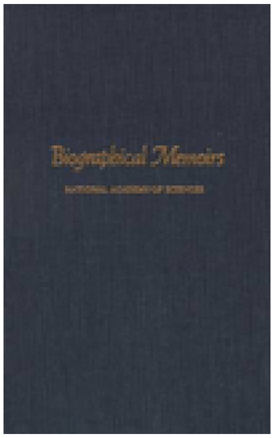

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Biographical Memoirs

VOLUME 64

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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 advixe 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.

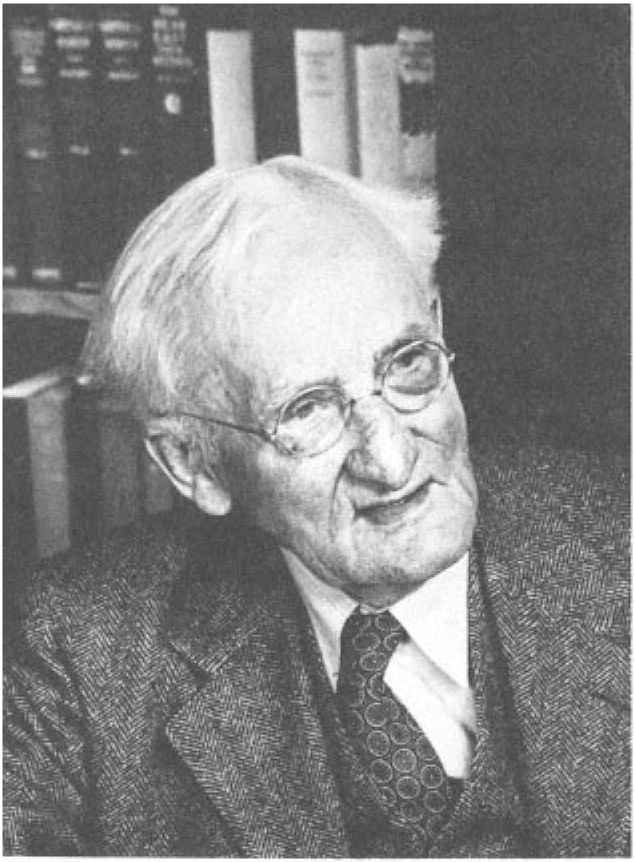
The Biographical memoirs, begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work of our most distinguished leaders in the sciences, and witnessed and interpreted by their colleagues and peers. They form a biographical history of science in America—an important part of our nation's contribution to the intellectual heritage of the world.

PETER H. RAVEN

Home Secretary

Biographical Memoirs

VOLUME 64



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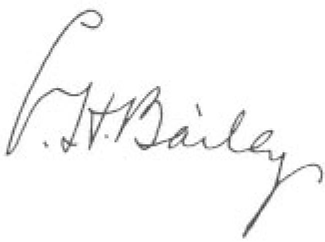

LIBERTY HYDE BAILEY

March 15, 1858-December 25, 1954

BY HARLAN P. BANKS

ON NOVEMBER 5, 1990, the American Society for Horticultural Science initiated a Hall of Fame designed to "honor distinguished persons who have made monumental and unique contributions to horticulture." Only two scientists were inducted at the initiation—Gregor Mendel, the Austrian monk who solved the riddle of heredity, and Liberty Hyde Bailey.

The career of Liberty Hyde Bailey—botanist; horticulturalist; plant breeder; teacher par excellence; visionary; astute, vigorous, successful administrator; lobbyist; prolific writer; superb editor; poet; rural sociologist; philosopher; environmentalist; traveler; and plant explorer—was remarkable for the magnitude of its accomplishments and the breadth and enduring quality of its influence. Bailey made his mark in botany with extensive publications on the systematics of sedges (*Carex*), palms of the new world tropics, blackberries (*Rubus*), grapes (*Vitis*), cabbages (*Brassica*), and pumpkins and squashes (*Curcurbita*), among others.

As author, editor, teacher, and frequent public speaker, Bailey helped create the science of horticulture. As an administrator, he established the New York State College of Agriculture at Cornell University, drawing on his skills as a lobbyist, and then, as dean, built it into an institution of world renown. He labored mightily to improve the image and climate of rural life in the United States. His lifelong concern for the environment was summarized in his book *The Holy Earth*, which long antedated today's tardy recognition of the vital significance of its protection. To all this, Bailey added the writing of poetry and frequent philosophical musings. It is one measure of the man that, recently, thirty-five years after his death, the American Horticultural Society held a symposium, "A Salute to Liberty Hyde Bailey," in his honor. By any measure, Bailey was a man of incomparable vision and prodigious energy.

Liberty Hyde Bailey was born on March 15, 1858, in South Haven, Michigan. His father, Liberty senior, had migrated from Vermont in his twenties and married Sarah Harrison in this new community carved out of the wilderness near the eastern shore of Lake Michigan. Bailey senior proved to be an exceptional orchardist as he assembled a large variety of apples, numbering over 300 cultivars. He became one of the most respected members of his community, which included the Indians whom he permitted to continue to occupy a portion of the land he had acquired.

