of Lake Roosevelt, making it one of the nation's first "salvage" archaeology projects. Haury's detailed comparative analysis of the Roosevelt 9:6 material paved the way for the much larger effort Gladwin initiated at the site of Snaketown on the Gila River Indian Reservation in 1934–35. The resulting landmark monograph defined and documented the Hohokam as a distinct culture that was widely accepted by the archaeological community. The dating of the early horizons was questioned and many scholars, including Gladwin, offered revisions of the Snaketown chronology. Haury returned to Snaketown in 1964–65 in an effort to clarify such problems, but was unable to resolve those relating to the chronology of the earlier periods, despite pioneering work in archaeomagnetism.

Although there was general acceptance for the Gladwin-Haury idea of a separate Hohokam culture, Haury's proposal of a Mogollon culture for the mountainous subregion between the desert and the plateau was met with considerable hostility. Haury planned his research at Forestdale and Point of Pines to generate new empirical data that would help legitimize the Mogollon concept. By promoting the Hohokam and Mogollon as cultures separate from the Anasazi, Haury emphasized the diversity of the prehistoric Southwest. By discussing these separate cultures in the context of the entire region he highlighted the underlying unity of the cultures in the Southwest. Emil's work in the Anasazi region was more limited, but by clearly defining Hohokam and Mogollon, he forced his critics, mostly Anasazi specialists, to clarify their conception of Anasazi. It is interesting to note that although Emil contributed little to the protohistoric period of southwestern prehistory, two of his projects were designed to provide evidence for that period. Papago (now Tohono O'odham) objections stopped the excavation of Batki, a site visited by Jesuit missionary Eusebio Kino, so Emil shifted attention to the excavation of Ventana Cave with its evidence for the earliest inhabitants of the Southwest. Similarly, the Bluff site was selected as a possible protohistoric Apache site, but it provided evidence that the Mogollon culture was not only distinct from Anasazi but in part predated it.

Haury's retirement years were active and productive. In 1989 the University of Arizona Press published his history of the Point of Pines Archaeological Field School. On July 6, 1990, he married Agnese Nelms Lindley, an old friend from Snaketown days. They traveled together throughout the Southwest, thoroughly enjoying visits to many of the sites he excavated. In August 1992, a few months before his death, they attended his last Pecos Conference held at Pecos National Monument on the sixty-fifth anniversary of his attendance as a student at the first Pecos Conference in 1927, symbolically closing the circle on his long and distinguished career.

Emil Haury's enduring contribution to the understanding of the prehistory of the Southwest derives neither from his pioneering Paleoindian research nor from his seminal definition of the Hohokam and Mogollon cultures, but rather from his clear delineation of a framework for the objective, rational, and creative study of the archaeology of an entire region. He presented a vision of archaeology and a definition of the Southwest as a whole that continue to stimulate new and exciting ways of recreating a more complete image of life in the ancient Southwest.

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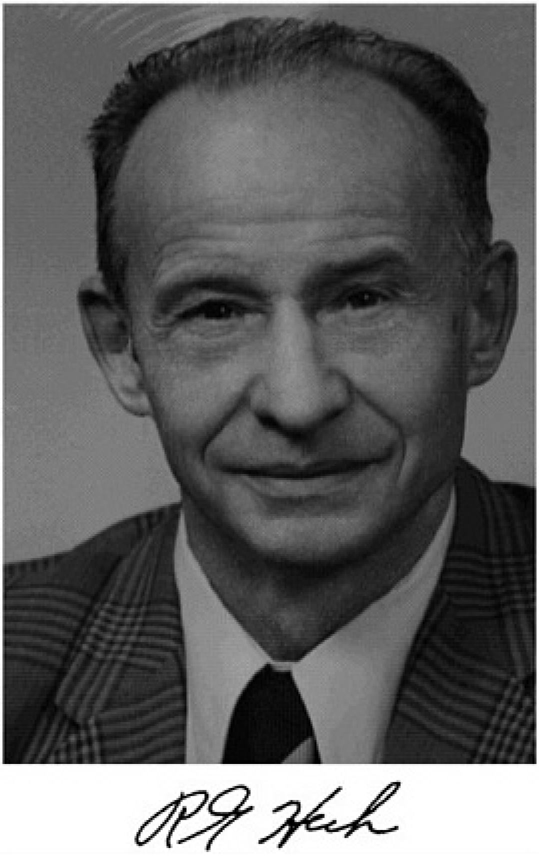

RAYMOND GEORGE HERB

January 22, 1908–October 1, 1996

BY HENRY H. BARSCHALL

Ray herb is famous for the design and development of pressurized electrostatic accelerators, which were the most widely used tools for nuclear physics research in the post-World War II period. This was, however, only one of his great contributions to physics and technology. He also used his accelerator to perform precision measurements in nuclear physics, supervised the Ph.D. research of over fifty physics students, and pioneered many advances in accelerator and vacuum technology. In his later life he founded a company that produced over 100 electrostatic accelerators not only for nuclear physics but for such diverse applications as detecting forgeries at the Louvre museum and inspecting cargoes passing through the Chunnel connecting England and France.

Ray was born in 1908 in Navarino on a small farm in north-central Wisconsin, one of eight children. He did his undergraduate and graduate work at the University of Wisconsin in Madison where he received a Ph.D. in physics in 1935. He spent his entire life in Wisconsin, except for the summer of 1935 when he worked at the Department of Terrestrial Magnetism of the Carnegie Institution in Washington and from 1940 to 1945 when he was at the Radiation Laboratory at the Massachusetts Institute of Technology working on microware radar development. He was on the faculty of the University of Wisconsin-Madison from 1935 until his retirement as the Charles Mendenhall professor of physics in 1972. (Charles Mendenhall, a member of the National Academy of Sciences, was head of the Wisconsin physics department and Ray's major professor).

Ray was elected to the National Academy of Sciences in 1955. He received honorary degrees from the University of Basel, Switzerland, University of São Paulo, Brazil, University of Lund, Sweden, as well as the University of Wisconsin; the latter was in recognition of his accomplishments following his retirement. He was awarded the Tom W. Bonner Prize by the American Physical Society.

In 1945 Ray married Anne Williamson, the daughter of a University of Florida physics professor. They had five children: Stephen, Rebecca, Sara, Emily, and William. Stephen followed in his father's footsteps and became an experimental physicist. Ray died in 1996 of multiple myeloma. Although disabled physically for many months, he remained active full-time in his work until the last days of his life.

The first particle accelerator used for a nuclear physics experiment was developed by Cockcroft and Walton, using high-voltage transformers, rectifiers, and a four-stage voltage multiplier to produce a voltage of 710 kV. In 1931 Robert J. Van de Graaff at Princeton attempted to attain higher voltages by transporting electric charges on a rapidly moving canvas belt to the high-voltage terminal. The attainable voltage of this electrostatic generator was limited by corona discharges from the high-voltage terminal. Henry A. Barton, D. W. Mueller, and J. C. Van Atta at Princeton were able to attain about 1 MV by enclosing Van de Graaff's belt-charging system in high-pressure air, but they did not attempt to accelerate charged particles.

In 1933, just after receiving his bachelor's degree, Ray worked with Glen G. Havens at Wisconsin on a vacuum-insulated electrostatic generator of the Van de Graaff design, but this device could not be pushed above 300 kV. There was no understanding of the discharges that limited the attainable voltage. Ray therefore decided to try to use high-pressure insulation. Two other graduate students, D. B. Parkinson and D. W. Kerst, joined Ray in this endeavor. Ray discovered accidentally that the dielectric strength of air could be greatly increased by the addition of carbon tetrachloride, an electronegative gas. Ray told the story that he tried other chemicals. When he threw a rag soaked in acetone into the tank, the first spark started a fire. He was easily able to reach 1 MV in a pressure tank 1 m in diameter and 2 m long filled with air and carbon tetrachloride. Ray then proceeded with the more difficult task of building an accelerating tube for this machine. It was limited in length to 50 cm. He used sections of Pyrex tubing, 6 cm long and 5 cm in diameter. The tubing was sealed to brass separators with red sealing wax. He then built an ion source and detection equipment and used the accelerator for his Ph.D. thesis, which involved the measurement of the yield of the Li(p,α) reaction at 400 keV.

Later in 1935 Ray with Parkinson and Kerst designed a larger machine. It used a tank 1.7 m in diameter and 6.4 m long. It could be operated at 2.6 MV. The accelerating tube for this machine used a design that was adopted for all future electrostatic accelerators (i.e., a column enclosed by closely spaced metal rings graded in potential). Although this machine did not attain the particle energies that could be obtained with a cyclotron, the energy of the particles accelerated with the electrostatic accelerator was much more uniform and could be controlled with high accuracy.

Enrico Fermi had performed experiments with slow neutrons that showed that the reaction probability of the neutrons increased sharply at certain bombarding energies, a process called resonance. These sharp resonances were believed to occur only at very low bombarding energies. Using the beam of well-defined energy from the electrostatic accelerator Ray and his coworkers showed that resonances as narrow as 1 keV occurred when light elements, such as fluorine and aluminum, were bombarded with MeV protons. These resonances persisted to the highest energies available, 2.6 MeV. This observation made it desirable to extend the studies to higher proton energies and to redesign the accelerator to reach higher energies.

The news of Ray's accelerator spread rapidly, and visitors from all over the world came to Wisconsin to admire the machine and to obtain copies of the drawings. The tank that Ray had used was limited in size by the requirement that it had to pass through a basement window in the physics building. Those who planned to build improved copies of Ray's machine did not have this limitation and were sure they could reach 10 MV or more. Ray gave the visitors much help and advice, but none of the improved copies worked nearly as well as the original —to Ray's quiet amusement. Ray, with the help of graduate students, especially J. L. McKibben and C. M. Turner, was able to remodel the accelerator to produce 4.5-MeV protons by 1940. This energy was surpassed only by cyclotrons until 1955 when a commercially produced electrostatic accelerator was installed at Oak Ridge National Laboratory.

The basic features of Ray's design, which have been incorporated into all modern electrostatic accelerators, include aluminum hoops surrounding the acceleration tube, a voltage gradient controlled either by corona points or resistors, a rotating vane generating voltmeter, high pressure insulation (originally air and carbon tetrachloride, but later nitrogen and Freon or sulfur hexafluoride).

In 1939 Ray with D. W. Kerst, D. B. Parkinson, and G. J. Plain published a seminal paper titled "Scattering of protons by protons." The analysis of these measurements by Breit et al.[1] gave accurate information about the short-range forces between protons and about the charge-independence of nuclear forces.

In the fall of 1940 war was imminent. Ernest Lawrence visited Wisconsin on a recruiting trip for the MIT Radiation Laboratory which was being organized for radar development. For the next five years Ray was at this laboratory, serving on the steering committee and as part of the field service group. He helped to install radar near the front lines in Europe and on a ship in the Pacific.

The Los Alamos Laboratory in New Mexico, known during the war as Project Y, was founded in 1943 to develop nuclear explosives. For the design of a nuclear explosive nuclear data, especially neutron cross sections, were needed. The founders of the laboratory decided to bring to Los Alamos accelerators that could provide the necessary data. The Harvard cyclotron, a Cockcroft-Walton accelerator from the University of Illinois, and the two electrostatic accelerators from the University of Wisconsin were secretly shipped to Los Alamos. The two electrostatic accelerators were dubbed "the short tank" and "the long tank." They ran around the clock at Los Alamos and provided the bulk of the needed nuclear data. Their advantage over the other accelerators was their production of neutrons in the energy range of greatest interest for the project. A target of lithium was bombarded with protons to produce neutrons. The energy of the neutrons could be varied by modifying the energy of the accelerated protons.

I had measured neutron cross sections with a Cockcroft-Walton accelerator at Princeton and was recruited by Los Alamos to perform similar measurements there. I soon realized that the electrostatic accelerators allowed the acquisition of more relevant data, and I spent much time taking data with the "long tank." I became quickly convinced that this machine opened up a new area of research.

After the end of the war Ray paid his first visit to Los Alamos to discuss the future of the two accelerators. This was the first time I met him. He agreed to the sale of the short tank to Los Alamos; this accelerator ran there for many years and produced a wealth of nuclear research. Ray arranged to have the long tank shipped back to Wisconsin. The success of the Manhattan Project had enhanced the standing of nuclear physics, and Ray was authorized to add a couple of nuclear physicists to the faculty of the Wisconsin physics department to reactivate the Wisconsin nuclear physics program. In 1946 the department offered positions to two nuclear physicists who had worked at Los Alamos with the electrostatic accelerators, Hugh Richards and me. I accepted the offer with enthusiasm and so did Hugh Richards. In addition, Ray added experienced students and postdoctorates, mostly from Los Alamos and MIT Radiation Laboratory.

It took a while for the nuclear research program at Wisconsin to get into full swing, but by the summer of 1947 it was well under way under Ray's enthusiastic leadership. One of the important developments Ray started at that time was precision measurement of the energy of the accelerated particles. The particle beam was bent through 90° by passing it between two concentric insulated electrodes, which were kept at a potential difference that could be measured with high precision. This made it possible to perform measurements at accurately known energy, which was particularly important for measurements involving sharp nuclear resonances.

The nuclear research at Wisconsin was generously supported by the Wisconsin Alumni Research Foundation, and Ray shared this support with all the members of the group, but we soon realized that we could not expect the foundation to support a program of the magnitude that was developing. In the fall of 1947 Ray and I visited Robert Bacher whom we both knew and who was a member of the newly created Atomic Energy Commission. Bacher encouraged us to make a formal application for financial support, and in April 1948 we received a grant of $50,000. It was the first grant the commission had made to a university in support of an academic research program. The grant was shared by Ray, Richards, and myself. It was renewed at increasing amounts for many years and provided funds not only for equipment and student and postdoc stipends but also for part of our salaries, so that we could devote more time to research.

Ray established close ties with nuclear physics laboratories in other countries and arranged for the exchange of graduate students and postdoctoral staff with the University of Basel, Switzerland, the Tata Institute in Bombay, India, and most actively with the University of São Paulo, Brazil.

In the following years Ray devoted most of his time to development of accelerator and vacuum technology. This program used many undergraduate and beginning graduate students who received a superb training in laboratory and research techniques. Often these graduate students would work on their thesis with another faculty member, and they could complete their research in a relatively short time because of the techniques they had learned from Ray.

An important event in vacuum technology occurred in 1953 when Ray developed the first practical getter-ion vacuum pump. Ray told the following story about the discovery that titanium could be used for getter-pumping of vacuum systems. Ray observed that a vacuum chamber that he machined rusted overnight. He wondered whether he could use the rusting for a worthwhile purpose. He gave the job of testing various materials as getter pumps to an undergraduate. Nothing worked well. At about that time titanium pellets became commercially available. He asked the student to evaporate titanium by electron bombardment. After the first try the student reported that, when he tried to evaporate titanium, the vacuum gauge broke. It just read zero. Fortunately, Ray realized that the zero pressure reading might not be caused by a broken gauge. For the next dozen years Ray and his students continued development work on vacuum pumps and vacuum gauges.

Ray made important advances in accelerator technology. In all the electrostatic machines built until the mid-1950s a source of positive ions was placed in the positively charged high-voltage terminal of the accelerator, and the positive ions were accelerated from the high-voltage terminal to ground. For example, to accelerate protons the orbital electron of the hydrogen atom was removed to form a proton in the ion source. In 1956 Ray built the first practical source of negative ions (i.e., hydrogen atoms to which an extra electron was attached). This source produced 20 μA of negative hydrogen ions. This made it possible to place the ion source outside the pressure vessel, a great advantage since ion sources require frequent servicing. A second equally important advantage is that, for a given terminal voltage, it now became possible to attain twice the energy of the accelerated particle. The negative ions were accelerated from ground to the positive high-voltage terminal. There the two electrons were stripped from the negative ion, resulting in a positive ion, which was accelerated back to ground potential.

With an external source of negative ions an accelerator that had a high-voltage terminal at 5 MV could produce 10 MeV protons. Accelerators using this principle were called tandem accelerators, and almost all subsequently built electrostatic machines were tandem accelerators. The High Voltage Engineering Corporation soon manufactured tandem accelerators. The first tandem accelerator was installed in Canada. In part because of Ray's contribution to the development of the tandem accelerator, the Atomic Energy Commission placed the first tandem accelerator in the United States at the University of Wisconsin.

Another important advance in accelerator technology that Ray made was the construction of an accelerating tube using metal-ceramic bonding. But the most important advance that Ray, with his student J. A. Ferry, made was the replacement of the canvas charging belt, which had been used in all electrostatic accelerators since Van de Graaff's first electrostatic generator, by chains of metal pellets. The charging belts had been a frequent source of problems in many machines; they often would tear, be difficult to replace, tended to absorb moisture when exposed to humid air, and sometimes would not carry the charge properly to the high-voltage terminal. Ray named electrostatic accelerators that used pellet chains "pelletrons." In 1965 Ray together with J. A. Ferry and T. Pauly founded the National Electrostatics Corporation, which was to manufacture pelletron electrostatic accelerators. The company was located in Middleton Wisconsin, a town adjacent to Madison. An important motivation for founding the company was Ray's loyalty to his home state and his desire to provide employment for scientists and engineers in Wisconsin and boost the economy of the area. It was actually a very risky decision. The demand for accelerators for nuclear research had dwindled, and there was no obvious market for pelletrons. Ray's hard work and good judgment made the gamble pay off. In 1972 Ray retired from the University of Wisconsin to devote full time to the National Electrostatics Corporation. He served as president and chairman of the board of the corporation until his death.

Ray's challenge was to persuade a customer to invest a large sum in an accelerator manufactured by a company without a track record. At that point Ray's long-time cooperation with the University of São Paulo came to his rescue. Oscar Sala, who had worked with Ray at Wisconsin and with whom Ray had maintained a close cooperation for many years, placed the first order for a pelletron for nuclear research. Once Ray had demonstrated that his pelletrons worked as promised, the company received orders for large machines from all over the world. The Oak Ridge National Laboratory ordered the largest machine, which was designed for operation at 25 MV and reached a terminal voltage of 32 MV without an accelerating tube.

National Electrostatics had produced by the time of Ray's death 130 pelletrons. The company at the height of its activity had 140 employees; at the time of Ray's death the number of employees was just under 100. Although the early pelletrons were designed to operate at high voltages for nuclear research, the most recently built machines were used for a variety of applications, such as ion implantation, accelerator mass spectroscopy, and as analytical tools.

When Ray returned to Madison at the end of the war, he moved into a house in the country just outside Madison. Before long his house was surrounded by other houses, and he decided to build a house on a hill outside of Madison overlooking the beautiful countryside. After a few years a shopping mall and a large new high school were constructed within his view. This time he decided to build a home on a hill twenty-five miles from Madison, where a large corn field was in front of the house and a densely wooded area was behind the house. Ray enjoyed watching the wildlife from his window, maintaining hiking trails in the woods behind the house, and almost until the end of his life to cut large piles of firewood. The Herbs' home in all three locations was not only the center of his family life, but Anne and Ray maintained effectively an open house for students and staff.

Ray was an ardent canoeist. On many weekends he would set out on camping trips, often on the nearby Wisconsin River. In 1993, when Ray was eighty-five years old, the award of an honorary degree in Sweden became the occasion for a lengthy canoe trip on Swedish lakes and rivers in the company of Anne, his son Steve, and Steve's wife. They had several adventures, including a severe storm, and the rescue of a young girl who had become lost and for whom a search had been started. This event was reported in the local papers.

In 1936 Eugene Wigner was dismissed from his position at Princeton and asked Gregory Breit at Wisconsin for help. According to Wigner's recollections[2] Breit persuaded the University of Wisconsin to offer Wigner a position. Wigner says, "The University of Wisconsin was rapidly becoming a center of nuclear physics research … A physicist named Ray Herb was the one who really kept the department together. He was about five years younger than me … But Ray was a great enthusiast about physics and life, enormously unselfish and tireless. He seemed to work day and night, and the whole department was infused with his spirit." On the occasion of the award of the Nobel Prize, Wigner paid tribute to the three teachers who had most influenced him[3]. One was Ray Herb. Wigner said, "In leadership, a young man at the time, Ray Herb was my tutor." Wigner's description of Ray remained appropriate for those who joined the University of Wisconsin physics department in later years.

D. A. Bromley[4] described Ray's contributions as follows: "His inventive genius has enabled him, perhaps more than any one other man, to give nuclear scientists everywhere the tools and the techniques which have been essential to major progress in the field."

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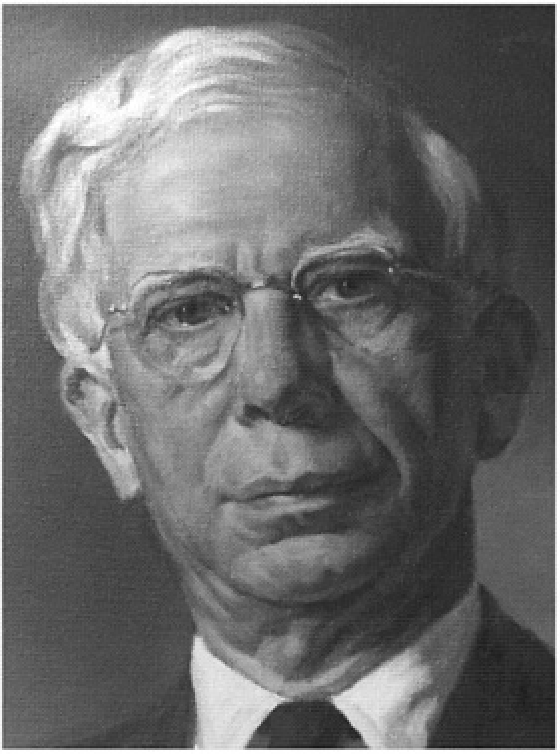

Oil painting by Deane Keller

James G. Horsfall

JAMES GORDON HORSFALL

January 9, 1905–March 22, 1995

BY PAUL E. WAGGONER

JAMES GORDON HORSFALL, who called himself a squirt gun botanist, fought the "rusts and rots that rob us, the blasts and the blights that beset us."[1] His writing inspired plant pathologists. He raised the quota of fundamental research in agriculture and the quota of agriculture in fundamental research.

CHILDHOOD

Horsfall was born January 9, 1905, in Mountain Grove, Missouri, where his father Frank, poor as a church mouse, worked at a tiny independent fruit experiment station. He grew up in Monticello, Arkansas, where his father presided over an agricultural school. His mother was Margaret Vaulx Horsfall. The strength of the father's example was demonstrated by three sons who became a plant pathologist, an entomologist, and an horticulturist. Horsfall, claiming the essential ingredient of a scientist was nonconformity, traced his own nonconformity to a grandfather sent to shoot birds along the Mississippi river by a well-to-do English great-grandfather. Horsfall entitled his autobiography "The Story of a Nonconformist."[2] Although he would spend most of his life in the northeast, Horsfall never forgot his agricultural roots; he featured his country connections, and when he died at ninety, his will sent his library to the experiment station back home in Mountain Grove.

A pear tree afflicted by fire blight introduced young Horsfall to plant pathology, and he followed the advice to prune it. The stump left after a few years of Draconian treatment encouraged his disrespect for conventional wisdom. Father sent him to the University of Arkansas in 1921 well enough prepared to skip his freshman year.[3]

COLLEGE

At the university Horsfall's luck continued. The fun of tinkering with Model T's had inclined him to engineering but math disinclined him. The luck was falling under the influence of Dwight Isely, an entomologist who loved science and stimulated Horsfall to love it, too. Pinning Chrysomelids into boxes for an insect collection bored Horsfall, and he later inveighed against "stamp collecting science." Riding a horse through cotton fields was more exciting. Pioneering the use of insect counts to schedule dusting for boll weevils, Isely employed Horsfall for two summers to scout the fields near Marianna, Arkansas, for signs of the weevil. His rewards of horseback riding, summer employment, and science practiced outdoors were augmented forty-eight years later when he heard from the stage of the National Academy of Sciences that he was the first scout of integrated pest management.

Horsfall claimed his nonconformity kept him from getting a graduate scholarship in entomology. The head entomologist at Arkansas had taken a dislike to him that Horsfall blamed on himself. Fortunately, however, plant pathologists H. R. Rosen and V. H. Young of Cornell found him a place and set him upon the road of the fungi. By the time he was granted a Ph.D. in 1929 he had traveled far with other students of H. H. Whetzel's *Principles of Plant Disease Control.*[4]

In 1927 Horsfall married Sue Belle Overton. Their children are Margaret Eleanor Horsfall Schadler and Anne Vaulx Horsfall Thomas.[5]

Near the end of his life Horsfall wrote of two great blunders. One was irritating the entomologist at Arkansas and the other was a remark that brought down the wrath of the head pathologist at Cornell. "Being a competitive character, my personality was pretty abrasive as a child and young man. It got me into several pecks of trouble until my wife about 1933 persuaded me that you capture more flies with honey than with vinegar," he wrote in his eighties. Sue Belle Overton redirected Horsfall's nonconformity from breaking his knuckles to breaking ground in research.[6]

FUNGICIDES

Luckily, the Agricultural Experiment Station in Geneva, New York, gave newly graduated Horsfall a job as assistant professor in February 1929, safely before the stock market crash in October. Although the economics of 1929 may have damped his nonconformity and heightened his appreciation of the practical, he gave much credit to two greenhouse growers. They first flattered the twenty-four-year-old scientist by calling him "doctor" and then asked, "Can you soak tomato seeds in a copper sulfate solution and control damping off?" Obligingly, if insecurely, he answered, "I think so." To test his opinion he proceeded to experiment. Decades later he still recalled how the thrill from the success of the first experiment caught his mind. When he reported his success at a national meeting, the presence of the eminent L. R. Jones in the front row endorsed the thought caught in his mind.[7]

Forever after he would label himself a squirt gun botanist.

In later years when administration palled, he would tell his secretary he was going to have "fun with fungicides" and slip away to his lab. [8]

Believing profoundly that institutions were the lengthened shadows of great men, Horsfall studied them. He found the man on the front row, L. R. Jones, "carried water on both shoulders." Jones could carry theoretical epidemiology on one shoulder and cabbage breeding on the other. Vowing to emulate Jones, Horsfall found theory in something as banal as damping off. He would do both theoretical and applied research and on crops and diseases that mattered to his state.[9] Later he joined in writing, "Our philosophy is to dig new knowledge from the face of the mine and convert it to fuel to power the society that pays for our groceries."[10]

Since P.-M.-A. Millardet in 1882 discovered that a mixture of lime and copper sulfate applied to grapes in the Medoc to discourage pilfering also discouraged downy mildew, Bordeaux mixture had been the elixir of plant pathology.[11] Deposited on leaves, it killed mildew spores when they alighted. Conforming, the new pathologist Horsfall began spraying canning tomatoes with Bordeaux, and although the dry weather of the 1930s discouraged disease and he had little disease to observe, he persisted. His genius, which he would have called nonconformity, was turning the lack of disease into opportunity. In the absence of mildew and thus the benefit of its control by spray, he could see that Bordeaux harmed the tomatoes. Remembering his vow to combine fundamental with applied, he delved into the harm.

He found that the spray of Bordeaux closed the leaf pores that admit carbon dioxide, the raw material for photosynthesis. The alkaline spray also weakened the cuticle around the pores, hardened the lamella within the leaves and stunted the tomato plants.[12]

Bordeaux was applied to far more acres of potatoes than tomatoes, and the motto was, "Spare the Bordeaux and spoil the potatoes." Horsfall could not believe that the spray stimulated potatoes but harmed closely related tomatoes. He believed the benefits of disease control, and also insect control by Bordeaux, simply hid the harm of Bordeaux to potatoes. He would find sprays that controlled the pests without harming the potatoes.

Attributing the harm to the alkalinity of the Bordeaux mixture, Horsfall tried copper oxide, but since it did not control insects as the mixture of copper sulfate and lime did, it could not succeed. Because the only chemical controls of plant disease had been sulfur, copper, and Bordeaux mixture for over a century, he was temporarily at a loss. Nevertheless, in the mid-1940s he risked excommunication by telling attendees at an inspection of fungicide trials that Bordeaux mixture on potato was a dead horse that had not yet fallen over.[13]

Despite the near excommunication Horsfall enthused in his 1945 book, "The story of organic sulfur compounds is being unfolded so rapidly that any discussion of them can hope only for a 'stop-action' snap-shot." Sulfur "wonder drugs" were in the public eye and Horsfall claimed, "Farmers are flooding the market with calls for the new 'thio' fungicides." [14] A book reviewer, however, wrote that not all would agree that Bordeaux mixture and elemental sulfurs would be turned out to pasture to spend their last years in leisure for a job well done. Thirty years later seventy-year-old Horsfall agreed that he had been an ebullient nonconformist.[15]

Fortunately, in 1945 at age forty he was unabashed. A few years earlier he had an experience on the road to Damascus. An ear infection that had endangered his small daughter was miraculously healed by a new synthetic organic compound called sulfanilamide. Undaunted by colleagues' claims that farmers would not pay $1.50 per pound for organic compounds when Bordeaux sold for 6¢, he soldiered on.[16]

A Horsfall maxim was, "Relate the unrelated."[17] Thus, he saw a similarity of sulfur in fungicidal action and in rubber vulcanization, of all things. With the help of W. C. O'Kane of the Crop Protection Institute he began collaborating with United States Rubber Company (now Uniroyal). Horsfall and his colleagues cited the dogma that copper in Bordeaux killed by oxidizing. United States Rubber replied that copper oxidizes rubber, too. So, why not try an organic pro-oxidant such as tetrachloroquinone? Accordingly, in 1938 Horsfall and colleagues treated pea seeds with it, buried the seeds, and discovered the protection imparted by what would be Spergon.[18]

A sidelight illuminated the always complicated marriage of academe and industry. Horsfall never published the results because United States Rubber would not release the chemistry, and he would not publish without it. Practicality overcame, however, and E. G. Sharvelle, then in Horsfall's lab, and H. S. Cunningham published the results under a code number. Farmers in New York State were soon buying Spergon, proving they would pay $1.50 a pound to protect pea seed.[19]

When the chemical that protected seed was sprayed on foliage in competition with Bordeaux, however, it failed. Sun and dew caused it to hydrolyze. Although related quinones did not deteriorate and found commercial application, they did not find it on potatoes.[20]

In 1939 Director W. L. Slate of the Connecticut Agricultural Experiment Station persuaded Horsfall to move to New Haven to succeed G. P. Clinton (and before him Roland Thaxter) as chief of the Department of Plant Pathology and Botany. Although both predecessors had been distinguished, Horsfall mainly enjoyed quoting Thaxter, demonstrating his own lively writing, and exposing Thaxter's acidic wit.[21] He found a comfortable home at America's first agricultural experiment station, whose founder believed, "Theory and practice must march together."[22]

In the same year Horsfall sat in a cheap restaurant outside Grand Central Station talking with his friend D. F. Murphy of Rohm and Haas Company about their cooperative work on cuprous oxide. Perhaps because he had changed addresses Horsfall felt it was time for other changes. He said to Murphy, "Let us try to develop organic fungicides. Sulfur is a fungicide. Let us try organic sulfur compounds."

Obligingly, Rohm and Haas sent 100 samples in January 1940. One was He-175, later labeled D-14. A. E. Dimond and J. W. Heuberger with Horsfall found D-14 was water soluble and so spread an invisible film evenly over leaves. When it dried, however, it became insoluble and, hence, resistant to removal by rain. It had a peculiar dosage-response curve, it controlled several diseases, and its invisible film recommended it to the eye.[23] D-14 is ethylenebisdithiocarbamate or nabam.

Soon, modifications of nabam (i.e., zinc and manganese ethylenebisdithiocarbamate) by Heuberger, D. O. Wolfenbarger, R. W. Barratt, and Horsfall completed the invention of successful controls of a range of diseases. Although the control of potato and tomato late blight by nabam had at first disappointed, the alterations of solubility by zinc and manganese saved nabam from almost certain failure, and Horsfall could later write, "A potato fungicide was born, and Bordeaux was in trouble." [24] About forty years later the National Research Council reported about the family of ethylenebisdithiocarbamates (EBDCs):

> There are over 40 manufacturers world wide … EBDCs are the

> most widely used group of fungicides in the world. The global market was estimated at $525 million in 1984. In the United States, more than 30 million pounds are used annually to control a wide variety of fungal diseases … Approximately one-third of all fruits and vegetables in the United States are treated with EBDCs.[25]

BORDEAUX MIXTURE DIED AND FELL OVER

Fungicides that lie in wait on leaves cannot control fungi like the Dutch elm disease pathogen, that an insect injects into the host. When Horsfall arrived in New Haven, the disease was decimating the trees that had given it the name Elm City. Joined by G. A. Zentmyer and A. E. Dimond, he tried chemotherapy, putting the fungicide into the water-conducting vessels of the elm where the pathogen lived. Rarely had systemic fungicides been tried, and it took a nonconformist to imagine he could save a tall elm. Undaunted, the team filled Cremo Ale cans with candidate elixirs and injected the fluid into the vessels inside the trunks by connecting ale cans to trunks with rubber tubes. [26]

The campaign to save the elms failed. Although trees alive were kept alive, they died as soon as treatment stopped. Horsfall attributed the failure to degradation of the compounds in the tree plus the lack in plants of the analog of phagocytes to clean up survivors. A consolation to the campaigners was Ainsworth's statement in a history of plant pathology that their unsuccessful attempts to control elm diseases by chemotherapy provided a stimulating example to others. In 1968 others finally discovered a successful systemic fungicide, benomyl.[27]

EXPERIMENTER

Horsfall believed in saving energy by using other people's data to draw new conclusions and applauded the plant pathologist who boasted none of his books had any experimental data of his own collecting.[28] Horsfall's generalizations could leave the listener awestruck. When a critic observed, "He leaps from crag to crag with the nimbleness of a mountain goat," Horsfall liked that.[29] Nevertheless, he was a shrewd experimenter.

To anticipate fungicidal success in the field he designed an apparatus for uniform deposit of fungicides on a glass slide and measurement of their action on spores.[30] His 1945 book, which found its way into several languages, featured dosage-response curves on logarithmic-probability coordinates for exploring the laboratory results. In an era dominated by randomized blocks and Latin squares of treatments in the field, he cleverly tested fungicides on spiral rows: "The hand-carried spray boom is flexible, the power pump untiring; the circular route of travel saves a return empty trip; and the water supply and drainage arrangements save work and time in loading."[31]

EPIDEMIOLOGY

His preeminence in fungicides could obscure Horsfall's contributions to epidemiology. In 1932 he coined the term "inoculum potential" to convey the idea of mass action—the greater the mass or virulence of the pathogen present, the more severe the disease regardless of environment. During the decades since, the precise meaning of inoculum potential has been smudged, with environment sometimes included and sometimes not. Through it all, however, Horsfall's graphic phrase on the banal dusting of tomato seed continues to convey the notion that an abundant supply of fungus can overwhelm a partially effective control.[32]

Determining the effectiveness of a fungicide brought Horsfall to the crux of epidemiology: How much disease is there? Measuring the changing quantity of disease in a crop of countless leaves requires a balancing of efficiency and accuracy. Without efficiency, the disease will out-race its measurement. Without accuracy, differences cannot be discovered. By 1942 Horsfall found that visually lumping plants into four equal grades of 25% each served fairly well.[33] By inverting the issue from one of seeing disease to one of what disease could be seen, however, Horsfall improved estimation.[34] "We stumbled onto two principles: (1) that the human eye is a photocell that reads in logarithms according to the Weber-Fechner law of human acuity and (2) that the eye reads the amount of diseased tissue below 50% and the amount of healthy tissue above 50%."[35] Decades later, fearing pathologists were spending too much time on minutiae while neglecting larger matters, Horsfall wrote, "Many pad around in air-conditioned laboratories seeking the third decimal place in disease physiology. Very few tackle the blue-jean job of searching for accuracy in disease appraisal. Suppose for a few years now we give triple credit toward promotion for the disease appraisers."[36] And another decade later, the Horsfall-Barratt grading system was still alive and a citation classic.[37]

The epidemic being assessed marches through a population of plants, integrating many factors in the environment and characteristics of the pathogen and host. This fabulous array boggles the mind. The arrival of fast computers, therefore, invited the integration of experimental evidence about the components of epidemics with mathematical simulators. They invited computation to reveal the controllable steps and also forecast epidemics. Accordingly, Horsfall participated in the review of knowledge of the life cycle and environmental influences on a tomato blight, experimented to fill in gaps, and assembled the first mathematical simulator of a plant disease. Histories of past weather and disease had been converted into statistical rules for forecasting disease, but the simulator EPIDEM was the first attempt to assemble physiological experiments on the components of the pathogen life cycle into a model that marched ahead as a virtual epidemic. A relative, EPIMAY, allowed forecasts of a new disease without a history, Southern corn leaf blight.[38]

Besides his own contributions to epidemiology, Horsfall inspired those of another. Collecting authors for a treatise on plant pathology, Horsfall invited the relatively unknown J. E. van der Plank in the Department of Agricultural Technical Services in far-off Pretoria, South Africa, to write a chapter, "Analysis of epidemics." Impressed by the chapter, Horsfall introduced van der Plank to his publisher. The outcome, *Plant Disease Epidemics and Control*,[39] taught plant pathologists how to interpret the logistic progress of an epidemic in terms of compound and simple interest, infection rates and latent periods, and horizontal and vertical resistance. Van der Plank inscribed a copy of this book, which transformed plant epidemiology, "To J. G. Horsfall, who with A. E. Dimond, started this in July 1957."

THE CONNECTICUT AGRICULTURAL EXPERIMENT STATION

Back in 1939, when Horsfall left Geneva, New York, for New Haven, Connecticut, he went from one experiment station to another. At the Connecticut station, however, he took up the tradition of the first of the American laboratories that S. W. Johnson named by translating *Landwirtschaftlich Versuchsstation* from the German. A student first of B. W. Silliman at Yale and then of J. von Liebig in Germany, Johnson spent a lifetime thinking, demonstrating, and writing how science and practice could most effectively march together.

After leading the plant pathologists in New Haven for nine years, Horsfall in 1948 became the fifth director of the Connecticut station and thus Johnson's successor. Ever the student of great men, Horsfall made Johnson's letters[40] his guide as he led an agricultural station "near but not part of the local university"[41] for twenty-three years as universities ballooned and farmers dwindled.

He eschewed the ambition to grow, saying "We don't want to be the biggest experiment station, just the best."[42] He spoke frequently of the "station charter," the Connecticut general statute establishing the station. He maintained the sanctity of an independent board of politicians, scientists, and farmers controlling the station and directly reporting to the state legislature. In an analog of priestly celibacy, he discouraged consulting and lecturing routine college courses to encourage concentration on research.[43]

Nevertheless, Horsfall knew people beyond the dwindling population of farmers had to support the station. When he had to make a difficult choice, he said he justified it to three imaginary state legislators, mostly urbanites, seated on the couch in the director's office.[44]

The gypsy moth and DDT presented a supremely difficult choice. The imported pest had been defoliating expanding areas of forest in Connecticut since the beginning of the twentieth century. The miracle of DDT and airplanes at the end of World War II presented to some a blessed opportunity to eradicate the pest; however, Horsfall and his colleague, state and station entomologist Neely Turner, who controlled aerial spraying, believed eradication was a phantasm. They would not lend their authority to federal authorities who wanted to spray the state. They received the encouraging support of the *Hartford Courant*, which editorialized, "If necessary, let us call out the [Governor's] Foot Guard and the Horse Guard to repel further forays by federal authority, even if it comes armed with DDT."[45] Horsfall lived to see a biological control of the gypsy moth discovered by station scientists.[46]

Even sooner, however, environmentalists understood that

Horsfall opposed heavy handed measures, and in 1962 Rachel Carson quoted approvingly his colleague state entomologist Turner.[47] Still earlier, the mayor of Meriden was pressured to ask Director Horsfall to approve the aerial spraying of his city with dieldrin. Horsfall replied, "First, that dieldrin was a pretty poisonous substance; second, that it would fall on babies and children playing outdoors; third, that it would fall on any cat caught out of doors; fourth, that the cat would lick the dieldrin from its fur and poison itself; and finally, that if the mayor would sign a letter to me and say, 'Let us spray,' I would approve." Horsfall never heard from the mayor. [48]

At the same time, obsessive fears of pesticides, of course, appalled the inventor of fungicides. He suffered picketing by the fearful. He drafted an editorial comparing them to Chicken Little, but wisdom and colleagues convinced him to leave it unpublished.[49]

In the end, leaders esteemed Horsfall's combination of a farmer's view of nature with a dislike of excess. When President John F. Kennedy set up a committee to confer with Rachel Carson, he appointed Horsfall to serve.[50] In 1970 Governor John Dempsey of Connecticut selected Horsfall to lead his Committee on Environmental Policy. Before he allowed the committee to recommend action, Horsfall led the members on a thorough diagnosis of societal functions that caused environmental problems. "In that way … haste for action could be tethered until we had a better diagnosis of causes on which to prescribe."[51] The consequent recommendations caused, among other things, a thoroughgoing revision of the way the Connecticut government dealt with parks, forest, water, and, broadly, the environment. It also led to a program of purchasing development rights on farmland. Recognizing his contributions to the state environment, the *New Haven Register* designated Horsfall Connecticut

Citizen of the Year for 1971.[52] Director Horsfall had broadened the station's field from farms to the whole landscape.

FUN WITH WORDS

In an autobiography Horsfall ranked "fun with words" with "fun with fungi" and wrote of his style: "Being a nonconformist, I have always tried to say it differently. I could never abide the stodgy stilted style of much scientific writing. The English language is an elegant medium for saying exactly what one wants to say—no need to use any of the standard circumlocutions."[53] In college he edited the student magazine, and by age forty he had published *Fungicides and Their Action* (1945). During the 1950s he and A. E. Dimond edited *Plant Pathology, An Advanced Treatise*. A score of years later, Horsfall and E. B. Cowling edited *Plant Disease, An Advanced Treatise*.[54] In the five volumes of the 1977–80 treatise he recurred to his theme of pathometry, indulged his hobby of genealogy, and concluded with a pithy philosophy of plant pathology.

During the 1950s up to 1962, Horsfall led committees of the American Phytopathological Society and Annual Reviews, Inc., that labored to create journals for synthesizing the knowledge about plant disease. The sensible and enduring result was the birth of a single journal, the *Annual Review of Phytopathology*.[55]

In 1973 Horsfall retired from the directorship of the Connecticut Agricultural Experiment Station and assumed the title of Samuel W. Johnson distinguished scientist. As an octogenarian Horsfall wrote the history of the pioneer experiment station, an invention for making inventions.[56]

THE NATIONAL ACADEMY OF SCIENCES AND SCIENCE POLICY

In 1953 the National Academy of Sciences elected Horsfall a member. His two brothers were scientists, too, and he wrote, "As a child, sibling rivalry played a role, I am sure."[57] He welcomed the honor of membership as he later welcomed other awards of distinction, perhaps in a continuing competition. Mostly, however, he welcomed the election as a route to affecting scientific policy. He believed a scientist should "carry water on both shoulders" because in President John F. Kennedy's words to the Academy, "Scientists alone can establish the objectives of their research, but society, in extending support to science, must take account of its own needs."[58]

During the 1950s and 1960s Horsfall served on committees of the Atomic Energy Commission and National Aeronautics and Space Administration, the President's Science Advisory Committee, and the National Advisory Commission on Food and Fiber. He served on the Academy's Latin-America Science Board, and he led its Board on Agriculture and lobbied for a commission on agriculture and renewable resources.[59]

Discerning an excessive emphasis on application during his early years as a scientific statesman, he urged more basic research. Later, however, he perceived a growing separation of science and application, which violated the maxim that theory and practice must march together. He made his case in an unforgettable essay, "Relevance: Are we smart outside?"[60] He related the parable of the little boy who was asked why he couldn't do as well in school as Alice. The boy answered, "Mother, Alice may be smart in school, but she is awfully dumb outside." To a scientist, who in 1932 searched out distinguished biochemist Z. I. Kertesz to join in studying "some effects of root-rot on the physiology of peas,"[61] basic research was holy. But four decades later he worried about irrelevance, deplored grantsmanship, and wrote, "Basic research at the old stand will no longer sell."

He urged his colleagues to fire up their relevancy, raise their quota of field research toward 50%, and give credit for publication of practical results.

As good as his word, Horsfall led committees at the Academy in two relevant inquiries. In 1970 an epidemic swept over the corn crop of the United States, threatening a great resource. In some sense, science and the technology of plant breeding were responsible because their success had caused genetic homogeneity of the crop. "In that it is the responsibility of the Agricultural Board [of the Academy] to watch for perturbations in the nation's agriculture and to suggest means by which to reduce them, the board established a committee to examine the blight epidemic." Horsfall led the committee of plant breeders, pathologists, entomologists, economists, and people knowledgeable in major crops to investigate the circumstances and also the more general issue of genetic vulnerability.[62]

When his colleague C. R. Frink called his attention to a slower rise of farm efficiency in the 1960s than in the previous decade, Horsfall encouraged the Rockefeller Foundation to fund a commission on agriculture and renewable resources of the Academy to perform an investigation of the nation's agricultural production efficiency. While Horsfall was leading the investigation, both food prices and exports soared, showing his prescience in anticipating the need of a nation that had been basking in sunny surpluses. Nevertheless, the report concluded optimistically that breakthroughs in cell fusion, photosynthesis, and biological nitrogen transformations could restore abundance. After the tally of basic breakthroughs hoped for, the last phrase of the report showed Horsfall's hand: "Being ever mindful of the need to seek practical field applications of major advances in knowledge." [63]

Happily the Horsfall clan of James, Sue Belle, daughters, and engineer sons-in-law were close knit. They shared a retreat on Lake George where an octogenarian could teach grandchildren such technology as repairing screen doors.

Horsfall channeled his proclaimed nonconformity into plant diseases and policy and wore a tie. He invented fungicides. He broadened the charter of his experiment station to encompass the whole landscape. He died a few weeks after his ninetieth birthday and was buried in New Haven's Grove Street Cemetery near the father of American experiment stations. They shared the belief that theory and practice must march together.

NOTES

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2. J. G. Horsfall. Fungi and fungicides. The story of a nonconformist. *Annu. Rev. Phytopathol*. 13 (1975):1–13. Hereafter cited as Story of a Nonconformist.

3. P. E. Waggoner (PW). Memo dated October 1, 1984, of conversation with Horsfall.

4. "To the memory of H. H. Whetzel, stimulating teacher and true friend." Dedication of J. G. Horsfall. *Fungicides and Their Action* . Waltham, Mass.: Chronica Botanica, 1945. Hereafter cited as Fungicides and Their Action.

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7. Story of a Nonconformist, pp. 5–6.

8. Recollection of Lois Pierson.

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14. Fungicides and Their Action, pp. 118, 124.

15. Story of a Nonconformist, p. 6.

16. Story of a Nonconformist, p. 7.

17. Epilogue: Anent a philosophy …., p. 440.

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19. Story of a Nonconformist, p. 7 H. S. Cunningham and E. G. Sharvelle. Organic seed protectants for lima beans. *Phytopathology* 30(1940):4–5.

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27. G. C. Ainsworth. *Introduction to the History of Plant Pathology*, pp. 120–21. Cambridge: Cambridge University Press, 1981.

28. J. E. van der Plank quoted in Epilogue: Anent a philosophy …., p. 436.

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30. J. G. Horsfall, J. W. Heuberger, E. G. Sharvelle, and J. M. Hamilton. A design for laboratory assay of fungicides. *Phytopathology* 30(1940):545–63.

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52. Sunday Pictorial. *New Haven Register*. January 2, 1972, p. 1.

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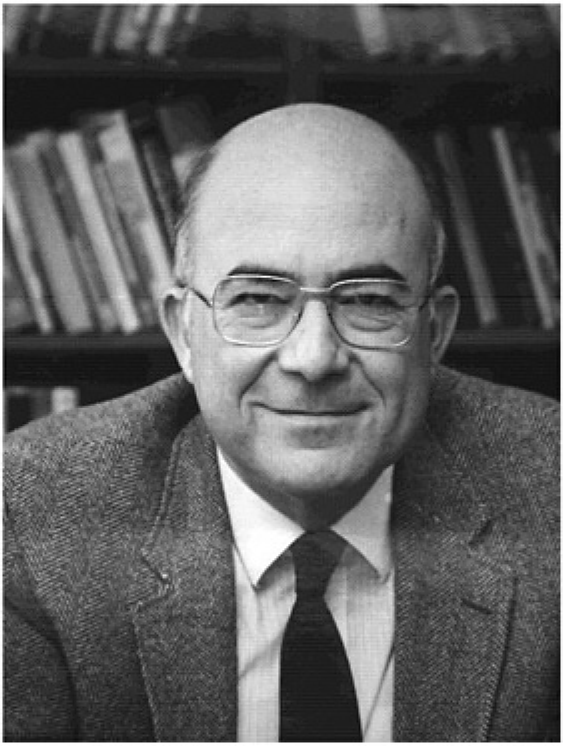

Courtesy of Rockefeller University/John Sholtis

EMIL THOMAS KAISER

February 15, 1938–July 18, 1988

BY F. H. WESTHEIMER

HUGH WALPOLE'S NOVEL *FORTITUDE* begins with the line, "'Tisn't life that matters! 'Tis the courage you bring to it." That may be true for most people, but it certainly isn't true for the tiny fraction of humanity who are highly creative; their lives matter even more than their courage because they change the world and the lives of others. Tom Kaiser was one of those creative people who changed the world—in his case, the world of science. But even if we were to measure him by Walpole's criterion, he would stand out; his courage was as great as his creativity.

EARLY YEARS

Emil Thomas Kaiser—Tom to his friends—was born in Hungary in 1938. His parents, both of whom were Ph.D. chemists, brought him, when he was an infant, to Canada, where his father had taken a job as a pharmaceutical chemist. Then, when Tom was two, the family moved to the United States, where his father began a long career with the research department of Armour Pharmaceutical Company.

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Obviously, Tom had chemistry in his blood, and it should come as no surprise that his career got off to an early start. His abilities and energy were apparent from the first. He graduated from the University of Chicago at the age of eighteen, and went to Harvard for his graduate work. He completed research for that degree, related to strain in cyclic sulfate esters,[1] with me in only two years, received his doctorate when he was only twenty-one, and began his independent research career. He decided to carry out postdoctoral research with E. J. Corey and afterward with Myron Bender. He and Professor Corey created a remarkable piece of physical-organic chemistry[2] that demonstrated that sulfone anions can retain their chirality, at least briefly; he and Professor Bender investigated the cinnamoyl intermediates[3] formed in the hydrolysis of cinnamoyl esters by trypsin and chymotrypsin. He was now well launched on his career in bioorganic chemistry, with experience in both physical-organic chemistry and enzymology.

At this time Kaiser accepted an assistant professorship at Washington University in St. Louis. His enormous capacity for productive research immediately became clear, and the University of Chicago offered him an assistant professorship in 1963, when he was twenty-five, and a professorship in 1970, when he was thirty-two. Those were among the department's best decisions.

Although his research at Washington University in St. Louis showed both his enormous productivity and his wide grasp of chemical problems, it was only after he came to Chicago that his startling originality came to the fore; it was here that he began the research that made him known in the scientific community. In 1982 he accepted a professorship at Rockefeller University, where he continued his spectacular research. At about the same time he became an editor of *Bioorganic Chemistry* .

Kaiser made two major contributions to that subject and authored a number of other advances that would have distinguished the career of a lesser scientist. One of his major contributions was the development of semisynthetic enzymes and the other concerned amphiphilic helices.

SEMISYNTHETIC ENZYMES[4, 5]

Enzyme kineticists separate binding and catalysis. Chemists have been quite successful in identifying the catalytic residues in enzymes and X-ray crystallographers have been successful in identifying the binding sites of substrates and coenzymes on the surfaces of enzymes. Kaiser devised a scheme for making useful new catalytic activities by combining the binding properties of one enzyme with the catalytic activity of an unrelated coenzyme. In particular, he converted a hydrolytic enzyme into one for oxidation-reduction by attaching a flavin coenzyme at the active site of a peptidase, papain. He utilized the binding properties of the peptidase and the oxidation-reduction properties of the coenzyme to make a new enzyme, a chimera, that would effect oxidation-reduction specifically and stereospecificity. This effort was largely successful, and he thus demonstrated how to go about constructing semisynthetic enzymes for many reactions.

In detail, what he did was to synthesize a modified flavin that was substituted with a bromomethyl, or preferably a bromoacetyl group. He then attached this coenzyme to papain. That enzyme has an essential sulfhydryl group at its active site, and this sulfhydryl group reacts readily with and specifically displaces the bromine from the bromoacethyl or bromoacetyl group of a modified flavin.[6, 7, 8] The reaction accomplishes two purposes. It destroys the active site of the protease and at the same time attaches the flavin in a position adjacent to the binding site of the enzyme. The resulting semisynthetic enzyme then serves to catalyze the oxidation of a number of substrates. In particular, the substrates that Kaiser chose, such as N-propyldihydronicotinamide, are related to NADH; they are, however, substituted on the nitrogen atom of the pyridine ring with alkyl groups rather than with the adenine-ribose-pyrophosphate-ribose substituent of NADH. The alkyl groups were chosen to match the specificity of papain, which hydrolyzes esters and amides with hydrophobic substituents. A semisynthetic oxidation-reduction enzyme, a chimera of papain and flavin, would be expected to bond, and thus react with hydrophobic substrates, and so it does. Kaiser's best semisynthetic enzyme and best substrate show an increase in rate by a factor of about 1000 over the corresponding uncatalyzed reaction, and a modest stereoselectivity with respect to the diastereotopic hydrogen atoms in the 4-position of the dihydronicotinamide ring. Although the modified papain is not comparably so efficient as natural enzymes, the work clearly demonstrates a principle and shows how to proceed in making new enzymatic activities.

Similar results can be obtained by attaching bromoacetyl flavins to glyceraldehyde-3-phosphate dehydrogenase. The resulting semisynthetic enzyme attacks NADH rapidly; a similar semisynthetic enzyme can be made from hemoglobin.

SYNTHESIS OF PEPTIDES AND PROTEINS

Another powerful idea with semisynthetic enzymes concerns a highly original way of utilizing thiosubtilisin. This modified enzyme had previously been prepared both by Daniel Koshland, Jr.,[9] and Myron Bender[10] and their collaborators by the chemical substitution of a cystein for the catalytically active serine in subtilisin. The resulting protein is still a protease, but a poor one. Kaiser and his coworkers[11, 12] showed how to use this semisynthetic enzyme to couple large peptides. One can only appreciate what can be accomplished with Kaiser's method by analogy to the field of nucleic acid chemistry. The successful synthesis of many polynucleotides depends on the ability of certain enzymes called ligases to join two polynucleotides of moderate size. No naturally occurring enzyme is effective as a protein ligase, and the lack of a ligase severely limits the synthesis of all but the smallest proteins by chemical methods. The solid-state synthesis of peptides, invented by Bruce Merrifield,[13] works beautifully for peptides composed of 20–50 amino acids, but less well for longer ones and has not so far been successfully employed for proteins larger than ribonuclease.

Kaiser and his coworkers showed how to use thiosubtilisin[14] to ligate (that is, to join) activated but unprotected peptides. The originality of this work lies in the appreciation of the utility of having a *poor* enzyme, rather than a good one (i.e., in capitalizing on the fact that the modified enzyme is *inefficient*). Thiosubtilisin reacts rapidly with an activated peptide to form an acylated enzyme and transfers the peptide residue to another peptide, completing the ligation reaction; but, since it is a poor peptidase, it does not attack the resulting product at all rapidly. Because of this work chemical synthesis can now complement the methods of molecular biology in forming proteins. Kaiser's premature death prevented him from exploiting his new methodology; it will remain for others to demonstrate the power of this invention.

In addition to this method for ligation, Kaiser and his coworkers invented a new resin, whereby the amino acids and peptides are built onto an oxime group, that supplements the resins introduced by Merrifield. [15]

AMPHIPHILIC PEPTIDES

Kaiser's opening with respect to amphiphilic proteins was even more important than his invention of semisynthetic enzymes and probably constitutes his major achievement. It offers an important breakthrough in the chemistry of proteins and effectively immortalizes him. Although the scientific community has some understanding of the way in which proteins work, both with respect to binding and catalysis, we are just beginning to understand the reasons for their secondary and tertiary structures. Kaiser's work showed the importance of secondary structure and in particular the reasons why amphiphilic helices are essential to biological activity. One face of an amphiphilic helix is hydrophilic and one hydrophobic; such a helix can lie down on a membrane with the hydrophobic side buried in the membrane and the hydrophilic side facing out to the aqueous solvent. The idea of amphiphilic helices was introduced in 1974 by Segrist et al.[16] based on the helical wheel of Schiffer and Edmundson.[17] But the concept was bare, a description perhaps, but without real substance until Kaiser and his coworkers synthesized peptides that demonstrated the importance of the idea. [18]

They took the naked hypothesis and clothed it. Fitch[19] and McLachlan [20] had previously and independently proposed that apolipoprotein-A, a protein 143 amino acids long, consisted of repeating units 22 amino acids in length in which each unit consisted of an amphiphilic helix. Kaiser and his coworkers[21] verified the hypothesis by synthesizing a peptide of 22 amino acids with minimum homology with the sequence of any of the repeating helical segments of apolipoprotein-A, but which nevertheless shares its biological properties, including in particular the activation of the enzyme lecithin-cholesterol acyltransferase. The synthesis demonstrated that the activity of apolipoprotein-A does not rest in its detailed sequence, but merely in its secondary structure (that is, in the amphiphilic nature of its repeated helices).

Melittin (that is, bee venom) is a peptide of 26 amino acids. Its toxicity depends on its ability to lyse erythrocytes. The structure shows a cluster of basic amino acids at the C-terminus and an N-terminal sequence of 21 amino acids that has the potential to form an amphiphilic helix. Kaiser postulated that the 4 basic amino acids at the C-terminus acted as a prosthetic, or catalytic group, and should be retained, but that the choice of the other amino acids was nearly arbitrary, provided they form an amphiphilic helix.[22] In collaboration with DeGrado and Kezdy he synthesized a polypeptide 26 amino acids long with the same 4 basic amino acids near the carboxyl terminus as those of melittin. The rest of the sequence was deliberately constructed to diverge widely from the natural. It was designed with 12 leucine residues and 1 tryptophane, properly spaced to mimic the hydrophobic portion of the amphiphilic helix, and 4 serine plus 3 glutamate residues for the hydrophilic portion. The result was a peptide with the properties of bee venom. In particular, it was even more active than the natural venom in the lysis of erythrocytes.

Similar spectacular results were achieved with peptides to mimic the action of calcitonin,[23] corticotropin-releasing factor[24] and endorphins.[25] In each case the mimic served the biological function of the natural product. Synthesis proved that in each case the essential feature of the sequence was an amphiphilic helix with a small prosthetic group in some cases; provided these features were preserved, the precise sequence was irrelevant. W. DeGrado has carried this idea to its logical extreme, imitating ion channels with helices composed of only two kinds of amino acids, leucine and glutamic acid, and ignoring natural sequences almost completely.[26]

The results are important not only with respect to the concept of amphiphilic helices, but also with respect to protein chemistry in general. The same enzyme in different species is represented by proteins that boast only partial homology. This is necessarily so; life would never have developed if only one combination and permutation of amino acids could serve a given function. A protein with only 100 amino acid residues—10 each of 10 different kinds—would allow 2×10^{92} permutations, an incredible number far beyond imagination. There has not been anywhere near enough time to examine even a billionth of a billionth of a billionth of these possibilities; after all, Earth is only 10^{17} seconds old. The living world can exist only because an enormous number—even if it is only a tiny fraction of the permutations for a protein—can carry out its essential function perfectly well. Here in Kaiser's work is part of the experimental demonstration that this is so.

SITE-DIRECTED MUTAGENESIS

Although semisynthetic enzymes and amphiphilic helices constitute the principal contributions from Kaiser's laboratory, at least one other aspect of his work (i.e., site-directed mutagenesis) demands attention. This technique is common today; it was relatively new in 1987. Kaiser and his coworkers substituted phenylalanine for tyrosine at position 198 in carboxypeptidase.[27] The hydroxyl group of the latter residue had been assigned by others to an important role in enzymatic catalysis. But the mutant enzyme that Kaiser and his coworkers created with phenylalanine in place of tyrosine, works perfectly well, and the mechanism of action of carboxypeptidase had therefore to be revised. Here in Kaiser's work is an early example of the power of site-directed mutagenesis in enzymology.

SCIENTIFIC SIGNIFICANCE

We have described Kaiser's work, but we have just begun the task of evaluating it. What he did really was to introduce a new field into chemistry. We (the bioorganic chemists) have been trying for some time to understand the action of enzymes and other active biological molecules. An enormous effort has been expended in our attempts to make enzyme models on the grounds that we cannot claim really to understand how enzymes work until we can build our own. We have had indifferent success so far in this undertaking; progress has been modest relative to the effort involved.

Kaiser introduced a different approach. If enzymes—most enzymes, at any rate—and receptors are proteins, we should then understand the way proteins interact with their receptors and membranes; we should at least understand the importance of secondary structures.

COURAGE

We have described the remarkable science Kaiser achieved and outlined the impact of his work on the future of chemistry, but we have told only half the story. The other half takes us back to Hugh Walpole and to courage. Before paying tribute to Kaiser's courage, let me say a bit about him as a person. He was friendly and smiled easily and often. He was devoted to his wife Bonnie and their children; admired his parents; and valued his graduate students. He was perceived as fair. He had no disputes other than friendly scientific ones. People trusted him; there was never a doubt that he would protect information given to him in confidence or give credit where credit was due.

When he became ill and his kidney problem had been diagnosed, he scarcely slowed up. His kidneys failed completely, and to clear his system of urea he had to undergo dialysis three times a week for four hours at a time. He wrote in 1988 that, "I have managed to utilize the time during my treatment for reading, but there is no question that having to go for dialysis on a regular basis is quite confining." Quite confining—no more complaint that that. It is hard to imagine facing such an ordeal with that much raw courage. He continued all his activities. He wrote, "Otherwise, everything is going well. I was able to travel to give seminars during the Fall [these included a lecture in Milan, named lectures at Virginia and North Carolina, the Calvin lectures at Berkeley, and more] since I could arrange to be treated … ." In other words, dialysis could be arranged all over the world; he did not allow anything—certainly not personal discomfort or risk—to interfere with his contributions to science. Tom Kaiser made these matter-of-fact comments concerning dialysis and then went on to discuss his discoveries. One can only shake one's head in awe. Kaiser's life during the year of dialysis would have stopped almost everyone; he faced it with so much courage that his life went on almost normally.

During the many months that he underwent dialysis three times a week, he talked about a kidney transplant. He was always upbeat, always optimistic. He quoted the favorable statistics on the operation—better than 90% successful—and felt absolutely confident that he would be one of the majority. His spirit—his smile and ebullient optimism—were contagious. Those who knew him were convinced by his enthusiasm that the operation would be a great success.

Kaiser was elected to the National Academy of Sciences in 1987, and he had planned to attend the meeting in April 1988 in order to sign the book and be formally inducted into the Academy. At the meeting of the Section of Chemistry we learned that he would not attend, but we were happy about the reason: a proper kidney had been found for him, and he would receive a transplant in Boston while the Academy met in Washington. The operation appeared to be a great success; he and the doctors were delighted. A few months later our optimism, and Tom, were gone.

But we can still be buoyed by that optimism. His friends will want to face any crisis in their own lives with half the courage he displayed in his. Science has profited by the openings he made; we—his friends and scientific heirs—can try to carry on in the pathways he pioneered. But we need, too, to hail his courage and will to overcome personal obstacles. We can only speculate on what he would have accomplished if he had survived. He was enormously productive, enthusiastic, and full of new ideas. We know that we have lost a friend, that the scientific community has lost a great scientist, and we have all lost a role model for facing adversity with courage.

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Courtesy of the Department of Physics, University of Wisconsin

DONALD WILLIAM KERST

November 1, 1911–August 19, 1993

BY ANDREW M. SESSLER AND KEITH R. SYMON

DONALD WILLIAM KERST died on August 19, 1993, at the age of 81. On that day the country lost one of its most influential physicists, one with a remarkable breadth of interests. Kerst will long be remembered for his development of the betatron, but he also made very important contributions to the general design of particle accelerators, nuclear physics, medical physics, and plasma physics.

In addition to these scientific and technical contributions, his deep understanding of physics, his know-how, and his enthusiasm have been a source of education and inspiration both to his students and his colleagues. His many students and junior colleagues during the last forty years have continued to make their own contributions to these fields. He was an enthusiastic and effective mentor who worked hard and expected his students to do likewise, and they did. His students liked and admired him. Thirty-three students completed Ph.D. degrees in the betatron group at the University of Illinois over a period of thirty years. Forty-two students completed their doctorates in the plasma group at the University of Wisconsin during the seventeen years that he led the group. Over the last forty years many of the leading scientists in the fields of accelerator physics, nuclear physics, medical physics, and plasma physics received their degrees under his direction.

Donald Kerst was born in Galena, Illinois, on November 1, 1911. He earned a bachelor's degree in 1934 and a doctorate in physics in 1937 at the University of Wisconsin. He was an instructor, assistant professor, and then professor at the University of Illinois from 1938 to 1957. He then was professor at the University of Wisconsin from 1962 to 1980. In short, except for some few years spent at the General Electric Company (1937–38 and 1940), at Los Alamos, New Mexico (1943–45), and at the General Atomic Laboratory, La Jolla (1957–62), he spent his life in the midwest.

THE BETATRON

Among the many investigators who attempted to accelerate electrons by magnetic induction, none were successful until Donald Kerst produced 2.3-MeV electrons in a betatron at the University of Illinois in 1940. He later constructed a number of betatrons of successively higher energies, culminating in the 300-MeV betatron at the University of Illinois. Kerst's success was due to a very careful theoretical analysis of the orbit dynamics in accelerators (including a study of the requirements for injection); to a preliminary analysis of all conceivable effects relevant to the operation of a betatron; and to a careful and detailed design of the magnet structure, vacuum system, and power supply. This was the first new accelerator to be constructed on the basis of a careful scientific analysis and a completely engineered design. Its success represented a turning point in the technology of particle accelerators from cut and try methods to scientifically engineered designs. All later accelerators, including the newest high energy synchrotrons, have been influenced by this early work of Kerst. It is only in the light of these later developments that we see the importance of the betatron not merely as a valuable instrument in itself but as a milestone in the development of particle accelerators generally. For example, the radial and vertical oscillations of the beam in all particle accelerators are now universally called betatron oscillations after the pioneering work of Kerst and Robert Serber, who together in 1941 published the first theoretical analysis of such oscillations as they occur in the betatron.

The betatron was quickly put to use in industry, medicine, and nuclear physics research. It was the first accelerator to provide gamma rays for photonuclear studies. In the late 1940s and early 1950s the betatron was used for much of the experimental research on photo disintegration of the deuteron, on photonuclear reactions (including the discovery of the giant dipole resonances), and important early work on nuclear structure from electron scattering.

Of great importance was the pioneering use of megavolt electron beams for the production of energetic X rays for the therapeutic treatment of cancer. His fascinating depiction of this treatment included a description of the first use of phantoms and the intense activity precipitated by a student afflicted with brain tumor, heroic efforts that achieved much, but were unable to save the student.

Kerst took a one-year leave of absence from the University of Illinois (1940–41), designed a 20-MeV betatron and a 100-MeV betatron working with the engineering staff at General Electric. He oversaw the construction and operation of the 20-MeV betatron, which he brought back to Urbana.

During World War II days, Kerst built a 4-MeV portable betatron for inspecting bomb duds *in situ* and, most importantly, built a 20-MeV betatron at Los Alamos for study of bomb assembly implosions. His work was described in the official history of Los Alamos as: "The technical achievements are amongst the most impressive at Los Alamos."

After World War II Kerst built a 300-MeV betatron at the University of Illinois that was brought into operation in 1950 and provided a facility for studying high energy physics until it was superseded by synchrotrons and then by electron linacs.

THE MURA YEARS

From 1953 to 1957 Kerst served as technical director of the Midwestern Universities Research Association, working on advanced accelerator concepts. His deep understanding of the physics of electric and magnetic fields and of mechanics and his vigorous technical leadership were responsible in large part for the many contributions to accelerator technology made by the MURA group during that period. In addition, the knowledge and inspiration gained under his leadership marked the beginning of productive scientific careers for a number of young physicists who were associated with the group at that time. The spiral-sector focusing principle, which now finds application in many spiral ridge cyclotrons in operation around the world, was originated by Kerst.

Among the contributions to accelerator technology of the MURA group under his leadership was the invention and analysis of the process of beam stacking by means of radio frequency acceleration in fixed field machines. The possibility of achieving intense circulating beams by means of beam stacking led to the first practical proposals for achieving greatly increased center-of-mass energies through the utilization of colliding beams. The successful storage rings for colliding proton beams at CERN in Geneva, Switzerland, and at Fermilab in the United States are a direct outgrowth of the MURA proposals. It is now recognized that colliding beams represent the major approach for further advances in experimental high energy physics.

PLASMA PHYSICS

In 1957 Kerst turned his attention to the area of plasma physics and its applications to the problems of developing controlled thermonuclear power. He brought to this field not only his deep physical insight into magnetic field structures but also his understanding, gained from his accelerator experience, of the importance of careful attention to detail in the design of magnetic structures so as to eliminate all possible sources of error and asymmetry in the magnetic fields. These are largely responsible for the success of the various toroidal machines that have been built under his direction, including a toroidal pinch device at General Atomic and a number of multipole machines (of which he was co-inventor with Tihiro Ohkawa). The first multipole machines were the toroidal octupoles completed at the University of Wisconsin under his direction and the toroidal octupole started by him and Ohkawa at General Atomic and completed by Ohkawa. These were the first magnetic confinement devices to achieve a quiet plasma undisturbed by the instabilities that had plagued previous machines. These were also the first machines to exhibit plasma lifetimes exceeding the Bohm diffusion limit.

PERSONAL DATA

Donald Kerst held honorary degrees from Lawrence College (1942), the University of Sao Paulo (1953), the University of Wisconsin (1961), and the University of Illinois (1989). He was awarded the Comstock prize by the National Academy of Sciences in 1945 and elected to membership in 1951. He received the John Scott Award of the City of Philadelphia in 1946; the John Price Wetherill Medal of the Franklin

Institute in 1950; the James Clerk Maxwell Prize in plasma physics from the American Physical Society in 1984; and the Robert R. Wilson Prize for accelerator physics in 1988. He was a member of the American Association for the Advancement of Science and the American Academy of Arts and Sciences, an honorary member of the American Association of Physicists in Medicine, and a fellow of the American Physical Society and the American Nuclear Society. In 1972–73 he chaired the Plasma Physics Division of the American Physical Society.

Donald Kerst was a well-rounded person. He was a sportsman who enjoyed skiing, deep-sea fishing, white-water canoeing, and ocean sailing. He had a low-key sense of humor that often delighted friends and colleagues. Even in recreation he remained the scientist. He taught a course in celestial navigation and wrote a program for his Hewlett Packard handheld calculator that provided location on the Earth to within a mile or so.

It would have been easy for one as accomplished as Donald Kerst to intimidate others, but that wasn't his style. He always treated graduate students as his equals. He found merit in even the craziest idea. He built others up; he did not put them down. He expected the best from people, and they worked hard to live up to his expectations.

He was a generous man. He always gave credit to those who worked for him. At scientific meetings he would let them give the talks. He seldom put his name on papers they wrote to describe the work done under his supervision. When important people from other laboratories visited, he would usually have the graduate students show them around and describe the machines and the research. And, of course, his love of physics was legendary. He got great pleasure from thinking about physics and in working out problems. When he did, he had to tell someone; often there were late night calls or pages at airports.

Donald Kerst is survived by his wife, Dorothy Birkett Kerst, his children Marilyn E. Kerst and Stephen M. Kerst, his grandchildren Rosalind and Susanna Sipe and David and Anita Kerst, and by his brothers Herman S. Kerst, Richard N. Kerst, and Kenneth A. Kerst.

IN PREPARING THIS MEMOIR we have drawn freely upon obituaries in a number of newspapers, a resolution by the faculty of the University of Wisconsin, and remarks made at the memorial service by Keith Symon and J. Clinton Sprott. In addition, we have profited by letters of nomination written through the years for the Enrico Fermi Award and the National Medal of Science by Keith Symon and Heinz Barschall. We found most informative an article titled, "Historical development of the betatron" [*Nature (London)* 157(1946):90] and an article titled, "Betatron-Quastler era at the University of Illinois" [*Med. Phys.* 2 (1975):297]. Finally, the photograph of Donald Kerst has been provided by the University of Illinois Archives (Donald W. Kerst papers, Record Series 11/10/30, box 5).

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Photo by Thad W. Sparks, Duke University, Durham, N.C.

PAUL JACKSON KRAMER

May 8, 1904–May 24, 1995

BY JOHN S. BOYER AND AUBREY W. NAYLOR

PAUL KRAMER WAS A gifted plant physiologist whose life was characterized by a special ability to conceive undeniable experiments and explain them simply and convincingly. He spent his entire professional career at Duke University, arriving at the new West campus in 1931 while the grass was being planted, "retiring" in 1974, but continuing his experiments with his many friends and devoted colleagues until he died. He was also a student of history and the arts, and his sixty-four years in science formed perspectives that few of us have. He could expound on the development of science in the United States or the history of politics in China, and he had a unique understanding of human nature that gave him uncommon persuasiveness when he developed a large project or administered complex organizations.

As an adviser to students, Paul was a listener and would say "Why don't you try it?" when an experiment was proposed. To the wilder ideas, he would lean back in his chair, fold his hands in back of his head as he looked at the ceiling, and whistle tunelessly. After a moment, he would say he needed to give it further thought and the student knew it was time to move to a different idea. This gentle guidance and intellectual liberalism gave many students the courage to work on diverse projects, and his lab bubbled with interactions between his own students and others from biochemistry, ecology, forestry, and elsewhere.

THE EARLY YEARS

Paul was born in Brookville, Indiana, near the Ohio border where his father farmed his mother's family farm. Paul milked the cows and weeded the garden at an early age, and was delayed in going to school until his younger sister also could go, because travel was so difficult. They rode by horse and buggy for four or five miles to a one-room school house. In 1912 his father, who was a devoted educationist, moved the family to a farm near Oxford, Ohio, where Miami University is located, "in order to educate his children," he often said.

They attended the village school in Oxford and eventually graduated together from high school. As he and his sister grew up, they saw the farm change from mostly hand labor to machine operations, and young Paul became good at mechanical repairs at the same time he learned about plants and animals of many kinds. His father had attended a small college for one year and Paul read often in the family's unusually large library. Family conversations ranged widely, with heavy use of the encyclopedia and dictionary, and the children enjoyed many cultural events on the Miami campus nearby. After completing high school, they entered Miami University but continued to live at home for financial reasons. They missed a lot of social life, but Paul felt the chance for an education far outweighed this disadvantage.

Paul enrolled in an economic botany course almost by accident in his first semester because of a chance conversation and recommendation by the president of the university

R. M. Hughes, whom he had met a few days earlier. The economic part of the course appealed to his background and gave him an early introduction to the subject. He finally majored in botany partly because he liked the laboratory work and partly because he was allowed considerable freedom and a small stipend as a teaching assistant. In fact, during his senior year, after the instructor in plant physiology became ill, Paul taught the remainder of the course.

One of Paul's problems in college was that he liked almost everything and was well acquainted with the arts and history. Science won out because plants were already so familiar and his mentor at Miami convinced him he had a promising future in the plant sciences, encouraging him to go to graduate school. The one subject he did not like was mathematics, which plagued him in his later life, although he insisted on doing the family taxes every year. Paul felt that one of the most important lessons from this breadth of interest was the ability to distinguish what was important from what was trivial. His writing and teaching were marked by exceptional clarity, reflecting this ability to think logically, and he often counseled students to identify what instructors regarded as important and focus on it.

In his senior year Paul decided to go to graduate school partly because he could be paid at the same time. Most schools offered $350 per year, but the University of Idaho offered $750, a magnificent sum in 1926. He worked there for a year with Prof. Floyd W. Gail, but decided to return closer to home. Before going he worked for the summer in northern Idaho in the U. S. Department of Agriculture's eradication program for white pine blister rust. In an isolated lab he studied the germination of wild *Ribes*, an alternate host for the disease. This gave him a little income and an enjoyable summer in the woods with other workers, thereby gaining some experience in dealing with people.

Paul's scientific career came increasingly into focus in 1927, when he entered graduate school at Ohio State University, where his adviser was the noted ecologist E. N. Transeau. Osmosis had been understood in the 1870s by J. Willard Gibbs at Yale and Wilhelm Pfeffer in Germany, but they did not explain how water moved to the top of tall trees. Dixon had subsequently shown that water columns could hold together and come under large tensions before breaking, thus providing a mechanism for "pulling" water to the tops of trees through the hollow vessels in the stem. It remained, however, to relate how living cells of the root absorbed water from the soil, because these cells could carry on osmosis and separate water in the root vessels from that in the soil. In 1928 this question was very much alive and Paul read the opinion of B. E. Livingston, supporting the view of Renner in Germany, that osmotic absorption is unimportant in transpiring plants.

As a model Paul chose a petiole from a large leaf and began by comparing the flow of water under tension or pressure with that caused by osmosis. He could fill the central cavity in the petiole with sucrose solutions to simulate the solution in the vessels of a root and he could pressurize the outer surface of the petiole while observing the cut end at atmospheric pressure. He found that much more water was moved across living cells by pressure and suction than by osmosis. He then similarly measured the water movement across roots. In all cases, the amount of water was much greater when suction was applied. If he killed the root systems, the flow increased under suction indicating that tensions could extend through the living tissues, "… reducing the role of the living cells of the roots to that of a mere absorbing surface, a role which might in some respects be filled as well by dead as by living cells …" (1932). He concluded that the living cells allowed rapid and passive water movement through them under tension in transpiring plants. At night they acted as a membrane, allowing osmosis into the vessels of the nontranspiring plants and creating "root pressure" and root exudation. The living cells mostly prevented air from entering the vessels and by growth extended the absorbing area of the roots.

THE MOVE TO THE SOUTH

Positions were few and pay was low in 1931 when Paul finished his Ph.D. He nevertheless refused an offer in peach research in Georgia for pay he considered too low ($1,600 per year) and held on for a few weeks until the young lady whom he later married noticed that the biology department of Duke University, recently formed from Trinity College, was looking for a plant physiologist. He was pleased to be offered the appointment at a salary of $2,000 per year, and he accepted.

Since Paul was about to be employed, Edith Vance and he married in June 1931; their honeymoon was a trip to North Carolina to inspect the university he had agreed to join. Later in the summer they moved to Durham with all their belongings in the rumble seat of a Model A Ford coupe purchased with money borrowed from a professor at Ohio State.

At Duke he was given a heavy teaching load. As the Depression was deepening, his salary was cut. Graduate students were few. He threw himself into his work and built on the experiments from his dissertation. Edith had graduate training and had taught botany at Vassar for two years. She encouraged him, knowing the purpose behind his dedication, and added her judgment to his thoughts. Soon daughter Jean Jackson was born and, later, son Richard Vance.

Paul recognized that the dead root experiments he had been conducting eliminated all theories of water absorption depending on live roots. He began more experiments of this type and found that intact plants with dead roots transpired copiously although not as much as plants with live roots. This indicated that forces originating above the roots were responsible for much of the water flow from the soil into the plant, which was consistent with the findings of his dissertation that tensions could be transmitted long distances through living root tissues and into the soil. He soon investigated whether temperature affected water movement into roots and could not distinguish between soil and root effects, so he investigated the soil alone using a porous clay surface to simulate a root surface. He observed a marked effect of temperature that he attributed to the viscosity and vapor pressure of the soil water. This paper was the first to measure temperature effects on soil water movement and is a good example of how easily he could isolate complex problems into simpler testable units.

As he worked, Paul became increasingly convinced that transpiration in the shoot was the origin of much of the force for water absorption, and he began to study the relationship between them in detail. He grew plants with an auto-irrigating reservoir to show how much water was absorbed by the roots and he weighed the plants and reservoir to obtain the amount transpired by the shoot. Transpiration always started before absorption, and the reverse occurred at the end of the day. This indicated that shoot dehydration was necessary before water could move. He postulated that the dehydration generated tension in the vessels and moved water into the roots, and the time to dehydrate caused a lag of about 2 h. He termed the effect the "absorption lag." This explained the role of the shoot in moving water into the roots and formed the basis for the present theory of water uptake by plants.

He began to think that the roots might be an important resistance to water uptake and measured the effects of excising the roots. Their removal shortened the absorption lag considerably, and absorption was transiently increased. He concluded in 1938 that "… the living cells between epidermis and xylem offer considerable resistance to the passage of water and are probably responsible for a large part of the lag of absorption behind transpiration … This resistance is much greater at low temperatures than at high temperatures, probably because the viscosity of both protoplasm and water increases as the temperature decreases."

Next, Paul quantified the water movement by transpiration and by osmosis by measuring transpiration in whole plants, removing the shoot, then determining how much water the roots delivered by forces generated in the roots alone. Initially, after removing the shoot, water was absorbed into the root through the stump and thus was opposite to that in the intact plant. Eventually, a small amount of exudation occurred from the stump. In no case could root exudation account for the water absorbed when the plant was intact and transpiring, and he said (1939), "Most, and possibly under some conditions all, of the water absorbed by transpiring plants is absorbed as a result of forces set in motion by the loss of water in transpiration." Further experiments showed that forces generated by transpiration moved less water when aeration was poor, temperatures were low, and water was deficient around the roots; the cause was resistance by the roots.

Thus was built the idea that water absorption in plants occurs slowly by osmotic means at night when transpiration is negligible and results in "root pressure" in the vascular system often leading to the formation of droplets around the margins of leaves (guttation). During the day water is absorbed by forces originating in the shoot because of the dehydration caused by transpiration, and these forces extend through the living tissues of the root into the soil water. The forces are much larger than can be generated with a vacuum pump and indicate that large tensions can be generated in the vessels and cause correspondingly large flows through the root. These concepts had a great impact on the field of plant physiology. They explained how water movement could be passive but still be affected by root metabolic activity. They also explained why plants could become water deficient in a flooded field. Farmers were amazed when they saw their tobacco leaves wilt after rain that flooded the soil, but Paul had the answer.

THE LABORATORY FLOURISHES

The decade of the 1930s marked Paul as a particularly capable scientist. His lab began to attract students. Paul called one prospective student on the telephone, saying, "I see that you want to be a physiological ecologist. What about becoming an ecological physiologist?" The student was so pleased that he accepted. Eventually more than forty students would receive their Ph.D. degrees from him. He would say, "I am not going to look over your shoulder. You know where I am when you need advice," and he was always willing to listen. He was kind in his suggestions and frequently gave a student a chance if he saw signs of commitment. He thought work could make up for a late start and would sometimes offer a summer fellowship to someone in need or encourage a promising technician to go on to graduate school. He and his students would often read scientific papers containing statements with which they could not agree. Paul would suggest that they write the author and would pen a letter that was polite but firmly in disagreement. Many of his students gained confidence from this display of scientific forthrightness.

Soon after arriving at Duke in 1931, Paul had made the acquaintance of C. F. Korstian, who had come from Yale to develop Duke Forest and establish the School of Forestry. Korstian thought physiology and ecology were essential for training in forestry and provided Paul with some equipment and support for his first two graduate students. A lifelong collaboration began with the forestry school and many original papers were written with students from forestry. With woody plants he found that tree dormancy was regulated in part by photoperiod and thus light was an important signal for the onset of winter. He and his students found that the eventual transformation of pine forests to hardwood forests resulted from the inability of pine seedlings to grow in the low light intensities under deciduous trees, which was one of the first physiological explanations of the succession of plant species in natural communities. Much forest practice is now based on this principle.

Paul was probably the first to measure the uptake of phosphorus by mycorrhizal roots of trees with radioisotopes. With his student H. H. Wiebe, Paul also investigated the absorption of water and radiolabeled phosphate along roots and discovered that much salt uptake occurred some distance behind the root tip where the xylem was well differentiated. He continued to study salt uptake in roots whose anatomy had been changed by becoming dormant or by developing suberized bark. Many of his findings had practical applications, such as the need for mycorrhizal organisms in tree culture, the importance of root temperature for salt and water uptake in plant culture, and the necessity of soil aeration in trees planted in urban settings.

NATIONAL ATTENTION

Paul's simple experiments were clearly described and interpreted with compelling logic, resulting in his publications being quickly recognized. By 1952 he had already published forty papers and one book. In the early 1940s he had become involved with the American Society of Plant Physiologists and served in several offices, including the presidency in 1945. The Botanical Society of America elected him president a few years later. He was appointed director of Duke Gardens, where he oversaw expansion during his twenty-nine years of service that made it a regional and national showplace. During the 1950s he became involved in faculty affairs, because he felt the faculty should have a voice in university governance. After a few years Paul became vice-chairman of the Faculty Council of the university. During this service a difficult power struggle began between the president and the vice-president of the university. Paul was caught in the middle of an unfortunate situation that incurred the dislike of several supporters of one faction or the other, and great diplomatic skills were required. When the National Science Foundation offered him a year as a program director for developmental biology in 1960, he was glad to leave the campus. While at the NSF he was asked to apply for the director's position for the Division of Biology, but he declined because of memories of the administrative problems at Duke. He continued to spend time on panels and committees in Washington and became president of the American Institute of Biological Sciences, an organization he had helped create as a voice for all biologists. Unfortunately, AIBS had overextended its finances based on grants for several years and Paul had the difficult job of sorting this out during his presidency. His persuasiveness with the sponsors and ability to prune the institute's activities brought the organization through.

THE FATEFUL SABBATIC

In the mid-1950s, as the number of graduate students was increasing, Paul decided he needed to get away and took the family on a sabbatic leave at the California Institute of Technology, where he worked in the Phytotron. This experience with controlled environments caused him to think that whole plants could be studied much as an enzyme is studied under controlled conditions in a test tube, and here was a way of bringing biochemistry and whole plants together. He wrote (1973), "Instead of being the master of whole organism biology, molecular biology really is its useful servant, helping to explain at the molecular level why organisms behave as they do," and the controlled environment promised to bridge this complexity.

Paul believed fervently in the concept that genetics and the environment play equal roles in growth and development, and he knew from his farm and research experience that environmental resources frequently control plant performance (1973), "There are specialists on the carbon pathway in photosynthesis, the energy transfer system, and the structure of chloroplasts, but the effectiveness of photosynthesis as a supply of energy for plant growth is limited more often by stomatal behavior, leaf structure, mineral nutrition, or water supply than by processes at the molecular level."

The experiences in Washington and at the NSF gave Paul confidence to apply for funds for a controlled-environment facility that he had so admired at Caltech. He campaigned for one in the eastern United States and in 1965 obtained a large grant from The NSF to build two! One was to be at Duke and the other at North Carolina State University. The two facilities have provided major boosts to plant research on the two campuses, and his own work made extensive use of them. In the Duke facility there were investigations of the coupling between solute and water flow that unified disparate results from other labs that had been unexplained for years. Further work, with his colleague Aubrey Naylor, showed that differences in membrane lipids caused differences in root resistances to water flow, which helped explain why roots had a high resistance to water flow and why killed roots transmitted more water than live roots. There were experiments with how CO_2 affected growth and why water deprivation inhibited photosynthetic CO_2 fixation.

Paul and his students showed that pineapple uses a specialized photosynthetic metabolism found in desert plants and thereby conserves water. Later, after reaching emeritus status, he became interested in nuclear magnetic resonance imaging as a way to follow paths of water movement from soil to roots, and he published with other colleagues the first pictures of water depletion zones around roots in undisturbed soil.

THE BOOKS

Despite all this heavy work, in 1949 he found time to publish a book on water relations of plants that was so well received that he did it again in 1969 and then in 1983. He published books on tree physiology and ecology with his student T. T. Kozlowski in 1960 and 1979 and with Kozlowski and S. G. Pallardy in 1991. These books were immensely popular because of Paul's ability to simplify complexity and concern himself with the main points of the arguments. He took it as a compliment when one reviewer said that he made complex concepts too simple. The books were translated into several languages, sometimes without his knowing it. He is probably as well known for these works as for his distinguished laboratory experiments.

IN CONCLUSION

Paul gave credit to many other scientists for his own contributions. He felt that any discoveries he may have made were initiated by the work of others. Others tended to recognize his accomplishments more than Paul, who sometimes would say that he never made a major discovery. Nevertheless, he was given an award of merit from the Botanical Society of America, an achievement award from the Society of American Foresters, a Barnes Life Membership in the American Society of Plant Physiologists, and a distinguished services award from the American Institute of Biological Sciences. He was elected a fellow of the Australian Academy of Sciences and was invited to membership in the American Academy of Arts and Sciences, the American Philosophical Society, and the National Academy of Sciences (1962). He was given honorary degrees by the University of North Carolina, Miami University, Ohio State University, and l'Université Paris VII. He served on the original board of editors for *Annual Review of Plant Physiology* and several committees of the National Academy of Sciences, including one that brought the American Institute of Biological Sciences into existence.

Paul felt that the best way to know oneself was to lose oneself in some kind of work, and his steady stream of papers and books demonstrates that idea. His last academic effort was a book with John Boyer on the water relations of plants, written when Paul was ninety. He was still hoping to dehydrate a plant in a magnet and see the effects on roots, which had not been done before, when the end came from pneumonia.

MOST OF THE MATERIAL for this memoir came from the "Autobiographical Statement of Paul Kramer" (National Academy of Sciences, Washington, D. C., 1987) and from personal conversations with Paul while John Boyer was his student and while Aubrey Naylor was his faculty colleague for forty-three years.

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Courtesy of Brookhaven National Laboratory

Milton Stanley Livingston

May 25, 1905–August 25, 1986

BY ERNEST D. COURANT

ON JANUARY 9, 1932, in Berkeley, California, a magnetic resonance accelerator (cyclotron) built by M. Stanley Livingston accelerated protons to 1.22 MeV (million electron volts), the first time that particles with energies exceeding one million volts had been produced by man. Twenty years later, in May 1952, the Cosmotron at Brookhaven National Laboratory, whose construction Livingston had initiated, became the world's first billion-volt (GeV) accelerator. By the time of his death in 1986 the world record had gone up by three more orders of magnitude to 900 GeV, thanks to an innovation by Livingston and others.

Milton Stanley Livingston was born in Broadhead, Wisconsin, on May 25, 1905, the son of Milton McWhorter Livingston and his wife Sarah Jane, née Ten Eyck. His father was a divinity student who soon became minister of a local church. When Stanley was about five years old the family moved to southern California, where his father became a high school teacher and later principal, having found that a minister's salary was inadequate to support a growing family. On the side he bought a 10-acre orange grove and ranch.

As the only son in the family—there were three sisters—Stanley grew up learning and doing all the chores on the ranch. He became acquainted and fascinated with tools and farm machinery; these skills became the foundation of his ability as a designer and builder of complicated scientific systems throughout the rest of his life. On the other hand, as he said in an interview many years later, having three sisters, "I never did a dish in my life." When Stanley was about twelve years old his mother died. His father remarried a few years later and continued to expand the family; as a result Stanley had five half-brothers.

After graduating from high school in 1921, Stanley went to Pomona College, initially majoring in chemistry. Toward the end of his years there his interest switched from chemistry to physics, stimulated by his college roommate Victor Neher and physics professor R. Tileston. He then went east to Dartmouth College, where he obtained a master's degree in physics and stayed on as an instructor for a year. To continue his graduate work he had a choice between Harvard and the University of California, and he chose California because it was close to home.

At Berkeley in the summer of 1930 he canvassed the professors of the Physics Department to look for a Ph.D. topic and chose one suggested by Ernest O. Lawrence. Lawrence had noted that ions of mass M and charge e moving in a uniform magnetic field B would circulate at a constant frequency $\omega = eB/Mc$ independent of energy, and therefore an applied radio-frequency field of that same frequency should be capable of accelerating the ions. If a radio-frequency electrode has a voltage V oscillating at the resonance frequency and a particle traverses it at the peak phase of the oscillation, the total energy gained by a particle in N traversals is N times e V. Thus, a small applied voltage V can lead to a large final energy. Lawrence suggested that Livingston verify this idea experimentally.

Another of Lawrence's students, Niels Edlefson, had made a preliminary attempt to find the resonance, but had obtained inconclusive results. Livingston used the 4-inch magnet previously used by Edlefson, built a metal vacuum chamber and installed a hollow D-shaped accelerating electrode in it, added an rf oscillator, and put the whole system together. With Lawrence's active help and supervision he first found evidence of resonant acceleration about November 1930. In January 1931 hydrogen molecular ions ($H2^+$ ions) were accelerated to the maximum energy available with the magnet, namely 80 keV using an applied voltage of just 1 keV—an energy amplification factor of 80! This was enough for Lawrence to apply for grants to build a new device of this type capable of going up to energies useful for nuclear disintegration experiments. He prodded Livingston to write his thesis and get his Ph.D. degree as quickly as possible so he could become eligible for appointment to an instructorship for the following academic year. Livingston, who had recently married and needed a more secure job, needed no urging. He wrote up the thesis in two weeks and underwent his Ph.D. oral examination in April. He had been so busy in the lab that he had not studied much of the basic literature of nuclear physics, and some members of his examining committee were critical of his lack of general preparation. But Lawrence's enthusiasm and persuasiveness saved the day, and Livingston got his degree and the instructorship.

He immediately went to work on designing and building the next machine, an accelerator designed to go to one million volts, for which Lawrence had obtained funds. This one was based on a magnet with an 11-inch diameter and included an rf accelerating system capable of providing the resonance frequency for protons, which is twice as high as for $H2^+$ ions. Acceleration took place in the gap between a pair of hollow D-shaped electrodes with grids at the edges of the gap to confine the electric field to the space between the "dees." Lawrence had emphasized that it was important to have no electric field inside the dees.

A crucial step came serendipitously. During the summer, while Lawrence was away on a trip, Livingston decided to see what would happen if he removed the grids, since they necessarily intercepted some of the particles and tended to reduce the number of particles accelerated. Surprise! The resonance still worked and the beam intensity leapt up by a factor of 100—far more than could be accounted for by the beam loss from the grids. The reason—quite unanticipated by Livingston and Lawrence, but recognized almost immediately—was that the electric field inside the dees produced electric focusing, which kept the particles from flying off to the top or bottom of the chamber as they had done before. This effect was most important at the center of the device.

An equally important development was the discovery of magnetic focusing. To smooth out possible imperfections in the uniform magnetic field Livingston inserted magnetic "shims," thin iron plates, between the magnet poles and the vacuum chamber. He found that he got the best beam intensity when these shims were shaped to make the field a little stronger at the center than toward the outside. After this empirical discovery it became clear that the curved lines of magnetic force associated with the field becoming weaker toward the outside would focus the beam, imparting a downward component of force to particles above the median plane and vice versa. And this effect becomes more pronounced toward the outside of the orbits, complementing the electric focusing, which is strongest at the center.

So, on January 9, 1932, to quote Livingston, "I recall the day when I had adjusted the oscillator to a new high frequency, and, with Lawrence looking over my shoulder, tuned the magnet through resonance. As the galvanometer spot swung across the scale, indicating that protons of 1-MeV energy were reaching the collector, Lawrence literally danced around the room with glee. The news quickly spread through the Berkeley laboratory, and we were busy all that day demonstrating million-volt protons to eager viewers." [1]

Lawrence lost no time raising money for a bigger magnet, and by the time the 11-inch accelerator had achieved its million volts the new magnet with 27.5-inch pole faces was already installed on the campus. Livingston switched his attention to the task of building a cyclotron with this magnet (the term "cyclotron" for the circular magnetic resonance accelerator was coined sometime during those years, and soon came into general use). The new cyclotron got its first beam in the summer of 1932 and by the end of the year it accelerated H_2^+ ions to 5 MeV.

As the Berkeley people were rejoicing in their world-record energy, news came that Cockcroft and Walton in England had accomplished the first artificial disintegration of nuclei with a high voltage generator of only half a million volts. Lawrence, Livingston, and the rest of the Berkeley crew immediately went to work bombarding all sorts of targets and duplicated and extended the Cockcroft-Walton results. An important contributor to this achievement was G. N. Lewis at Berkeley, who had found a way of producing substantial quantities of heavy hydrogen (deuterium) by electrolysis. He made deuterium available to the cyclotronists, and they found that deuterons were even better projectiles for nuclear disintegration work than protons. In addition neutrons were produced copiously by deuteron bombardment of many different targets. Lots of experiments were done with deuterons and neutrons, and the cyclotron's performance was continually improved, largely by Livingston's efforts. Livingston, together with Lawrence, M. G. White,

M. C. Henderson, and others, was an active participant in these nuclear physics experiments, as well as the cyclotron development work. They kept in constant touch with English and French groups doing nuclear disintegrations with radioactive sources and Cockcroft-Walton generators.

A big controversy arose. Livingston, Lawrence et al. had found protons of the same energy emitted when different elements, light or heavy, were bombarded with deuterons. They interpreted this as evidence for instability and spontaneous breakup of the deuteron and deduced a value for the mass of the neutron that was appreciably lower than the Europeans' value. Eventually it was found that the Berkeley result was due to target contamination, and the European value of the neutron mass turned out to be correct.

In early 1934 Berkeley was scooped again—this time by the discovery of artificial radioactivity by the Joliots in France. Again this was something they had overlooked. The moment they heard about it they also found radioactivity in their targets and, of course, the Berkeley cyclotron was the ideal instrument for studies of induced radioactivity and production of radioactive isotopes.

Altogether in the years 1932–34 Livingston was author or co-author of more than a dozen papers on nuclear physics and the apparatus. During this time he systematically compiled a file of all the nuclear reactions and radioactive isotopes that had been found.

After four years at Berkeley, Livingston decided it was time to be more independent. He felt he had not received as much recognition at Berkeley as was warranted. Lawrence, who had become world-famous for the cyclotron, did not generally credit Livingston much for his part in the development. He moved to Cornell, where he built a cyclotron on his own—the first successful cyclotron away from Berkeley, a 2-MeV machine. He did this with a grant of just $800 and with the help of the departmental shop and graduate students.

At Cornell, Livingston, together with Robert Bacher and Hans Bethe, established nuclear physics as a field of study in the department and Cornell as a major center of this discipline. Livingston's compilation of data he brought from Berkeley proved to be very useful for the monumental series of three review papers by Bethe, Bacher, and Livingston that appeared in *Reviews of Modern Physics* in 1936 and 1937, and have been reprinted many times since. Livingston credits Bethe with really broadening his understanding of nuclear physics. One of the significant accomplishments at Cornell was the demonstration that the neutron has a magnetic moment.

In 1938 Robley Evans at MIT began a project to build a large cyclotron, financed by the Markle Medical Foundation and intended for medical applications and nuclear physics. He persuaded Livingston to come to MIT to build this machine. So, even though he was in no way dissatisfied at Cornell, Livingston made another move. The MIT cyclotron was finished by 1940 and worked very well. Livingston continued his evolution into an all-around nuclear physicist and professor, teaching courses and supervising graduate students.

When many of his colleagues transferred to radar work or the Manhattan project, Livingston stayed with the cyclotron. It too had an important role to play in the war effort—the production of radioisotopes for medical purposes. Soon the cyclotron was running around the clock. In 1944 his senior colleague Philip Morse enlisted Livingston in his operations research effort with OSRD and the Navy Department. He spent two years in Washington and London working on antisubmarine radar and countermeasures.

Not long after Livingston returned to MIT, physicists from

Columbia, Harvard, MIT, and other universities explored the possibility of setting up a new laboratory for nuclear science somewhere in the northeast. By the middle of 1946 a site was chosen, namely the future Brookhaven National Laboratory on Long Island. Philip Morse became director of this new laboratory. Its mission was to establish research facilities on a scale too large for a single university and make those facilities available to researchers from all universities on a cooperative basis. The initial facilities were to be a nuclear reactor—the first one to be dedicated to research not associated with weapons—and a large particle accelerator. Morse persuaded his colleague Stan Livingston to take charge of the accelerator project at the new lab, and Stan took a leave of absence from MIT and moved to Bayport, Long Island.

What sort of accelerator should be built? At Berkeley, Lawrence had built a huge cyclotron with a 184-inch magnet, which was started in 1940, but was interrupted by the war. In fact, that machine turned out to be feasible only because in 1944 and 1945 Veksler in the Soviet Union and McMillan at Berkeley had come up with the principle of phase stability and synchronism, which overcomes a limitation on the energy that a cyclotron can attain. This principle had made it possible for the Berkeley cyclotron—now converted to a "synchrocyclotron" or frequency modulated cyclotron—to reach energies around 300 MeV. Several other synchrocyclotrons in that range were also being built at various universities, such as Columbia, Carnegie Institute of Technology, Rochester, Chicago, and Liverpool. Livingston felt that, with the resources available at Brookhaven, he could outdo Berkeley with a 600-700 MeV synchrocyclotron, and he began to assemble a staff to design and build it.

The Veksler-McMillan principle could also be applied to a ring accelerator for either electrons or protons. The ring has the advantage that the magnet only needs to cover the orbit corresponding to the full energy of the machine, rather than the whole area inside that orbit; thus, higher energies are practicable. Lawrence and his people at Berkeley were exploring the possibility of a 10,000-MeV (10-GeV) ring proton synchrotron. I. I. Rabi in particular argued forcefully that this was also the way to go for Brookhaven. Livingston agreed, and soon the proton synchrotron became the primary focus of the Brookhaven accelerator project. The cyclotron was eventually dropped.

In the spring of 1947 Morse, Livingston, and Brookhaven's personnel director R. A. Patterson went on a recruiting trip that took them to Cornell among other places. I was in my first year of postdoctoral work in nuclear physics there, under Hans Bethe, and at Bethe's suggestion I had looked at some problems in connection with the dynamics of synchrotrons. Livingston offered me a summer job at Brookhaven, and I jumped at the opportunity.

Brookhaven in the summer of 1947 was an exhilarating and stimulating place. I was swept along by Livingston's enthusiasm, and found myself working on this marvelous new project. There were frequent meetings to discuss and explore all aspects of the project. The main thing I learned from working with Livingston that summer was how one field—say, theoretical orbit dynamics—can impact on a very different field, such as vacuum technology, and vice versa. I went back to Cornell for the academic year, but the next summer I joined Brookhaven for good.

Shooting for a 10-GeV proton synchrotron put Livingston and Brookhaven into direct competition with Lawrence and Berkeley. The government (i.e., the Atomic Energy Commission) was unwilling to underwrite two large projects, and a meeting took place at Berkeley to decide how to proceed. Brookhaven was represented by Livingston, Morse, and Leland Haworth, the associate director for projects, and Berkeley by Lawrence, E. M. McMillan, and others. There was only enough money available for one 10-GeV machine. For both laboratories to get something, two smaller machines would have to be built—say, 6 and 2.5 GeV. Who would get which?

Haworth volunteered to take the smaller piece of the pie in the hope that Brookhaven would finish its record-breaking machine before Berkeley completed the more ambitious project. Then Brookhaven would be first in line for the next, even bigger, step. Livingston agreed reluctantly. In the end it turned out just that way. In later years Haworth often called this the best decision he ever made.

Livingston got the Brookhaven project going at full speed with a fresh and enthusiastic team made up of a number of young engineers and physicists; I was one of two theorists, and there were just two or three people besides Stanley Livingston who had any substantial experience. Somewhere along the line the name "Cosmotron" was coined to indicate that this machine would duplicate cosmic rays (well, almost). The design energy was upgraded from 2.5 to 3 GeV.

Stan had been on leave from MIT. By the end of 1948 this leave had stretched to two years, and he could not stay away any longer without forfeiting his tenure position. So he chose to return, leaving the cosmotron in what he hoped were capable hands. Back at MIT he Concentrated on teaching and supervising graduate students. At the same time he continued to think about accelerators and worked in other fields. In 1950 he participated in a Los Alamos experiment to investigate the lifetimes of short-lived fission products. He also began to explore the possibility of building a large accelerator in Cambridge as a joint effort of MIT and Harvard.

In May 1952 the Cosmotron at Brookhaven was finished and succeeded in accelerating a proton beam to a little over a billion volts—the first time such an energy had been reached in the laboratory. Soon it got to 2.5 GeV, close to the design energy of 3 GeV. (The larger Berkeley project was not yet finished, and was to take two more years).

Meanwhile, in Europe physicists from several countries began to explore the possibility of setting up an international laboratory for nuclear physics, aiming at a facility that might be beyond the reach of any one country but within the reach of all of them cooperatively. The concept was modeled on Brookhaven, which had been set up as a group effort by nine universities. In Europe it was to be twelve countries. The centerpiece was to be a new accelerator like the Cosmotron but bigger; the energy they had in mind was about 10 GeV.

The nascent Conseil Européen pour la Récherche Nucléaire (CERN) decided to send a team of experts to Brookhaven to see how we had done it, in the hope of getting some useful advice. Stan Livingston went to Brookhaven that summer to set up a study group to learn more about how the Cosmotron worked, what improvements might be suggested to the expected visitors, and to lay the groundwork for the Cambridge accelerator project.

One feature of the Cosmotron—designed into it by Livingston back when it was started—was that the magnet that had a C-shaped cross section with the iron yoke closing off the inside of the aperture but not the outside. This made it easy to get at the beam from the outside and for negatively charged secondary particles to emerge, but it led to an asymmetry in the magnetic field configuration that limited the useful part of the aperture (i.e., the space available for particles to circulate stably). Now it occurred to him that, if the ring were divided into sectors with the direction of the C-shape reversed in half the ring (i.e., the magnet yoke on the outside in half the sectors and inside in the other half), the asymmetry in the field would even out so that the useful aperture might be increased. Furthermore, in the sectors where the opening was to the inside, positive secondaries would be just as accessible as negative ones in the other half.

I pointed out immediately that a parameter known as the focusing gradient *n*, which governs the orbit focusing properties first enunciated by Livingston and Lawrence in connection with the original cyclotron, would be different in the sectors with inside- and outside-facing C's. This might well weaken the overall orbit stability. But then I did some quantitative calculations, using a formalism I had previously used in connection with other orbit stability calculations, and I found that the alternation of *n* could enhance stability rather than weakening it. Without alternations it was well known (from work by Kerst and Serber) that *n* had to be between 0 and 1, and it had been chosen as 0.6 for the cosmotron.

Using values of *n* that alternated between +1.2 and 0 (averaging 0.6 as before) I found slightly enhanced stability. The next day I did the calculation for +10 and -10 with more alternations, and the results were even better (the average of 0.6 turned out to be irrelevant). One day later I found a general recipe: Alternate the gradients in N pairs of sectors with the value of *n* alternating between about $+(N/2)^2$ and $-(N/2)^2$. Then the stability of the oscillations is enhanced by a factor on the order of N/2 (i.e., the transverse space needed for oscillations is reduced by that factor). If N and *n* are made very large the reduction factor is also large. Hartland Snyder, another Brookhaven theorist, pointed out an analogy between this and optical focusing of light beams.

Stan Livingston recognized this as something qualitatively new that would make it possible to build new accelerators with more compact magnets. These compact magnets would be much less massive and were bound to be much cheaper than the behemoths of the Cosmotron and Berkeley, and it should be possible to think of much higher energies at reasonable cost. Stan and the rest of us put these new ideas together with the engineering and design principles that had gone into the Cosmotron and in less than two weeks we drafted a paper for the *Physical Review*.

This paper described the new "strong focusing" principle and presented a conceptual design for a machine that could go to 30 GeV, ten times the Cosmotron's record and five times the energy of the coming Berkeley machine. For our example we took 120 pairs of sectors and $n = \pm 3600$. The space needed for the particles in the aperture of the magnet was calculated as less than an inch, as against 6 by 36 inches in the Cosmotron and 12 by 48 inches in the (more conservatively designed) Bevatron, the 6-GeV machine being built in Berkeley. In the same paper it was also recognized that the new focusing principle was quite separate from the problem of acceleration and it could be applied to beams of particles being guided in paths of any shape, keeping them focused with what came to be known as quadrupole lenses.

The CERN delegation arrived from Europe: Rolf Wideröe, whose concept of multiple acceleration has stimulated Lawrence to invent the cyclotron; Odd Dahl, who had worked on some early high-voltage machines in the 1930s, and F. K. Goward, the first to make the McMillan-Veksler synchrotron principle work. They found more in the way of ideas for their machine than they expected. Goward said, "I could kick myself for not having thought of that!" or words to that effect.

Our visitors left convinced that they should adopt this new principle for their laboratory and aim at 25–30 GeV rather than the 10 GeV originally planned. A study group had been set up in Europe even before the CERN laboratory was established in Geneva. In the course of their explorations they found that maybe we had been a bit overenthusiastic in our initial choice of numbers; a 1-inch aperture was too small, and a practical value of n should be closer to 300 than 3600. But it was still a big advance over the conventional ways.

Of course, Brookhaven went on with these studies and came up with the same conclusions and a design for 30 GeV. Stan went back to MIT after the summer and argued that the new principle now made it economically possible for a single university—or a pair, such as Harvard and MIT—to build its own multi-GeV accelerator at home. He got a number of talented people together to explore the possibility. So there were three groups: CERN, Brookhaven, and Stan's team at Harvard-MIT. We found that the best method of competition was complete openness and sharing of results. Reports produced at Brookhaven, CERN, or Cambridge were routinely and promptly sent to the other places. There was no secrecy!

This did not sit well with the Atomic Energy Commission (AEC). Accelerator physicists at Berkeley had been engaged in a classified project (known as MTA) to build accelerators for the production of plutonium for nuclear weapons (their preoccupation with that project was probably one of the reasons why the Berkeley Bevatron was two years behind the Cosmotron). Therefore, the reasoning went that innovative accelerators had to be classified and certainly should not be disclosed to foreign countries. Fortunately, Haworth (director of Brookhaven by that time) was able to put out that fire.

Early in 1953 an engineer from Greece, Nicholas Christofilos, appeared on the scene. He claimed he had invented the strong-focusing principle two years earlier and had sent a description of it to Berkeley. Indeed, people at Berkeley found his paper in their files. They had examined it superficially and dismissed it as one of the many crackpot letters that laboratories get. They and we were most embarrassed, and we published a letter in the *Physical Review* acknowledging Christofilos's priority. An agreement was reached with the AEC whereby he received a substantial payment, and he was hired to join the Brookhaven staff (he later left for Livermore).

Back at MIT, Stan embarked on a design study for a 15-GeV alternating-gradient proton synchrotron, which he hoped could be built there. A design study report, supervised by Stan and containing contributions by a dozen MIT and Harvard authors, came out in 1953. At Brookhaven we worked hard on a 30-GeV machine, and CERN embarked on a parallel study with the same goal.

In October 1953 a conference on "Theory and Design of an Alternating-Gradient Proton Synchrotron" was held at the University of Geneva. Stan and I and a few other Americans were invited. Numerous papers were presented showing that the new concept really seemed capable of fulfilling its promises. John and Hildred Blewett from Brookhaven had spent a few months with the CERN group, and also participated. On the weekend they took Stan and me on a drive around the Lake of Geneva, traversing mountain passes with magnificent views of the Alps. Stan exclaimed, "A mountain like Mont Blanc would really *make* Long Island," alluding to the fact that Brookhaven could hardly compete with CERN in the matter of scenic appeal.

Brookhaven applied to the AEC for authorization to build the 30-GeV alternating-gradient synchrotron (AGS) on which our design explorations had converged. In early 1954 this proposal was approved. Stan's plan for a 15-GeV machine became moot; there seemed to be no point in doing almost the same thing as Brookhaven but on a smaller scale. Instead, he began to think about an electron synchrotron for Cambridge, a 6-GeV alternating-gradient machine much higher than any other electron accelerator, but not too big a bite for a university-based project.

It took a few years before the AEC approved funding, and the project to build the Cambridge electron accelerator (CEA) finally got under way in 1956. It was a joint Harvard-MIT project located on the Harvard campus. Livingston, as director, was in charge of building the accelerator; Norman Ramsey oversaw the experimental program and physics applications. Stan assembled a staff of talented physicists, and one of them (John Rees) described Livingston's leadership style:[2]

Stan's most important and noteworthy trait as director was his irrepressible enthusiasm. It was that characteristic that enabled him to attract so bright and promising a staff of young physicists to design and build the CEA: Tom Collins, Gerry Fischer, Ewan Paterson, Ken Robinson, Gus Voss, Herman Winick and the list goes on. Most of these people went on to brilliant and creative careers of their own… Stan did something that was of great importance to our development: he gave us responsibilities and the authority to go with them … That is essential to developing leaders.

During this time Stan welcomed visits, short- and long-term, from people who were embarked on similar projects in Daresbury, England; Hamburg, Germany; and Yerevan, Soviet Armenia. All of these built 4–6 GeV electron synchrotrons similar to the CEA.

The CEA went into operation in 1962, and it held the record for the highest energy electron and photon beams for several years until the Stanford two-mile linear accelerator came into operation. The accelerator was used for numerous physics experiments, but a disaster happened when a beryllium window on a large liquid-hydrogen bubble chamber failed and the chamber exploded, killing one person and devastating the laboratory.

Fortunately, Livingston and his crew were able to rebuild and restore the laboratory. The crowning achievement was the installation of a beam bypass that made it possible to store counter-rotating beams of electrons and positrons in the ring and observe their collisions at 3 GeV per beam, the first multi-GeV colliding beams and the prototype for many electron-positron colliders that exist today.

By the time the Brookhaven and CERN proton synchrotrons had come into operation in the 30-GeV range in 1959–60, it was clear that the strong-focusing principle on which they were based could be stretched to much higher energies. Studies for a large proton synchrotron began in the early sixties, and in 1966 it was decided to build a new laboratory, the National Accelerator Laboratory, which was dedicated to the construction of a proton accelerator of at least 200 GeV. A site was eventually selected in the Chicago area, and R. R. Wilson became its director. Wilson recruited Stan Livingston as associate director, and in 1967 Stan moved to the new project, now known as Fermilab. He helped in setting up the new laboratory. The 200-GeV accelerator there has by now been upgraded to almost 1000 GeV (the Tevatron), and is still the highest energy accelerator in the world.

In 1970 Livingston retired. He and his wife had acquired a piece of land on the outskirts of Santa Fe, New Mexico, and had designed an imaginative, comfortable adobe house there. Stan proudly showed a model of it to visitors during his last years at Fermilab. In retirement he devoted himself to a newly acquired skill. He became an accomplished silversmith and created jewelry in the style of the local Indians. My wife's collection now includes a pair of Livingston earrings and a matching brooch. He did not abandon physics altogether. He spent several years as a consultant to the nearby Los Alamos Laboratory and on occasion acted as an administrative judge for the U.S. Nuclear Regulatory Commission.

Stan was also interested in and concerned about the role of science in society at large. In the 1950s he became active in the Federation of American Scientists, working on the problems of the interaction between science and public policy. He served as chairman of the federation in 1954 and again in about 1959. He was particularly concerned with problems of undue secrecy classification of scientific knowledge, unjustified political harassment and persecution of some scientists, and international control of atomic energy.

Stanley Livingston received a number of honors, mostly late in life. He received honorary degrees from Dartmouth College (1963), Hamburg, Germany (1967), and Pomona College (1971). In 1970 he was elected to the National Academy of Sciences. In 1985 the U.S. Summer School on Particle Accelerators presented him with a citation signed by the directors of seven high-energy physics laboratories all over the world. In 1986 the U.S. Department of Energy decided to honor him (and me) with the Enrico Fermi Award, previously given to Fermi, Lawrence, Wigner, Oppenheimer, and other pioneers. Alas, he was never to know this. He died on the very day the award committee made its decision, and on December 18, 1986, the award was presented, posthumously for the first time, to Mrs. Lois Livingston.

Stanley Livingston married Lois Robinson in 1930, while he was a graduate student at Berkeley. They had two children, Diane (Dee), born in 1935, and Stephen, born in 1943. In 1949 Stan and Lois were divorced, and he married Margaret (Peggy) Hughes in 1952. But Peggy was in poor health and died a few years later. In 1959 Stan and Lois were married again, followed by another twenty-seven years together. He remained in good health until his last year, but after a supposedly successful prostate cancer operation he never recovered completely. He gradually became sicker and sicker and after many difficult months he died on August 25, 1986.

Stanley Livingston's contributions—the cyclotron and the alternating-gradient synchrotron—have revolutionized modern physics and led to our present ability to probe not only nuclei but many esoteric particles—with promises of more to come.

I WANT TO EXPRESS my appreciation to Dee Livingston for sharing some of her memories with me and lending me her collection of her father's memorabilia. I also had instructive conversations with John Rees, Norman Ramsey, and Lois Livingston shortly before her death. The Niels Bohr Library of the American Institute of Physics made available a transcript of an interview with Stanley Livingston held in 1967. Robert Crease lent me a videotape of an interview conducted at Livingston's Santa Fe home in October 1982.

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1. M. S. Livingston. *Particle Accelerators: A Brief History*, p. 29. Cambridge: Harvard University Press, 1969.

2. J. Rees. Letter to E. Courant, 1996.

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Courtesy of UCO/Lick Observatory, University of California, Santa Cruz

WILLIAM WILSON MORGAN

January 3, 1906–June 21, 1994

BY DONALD E. OSTERBROCK

WILLIAM W. MORGAN was born and raised in the South, but went to Yerkes Observatory in Wisconsin as a student and spent the rest of his life there. He was an outstanding observational astronomer who became a master of spectral classification, a field he dominated for many years. He made important contributions to galactic structure, stellar populations, and galaxy research. Fiercely independent, he insisted on describing rigorously "the thing itself," whether it was the spectrum of a star or the form of a galaxy; and he demonstrated that an astronomer who consciously rejected astrophysical theory if it conflicted with his own observational data could often be right in the twentieth century.

EARLY LIFE AND EDUCATION

Morgan was born on January 3, 1906, in Bethesda, Tennessee, a tiny hamlet that no longer exists. His father, William T. Morgan, and his mother, Mary Wilson Morgan, were both home missionaries in the Southern Methodist Church, who went from town to town to spread the Good News. His father, originally a Southern Presbyterian, was a minister who received his training in Chautauqua courses (essentially short-term summer schools) in the South. The family moved frequently, and young William's mother taught him and his sister Mildred, two years younger, at home until he was nine. He first attended school at Perry, Florida, in 1915, then in Colorado Springs, and finished eighth grade at Poplar Bluff, Missouri, in 1919. During World War I his father served as a minister for soldiers in training camps in the South and as the head of a church-sponsored hospitality center. After the war he called himself Major Morgan, presumably his rank in a Salvation Army-like organization. He became an itinerant lecturer, preaching moral uplift and self-improvement, both then including abstinence from alcoholic beverages. William W. Morgan had his first two years of high school at Marvin Junior College, Fredericktown, Missouri, where his mother supervised a girls' dormitory, and his last two at Central High School in Washington, D.C. He was a good student who took four years of Latin, three and a half years of mathematics, three years of science (general, physics, and chemistry) as well as the English and history all his classmates studied.

In 1923 Morgan entered Washington and Lee University in Lexington, Virginia; it dated back to 1749, and Robert E. Lee was its president after the Civil War (when it was still called Washington College). It was an all-male institution, and Morgan's classmates came from all over the South, with a tiny sprinkling of Northerners, mostly the sons of expatriate Southerners. Morgan lived in Lee's Dormitory on campus his first year; after that in a college-approved rooming house, but ate his meals in the Washington and Lee dining hall. He was an especially good student in English and was elected to the freshman English honorary fraternity; after two years the career he visualized for himself was teaching literature. Contrary to Morgan's stories in later life that he had almost no training in science, he was in fact a nearly straight-A student in mathematics, who was awarded a scholarship for excellence in that subject his third year. He also did average work in chemistry (better in class than in the laboratory), good work in physics, and excellent in astronomy. Morgan was dedicated, eager to learn, "gentlemanly … in his manners and breeding"; he impressed Benjamin A. Wooten, his professor in physics and astronomy, as a real prospect for the future. Wooten, a Columbia University Ph.D. in physics, obtained a small, professional-quality refracting telescope for the university while Morgan was a student, and he began observing with it. On visits home in Washington Morgan frequented the Naval Observatory. Washington and Lee was no MIT, but when he left college Morgan was reasonably well prepared for graduate work in the astronomy of his time.

Wooten spent the summer of 1926 at Yerkes Observatory, the astronomical research center of the University of Chicago. Situated in little Williams Bay on beautiful Lake Geneva, a resort area in southern Wisconsin, it was more to Wooten's taste than Ryerson Physical Laboratory on the campus, where he previously had put in an occasional summer. Just before returning to Washington and Lee, Wooten learned that Edwin B. Frost was desperately searching for an assistant to take over the routine program of obtaining daily spectroheliograms, which had been going on for thirty years. Under Frost, Yerkes Observatory was in its doldrums as a research and graduate training center, and there was no new incoming student to continue the work. Wooten recommended Morgan, and Frost snapped him up, particularly when he learned the young paragon was nearly twenty-one and was "very strong and healthy." His father did not want to let him go, with a year still to finish before he would have his undergraduate degree. He could not visualize his son as a scientist; he had expected him to follow his career as a lecturer. But young Morgan was determined to get away; he insisted, and won out. By early September he was at the observatory, learning the ropes. His salary was $900 a year plus tuition, then less than $100 a quarter.

Morgan was quick to learn, keenly interested in astronomical observing, industrious, and highly intelligent. He picked up the necessary photographic techniques quickly and became expert in them. Although he could not be officially admitted as a graduate student without a bachelor's degree, Morgan was allowed to take a full program of graduate courses, all research in astronomy, working with different professors. At the end of his first year Morgan received his bachelor's degree from Chicago on the basis of his transfer credit from Washington and Lee plus the astronomy graduate courses he had taken at Yerkes. He had never set foot on the campus before he went there in May to sign up for his degree.

Morgan continued for two more years as a graduate student at Yerkes, by then observing regularly in the spectroscopic program with the 40-inch refractor. In the middle of that period, in the summer of 1928, he married Helen M. Barrett, daughter of astronomer Storrs B. Barrett, who was the secretary of the observatory. Then Morgan and his young wife spent the 1929–30 academic year on the campus in Chicago, where he took graduate courses in celestial mechanics, physics, and mathematics. Returning to Yerkes in June 1930, he began a Ph.D. thesis on the spectra of A stars under the supervision of Otto Struve, the hard-working, Russian-born staff member. Struve had earned his own Ph.D. at Yerkes in 1923; Morgan was his second thesis student. In this thesis, an excellent one, Morgan concentrated on the "peculiar" A stars, which did not appear to fit into the standard ionization interpretation of stellar atmospheres then being worked out by theoretical astrophysicists. He carefully studied all the spectrograms of A stars in the Yerkes plate files and obtained many more at the telescope. He showed that a few of the A stars previously called peculiar in fact did fit the theory, but more did not. Morgan found that he could recognize many additional peculiar ones and that they were a sizeable minority compared with the "normal" A stars. Most show stronger-than-average spectral lines of various heavier elements, including manganese and some rare earths. Some of these stars have variable spectral lines. He classified the brightest peculiar A stars into several groups, and studied the spectra of a few of them in detail. He emphasized the importance of trying to understand physically how these stars fit into stellar evolution, a remarkably prescient statement in the early 1930s.

Morgan completed his thesis and received his doctoral degree in December 1931 during one of the darkest periods in the Great Depression. Nevertheless, he was kept on the staff, but in the same assistantship he had held as a graduate student. In the summer of 1932 he was promoted to instructor and in 1936 to assistant professor.

SPECTRAL CLASSIFICATION

As a young faculty member Morgan continued working on peculiar A stars and published several more papers on them. The classification system he set up for them was widely adopted and is the basis of the nomenclature for these stars still used today. In a long paper summarizing this work he described painstakingly the spectra of the normal and peculiar A stars in detail, comparing and contrasting them. He concluded that the peculiarities resulted from some physical factor other than the star's surface temperature and gravity, probably variable effective abundance of the elements, but he included no equations or calculations in his paper. Struve, now director, praised Morgan's research, but tried to get him to learn more theoretical astrophysics, particularly the quantitative interpretation of stellar spectra in terms of stellar atmospheric temperatures and densities. It was a new, rapidly growing subject; Struve was attracted by its early successes and saw it as the wave of the future, but Morgan was repelled by its failures (which were many in the 1930s) and avoided it. His interests shifted more and more to spectral classification.

Morgan tested the spectral types of the widely used Henry Draper catalogue, and the improved two-dimensional types of the Mount Wilson system and found he could do still better himself. He learned that, by using only well exposed, widened spectrograms taken with a spectrograph optimized for his program and then carefully developed under controlled conditions, he could obtain homogeneous data that was much better for spectral classification. Morgan pioneered in setting up a completely symbolic, two-dimensional spectral classification system in which he assigned stars types like G2 V, B2 Ia, or A2 IV purely by visual inspection of these high signal-to-noise-ratio spectrograms (in modern terms). Unlike the Mount Wilson observers he completely separated the spectral type and luminosity classification from the absolute-magnitude calibration. Thus, later determinations of the absolute magnitudes of stars would improve the calibration of Morgan's types but would not change them. Likewise, there was no hint in his system that (for instance) a G2 V main-sequence dwarf and a G2 I supergiant had the same effective temperature; that also was a separate calibration and the effective temperatures of either or both of these types, for instance, might change on the basis of later and better physical measurements, without changing the classification system, but making it more useful. Classifying a star's spectrum meant finding the standard star whose spectrum best matched it from the small list determined by Morgan himself. Thus, he consciously rejected any theoretical ideas of "quantitative" data in his classification scheme, but concentrated entirely on what he later came to call "the thing itself," the appearance of the spectrum alone. The rejection bewildered many of his contemporaries, but was an intelligent response to the state of astrophysics at that time. If freed Morgan and his results from the misconceptions based on incorrect theories and faulty data that plagued those same contemporaries' results.

In 1938 he showed that his two-dimensional spectral types and luminosity classes were better correlated with the color indices of nearby stars than the earlier Harvard and Mount Wilson types. His types could therefore be used to determine the stars' intrinsic colors and thus the extinction along the light paths to them. From the calibration of his luminosity classifications he could then determine the best available distances to the stars. In this paper Morgan used only highly accurate photoelectric color indices measured by Kurt F. Bottlinger and Paul Guthnick at Berlin-Babelsberg Observatory. Previous discrepancies disappeared when Morgan's types and these measured colors were used; he always believed in basing his work on the best data only, ignoring the rest. Henry Norris Russell, the outstanding leader of American astrophysics, was greatly impressed by his new results. This recognition helped Morgan in Struve's eyes.

Philip C. Keenan, who had started as a graduate student at Yerkes a few years after Morgan and had also remained on its staff, joined him in the later stages of this work. By 1940 Morgan had the concept for an *Atlas of Stellar Spectra* based entirely on spectrograms taken with his new classification "spectrograph" on the 40-inch refractor. It put to very good use a telescope that most of the other Yerkes faculty members were abandoning for the new 82-inch McDonald Observatory reflector in the clearer skies of Texas. In 1943 Morgan, Keenan, and Edith Kellman (Morgan's assistant, who made the original photographic prints that formed a large part of it) published their spectral atlas, with its "outline of spectral classification" written almost entirely by Morgan. The standard stars listed in it defined what came to be called the MKK system, which very quickly was adopted by astronomers almost everywhere.

SPIRAL ARMS

In 1944, just a year after the publication of the MKK atlas, Walter Baade announced his identification of two stellar populations, which soon turned out to be young stars and old. Within a very few years his surveys of M 31 showed that the young OB stars and H II regions of Population I are concentrated in its spiral arms. They define the spiral arms in it and other similar galaxies. Morgan was inspired by Baade's work and quickly realized that he could use these very same markers to trace the spiral arms in our galaxy. His accurate spectral types of individual OB stars, with their photoelectrically measured color indices, provided the data to determine accurate distances to these stars. Plotting them on a map with the sun at the origin would reveal the spiral arms, or at least the parts of them close enough to find OB stars in them. The distances depended critically on having an accurate absolute-magnitude calibration for the OB stars. Morgan worked very hard to set it up, using data he and others obtained from proper motions, galactic rotation, and clusters and even associations with distances obtained from fainter stars within them. Always he insisted on using only high quality measured photoelectric color indices and spectral types and luminosity classes determined by himself, his graduate students, and his collaborators. Nearly all the color indices came from Joel Stebbins, C. Morse Huffer, and Albert E. Whitford at the University of Wisconsin. Morgan had worked with Jason J. Nassau at the Case Institute of Technology and later with Guillermo Haro, Luis Münch, Graciela and Guillermina Gonzalez, and other Mexican astronomers on the Tonanzintla Observatory staff in finding candidate OB stars for this program, on the objective-prism spectral plates they had taken with their respective Schmidt telescopes. Morgan had taken part in the dedication of Tonanzintla Observatory in 1942 and had presented a paper on spectral classification that greatly influenced the early direction of the research program there.

In the later stages of this program Morgan decided that the OB "aggregates" (the term he used to mean large complexes of nebulae and young stars containing many OB stars), were the best markers to use to map the spiral arms. Measuring independently the distances of many stars in such an association and adopting the mean gave the best measure of the distance to use in plotting the aggregate in the galactic plane. Morgan encouraged two young graduate students at Yerkes Observatory, Stewart Sharpless and me, to find additional candidate associations using filters that isolated the characteristic Hα, [N II] emission lines of gaseous nebulae with a special very wide-field camera designed by Jesse L. Greenstein and Louis G. Henyey. Morgan had recognized its potential for this program immediately. He generously included both of us as co-authors of the resulting paper, which mapped portions of the Orion spiral arm through the sun and the Perseus arm, the next one beyond it in the galactic plane. Morgan presented the resulting map at the American Astronomical Society meeting in Cleveland in December 1951, where it received unprecedented acclaim in the form of a standing ovation, but sadly he published only an abstract of it. As he revealed years later to David H. DeVorkin in an interview, Morgan suffered a nervous breakdown soon after this meeting, was hospitalized briefly, and then convalesced at home, unable to do scientific work for a time. He soon recovered, but the data he had used in the oral presentation of the paper were rendered obsolete by later, more numerous color indices, spectral classifications, and identifications of additional associations.

Morgan did much of this later spiral arm work in collaboration with Albert Whitford and Arthur D. Code, who measured the color indices, and the three of them published it soon thereafter. Besides the first two arms, their paper also included the next spiral arm inside the sun's distance from the galactic center, the Sagittarius arm. As part of this program Code and Theodore E. Houck, on an expedition to South Africa, obtained better wide-angle photographs showing the southern as well as the northern Milky Way. In all this work Morgan demonstrated his ability to inspire and work very effectively with collaborators to solve important problems.

In applying his technique of spectral classification Morgan early realized that spectrograms taken with his spectrograph, widened and processed with his special low-contrast, fine-grain developer, gave far superior results than the narrow, grainy, and high-contrast plates taken for radial-velocity work. Everyone could grasp that. But he went beyond it to the idea that the number of different spectral types that could be distinguished depended on the instrument, and that certain "natural groups" could be recognized on extremely low-dispersion objective-prism plates. The outstanding examples were the high-luminosity OB stars so important for his galactic-structure studies that he, Bengt Strömgren, and Hugh M. Johnson demonstrated could be segregated with spectra taken at the fantastically low dispersion of 30,000 Å/mm. All that these spectrograms showed was the continuous spectrum of the star, but that was enough to recognize faint distant OB stars that lay in the spiral arms, as Daniel H. Schulte later demonstrated with a small, specially designed Schmidt camera.

Morgan became managing editor of the *Astrophysical Journal* in 1947, under a reorganization of faculty responsibilities that Struve put into effect. The journal belonged to the University of Chicago and ever since George Ellery Hale, the Yerkes director had been its managing editor. After World War II, however, Struve was tired of administrative tasks that kept him from research, and he handed the job over to Morgan. At the same time the university signed a new contract, negotiated by Struve, under which the American Astronomical Society shared partly in the control of the *Journal* and helped in financing it through its member subscriptions.

Morgan took his editorial responsibilities seriously and worked hard at them. He improved the scientific standards of the *Journal*, particularly in the observational papers. However, pressures connected with rising costs of postwar publication and an extension of this agreement undoubtedly contributed to Morgan's breakdown. There were also various internal tensions among the senior Yerkes faculty members, which led to Struve's resignation and departure for Berkeley in 1950. They must have added to Morgan's mental problems at the time. He resigned as managing editor in 1952, and Subrahmanyan Chandrasekhar succeeded him.

PHOTOELECTRIC PHOTOMETRY AND THE MK SYSTEM

Morgan and Keenan continued improving their system of spectral classification, with Morgan concentrating especially on the early-type stars and Keenan on the later-type. They published the essentials of their revised MK system, defined by its standard stars, in 1951. Morgan's Ph.D. students William P. Bidelman, Arne Slettebak, and Nancy G. Roman worked with him on parts of this program and made important contributions to it. Photoelectric photometry became a rapidly growing field after World War II. Morgan naturally took a keen interest in it, since the color indices it provided were so closely related to spectral types, and with them provided the color excesses that led directly to quantitative measurements of interstellar extinction.

With Harold L. Johnson, who joined the Wisconsin faculty in 1949 and moved on to Yerkes in 1950, Morgan set up the *UBV* system, based on filters chosen to give the best match with the MK types. A crucial element of it was that the *U* (ultraviolet) filter was selected to maximize the effect of the higher Balmer lines and Balmer continuum, while the *B* (blue) filter excluded them as much as possible. As a result the combination of *U* — *B* and *B* — *V* (visual) color indices formed an excellent basis for discriminating between giants and dwarfs and for providing interstellar extinction for "normal" stars. Like the MK system of spectral classification, the *UBV* photometric system was adopted everywhere and quickly became the most widely used system. Their paper on the *UBV* system also contained the complete list of standard stars that defined the MK spectral classification system in full. In a review paper published in 1973 Morgan and Keenan further refined this system, and in 1996 it remains in use as the revised MK system.

STELLAR POPULATIONS

Morgan was tremendously stimulated by Baade's invited lectures on his new concept of stellar populations at the American Astronomical Society meeting at Perkins Observatory, Ohio, in December 1947 and at the subsequent meeting in California in June 1948 for the dedication of the 200-inch telescope. The Yerkes spectroscopist was always eager to apply his methods at the frontiers of the newest fields of astronomy, as he had done in locating the nearby spiral arms in our galaxy. He was the leading authority in the world, from the observational viewpoint, on O and B stars, the hottest, most massive young stars, which are the markers of Population I. Many of his papers dealt with these stars and the groups and clusters in which they occur, their association with interstellar matter (observable as H II regions and as "dust"), how to find them, and how to measure their distances.

In the MKK spectral atlas, even before Baade's identification of the two populations, Morgan and Keenan pointed out a few specific examples of stars with very weak spectral lines and quite high velocities. Baade recognized them as nearby members of his Population II, showing the importance of the MK system of spectral classification in this problem, and Keenan, Morgan, and Guido Münch presented a paper on the spectra of the high-velocity giants at this same 1947 meeting. Morgan reviewed and extended the results in this field in a paper he presented at the conference on stellar populations held in Rome under the auspices of the Pontifical Academy of Sciences in 1957.

Although Morgan did not have access to a large telescope at Yerkes, he hungered to do research on galaxies and globular clusters. This he achieved as a visitor to Lick and Mount Wilson Observatories. Nicholas U. Mayall had obtained a large collection of spectrograms of globular clusters at Lick to measure their radial velocities. Morgan, whom Mayall allowed to use the plates for spectral classification, found that he could estimate fairly accurate integrated spectral types from them and could also assign values of a line-strength parameter. He supplemented these with better, widened spectrograms he obtained of some of the clusters with the 82-inch McDonald Observatory reflector. These plates were optimized for his type of spectral classification. At that time globular clusters were, by Baade's definition, extreme

Population II objects and therefore were considered to be old weak-line, metal-poor groups of stars. The available spectra of stars in the relatively nearest globular clusters confirmed their weak-line property. However, Morgan found that, although this correlation held up quite well for distant clusters far from the galactic plane and the galactic center, it did not apply to a small group of clusters close to the galactic center, most of them also at relatively small distances from the galactic plane. Ten years earlier Mayall classified some of them as "later" in spectral type than the other globular clusters. In fact, these clusters near the galactic center showed "normal" strength absorption lines, Morgan insisted, indicating approximately solar-type abundances. Baade, who still held firmly to the idea of two populations and not a multitude of intermediate ones, could possibly accept several, but not the idea that an "old" globular cluster could have a "normal" metal abundance. Jan H. Oort, the leading interpreter of the populations concept, was naturally skeptical, and tended to question endlessly any new idea that did not fit his current theory, which he had worked out to fit the facts as Baade had stated them.

Only Morgan with his years of experience in classifying spectra, his supreme self-confidence in this field, and his aversion to or even downright contempt for theory could have withstood their attacks. But he stuck to his guns, invited questioners to look at the spectrograms themselves (some of them did, but could not see what was obvious to him), and was sure he was right. And he was, as many much more quantitative measurements and long discussions have subsequently confirmed.

Similarly, Morgan, working with George H. Herbig at Lick Observatory, obtained a good spectrogram of the nucleus of M 31, the spiral galaxy that Baade had systematically observed as the analogue of our galaxy. Morgan classified the nucleus as having a K2 III integrated type with strong metal lines, not weak. This conclusion was even harder for Baade to accept, because the central bulge of M 31 was pure Population II according to all his direct photographic tests; and he believed that the nucleus was the densest concentration in it, composed of the same types of stars as the bulge at large. Again, Morgan was right. At the time of his death much of the research on the variety of ages, metal abundances, and kinematic properties of the stars in the central regions of our galaxy can be traced back to his highly individualistic, paradigm-shattering spectroscopic research on stellar populations in the 1950s.

GALAXIES

Naturally, Morgan was keenly interested in the classification of galaxies. His earliest published paper in this field, in collaboration with Mayall, classified their spectra on the basis of the latter's Lick spectrograms. True to his guiding philosophy of "the thing itself," Morgan emphasized that galaxy spectra, like globular-cluster spectra, are the integrated sum of the contributions of multitudes of spectra of individual stars and cannot be assigned unique stellar types, because they do not match the spectra of any standard stars. He therefore used a different notation for galaxy spectra, but was somewhat inhibited by the more conservative Mayall, the provider of the data. In this field Morgan always faced the problem of having to collaborate or use borrowed spectrograms, and he generally tempered his published criticisms of earlier work because he feared, perhaps incorrectly, that such criticism might antagonize his sources. Later Mayall did turn over his spectrograms to Morgan, who used them to discuss further what they revealed about the stellar populations in the galaxies.

Morgan's work on the classification of the forms of galaxies, based largely on close inspection of direct photographs in the Mount Wilson and Palomar Observatories plate files, was one of his most important contributions to galaxy research. He rejected much of the Hubble classification system, arguing convincingly that the objects the great observational cosmologist called SO were in fact a mixture of galaxies with dissimilar forms and that his Sa, Sb, Sc sequence was based on two criteria that were often in disagreement. Morgan based his classification on only one of them, the strength of the central bulge relative to the disk. Probably he somewhat hindered the acceptance of his galaxy classification by assigning symbols for form types that were quite different from Hubble's, but were related to spectral types, like fS1 or kE5.

Certainly, his symbols DE, D, and (later) cD for some of the types of galaxies that Hubble had included in the SO class proved extremely useful in finding and analyzing radio galaxies. By the time of his death Morgan's cD notation was used by astronomers everywhere, but most of his other types were never widely accepted, probably because the Hubble system was too well entrenched. Laura P. Bautz, then a Northwestern University faculty member, and several of his graduate students, beginning with Janet Rountree Lesh, collaborated with Morgan in a number of papers on cD galaxies; as a result, many of these objects have published classifications that radio astronomers adopted and used extensively. Morgan also studied and emphasized the importance of the luminosity sequence of active galactic nuclei from Seyfert galaxies to quasistellar objects and coined the name "N galaxies" for intermediate objects.

CAREER

In his early years at Yerkes Morgan felt he received little recognition. Struve assigned him the duties of assistant director

rector without giving him the title, salary, or guidance on policy he deserved. Morgan did not like the post, which he considered a drain on the time and energy he preferred to devote to research. In this position he did little beyond carrying out the basic directives Struve gave or sent him and, after publication of the MKK *Spectral Atlas* brought him his first fame in the astronomical community, he shunned administrative tasks. Yet Morgan loved Yerkes Observatory and believed he owed his career to it. To his dying day he thought it had a special mystique for research. Thus he accepted the directorship of the observatory from 1960 to 1963 and the chairmanship of the department of astronomy from 1960 to 1966 after his colleagues convinced him there was no other suitable faculty member available. He was promoted to full professor in 1947 and was named a distinguished service professor in 1966.

Morgan was an excellent teacher in the small classes that the graduate students at Yerkes Observatory took. He taught only photographic photometry (in his earlier years) and his own subject, spectral classification, which he made every student learn by doing it. To interested, involved students he could be charismatic. When he taught spectral classification one quarter every other year, small groups of University of Wisconsin graduate students would drive from Madison to Williams Bay for his weekly classes. Morgan especially liked to attract Yerkes students who were seen as budding theorists, such as Code, Marshall H. Wrubel, D. Nelson Limber, Jeremiah P. Ostriker, and the writer, into his subject, and we all agreed we learned new ideas as well as new techniques from him. On the other hand, students who resisted involvement in the classwork depressed him, and he was quite capable of abruptly canceling the remaining lectures for the quarter if he felt he was not getting through to the participants. Most of the practitioners of spectral classification in the years after World War II were former students who had learned the subject from Morgan and from Keenan at Ohio State University.

Morgan was elected to the National Academy of Sciences in 1956, and was awarded the Bruce Medal of the Astronomical Society of the Pacific in 1958, the Henry Norris Russell Lectureship of the American Astronomical Society in 1961, and the Henry Draper Medal of the NAS in 1980. He received three honorary doctor's degrees and was named a member or an associate of the academies of several other nations; of the latter the most meaningful to him probably were the Pontifical Academy of Sciences and the Royal Danish Academy of Sciences and Letters.

In 1960 he was the astronomer named by his colleagues to give the main scientific address at the dedication of the Kitt Peak National Observatory, and in 1971 he was again chosen as one of the four invited speakers at the ceremonies marking Mayall's retirement as its director. Later Morgan and Keenan were honored by a workshop on spectral classification, dedicated to them, at the University of Toronto, and by another held under the auspices of the Vatican Observatory in Tucson in 1993, the fiftieth anniversary of the publication of the MKK *Spectral Atlas*.

Among the organizers of both these conferences was Robert F. Garrison, Morgan's Ph.D. thesis student who continued his adviser's approach to spectral classification, concentrating on the data and suppressing theoretical or prior observational prejudices to the extent he could. Another even later Morgan Ph.D. in this mold was Nolan R. Walborn. They both demonstrated in their work that Morgan's principles are just as applicable with the large ground-based and space telescopes of the 1990s as they were with the 40-inch Yerkes refractor in his time.

Morgan and his wife Helen had two children: Emily and

William. Helen Morgan died in 1963, and in 1966 he married Jean Doyle Eliot, who, with his children, survived him. She had been a schoolteacher in Williams Bay and later became a faculty member at Roosevelt College in Chicago, commuting back and forth from their home.

OTHER INTERESTS

Although Morgan worked very hard in astronomy all his life, he had many other interests as well. A former English major, he loved literature, particularly Shakespeare and Christopher Marlowe. In a retrospective account of his life published in 1988 Morgan wrote that Marlowe's play *Doctor Faustus* was one of the most important influences on his life, from the time he read it as a young high school boy in Missouri. The lesson of *Doctor Faustus*, as he understood it, was the drive to continue learning, to increase one's knowledge of the universe. It made him realize, he told me more than once, that there is no limit to how far a person can go in following his own ideas. In the postwar period he particularly favored T. S. Eliot and Marcel Proust.

Morgan had very wide reading horizons not only in literature but in art, music, and philosophy. He was particularly attracted by the writings of Ludwig Wittgenstein, the apostle of logical positivism. In the 1940s and 1950s Morgan was especially interested in photography and took numerous sensitive pictures, particularly of flowers and groups of flowers in nature. His interest in art was intense, especially the patterns in the work of Mondrian and the cubists, and over the years he accumulated an enormous collection of art books. He organized a local photography club and was its first president. Morgan, a connoisseur of cinema, was also highly involved in showings of historic films at Yerkes Observatory evening social and cultural affairs. All his reading and his other activities he undertook for their own sake, but he believed that they broadened his horizons on astronomy and science. After his breakdown he began writing his thoughts, experiences, ideas, and insights in private notebooks as a form of therapy and had accumulated more than a hundred of these books by the time of his death.

Morgan also had many interests in the more conventional American pastimes, such as detective stories and spectator sports. I well remember watching an early television broadcast of a college football game with a group of graduate students at his house, with the sound turned down so we could simultaneously listen to a radio broadcast of a symphony concert. Morgan, of course, chose both programs. In his mid-forties he continued to play in informal softball games with the graduate students and for years afterward took pleasure in lamenting how, as pitcher, he "had to" strike out his son Billy, then a beginning high school student, in a crucial game.

Morgan was well liked and respected in the little village of Williams Bay. He played a key role in organizing a Boy Scout troop when his son reached that age, and afterward was elected and reelected to the Village Board for four two-year terms. Its other members elected him president for his last two terms, and he was thus unofficially the mayor from 1947 to 1951. Morgan was active in the Williams Bay Congregational Church and, particularly in the years after his official retirement, occasionally gave talks on the universe in lieu of sermons. He was, much more than most observational astronomers, an all-around man.

CONCLUSION

Morgan was an outstanding research astronomer. He was one of the leading experts in the world in the classification of stellar spectra, integrated spectra of clusters and galaxies, and the forms of galaxies. Through his work, much of it done with a relatively small, outmoded telescope and the rest with borrowed observational material, he made many important discoveries on the structure of our galaxy and on the nature of stellar populations. He insisted on describing rigorously what he observed independently of previous observers' possibly incomplete or even incorrect analyses. Morgan tried to free himself of theoretical preconceptions. His unconventional but careful, controlled way of doing research enabled him to make many new discoveries about stars, our galaxy, and other galaxies.

THIS BIOGRAPHICAL MEMOIR is based largely on the written record of Morgan's research, published in his many scientific papers. It is also based on the hundreds of letters to, from, or about him in the Yerkes Observatory Archives, dated from 1926 to the mid-1950s, and on my own conversations and correspondence with him from 1949 until the year of his death. Many of his former colleagues and students have helped me with their reminiscences of him, particularly Philip C. Keenan and Robert F. Garrison. Above all, I am grateful to Jean Morgan, who answered many of my questions about her late husband. D. Scott Dittman, registrar of Washington and Lee University, and Maxine Hunsinger Sullivan, registrar of the University of Chicago, very kindly made Morgan's student records (which included synopses of his high school work) available for this memoir. Morgan himself published two fairly brief accounts of some aspects of his life and philosophy, but he wrote them years after the early events described in them, and evidently almost entirely from memory, for they contradict in some ways documentary evidence, or much earlier autobiographical data that he left in his papers.[1] Morgan's research for and discovery of the spiral arms in our galaxy were well described by Owen Gingerich in a historical paper, which includes a summary of David H. DeVorkin's interview.[2]

NOTES

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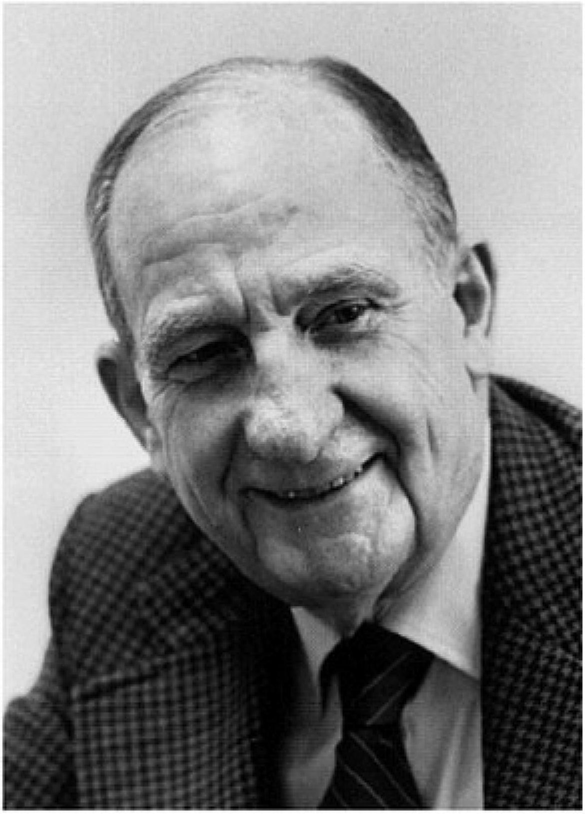

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M. M. Rhoades

MARCUS MORTON RHOADES

July 24, 1903–December 30, 1991

BY WAYNE R. CARLSON AND JAMES A. BIRCHLER

THERE ARE MANY ways to characterize scientists. Some could be said to choose a problem and begin to apply various techniques to understand it. Others explore the field with open eyes and open mind, grasping the unexpected for investigation. Marcus M. Rhoades was the latter type. His oft-repeated entreaty to beginning graduate students, "Just get in the lab and start work; you can't help but discover something," gave evidence of his belief in this approach. Certainly his own discoveries were a diverse group of seminal contributions to genetics.

Marcus M. Rhoades was born on July 24, 1903, in Graham, Missouri, and spent his childhood in Downs, Kansas. He developed a strong affinity for the Midwest and often boasted of the fertile fields and wide expanses of that part of the United States. Rhoades attended the University of Michigan, majoring in botany and mathematics. When he was a senior and uncertain of his interests, he was befriended by Prof. E. G. ("Andy") Anderson. Anderson introduced Rhoades to plant genetics and, when Rhoades later wrote a memorial resolution on Anderson, he praised him warmly. After receiving his B. S. and M. S. degrees at Michigan, Rhoades studied for his Ph.D. at Cornell University under

Anderson's major professor, R. A. Emerson, a maize geneticist. During his graduate years Rhoades lived for three years in Emerson's home as "a member of the family" (1984). He was part of a brilliant group of maize cytogeneticists, which included fellow students Barbara McClintock, Charles Burnham, and George Beadle. Rhoades interrupted his Ph.D. work for one year to visit the California Institute of Technology as a teaching fellow. At this time he worked on *Drosophila* under the guidance of A. H. Sturtevant and T. Dobzhansky, with occasional support from T. H. Morgan and C. B. Bridges. By the time he received his Ph.D. Rhoades had written five research papers. During his graduate years Rhoades worked with the best minds in both maize and *Drosophila* genetics. No wonder he frequently advised his graduate students that they would look back on their graduate years as the best years of their lives.

Rhoades met his future wife Virginia at Cornell University, and they were married in 1932. Virginia Hatcher Rhoades was a graduate student in L. F. Randolph's laboratory and she made significant contributions to maize genetics, with work on pollen development and genetic factors of chromosome 10. She subsequently gave up research in favor of raising their two sons, Marcus junior and William.

Following completion of his Ph.D. in 1932 Rhoades stayed at Cornell as an experimentalist in plant breeding until 1935. In that year he joined the U. S. Department of Agriculture as a research geneticist and was stationed at Iowa State University until 1937. While there, he participated in setting up the yearly Iowa corn yield test. In 1937 the USDA transferred him to the Arlington Experimental Farm outside Washington, D.C. At the farm his basic cytogenetic research flourished, with considerable support from both his supervisor M. T. Jenkins and bureau chief F. D. Richey. Rhoades returned to academics in 1940 as associate professor at Columbia University. He was promoted to professor in 1943, and remained at Columbia until 1948, when he was appointed professor at the University of Illinois. He spent ten productive years of teaching and research at Illinois and next served as chairman and professor of the botany department at Indiana University from 1958 to 1968. In 1968 he resigned the chairmanship and was given the rank of distinguished professor at Indiana University. Rhoades retired in 1974, but he continued his research activities at Indiana until shortly before his death in 1991.

During his career Rhoades worked on a wide variety of topics in maize cytogenetics, including crossing over and basic cytogenetic principles; cytoplasmic male sterility; centromeric misdivision; the first transposon-type mutator system; a nuclear gene, *iojap*, that affects the chloroplast genome; meiotic mutations, including *ameiotic 1*; meiotic drive by abnormal chromosome 10; properties of heterochromatin; and the effect of B chromosomes on heterochromatin. The listing of Rhoades's work demonstrates the variety of his interests. It does not, however, reveal the thoroughness with which he approached each topic. One has to examine only a few of his papers to understand the high standard of proof that Rhoades demanded before publication.

Rhoades developed certain friendships and associations during his lifetime that had a far reaching effect on his career. In 1946 he selected a student from his cytogenetics class to help with pollinations in the corn field. Ellen Dempsey was later promoted to research associate. In that position she performed genetic crosses and cytogenetic analyses plus various duties, including the tutoring of graduate students, review of thesis manuscripts, and much of the assembly of the *Maize Genetics Cooperation News Letter*. Rhoades and Dempsey published a number of joint papers from 1953 to 1990. Several reviews of the life of Marcus Rhoades were written by Dempsey, including two Festschrift publications and one memorial resolution (Dempsey, 1973,1983,1994). We have borrowed liberally from these publications, since they come from the person who knew Rhoades's work better than anyone. Peterson and Peterson also wrote an article on the life of Rhoades (Peterson and Peterson, 1973).

Another longtime association for Rhoades was with Drew Schwartz. Rhoades was Schwartz's Ph.D. supervisor at Columbia and later at the University of Illinois. Subsequently, he was instrumental in bringing Schwartz to Indiana University. Schwartz played a major role in developing the area of biochemical genetics in maize. His work was a bridge to the later study of molecular biology in maize, and a number of Schwartz's students went on to become leaders in the field. The synergism between Rhoades and Schwartz carried over to benefit Schwartz's students.

Other important associations were with John Laughnan at the University of Illinois and Jim Peacock at the Commonwealth Scientific and Industrial Research Organization in Canberra, Australia. Rhoades and Laughnan shared field plots at Illinois and gave a joint seminar. They often advised each other's graduate students. Peacock provided Rhoades with a place to work away from the considerable administrative duties of the chairmanship at Indiana University. Two visits to Canberra allowed Rhoades the time to write several papers and to become acquainted with molecular cytogenetic methods.

The honors given Rhoades were numerous. Among the most significant was his election to three prestigious societies: the National Academy of Sciences, American Philosophical Society, and American Academy of Arts and Sciences. In addition, Marcus Rhoades and Barbara McClintock were given the first T. H. Morgan Medal from the Genetics Society of America in 1981. Rhoades was also honored with two

Festschrift publications, on the occasions of his seventieth and eightieth birthdays. Contributions to the publications came from former students and colleagues. At one of the gatherings for presentation of a volume Rhoades stated that his students had made him proud and maintained that some of their research contributions had exceeded his own. We all knew the latter was untrue, but the comment was typical of his modesty.

Following the example of his mentor, R. A. Emerson, Rhoades provided much help to the maize genetics community and the genetics community at large. It was Emerson who began the spirit of cooperation among maize geneticists. He organized the first meeting of maize workers in 1928 during the meetings of the American Association for the Advancement of Science. The cooperation that began in 1928 was formalized at the Sixth International Genetics Meeting in 1932, when a group of maize geneticists formed the Maize Genetics Cooperation. The Cooperation, originally located at Cornell University, established a center for the preservation of seed stocks and published the *Maize Genetics Cooperation News Letter*. At the 1932 meeting Rhoades was asked to take charge of the newsletter. He served as its editor from 1932 to 1935 and again from 1956 to 1974. Rhoades also participated in the collection and maintenance of seed stocks and later was primarily responsible for moving the stock center to its present location at the University of Illinois.

Rhoades was editor of *Genetics* from 1940 to 1948 and was a member of numerous journal editorial boards over the years. He served on the committees of many organizations, including the Guggenheim Memorial Foundation, National Institutes of Health, National Science Foundation and Atomic Energy Commission. Another type of service, performed not infrequently, was the authoring of biographical papers on colleagues. Among these were the National Academy of Sciences' *Biographical Memoir* for R. A. Emerson (1949) and L. J. Stadler (1957) and memorial biographies of E. G. Anderson (1973, Stadler Symposium) and B. McClintock (1986, *Maydica*).

Rhoades was also known for his teaching. As recalled by Ellen Dempsey, Rhoades's two-semester cytogenetics course at Columbia was very popular. By the time Rhoades was teaching at Indiana University and serving as chair of the botany department the course was one semester in length. We (JB and WC) both took the course at Indiana and recall the lectures as well organized, intellectually challenging, and taught with humor and concern for the student. In terms of graduate education Rhoades sponsored twenty-six Ph.D. students during his career, and their names were recorded by Dempsey (1973). In dealing with new graduate students Rhoades would start a student on a project and provide the materials needed. After that he considered that the research belonged to the student and never asked that his name be placed on papers his students wrote. Consequently, he is senior author on almost all the papers in his bibliography.

Formal retirement freed Rhoades for greater interaction with Schwartz's students. One of the authors (JB) recalls that visits to the Rhoades lab were greeted with "Have a seat, young man" and inquiries about the latest experiments. He freely gave advice about available stocks that might be useful. These visits were often characterized by recollections about notable geneticists and their work. It is clear he had some strong favorites. In the field he would wander into one of JB's plots and offer judgment on the corn and weeds and give tips on field work in general.

Rhoades was modest and did not view himself as an extraordinary talent, but more as a gatherer of data. He did not consider himself a gifted theorist, nor did he particularly approve of "theorizing." Rhoades's approach to science was described by Dempsey (1973). He was an acute observer and studied any anomalies in his crosses assiduously until they were explained. This approach led to the discovery of the *Dotted* mutation, the neocentromeres of abnormal chromosome 10, and the high loss phenomenon of B chromosomes. In the last case Rhoades found some recessive kernels on an ear that should have had only the dominant phenotype. The simplest explanation would have been that pollen (self) contamination by the recessive parent occurred. Rhoades would not accept this explanation and one can certainly imagine him confidently dismissing any criticism of his technique. While Rhoades was well known for his modesty, this trait did not extend to his view of his own technical skills, whether they were field techniques, such as pollinating and collecting sporocytes, or laboratory skills, such as staining cells and chromosome analysis.

A brief, but by no means comprehensive, review of research discoveries by Marcus Rhoades follows. Rhoades began his career with a Ph.D. thesis on the topic of cytoplasmic male sterility in maize. This was the first study of its kind on a topic that has been of enormous economic significance in the cultivation of maize.

Rhoades loved to recount the story of his thesis defense. After Rhoades had left the room for the decision a chemist on the committee, who was unfamiliar with genetics but intrigued by Rhoades's work, drew the group into an extended discussion for his own edification, forgetting that Rhoades was waiting in the hallway and enduring the inquiries of fellow graduate students as to whether he was "in trouble." As a result of this experience Rhoades always kept the discussion following student committee meetings to a minimum.

During studies for his Ph.D. Rhoades also worked on some basic principles of crossing over, principles that were just emerging at the time. For example, he demonstrated in maize that crossing over occurs at the four-strand stage (1932). Subsequent work covered a variety of topics, including mutator genes. The *Dotted* mutator was first identified by observation of a variegated endosperm color phenotype (purple dots on a colorless background) on seeds of a single ear (1938). *Dotted* was shown to be an unusual mutator gene because it affects a specific locus, *a1*, and does not have a general effect on other genes. It was later found that the dotted phenotype is due to the movement of a transposable element at *a1* under the influence of *Dt* . A completely different mutator system, affecting plastids, was also discovered by Rhoades. It was one of the first cases of nuclear-cytoplasmic interaction found and one that is often cited in textbooks. In this case a nuclear gene, *iojap*, causes a change in the chloroplast. These alterations are heritable in the cytoplasm, even after the nuclear mutation is replaced.

Few organisms are as well suited to cytogenetic work on meiosis as maize. The combination of excellent meiotic cytology with a strong genetic tradition has produced a long line of cytogenetic studies. Rhoades's focus as a cytogeneticist was very much on meiosis and he often studied mutations that disrupted the process. In addition, he discovered one of the key genes in the switch from a mitotic to a meiotic program, *ameiotic 1*. Another focus of Rhoades's work was the centromere. He discovered a telocentric of the short arm of chromosome 5 (1940) and used it to study centromeric stability.

No mention of the research of Rhoades would be complete without reference to a long commitment to analysis of abnormal chromosome 10, an intrigue that began at the USDA and continued until his death. This chromosome, discovered by A. Longley in 1937, differs from the normal chromosome 10 by the addition of chromatin to 10L. The abnormal chromosome was shown by Rhoades to possess a unique system of meiotic drive. Rhoades delighted in informing students that the meiotic drive aspect was discovered in his Arlington, Virginia, USDA plot "on which the Pentagon now stands." Due to the findings of Rhoades this work remains one of the best explained systems of meiotic drive. It was shown that the abnormal chromosome, when heterozygous with a normal 10, is recovered about 70% of the time in the female gametes versus 30% for the normal chromosome. Transmission through the male is normal (50/50). Cytological studies showed that the abnormal chromosome causes the production of neocentromeres in both the male and female meiosis. These neocentromeres (accessory centromeres) migrate precociously to the poles at the meiotic anaphases. Neocentromeres pull chromatids attached to them to the outer poles of the linear female meiotic quartet. Since the basal product is destined to produce the egg cell, with the other three cells deteriorating, chromatids with neocentromeres are favored for transmission through the female. No advantage to neocentromeric chromatids occurs in the male, because orientation of the meiotic cells is not linear and because all meiotic cells survive. Rhoades also showed that the neocentromeres only form on chromatids that carry chromosomal knobs (i.e., large heterochromatic regions that are separated by some distance from the centromere). Therefore, when a bivalent is heterozygous for one chromosome carrying a knob and one lacking the knob, preferential recovery of knobbed chromatids occurs in the female following a crossover between the centromere and the knob. Abnormal chromosome 10 has a large knob whereas the normal chromosome lacks a knob.

Consequently, abnormal 10 is recovered in excess through the female.

Rhoades and Dempsey discovered a system that could be used for studying the structure of abnormal chromosome 10. While working with the maize B chromosome they found that a certain stock gave frequent chromosome breakage in the presence of B's. The chromosomes being broken were the knobbed chromosomes, with breakage occurring at the second pollen mitosis. According to Rhoades and Dempsey the B chromosomes caused knobs to stick together at the second pollen mitosis, giving bridge formation and bridge breakage. The timing of the breakage was quite useful, because the broken chromosome subsequently enters the zygote. Entry into the zygote "heals" the chromosome presumably by the addition of telomeric DNA to the broken end. Consequently, the breakage-fusion-bridge cycle cannot occur. Therefore, all the deficiencies caused by chromosome breakage in this system are simple terminal deficiencies. Rhoades and Dempsey (1985) used this fact to make terminal deletions of the long arm of abnormal 10. They showed that the additional chromatin of abnormal 10 is interspersed with normal chromatin and is not a simple terminal addition to the chromosome.

Rhoades demonstrated that the system producing the chromosomal breakage contains two components; it requires at least two B chromosomes plus a specific inbred genetic background to be effective. Under these circumstances chromosomes with knobs undergo frequent chromosome breakage in the pollen. The breakage is visualized by the expression of a recessive phenotype in a homozygous recessive x homozygous dominant cross. The system became known as "high loss" due to the frequent elimination of dominant markers from knobbed chromosomes. A further value of this system was the discovery of new transposon systems.

Chromosome breakage in maize seems to stimulate the activation of transposons, and new transposons were reported and analyzed in 1989.

A remarkable aspect of Marcus Rhoades's career was his ability to continue producing imaginative and significant research for more than fifty years. From his first publications in 1931 until his last in the late 1980s Rhoades maintained a standard of quality in his research that was unwavering and was an example to all his students and colleagues.

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Henry Stommel

HENRY STOMMEL

September 27, 1920–January 17, 1992

BY CARL WUNSCH

HENRY MELSON STOMMEL, probably the most original and important physical oceanographer of all time, was in large measure the creator of the modern field of dynamical oceanography. He contributed and inspired many of its most important ideas over a forty-five-year period. Hank, as many called him, was known throughout the world oceanographic community not only as a superb scientist but also as a raconteur, explosives amateur, printer, painter, gentleman farmer, fiction writer, and host with a puckish sense of humor and booming laugh.

Stommel entered oceanography when the field still had much of the atmosphere of an avocation for wealthy amateurs who used their own yachts for research; he left it at a time when it had been transformed into a modern branch of science, often driven by perceived needs of national security, and of global, organized, highly expensive programs requiring massive government funding. In a sociological sense he was a transitional figure, being probably the last of the creative physical oceanographers with no advanced degree, uncomfortable with the way the science had changed, and deeply nostalgic for his early scientific days. The paradox of his life is that the huge changes that had taken place were to a great extent of his own making, and are a testament to the major advances his ideas had made possible. He was a man of deep ambivalences and contradictions. He sometimes recognized, but often did not, that his intellect was driving him and the study of the ocean in one direction—toward the use of modern sophisticated instrumentation and computers and to the organization of giant field programs—while his heart clearly lay with the science of his youth, which involved intense work at sea with gifted amateurs and crusty old fishermen using primitive instruments made by clever local machinists and craftsmen. All of his personal inclinations led him to identify most closely with the large group of "amateur" scientists who represented oceanography in the years just following World War II. They and their successors, despite their having doctoral degrees, came closest to representing what he loved: serious observational work at sea by close teams of like-minded, unpretentious people. His writings and talks are full of contradictions: exhorting fellow scientists to eschew organization and bureaucracy and get to sea, while simultaneously complaining that the science was being strangled by the focus on purely local problems, inadequate theory, and poor instrumentation, the remedy for which was professionalism and large-scale organization.

Stommel was born in Wilmington, Delaware, on September 27, 1920, into what today would be labeled a dysfunctional family. His ancestors were from the Rhine Valley, Poland, Ireland, the Netherlands, England, and France, with a trace of Micmac Indian. Walter Stommel, his father, was a chemist born in northern Germany and trained in Darmstadt and Paris. In the upheaval of the First World War, he emigrated to Wilmington where he found employment with Dupont Chemical. While there he married Marian Melson whose family had lived on the Eastern Shore of Maryland and nearby Delaware since colonial times. Their son Henry

Melson Stommel was born shortly thereafter. For reasons which are not entirely clear, perhaps anti-German sentiment following World War I, the family moved to Sweden, where Walter rose to become chief chemist of a leather factory. But Henry's mother, just prior to the birth of a daughter, Anne Stommel, left Sweden; with Henry she returned to Wilmington, choosing never again to see her husband. (Among other problems, she hated the primitive life in rural Valdemarsvik, Sweden.)

Henry thus grew up in a single-parent family. Although he states in his autobiography that he did not know his father was alive (and with a second family) until he entered high school, it is clear that Henry and Anne exchanged Christmas cards with him from a very early age (E. Stommel, private communication, 1995).

In 1925 Henry's mother moved with the two children to Brooklyn, New York, where the household consisted of Henry, his sister Anne, and their mother, but also included their maternal great-grandmother, a divorced Aunt Beck and her daughter, and a maternal grandfather and grandmother. As described by Henry, the household was supported wholly by his mother working as a fund raiser and public relations officer for a hospital (this was during the depression) and dominated by female disputation. His grandfather, Levin Franklin Melson, was apparently a peaceful man who retreated upstairs to his room. The discussions he and Henry carried on there until Melson's death, when Henry was eleven, provided a refuge for both of them. Melson was an important person in Henry's early years. He had been trained as a lawyer, worked as a bank clerk, struggled with alcoholism, but apparently had a true love of knowledge and a bit of scientific understanding, including a taste for simple chemistry and *Popular Mechanics*. Science was both interesting and a protection against the discords of the world.

EDUCATION AND EARLY PROFESSIONAL YEARS

Stommel's education was in the public schools of New York City at a time when that system had many highly educated, articulate teachers who had sought security during the depression through careers in public education. He spent one year at the competitive-entry Townsend Harris High School, but finished high school at Freeport, Long Island, where the household had moved. He proceeded then to Yale University largely on the basis of his mother's successful efforts to obtain a full scholarship for him. There a major in astronomy failed to provide a focus for his interests. Graduating in 1942 at the height of the Second World War, he was faced with a conflict between his basic pacifism and self-awareness of a streak of aggression; he compromised by remaining at Yale for two years teaching analytic geometry and celestial navigation in the Navy's V-12 program. Six months spent at the Yale Divinity School showed that the ministry was an unsuitable vocation (he had a lifelong ambivalence towards religion, organized or otherwise). In 1944, at the suggestion of the well-known astrophysicist Lyman Spitzer, he applied for work at the Woods Hole Oceanographic Institution in Woods Hole, Massachusetts, an organization that had been transformed rapidly from a summer-only field station into an arm of the U.S. war effort. Assigned to work with Maurice Ewing on acoustics and antisubmarine warfare, he found both the work and Ewing's imperial style quite unpalatable. He escaped as quickly as possible into other groups and endeavors.

Stommel flirted with a number of different aspects of physical oceanography during the period immediately following the war. These included a major effort on modeling tides, atmospheric convection, Langmuir cells, and speculations about computer possibilities, but he did nothing that either he or his supervisors regarded as particularly noteworthy. Then, almost "out of the blue," he published a paper (1948) that marked the birth of dynamical oceanography and the start of his continuing avalanche of new ideas over the next forty-five years. During these early years he was fortunate to have encountered a few individuals who acted as mentors and advisers. These included Lyman Spitzer at Yale, who sent him to Woods Hole; Jeffries Wyman, who put him on to the theory of convection and remained a good friend thereafter; and, especially, Carl Rossby, who inspired him.

Stommel remained at the Woods Hole Oceanographic Institution (WHOI) until 1959, when he left to become a professor at Harvard University. He clearly loved WHOI and the life of the small town of Falmouth. His departure was the culmination of a deep-seated antipathy for the director (Paul Fye) and his policies, coupled with the lure of being a professor at an institution with the reputation of Harvard University. (A significant number of the best scientific staff at WHOI also left around the same time.)

The four years he spent at Harvard were clearly distasteful and unhappy (he wrote about this time in some detail in his autobiography, which was published posthumously[1]) and he "fled" to the much more congenial and democratic environment in the Department of Meteorology at the Massachusetts Institute of Technology (MIT). There his closest colleagues were to be people such as Jule Charney, Norman Phillips, Edward Lorenz, and Victor Starr. The major problems at Harvard (his departure was something he continued to rationalize for many years) appear to have been the arrogance of both the institution and of individuals there—which clashed with his deeply democratic instincts—coupled with a sense of not belonging in such a place without a proper doctoral degree.

Stommel worked at MIT for sixteen years as professor of physical oceanography, returning to WHOI nearly instantly upon the retirement as director of his *bête noire*, Fye. He remained there actively doing science almost to the day of his death, only rarely and grudgingly leaving Cape Cod. Some of his most interesting work was done toward the end of his life.

Stommel married Elizabeth Brown, daughter of Huntington Brown, professor of English at the University of Minnesota, and Elizabeth Waldo Wentworth Brown, originally of Boston, on December 6, 1950. They had three children: Matthew (a professional fisherman in Falmouth, Mass.), Elijah (a physician at the Dartmouth-Hitchcock Medical Center), and Abigail Stommel Adams (a nurse practicing in Falmouth). Hank's devotion to his wife (universally known as Chickie) was complete, manifested in part by his insistence every day, when it was humanly possible, on bolting home for lunch precisely at noon. Her own work, apart from the family, has been as a writer, church organist, and hospital chaplain.

Henry Stommel's work can be divided crudely into several overlapping categories. The Collected Works contain expert commentaries on his work, and I will therefore simply summarize the high points. A general comment is that he made observations at sea, designed (with the help of talented engineers) new instruments, worked in the laboratory (again with the assistance of skilled experimentalists), and did theory. The work of such a person is not easily summarized. I do think it fair, however, to assert that his sea-going was important mainly for the inspiration it gave him, rather than for the power of the data per se. He was not temperamentally suited to the infinite taking of pains reflected in the very best at-sea work, which he so admired in others.

THEORY OF THE GENERAL CIRCULATION OF THE OCEAN

The general circulation of the ocean was the focus of Stommel's efforts for decades and our present understanding of it is his greatest monument. In this field as in the rest of his science he combined a deep, sometimes wholly inexplicable, physical intuition with a love of field work and just enough mathematical skills to suit his needs. Constantly complaining about his lack of mathematical abilities, he always found either the precisely right, just-simple-enough problem or a suitable, more mathematically adept collaborator to generate a series of papers that constitute a history of oceanographic theory and observation in the middle to late twentieth century.

This body of work begins with the 1948 paper already mentioned, in which he showed that the Gulf Stream was a phenomenon that could be explained deductively by fluid dynamics. In particular, he found the mechanism (the latitudinal change of the Coriolis force on the rotating Earth) that produced the westward intensification of oceanic currents. This first paper is prototypical; he fingered an essential phenomenon, which somehow no one had ever thought to try to understand, and he then formulated an extremely simple model that was reduced by him to nothing more than a linear two-dimensional partial differential equation whose solution provided the essential insight. There is a long list of powerful and sophisticated scientists who must have kicked themselves for not having seen the problem and its mathematically easy solution.

This paper is also prototypical of his approach to finding problems to work on. Stommel attributed to the late Raymond Montgomery the suggestion that the Gulf Stream was something important and in need of explanation. (But Montgomery attributed it to Columbus Iselin.) On completing a piece of work, Stommel would go searching for something to take up next; he relied on colleagues to an astonishing degree, given his creativity, to point him in new directions. He roamed the corridors of MIT and WHOI, asking in effect, "what's interesting?" Often he would get intrigued, hooked, and would become obsessed with a problem to the point where he was preoccupied with it day and night. More than one collaborator can attest to the late-night or 6:00-a.m. phone call that would start without so much as "hello," but would come out something like "you know I think the second term in that equation can be neglected, because…"

In the decade following the 1948 paper, Stommel and his collaborators had gone from a primitive and stumbling beginning to a sophisticated theory of the thermocline, the gross thermal structure of the ocean (1959), to a theoretical view of the global abyssal circulation (1960). His important book, *The Gulf Stream*, had already been written by about 1954 and was probably the first true dynamical discussion of the ocean circulation. He embedded the Gulf Stream in the wider context of the general circulation and already clearly had in mind what became the so-called thermocline theories. Following these theories of the late 1950s and early 1960s, there was an extended pause in the theoretical work of Stommel and of others; the theories were based on similarity solutions to an otherwise intractable set of nonlinear partial differential equations. They were a considerable, almost astonishing achievement, a clear beginning on a full theory, but were very difficult to with and their extension obscure. For a long period ending in the early 1980s Stommel's attention turned to more specific elements of the general circulation. The results included an important paper pointing out the great significance of the very small area in which the ocean underwent convective sinking; a study of the balance of forces in the

Antarctic Circumpolar Current;[2] a series of studies of the nature of convection in the Mediterranean; and in 1979 the introduction of the important concept of the "Ekman demon," which opened the new field of ocean subduction. During the period from about 1963 to 1980 his focus on the general circulation was largely observational (discussed below).

Then in 1983 Luyten, Pedlosky, and Stommel reopened the study of the oceanic thermocline structure through the seemingly ruthless means of replacing the equations for a continuously stratified fluid by those for one constructed of layers. This simple step, coupled with a highly developed physical intuition, suddenly made the study of the oceanic thermohaline structure blossom once again; this paper was followed by a torrent of papers by Stommel and collaborators, as well as many others. Although this theoretical vein may now be nearly mined out, Stommel had clearly rejuvenated, late in his career, the study of a fundamental problem in oceanic physics.

OBSERVATIONS OF THE OCEAN CIRCULATION

Henry Stommel was constantly looking at data, his own and that of others, speculating about possible new instruments and incessantly planning expeditions around the world. The advent of modern long-endurance oceanographic vessels, electronic instrumentation, and the appearance of jet passenger airplanes beginning in the early to mid-1960s made it practical for the first time to study distant oceans without spending years away from home. (He bemoaned the disappearance of the evocative old oceanographic sailing vessels such as the *Atlantis*. But he confided to me, standing on her deck as she was preparing to leave Woods Hole for the last time on her way to oblivion in Argentina, that the new vessels were far better for serious work at sea.

Similarly, the advent of modern computers and electronic instruments meant that the sort of mechanical devices he liked to tinker with had become obsolete and nearly irrelevant.)

He instigated and participated in many cruises all over the world. The most notable of these were the Swallow-Worthington float measurements that offered confirmation of his abyssal circulation theory; the multiship, multinational studies of Mediterranean convection;[3] the first-ever, true trans-Pacific hydrographic sections; and the wonderfully romantic operations in the Seychelles using the marginal vessel *La Curieuse*. In his autobiography he gives an account of this mode of doing oceanography in a small, exotic, faraway port, using simple equipment and dealing with all the characters one must in such an operation. This was oceanography in the middle 1970s carried out in a style as close as possible to that of 1950, and he loved it. On the other hand, he felt compelled to admit that because of their crude equipment they missed the really important discovery: the equatorial jets found by Luyten and Swallow with a large crew and scientific party using a state-of-the-art profiling device on a modern oceanographic vessel.

Stommel's invention with Fritz Schott of the so-called beta-spiral method for determining absolute flow in the ocean was a high point of this period. Although the method has been used frequently to make estimates of the actual oceanic flow, perhaps its most important result was to demonstrate forcefully that the classical problem of physical oceanography—the inability to determine absolute current at sea from temperature and salinity measurements alone—was, like the Gulf Stream in his 1948 paper, a problem susceptible to theoretical analysis and solution. In a more recent context the beta-spiral is an example, when modified, of an inverse method. It spawned a host of extensions and applications.

By around 1969 Stommel had concluded that the new technologies rapidly becoming available to oceanographers (the invention of solid-state electronics has had a greater impact on oceanography than any other technical innovation of the twentieth century) would make it possible to observe the ocean circulation at sea in a qualitatively new way. Gradually seeping into the oceanographic consciousness was the realization that the ocean was highly time-dependent and probably turbulent—a picture at odds with the prevailing mind-set of a steady, essentially slow, laminar flow. Over the previous twenty-five years Stommel had published a series of exhortative articles urging his colleagues to recognize that one could not understand the ocean by summing up results from a series of small regional experiments. He thought the time had finally come to put into practice the vision he had been preaching. One result, albeit peripheral to his own immediate scientific interests, was the global-scale Geochemical Sections Program (GEOSECS). Another was the Anglo-U.S. Mid-Ocean Dynamics Experiments (MODE) and its U.S.-U.S.S.R. successor POLYMODE, instigated, organized, and overseen by Stommel over many years.

By most measures these programs were a great scientific success (particularly GEOSECS and MODE) and became prototypical of successor generations of organized international oceanographic programs. But Stommel found himself embroiled in bureaucracy, paperwork, meetings, and intellectual compromises in the name of international and national comity, all completely contrary to his taste in science. His return to Woods Hole in 1978 led him to resign from all such programs and thereafter he would serve on no committees or participate in any organizations; he would not even take on another graduate student or postdoc.

MIXING AND MICROSTRUCTURE

Perhaps his most famous experiment was one he deprecated: the study with L. F. Richardson in 1948 of lateral mixing in large bodies of water, using nothing but cut-up parsnips. This work is perhaps the ultimate in the strings-and-sealing-wax school of oceanography, but remains an important achievement in a field that has grown increasingly important over the years. He professed the greatest admiration for scientists who did precisely what he himself did not do: spend long months at sea, making exquisite, high-quality observations. But these scientists (although he did not say this) could not take the intellectual leaps that were his own forte.

The entire field of what is usually called double-diffusive convection is often traced to a one-page paper with the unusual title "An oceanographical curiosity: The perpetual salt fountain." Stommel himself attributed the main idea to his longtime collaborator Arnold Arons, and it was Melvin Stern who later, in 1959, recognized the much more fundamental nature of the phenomenon. (He praised Stern highly for this; clearly he was somewhat chagrined not to have had that insight himself.) Out of these efforts—and with a kind of absentminded, intermittent interaction with collaborators—grew the laboratory experiments and later theories that have developed this field into a branch of fluid dynamics in its own right (J. S. Turner in the Collected Works well describes Hank's approach to laboratory work).

Although Stommel preferred to work with simple instruments (parsnips, buckets of salty water, etc.), he clearly recognized that much more sophisticated measurements were required to understand the ocean. He was always on the lookout for clever new instruments (e.g., early foreseeing and helping to bring into being what became in the hands of Tom Rossby and Doug Webb the SOFAR float, an acoustically tracked instrument that floats at a predetermined midwater depth). Neil Brown by the middle 1960s had produced the first of the revolutionary continuously profiling devices, then called an STD (salinity-temperature-depth). Despite the intense skepticism of many of his colleagues, Stommel determined to use these instruments to study mixing of Pacific and Indian Ocean water in the Banda Sea in the Indonesian archipelago. Somewhat concerned about their reliability, he succeeded in getting three of these new instruments on board the *Atlantis II*. At the very last minute, refused permission by the Indonesians to work in their territorial waters, he proceeded to use STDs on the northwest Australian coast, making repeated surveys of the interleaving water masses. With his collaborator, the late Soviet oceanographer Konstanin Federov (who had become so enamored of the instrument he undertook its virtual single-handed operation), he produced an extremely important discussion of the implications of their measurements. Out of this work, and in the hands of skilled instrument designers and users such as Charles Cox, eventually came the new field of fine- and microstructure studies.

For a long period Stommel was fascinated by the Indian Ocean, mounting repeated expeditions there, often in collaboration with his longtime friend John Swallow. He sometimes tried to work independently of modern oceanographic tools by running small boat operations out of such outlandish (and dangerous) places as Somalia.

TIDES, ELECTROMAGNETIC METHODS, EDDIES, ESTUARIES

Henry Stommel worked on a vast variety of problems. These included tides, pedagogical problems (how to explain the Coriolis force), numerical methods, and internal waves. The breadth of his interest can be understood simply by reading the titles of his unpublished technical reports as listed in the Collected Works. Nonetheless, a number of major foci do stand out. These include the general application of electromagnetic measurements to oceanic flows, the dynamics of estuaries and the related problem of hydraulic controls, and the interaction of nonlinear eddy-like phenomena (hetons). The last category generated in part his late-in-life fascination with computers, machines whose influence he had thitherto found rather distasteful.

ACADEMIC YEARS

Although he was professor for nearly twenty years at two leading academic institutions (Harvard and MIT) he rarely wrote or spoke of his role as teacher. Perhaps his deepest ambivalence emerged here. He was advising students on how to obtain a Ph.D., which he lacked himself. In explaining his presence at MIT, he would admit, slightly grudgingly, that his personal goal of real progress in the field demanded a level of sophistication in mathematics, fluid dynamics, statistics, and electrical and mechanical engineering that was simply beyond the amateurs, although the amateurs were often more fun.

Hank Stommel was not a very good lecturer. He often stumbled, reversing thought in the midst of a sentence—thinking aloud. For strong students who could cope, he was nonetheless a superb teacher in the wider sense—a source of stimulation, ideas, love of the ocean. A number of his

Ph.D. students have gone on to successful careers of their own.

Hank Stommel had a sense of fun in almost everything he did. He clearly enjoyed life and being around people. He wrote incessantly, producing several non- or semi-technical books, including *Volcano Weather: The Story of 1816, the Year Without a Summer* (1983) in collaboration with his wife Elizabeth and a series of brief essays on the passing scene in the *Falmouth Enterprise* under the pseudonym "Starbuck." (The series ended prematurely when the newspaper foolishly disclosed his identity.) He had a taste for the absurd, being fascinated especially with a nineteenth-century character named William Leighton Jordan, who attacked the British Admiralty for allegedly falsifying temperature measurements made on the *Challenger* expedition. Stommel wrote an entire book on islands that never actually existed.[4] He loved making and setting off fireworks for the amusement of his own and visiting children, as well as for himself. There was a period in which he printed newsletters, some anonymously, poking fun at various people and institutions. He built a railroad in his backyard for the entertainment of his grandchildren and visiting oceanographers. His skill as an amateur painter was considerable, sometimes manifesting itself in unexpected ways, such as the kitchen refrigerator he decorated with tropical birds and animals on a brilliant yellow backdrop. The list of his interests in almost endless.

Apart from his own science, Stommel's greatest legacy was his inspiration to others struggling to make their way scientifically. Anybody who would listen became the object of a passionate lecture on what was exciting him and what he was doing, with both parties usually emerging with renewed enthusiasm. He was unassuming, normally unwilling to impose his views on others, and unhappy with bureaucracy and organization. Stommel did, however, have an acute sense of his own worth. In private, and only in private, he could be scathing about individuals who he felt did not treat him with the respect owed him or who he believed had reputations far beyond what their own work merited. But basically he was a kind man who did not want to deliberately make anyone unhappy. If asked to write a letter of recommendation for a person whom he really did not admire, he would nonetheless find some way to say something positive. To Hank's dismay, these letters were sometimes seriously misinterpreted. But even his privately expressed reservations made him acutely uncomfortable. One day one could hear him expressing outrage about what someone had said or done; the next day, seemingly as a form of penance, he would be going out of his way to assist that very same person in a promotion or career advance. Consistency was not his chief virtue; compassion perhaps was.

During his lifetime, Henry Stommel received many honors and awards. Among them were the National Medal of Science, the Craaford Prize of the Royal Swedish Academy (shared with Edward Lorenz), election to the National Academy of Sciences (1959), and foreign membership in The Royal Society, London (1983), the Soviet Academy of Sciences, and the Académie des Sciences de Paris.

I WAS GREATLY ASSISTED in the writing of this memoir by Elizabeth (Chickie) Stommel who agreed to several hours of oral history (the tapes of which will be deposited in the WHOI archives) and the answering of endless questions. Henry's sister, Anne Melson Stommel of Red Bank, New Jersey, who corrected details and who kindly provided an extensive written background on the Melson family history, a copy of which will also be placed in the WHOI archives. The publication of Henry Stommel's Collected Works with his autobiographical essay and the commentaries by a number of individuals was of great help. The autobiography is somewhat "raw"—it was not published in his lifetime—and the reader is warned that Stommel's

memory was not always reliable. I also drew on the personal essays about him in *Evolution of Physical Oceanography, Scientific Surveys in Honor of Henry Stommel*, edited by B. A. Warren and C. Wunsch (MIT Press, 1981). The manuscript was read for accuracy by Elizabeth Stommel, Joseph Pedlosky, Henry Charnock, Anne Stommel, and Nelson Hogg.

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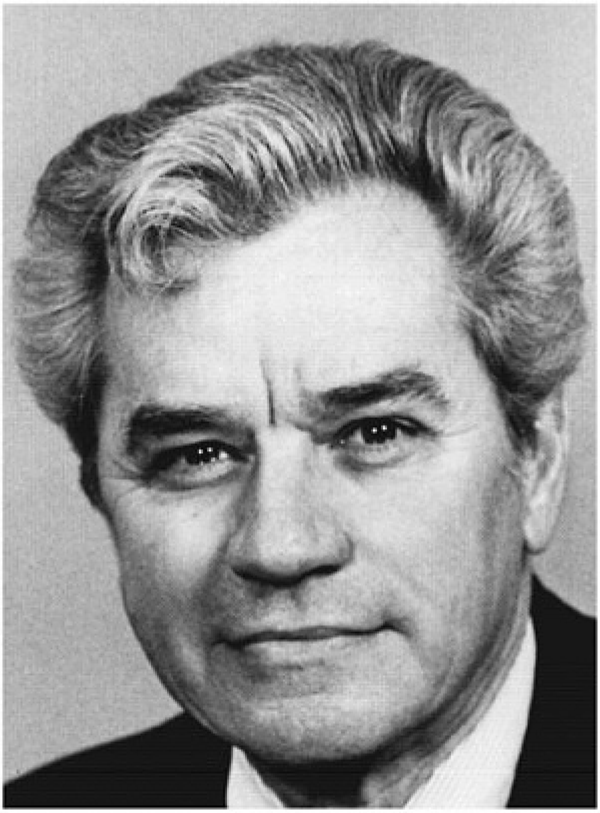

JOSEPH E. VARNER

October 7, 1921–July 4, 1995

BY MAARTEN J. CHRISPEELS

JOSEPH E. VARNER'S fifty-year career (1945–95) spanned the emergence and development of plant biochemistry, and he was one of the major contributors to this field. His most notable research achievements were the definition of cell death as an active process; discovery that the hormone gibberellin regulates the expression of α-amylase in barley aleurone cells at the level of the gene; and cloning of the cDNA for the cell wall protein extensin, which laid the foundation for the study of the role of cell wall proteins in plants. Together with James Bonner, Varner edited *Plant Biochemistry*, which remained the standard single-volume textbook in the field for fifteen years. During the last ten years of his life he was probably the most widely admired and loved plant biologist in the country, the elder statesman of his discipline. He was extremely knowledgeable about biochemistry and whenever he talked to colleagues or students he generously shared his many ideas. He was a tireless promoter of the study of plants and talked about experiments until the final days of his life. In addition, Varner was a sought-after advisor to government, universities, and industry. He was a major supporter of the American Society of Plant Physiologists, which he served as president in 1970–71 and which awarded him its highest honor, the Stephen Hales Prize, in 1990.

GROWING UP IN OHIO AND STARTING A FAMILY

Joe Varner was born and grew up in Nashport, Ohio, on a farm that had been in the family's possession for several generations. He was the second of four sons, one of whom (Robert Varner) carried on the farming tradition; thus, Joe Varner always maintained his ties to the land. His parents, George and Inez Gladden Varner, were both school teachers, and the Varner children were educated first in the rural one-room schoolhouse where their father was the teacher. They later attended the local high school. Inez Varner stayed home to help run the farm and care for her family. Joe's love of science was apparent early on and he won an award for "best student in the county in chemistry and physics." He continued his education at Ohio State University (OSU), where he majored in chemistry and received a bachelor's degree in 1942 and a master's degree in 1943. About his education at OSU he wrote, "It was possible to earn a B.Sc. in chemistry without hearing a single word about physiological chemistry or photosynthesis. It was also possible to sit through an entire year of elementary botany without hearing a single instance of how a chemist might make a contribution to botany."

Joe joined the U.S. Marine Corps in 1944, and while he was in the service he found a book on physiological chemistry (Hawk, Oser, and Summerson) at the Santa Ana Public Library that opened his eyes to new possibilities for research. "Wouldn't it be nice to do that sort of thing with plants," thought Varner. In 1945 Joe married Carol ("Ray") Dewey and together they raised a family consisting of son Lee and daughters Lynn, Karen, and Beth. Joe was first employed as an analytical chemist by the Battelle Memorial

Institute, but after a year he returned to OSU to work on his doctorate supported by the G.I. Bill. He wanted to know "how plants work" rather than "what they are made of," and he was awarded a Ph.D. in biochemistry in 1949.

THE FIRST TEN YEARS: FROM ORGANIC ACIDS TO ENZYME SYNTHESIS

Varner started his career when plant biochemistry was emerging as a new branch of experimental plant biology. At that time plant physiology concerned itself with mineral nutrition of plants, the environmental stimuli that induce plants to flower, and the idea that hormones control plant development. The availability of radioactive CO_2 led to the study of plant metabolism and in the late 1940s and early 1950s understanding metabolism was seen as an important step in elucidating the control of plant growth and development.

Varner's doctoral dissertation, carried out under the guidance of Prof. Robin C. Burrell and presented in 1949, dealt with the metabolism of organic acids in *Bryophyllum calycinum*, a plant that fixes carbon dioxide into malic acid during the night, then breaks down the malic acid again during the day to re-fix the released CO_2 with ribulose bisphosphate oxygenase. For this study Varner used radioactive CO_2 supplied by the Oak Ridge National Laboratory. No one at OSU had any experience with ^{14}C, so Joe used his own money to go to Oak Ridge for a thirty-day training course in radioisotopes. Later in his career he would continue to use isotopes in very clever ways.

During his first three years as an assistant professor of biochemistry at OSU, where he was appointed to the faculty in 1950, Varner continued to work on organic acids and he developed a method for their separation by chromatography. However, after spending a year (1953—54) at the California Institute of Technology in the laboratory of James Bonner, Varner changed his research direction quite dramatically. In the 1950s Caltech was a hot place for plant biology with three active laboratories, those of James Bonner, Arthur Galston, and Frits Went. James Bonner's laboratory was a magnet for plant biochemistry with graduate students and postdocs, such as Sam Wildman, George Laties, Bernard Axelrod, Robert Bandurski, George Webster, and many others who went on to make major contributions to this new field. By all accounts the research environment was enormously stimulating. Ideas flowed freely between genetic, structural, and biochemical laboratories, and the sky seemed the limit. The young scientists could hardly wait to answer all of plant biology's pressing questions. The year at Caltech had a profound impact on Varner's career, and his lifelong friendship with James Bonner resulted in the joint editing of *Plant Biochemistry*.

After returning to OSU from his sabbatical at Caltech, Varner convinced George Webster to join him there. Together they started working on the biosynthesis of glutamine, asparagine, and glutathione. They saw the tripeptide glutathione as a simple model to study peptide synthesis. This work is evidence of Varner's desire to get beyond metabolism and to look at how processes in living organisms are controlled. When Varner arrived at Caltech, Watson and Crick had just published their model of the structure of DNA, and soon after he returned to OSU different laboratories started reporting that proteins could be synthesized in vitro. Furthermore, the one-gene-one-enzyme theory of Beadle and Tatum was much talked about, although the discovery of mRNA, the connection between DNA and protein, was still ten years away. The work on glutathione biosynthesis was important in its own right, but it did not lead to a better understanding of protein synthesis, because no mRNA template is required to order the amino acids.

Webster went on to work on in vitro protein synthesis, but Varner turned his attention to the role of oxidative phosphorylation and protein synthesis in development (fruit ripening) and senescence (pea cotyledons). His main contribution here was to define cell death as an active process that requires respiration and the synthesis of new enzymes. His work on the synthesis of enzymes in pea cotyledons during seedling growth followed closely on the heels of work by Harry Beevers who demonstrated the induction of glyoxylate cycle enzymes in castor bean endosperm, another senescing tissue. In 1961 Varner published "Senescence in plants," a major review on this topic in the *Annual Review of Plant Physiology*. In the *Plant Biochemistry* textbook, edited with James Bonner in 1965, he devoted an entire chapter to "death." Those who recently "discovered" apoptosis in plants can profit from reading it. Subsequently, Varner's lab found that a diffusible factor from the axis regulates cotyledon senescence.

At this time Varner was also working on oxygen exchange reactions. He investigated the transfer of oxygen from ^{18}O-labeled arsenate in the arsenolysis of glutamine. Throughout his career Varner used isotopes in many creative ways, not only for metabolic labeling but also for exchange reactions, density labeling, protein turnover, and *in planta* enzyme assays. Joe's older brother David was a successful inventor, and Joe had a touch of the same creative streak. In 1952 he published a paper entitled "An automatic constant volume fraction collector" in the *Journal of Chemical Education*.

CAMBRIDGE UNIVERSITY AND THE RESEARCH INSTITUTE FOR ADVANCED STUDIES

In 1959 Joe took his family to England for a sabbatical leave at Cambridge University. After returning to Columbus he became dissatisfied at OSU. He told the dean he was underpaid and unappreciated (Varner apparently had not yet been promoted to associate professor). The dean replied that if Varner thought he was worth more money, he should find an employer willing to pay more. By his own account, Varner promptly wrote a letter of resignation and somewhat later found a position with the Research Institute for Advanced Studies (RIAS), a division of the Martin Marietta Corporation. RIAS was housed in a large suburban property in Baltimore and consisted of a small community of physicists, chemists, mathematicians, and a few biologists. The biology group was led by Bessel Kok, a feisty, brilliant Dutchman (later elected to the National Academy of Sciences), who, like Bonner, had a profound impact on Varner's career. RIAS housed a lively group of scholars; ideas and experiments were hotly debated in the cafeteria and at social gatherings. Kok and Varner, along with George Cheniae and Dick Radmer, constituted a true debating society. Varner's critical thinking skills were sharpened by these lively exchanges. During four productive years at RIAS, Varner poured his creativity into two scientific problems: hormonal control of enzyme synthesis (see below) and the detection of life on Mars. The work on the detection of life on Mars was triggered by a call for proposals from NASA to design a 10-lb instrument that could detect "life" (not just life as we know it). With his background in chemistry and his interest in exchange reactions Varner argued persuasively that we should not look for metabolism (e.g., CO_2 assimilation or release), but rather measure exchange reactions. About this time it was discovered that phosphoryl/phosphate group transfers resulted in $H_2{}^{18}O$ formation when ^{18}O-phosphate-labeled substrates were used, and Varner suggested that such exchange between water and oxy-anions (phosphate, sulfate, nitrate) could possibly constitute the simplest reactions of "life," whether on Earth or elsewhere. These ideas were published in an article in *Science* in 1967, but the probe that was eventually built (but not used because of NASA budget constraints) relied on the "sniffing" of gases and their analysis by a 10-lb mass spectrometer.

HORMONAL CONTROL OF ENZYME SYNTHESIS

The research for which Varner is best known was his demonstration that the plant hormone gibberellin induces cereal aleurone cells to synthesize massive amounts of α-amylase through the action of the hormone on gene activity. I had the good fortune to join this project as a postdoc in his laboratory. This work finds its origins in the independent observations by L. G. Paleg and H. Yomo that addition of gibberellin to barley grains, from which the embryo had been removed, greatly enhanced the release of sugars and the production of amylolytic enzymes. Varner, who was fully conversant with recent developments in molecular biology, suspected that gibberellin was inducing α-amylase release (activation or synthesis) probably by a process of gene activation. He quickly adopted the barley endosperm system as a model to study the genetic basis of hormonal control of enzyme synthesis, and in 1964 he published a seminal paper on this topic in the *Proceedings of the National Academy of Sciences* (the paper was communicated by James Bonner). Using the available tools, inhibitors of protein synthesis (amino acid analogs) and RNA synthesis (actinomycin D), he was able to conclude that "the effect of gibberellic acid is therefore upon the expression of the genetic information which controls α-amylase production." The paper also demonstrated that incubation of endosperm tissue with radioactive amino acids resulted in the production of radioactive α-amylase, suggesting de novo synthesis of the enzyme. A major point of discussion at the time was whether the appearance of enzyme activity in storage organs of seeds during seedling growth resulted from the activation of an inactive enzyme precursor (zymogen) or from de novo synthesis of the enzyme.

This elegant work, which was initiated at RIAS in Baltimore, drew the attention of more classically oriented plant physiologists such as Anton Lang, who had just been named director of the newly created Atomic Energy Commission Plant Research Laboratory at Michigan State University (MSU), and Lang offered Varner a position at MSU. Varner left RIAS in the spring of 1965, and much of the work on the barley system was done in the next eight years at MSU by his graduate students (U. Melcher, W. Evins, and D. C. Koehler) and postdocs (J. V. Jacobsen, G. R. Chandra, and myself). Nevertheless, it was ten years before David Ho, another Ph.D. student, showed that gibberellin induces the synthesis of α-amylase mRNA, primarily because the molecular tools to answer that question were not available until then.

Varner combined his penchant for devising simple yet elegant techniques and his love affair with isotopically labeled metabolites to measure, in collaboration with Philip Filner, protein synthesis using density labeling. They used heavy water ($H_2{}^{18}O$) to demonstrate that the increase in α-amylase activity induced by gibberellin in aleurone layers was due to de novo synthesis of the enzyme. Varner reasoned that the ^{18}O would be incorporated into amino acids during hydrolysis of the reserve proteins of the endosperm and would then appear in all newly synthesized proteins.

Newly synthesized proteins should, therefore, have a greater density than did pre-existing proteins, and the technique would settle the zymogen activation question. The proteins were fractionated on isopycnic CsCl gradients in an adaptation of the Meselson-Stahl experiment demonstrating the semi-conservative replication of DNA; the average density of α-amylase synthesized in the presence of 80% $H_2{}^{18}O$ was found to be 1.1% greater than that of the enzyme synthesized in the presence of $H_2{}^{16}O$. The whole experiment was conducted with two aleurone layers and 100 μl of water! The so-called density labeling technique was widely applied in many plant biochemistry laboratories to demonstrate *de novo* enzyme synthesis. However, because of the expense of $H_2{}^{18}O$, D_2O was used for most experiments.

Gibberellin not only turns on the expression of the genes for α-amylase (and other hydrolytic enzymes) in aleurone cells, but also induces the formation of the endoplasmic reticulum, the site of synthesis of these secreted enzymes. Plant cells were known to possess isoforms of enzymes that remain inside the cell, as well as isoforms that are secreted. Varner coined the terms "inzymes" and "outzymes" for such isoforms and discussed with his associates at length his idea that there must be subtle differences in protein structure between the two that allow them to be routed to these two different destinations. We now call these structural differences "targeting signals." My Ph.D. thesis in the laboratory of John Hanson at the University of Illinois on changes in microsomes during cell elongation and my postdoctoral research in Varner's laboratory on α-amylase secretion led to a career in plant cell biology and a study of protein targeting signals and the role of the Golgi apparatus in glycosylation. Varner remained interested in secretion, and in 1971-72 he took a sabbatical leave at the University of Washington to become more familiar with yeast (*Saccharomyces cerevisiae*) because he thought that it might be a more suitable system for studying this process.

In 1973 Varner left the Plant Research Laboratory and moved to the Biology Department of Washington University in St. Louis. At Washington University he started with a small research group, but he had plans for his new department. Soon after arriving in St. Louis, he convinced the then chancellor William Danforth that he could build a first-rate plant biology program if the department were given additional faculty positions. Varner clearly saw that plant biology was nearing a new takeoff point and he wanted Washington University to be part of it. He attracted a number of first-rate junior plant biologists to the department, including Roger Beachy, Mary Dell Chilton, William Outlaw, and Virginia Walbot. Subsequently, additional plant biologists joined this group. Soon after coming to St. Louis, Varner met Jane E. Burton and in 1976 they were married. They spent twenty happy years together, and he was a caring stepfather for her two children. Scores of plant biologists from all over the world enjoyed the hospitality Joe and Janie provided in their lovely home on Kingsbury Avenue. At Washington University he carried on with the work on gibberellin and aleurone cells for a few years and started his research on cell wall proteins and cell wall architecture.

HYDROXYPROLINE-RICH GLYCOPROTEINS AND EXTENSIN

While on sabbatical leave at Cambridge University, Varner met Derek Lamport who was then a Ph.D. student of D. H. Northcote. Lamport had just discovered that the most abundant amino acid in a hydrolysate of purified sycamore cell walls was hydroxyproline and had postulated that the cell wall contained a structural protein, which he called extensin. Varner was fascinated by the idea and invited Lamport to become an independent postdoc in his laboratory at RIAS.

Later, he persuaded Anton Lang to appoint Lamport an assistant professor at the AEC Plant Research Laboratory, where they both moved in 1965. While at MSU, Varner and Lamport worked in adjacent laboratories and interacted on a daily basis. Lamport continued the biochemical characterization of extensin, proving its existence to early skeptics.

After Varner moved to Washington University he sensed that aleurone layers and gibberellic acid had run their courses, at least in his laboratories, and after some hesitation he moved to the cell wall protein problem. The hesitation probably stemmed from a reluctance to compete with his long-time friend. However, he knew better than anyone else that Lamport was too set in his biochemical ways to utilize the new molecular tools to push the analysis of extensin into new terrain. Varner's lab used two approaches to get at the extensin protein: the purification of a precursor protein before it becomes covalently linked to the cell wall matrix and the cloning of a cDNA. He switched to the aerated carrot disk system used in my laboratory because we had shown in the late 1960s that wounding (when the disks are cut) induces massive synthesis of hydroxyproline-rich glycoproteins (HRGP, Varner's new term for extensin). They made several attempts to obtain the extensin cDNA. Realizing that a Hyp-rich protein should have a cytosine-rich message, David Stuart attempted to use polyG columns to isolate the message using in vitro incorporation of amino acids. They also devised a way to identify clones that have proline-rich and leucine-poor translation products. These approaches failed, and the cDNA clone for extensin was finally obtained through a library screen by Jychian Chen, a graduate student from Taiwan.

The findings were published in the *Proceedings of the National Academy of Sciences* and were communicated by Varner himself, having been elected to membership in the Academy in 1984. They confirmed the work of Lamport and showed that the pentapeptide Ser(Pro)$_4$ was repeated 25 times in the derived amino acid sequence of 306 amino acids. TyrLysTyrLys and ThrProVal were also found as other major repeating units. This first cloning of extensin opened up the whole field of cell wall structural proteins. Other students and postdocs worked on many different aspects of HRGP biosynthesis, including insolubilization in the wall, structure of the protein, and the induction by pathogens. With Gladys Cassab, a Ph.D. student from Mexico, he described an entirely new glycine-rich cell wall protein, which he referred to as plant silk. As a result of these important contributions Varner was asked to write a review on cell wall architecture for *Cell*.

While in St. Louis, Varner became a consultant for Monsanto and initiated a joint research project with Jake Schaeffer and others to investigate nitrogen metabolism (glycine and asparagine utilization and protein turnover) in soybean using ^{15}N and ^{13}C NMR. Again, he cleverly used isotopically labeled metabolites, this time coupled to a hightech analytical technique.

In 1977, in recognition of Joe's numerous contributions to plant biochemistry, the University of Nancy awarded him a doctor *honoris causa* degree. Together with Jane he traveled to France and enjoyed the French hospitality.

TISSUE PRINTING, LIGNIN BIOSYNTHESIS AND CELL DEATH (REPRISE)

As noted earlier, Varner had a penchant for simple yet elegant techniques designed to answer interesting questions. In 1986, with Gladys Cassab, he revived the technique of tissue printing. The question they wanted to answer was whether cell walls of different cell types differ in their macromolecular constituents. If mesophyll cells and bundle sheath cells have different cytoplasmic structures, do they also have different cell walls? Tissue printing had been used off and on to detect enzymes on substrate films (e.g., gelatin), but Varner turned to the nitrocellulose sheets already in common use for immunoblotting to make tissue prints. When a thin tissue slice, especially from a stem, is pressed against nitrocellulose paper, the hard cell walls make a slight indentation, and the proteins that are not covalently bound to the wall are transferred to the nitrocellulose (as are the cytoplasmic proteins). The proteins can then be detected by relying on their enzymatic activity (e.g., peroxidase) or with antibodies (as with immunoblotting). Using side illumination and a low-power light microscope, Varner obtained amazingly beautiful images. Getting good results is not as easy as it sounds, but Varner and a few of his students (Gladys Cassab and Rosannah Taylor) became experts and published several articles demonstrating the utility of the technique in showing cell wall differentiation.

Around 1990, when Zeng-hua Ye came to his laboratory, Varner combined his interest in cell wall architecture with a much older interest in programmed cell death. Together they started working on lignin biosynthesis in differentiating xylem elements of cultured *Zinnia elegans* mesophyll cells. With this cell system, developed in Japan by H. Fukuda and A. Komamine, they studied O-methyltransferases in xylogenesis. Their intention was to use the tools of molecular biology to unravel this intriguing developmental program in which the cell first elaborates a complex cell wall and becomes fully functional in water transport after it dies.

MENTORING

Joe Varner was an unusually effective mentor of young scientists. He was the advisor for three masters students, seventeen doctoral students, and forty-six postdocs and sabbatical visitors. His influence was felt beyond his own laboratory because the impromptu scientific discussions around the coffee table or at lunch attracted graduate students and postdocs from many other laboratories. Together they would dissect a scientific question and illuminate it from different angles. How can enzymology, cell biology, biophysics, chemistry, and structural biology help us get an answer? His favorite term was "brain candy," the reward the brain gets for thinking up clever solutions to difficult problems. At a symposium held in Varner's honor at the time of his retirement in 1993 the many participants referred to the influence that Varner's ideas–his brain candy–had on their research.

PUBLIC SERVICE

Throughout his career Varner was a sought-after advisor who contributed substantially to government, industry, and academic advisory groups. He was a member of the National Science Foundation's Developmental Biology Panel (1968–71) and the Genetic Mechanisms for Crop Improvement Panel of the U.S. Department of Agriculture (CRGO) (1982–85). Realizing the importance of the Department of Agriculture's competitive research grant organization, he volunteered to serve as a program manager (1984–85) and as chief scientist (1986–87). In this last capacity he persuaded the Department of Agriculture to start a postdoctoral grant program. He served as chair of the Scientific Council of the Plant Gene Expression Center of the Department of Agriculture's Agricultural Research Service in Albany, California (1985–90), and was on the visiting committee of the Department of Plant Biology at the Carnegie Institution of Washington in Palo Alto, California (1981–86). For sixteen years he was an associate editor of *Plant Physiology* (1967–84) and for five years served on the editorial board of the *Annual Review of Plant Physiology* 1970–75). He was sought for these positions because of his renown for fairness and absolute integrity. The betterment of plant biology was his only agenda.

Varner was an engaging lecturer who on eight occasions took a month from his busy schedule to give an upper division/graduate course in plant biochemistry at other universities, including the National Taiwan University (1960), National University of Mexico (1976), University of California, Riverside (1978 and 1982), University of California, San Diego (1979), University of Chile (1981 and 1983), and North Carolina State University (1984). I had the good fortune to attend the 1979 course at San Diego. Several hours of reading and preparation went into each lecture and the chemical basis of all phenomena was explored in depth. During these extended visits he always took the time to share his extensive biochemical knowledge with his colleagues.

In the late 1980s he became concerned that plant biochemistry was being neglected. "Soon, every graduate student will know how to clone a gene, but no one will know how to investigate function" was his rationale for approaching the granting agencies for support for a national plant biochemistry course. The course has been held annually in different locations and has attracted students from everywhere.

Varner's death from cancer at the age of seventy-four was an enormous loss for plant biology. An excellent and generous scientist, he was universally admired by his colleagues. He was a tireless promoter and spokesman for his discipline and a mentor and friend to many, especially the young.

THE FOLLOWING PEOPLE HELPED me by providing details or reading the finished manuscript: Roger Beachy, Jane Burton, Joe Chappell, James Cooper, George Cheniae, Jack Hanson, David Ho, Hans Kende, Frank Salisbury, and Paul Saltman.

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PAUL ALFRED WEISS

March 21, 1898–September 8, 1989

BY JANE OVERTON

PAUL ALFRED WEISS was a gifted biologist who worked in the fields of growth, differentiation, and neurobiology over a period of five decades. The precision and breadth of his thought and elegance of his experimental design had a major influence on the development of these fields and on aspects of medicine as well. Some of his early views, which seemed innovative at the time, have become commonplace today. In addition, he took an active role in the affairs of scientific societies, where he promoted interaction between diverse disciplines.

Paul Weiss was born on March 21, 1898, in Vienna, Austria, the son of Carl S. Weiss, a successful businessman, and Rosalie Kohn Weiss. The major cultural interests of the family lay not in science but in music, poetry, and philosophy. An uncle stimulated young Paul's interest in Science. In 1916 Paul received his bachelor's degree and immediately entered the Austrian army, where he served for three years during World War I as an officer in the artillery.

At the end of the war Weiss began his university study at the Technische Hochschule in Vienna, having decided on a career in mechanical engineering. He soon shifted his interest to biology, where he was introduced to the newest results of Edmond B. Wilson, Edwin G. Concklin, and Theodor Bovari, which led to his continuing interest in dynamic causation in biology. He chose physics as his minor subject, and this, with his training in mathematics and engineering, had a strong effect on his later work in biology. Weiss's work was carried out under Hans Prizbram, director of the Biological Research Institute of the Academy of Sciences in Vienna. In his thesis, published in 1922, he studied responses of butterflies to light and gravity, arguing that the nervous system cannot be reduced to a rigid tropistic machine, but that the elementary steps in behavior are subordinated to the state of the whole, a view he extended later in studies of the vertebrate nervous system.

Weiss's nonscientific activities centered on music (he played the violin), sports, and travel. In 1926 he married Maria Helen Blaschka. After receiving his degree he worked in a number of European laboratories and traveled and lectured widely. Weiss later moved to the United States where career opportunities were greater. He became an American citizen in 1939. He taught first at the University of Chicago and later moved to Rockefeller University. In 1947 he was elected to the National Academy of Sciences.

During the period in which Weiss worked in Europe he initiated research in several areas that he was to return to later, most notably in cellular differentiation during regeneration, neural coordination of movement, and patterns of cell growth in tissue culture. In his studies of regeneration in newts he demonstrated that regeneration of the limb involved not merely the reformation of each tissue from the corresponding tissue of the stump, but differentiation of skeletal elements from non-skelatinous tissue, since a normal limb could form after removal of all skeletal components from the stump.

In subsequent work with the newt limb, Weiss was able to show that whole limbs could be transplanted and would become enervated and capable of movement. Movement of the grafted limb was fully coordinated and synchronized with the adjacent host limb, suggesting a relation between central and peripheral coordination. He tested this relation by muscle transplantation in mammals in the surgical clinic of Bier and, while at the Hungarian railway station there, obtained an adult frog found in nature with two supernumerary limbs in full functional condition, which moved in a manner that confirmed his experimental findings. The frog was featured in his European lectures and the idea of the "natural experiment" became a teaching device and later found its way into his text and teaching lectures.

Weiss's initial studies in tissue culture involved observation of the pattern of outgrowth of cells into the medium. He was able to modify the direction and intensity of outgrowth of chick heart fibroblasts by stretching the plasma clot in which cells were cultured over a small triangular frame. This produced three areas of preferential outgrowth with a marked orientation, indicating that fibroblasts responded to the orientation of the substrate.

In 1931 Weiss received a Sterling fellowship to work with Ross G. Harrison at Yale, where he continued his earlier tissue culture work, now applying it to the study of nerve growth. He obtained negative results in his study of effects of chemical attraction and electrical orientation, but was able to show that nerve fiber outgrowth, just like cell growth studied earlier, can be guided by the ultrastructure of the medium.

In 1933 he was offered a teaching position on the zoology faculty at the University of Chicago to replace Benjamin H. Willier, and in 1942 Weiss was promoted to professor of zoology. He remained at Chicago until 1954. During his twenty-one years of teaching and research at Chicago he was involved in investigation of many new areas in embryonic development and regeneration. However, the subject of neural organization and nerve outgrowth remained a central interest.

Weiss continued his studies of coordination of grafted limbs, comparing the movement of graft and control limbs using slow motion cinematography to identify the time of onset and duration of key muscles controlling each joint. Using a musical analogy, he recorded a "score" on a "staff" for each muscle; just as in orchestral music, the score records the onset and duration of each instrumental part. By this means he was able to show that activity of a grafted muscle corresponded exactly with that of the corresponding muscle of the host. Another important result of these studies was that, if movement of grafted limbs was unadaptive—for example, if reversed limbs caused an animal to approach rather than retreat from a noxious chemical—this behavior was never changed. These findings were consistent with the view that basic neural patterns of coordination were self-differentiating rather than learned.

Coordination of muscle activity was then studied in mammals. Tendons or nerves were crossed so that a flexor muscle, when activated, would have the effect of extending rather than flexing the joint. These studies confirmed the fixed patterns of reflexes, while demonstrating the much greater plasticity in higher vertebrates. Another natural experiment (in the form of a girl with three supernumerary fingers) was analyzed and confirmed earlier observations that homologous muscles have similar scores of activity. However, in this case, the pattern of simultaneous activity, which is fixed in lower vertebrates, could be modified by training, although the learned behavior failed when the girl's attention was diverted. At this time a study was undertaken of twenty-two patients with poliomyelitis in which healthy muscles were transplanted to replace paralyzed ones. A new technique of electromyographic determination of muscle action was developed to analyze these cases. Weiss's interpretation of these carefully analyzed studies of muscular coordination in a variety of systems was based on the assumption that the path of nerve outgrowth in development and regeneration was determined by orientation of the substrate, since he had shown that substrate orientation could be a decisive factor in tissue culture. Contrary evidence was discovered later by his postdoctoral student Roger Sperry, who went on to receive a Nobel Prize.

In further studies of nerve outgrowth Weiss grafted segments of neural cord and limb some distance apart in the gelatinous matrix of the newt dorsal fin. This provided satisfactory material for observing formation of neural connections between the two grafts, but also revealed the unexpected observation that the limb, when enervated, underwent continuous spontaneous convulsions, indicating a new finding, namely that spontaneous rhythmic discharges were a characteristic property of nerve pools. This was in contrast to prevalent views at the time. Studies of this kind, as well as many observations under a variety of experimental conditions of nerve growth in tissue culture in the embryo and in regeneration, led to the concept that in nerve formation pioneering fibers first grew out followed by application of later fibers, termed "fasciculation," and finally, upon reaching the end organ, by towing.

At this time Weiss was also concerned with a number of other problems related to morphogenesis, among them growth control of homologous organs and the dependence of cell patterns in cartilage on mechanical factors. In addition, he wrote his textbook *Principles of Development,* which was regarded as the leading text in the field.

With the entry of the United States into World War II he served as a member of the Conference on Peripheral Nerve Injuries of the National Research Council and took part in the planning and development of work related to repair of injured nerves, a major problem of war surgery. Under a program of the Office of Scientific Research he undertook a study of nerve growth and regeneration that might apply to nerve surgery and be of potential clinical value. New techniques of nerve repair were devised, the most successful of which was a sutureless one in which the two free ends of the severed nerve were united by an arterial sleeve that became attached to the nerve ends by clotted blood and lymph. In such preparations the erythrocytes degenerated, but the fibrin network persisted and became separated from the arterial sleeve by proteolysis, leaving a cylindrical clot subject to mild tension by the nerve. Tension oriented the fibrin and formed a straight bridge that the outgrowing axons followed, just as had been demonstrated a decade earlier by subjecting a fibrin clot in tissue culture to tension. Frozen dried and rehydrated arterial sleeves were also used with the view of developing tissue storage. Later, tantalum foil was used to form the sleeve. If heated under specific conditions, this normally pliable foil became elastic and could be formed into a coil of appropriate diameter. A patent was granted for this process. Surgical techniques developed in animal models were later extended to clinical application in selected Army and Navy hospitals.

The fundamental research that came from his laboratory during this same period included many aspects of nerve outgrowth and neural connections, but a major finding at this time was the demonstration that the outgrowth of neurons and their maintenance depended on axonal flow from the nucleated cell bodies in nerve centers. A pressure block (by suture, by passage into a fibrotic zone such as a scar, or by other means) caused piling up of the material of an intact axon on the nuclear side and reduction in diameter on the distal side of the constriction. With release of the constriction the characteristic conformation was regained. The pile up of axonal material formed complex nodules and spirals, which, when analyzed and reproduced in a mechanical model, showed that the effect could be produced by translational movement of the axon as a whole. Application of a second block distal to the first had the same result, indicating that the growth process was continuous.

During World War II Weiss concentrated on neurological problems exclusively, but, when the war was over, he returned to following up his interests in morphogenesis. He focused on cell-cell and cell-substrate contacts and developed a concept of molecular interaction, applied to problems of growth and morphogenesis. This scheme was termed "molecular ecology" and was presented in very general terms with diagrams of complementary geometric shapes between adjacent cell surfaces and between cell and substrate. There was widespread interest in this paper at the time.

Weiss designed a number of experiments consistent with his molecular hypothesis illustrating the specificity of cell behavior. In amphibian larvae grafts of epithelial sheets fused readily with adjacent tissue if the tissues were adjacent to each other normally; otherwise, the grafts were expelled. Chick embryonic pigment cells injected into early chick embryos by a vascular route persisted and matured only in regions that normally formed pigment. Cartilage cells of embryonic chick sclera and limb formed very different conformations and, when dispersed and allowed to reaggregate, each cell type formed patterns characteristic of the tissue of origin. In this group of experiments perhaps the most dramatic was the finding that, when organs from chick embryos in advanced stages of morphogenesis were dispersed into single cell suspensions, pelleted, and allowed to reorganize on the chick chorioallantoic membrane, remarkably complete organ structure of the same type was reconstituted. He also initiated studies of cell substrate contacts using an early tabletop electron microscope.

In 1954 plans to change the Rockefeller Institute to Rockefeller University drew him to New York; he became one of the university's first professors and directed a new laboratory specializing in wound healing, cancer, and development and repair of the nervous system. A major interest during this time was the further analysis of axonal flow. Numerous experiments were carried out, and involved whole animal studies and tissue culture using currently available techniques, including electron microscopy, phase contrast with cinematography, and isotopic labeling. Distribution of neurofilaments and microtubules within the axon were recorded, as well as detailed behavior of neurons in tissue culture. A particularly innovative approach was the injection of leucine-H 3 into the posterior chamber of the eye where it was confined by the sclera surrounding the eyeball, yet could bathe the optic ganglion cells and permit a quantitative study of axon labeling. Although involved with detailed quantitative studies in much of his work, Weiss never lost sight of the larger picture, stating that "the realization of the intensive and incessant activity of the neuron has an immediate bearing on the problem of specific qualitative adaptability of neurons such as underlies functional plasticity of the central nervous system as manifested in learning, habituation, acquired hypersensitivities, idiosyncrasies, addictions, and so forth."[1]

He remained an active faculty member at Rockefeller for the next fifteen years, alternating his research there with occasional interruptions to teach elsewhere around the world. He served as visiting professor at ten major universities and as dean of a new graduate school of biomedical sciences at the University of Texas. He was elected a member of the American Philosophical Society (1953) and the American Academy of Arts and Sciences (1954), and received many other honors and awards. In 1979 President Carter awarded him the National Medal of Science.

Weiss's close association with European laboratories in the 1920s and early 1930s was maintained, and this brought postdoctoral students and many visitors from Europe to his laboratories at Chicago and Rockefeller. After World War II he served on a Marshall Plan commission set up to revive scientific activity in Europe.

In 1930 he came to the United States because negotiations for a job at the University of Frankfurt were terminated due to the general financial collapse in Germany. Realizing that conditions for scientific work on the continent were deteriorating, Weiss decided to devote his energy to a country where the scientific spirit was in the ascendancy—the United States. Jane Oppenheimer reports that in 1950 Weiss was asked at an informal gathering why he worked so hard and was overheard to reply that his reason for wishing to be a good embryologist was that by doing so he might help repay the United States for what it had done for him.[2]

Paul Weiss was a gifted investigator and a stimulating and supportive teacher. His interest and enthusiasm for the subject was contagious. He put a high value on spacial organization in biological systems and emphasized its importance to students and colleagues alike. He felt that, "The complex engineering performances of technology are a much more pertinent model of the nature of morphogenesis than are the more elementary phenomena dealt with in basic physics and chemistry."[3] At a time when respiratory intermediates were still being discovered he deplored the fact that some biologists seemed to consider the cell a bag of enzymes. When demonstrating tissue culture procedures for students in the days before commercially available media, he personally prepared plasma and embryo extract. While doing so he invariably held up two tubes, one with intact embryos and the other with embryos after homogenization, and pointed out with delight that both tubes contained the same molecular components. He took a serious interest in teaching and in programs that integrated the various specialties in biology. He also put great emphasis on terminology. In his general embryology course he devoted several lectures to topics such as how the meaning of a given embryological term had changed over time or how a given term used currently by different investigators might have somewhat different meanings. Bernice Grafstein points out that many of his terms have become part of our scientific discourse. "Axonal flow," for example, although neither strictly axonal nor strictly flow still crystalizes a complex set of related ideas and is used today in cataloging. Other terms such as "neurobiology" and "developmental biology" have this same characteristic.[4] In referring to Weiss, James Ebert has noted that "his life has been a life devoted to improving science and its language."[5]

Weiss gave considerable time to the biological community and served as a member and officer of the Society for Development and Growth (president, 1939–41). He was president of the American Association for the Advancement of Science (1952–53), the Harvey Society (1962), and the International Society for Cell Biology (1962). He emphasized to his colleagues and students the importance of interaction between various biological disciplines. He wrote, "While scientific workers are more and more constrained into narrower and narrower confines in which to pursue their specialties, science as a whole cannot develop into a healthy and proportionate organism unless specialists will leave their burrows on periodic occasions and meet on common ground."[6]

In the case of the Society for Development and Growth (now the Society for Developmental Biology), he was strongly instrumental in designating the society, and more important the discipline, as developmental biology and not plant and animal embryology. The study of bacteria, fungi, and viruses were promptly incorporated into the new discipline, which played an important role in promoting the later development of molecular genetics as an integral part of the study of development. [7]

I HAVE DRAWN ON background material provided by the National Academy of Sciences and on part of Weiss's extensive publications. He republished more than fifty of his review articles that he considered scattered in less accessible locations in book form, *Dynamics of Development* (1968) and *Biomedical Excursions* (1971). His text book *Analysis of Development* was reprinted in facsimile in 1969.

NOTES

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