Young Liberty grew up in this rural atmosphere, gardened with his mother, who died when he was but five years old, and reveled in the joys of natural history as he roamed the streams and woods learning the habits of both plants and animals. One of the most striking of his experiences with wildlife was witnessing the extermination of the carrier pigeon. An industrious youth, by his early teens Bailey had become expert at grafting. Many farmers planted apple seeds because of the expense of buying named seedlings. This necessitated grafting cuttings on to the trees in order to obtain desired varieties. Young Bailey was soon in demand to do the necessary grafting in neighbors' orchards. He, himself, had one tree on which he had grafted forty varieties.

Bailey's grade school teacher, Mrs. Julia Fields, cognizant of his learning skills, challenged him by suggesting that he was growing up blind. When he protested that he was continually observing, she asked him the names of the various trees, the height they reached, and how they grew. These questions proved a remarkable stimulus. For example, when Bailey found a copy of Darwin's *Origin of Species,* he read it with care, marveling at Darwin's knowledge but wondering at the meaning of *a priori*. *H*is teacher then agreed to teach him a little Latin, an experience that only one other classmate was willing to share.

Bailey's first public speech was an enthusiastic talk on grafting at the South Haven Pomological Society. Subsequently, he gave a paper on birds in which he made a plea for cessation of their wanton slaughter. Soon, at age fifteen, he was invited to repeat that paper at the State Pomological Society, and it was published in the society's annual report for 1873.

During his teens, Bailey saw the squeeze to which farmers were subjected, as low prices for their products were accompanied by high prices for the newly evolving mechanical equipment and for shipping by rail. He saw rural youth migrating to the cities and foreclosures on farms. He was further influenced by the Grange, as it fought for better roads, broader educational opportunities, equality for women, and dignity for rural life. He became secretary of the local Democratic committee. These influences are all reflected in the actions of his later life.

At age sixteen Bailey found a copy of Asa Gray's *Field*, *Forest and Garden Botany*, which he used avidly to identify plants. Lucy Millington, a botanist, arrived in South Haven and soon began helping Bailey identify difficult plants, but with the challenge that the sedges (*Carex*) were too difficult for a beginner. Naturally that remark resulted in his following through to eventual technical monographs of the

Carices. At age eighteen he invited Professor William James Beal of the Michigan Agricultural College to lecture at South Haven in the hope that he might learn more about plants. The meeting was a success. Beal told Bailey about his own mentor, Asa Gray, about Louis Agassiz, and about current developments in botany. The result was Bailey's entrance to the college in September 1878 at age twenty. It is said that on the way to college Bailey outlined his goal in life— to spend twenty-five years in preparation, twenty-five in earning a livelihood, and twenty-five in using his abilities as he chose—a goal he approached closely, although he outlived the final third by more than two decades.

On his first day in college Bailey met Annette Smith, destined to become his bride a few years later. An early paper submitted to his professor of English elicited the comment, "That boy will either be a great man or he won't amount to shucks!"[1] Bailey excelled in college and flourished under the tutelage of Dr. Beal, who taught botany with a new experimental approach using living plants and laboratory work rather than with the then-dry textbooks. In his senior year he helped organize and edit *The College Speculum*, a quarterly paper established to provide both scientific and general reading. He wrote many of its articles himself. Graduating in August 1882, Bailey promptly secured a post as reporter on the Springfield, Illinois, *Morning Monitor*. Within a few months he was offered the post of city editor. Then fate intervened. Asa Gray, then America's leading botanist, needed an assistant and Beal recommended Bailey. By February 1883 Bailey was working in Cambridge, arranging and classifying a large collection of pressed plants from Kew. He was to make a set for the Missouri Botanical Garden, one for the National Museum, and a third could be his. By June of that year he had married Annette Smith, ending their five-year courtship. They set up residence in

Cambridge, and Bailey continued to learn from long discussions with Gray about systematic and structural botany. He profited greatly from William Gilson Farlow's cryptogamic botany and George Lincoln Goodale's physiological approach to botany. No less significant to his training were the vast collections at the Arnold Arboretum, the Cambridge Botanical Garden, the greenhouses and scientific agricultural work of the Bussey Institution, and the noted market gardens in nearby Arlington.

Late in 1884 Bailey was offered a professorship of horticulture at Michigan Agricultural College. Gray told him that a botanist is a scientist, an intellectual, whereas a horticulturist is merely a gardener, a practical man. John Merle Coulter, a fellow student and later an outstanding botanist at the University of Chicago, told Bailey he would never be heard from again if he took the position. Nonetheless, Bailey accepted the offer. The point missed by Gray and Coulter was that Bailey did not leave botany. Rather, he joined forces with the study of cultivated plants and, in the end, removed the barriers between theoretical botany and horticulture, as he rose to the peak of recognition in both pure botany and the applied plant sciences.

In early 1885 Bailey started working at Michigan Agricultural College at age twenty-six. His success was immediate. Students flocked to his classes. He brought pumpkin vines to lecture to illustrate huge fruits on small plants and then pumpkin seeds to stress the small beginnings. He taught physiology graphically, as when he proclaimed that placing fertilizer close to the trunk of a tree was comparable to tying a bag of oats to a horse's leg. A lecture at the Massachusetts State Board of Agriculture expressed one of his major objectives clearly. It was entitled *The Garden Fence*, *by* which he meant the wall of prejudice that separated botanist and horticulturist. Bailey insisted that each needed the work of the other. By the end of his first year at M.A.C. he had published his first book, *Talks Afield*: *About Plants and the Science of Plants*. The following year saw publication of another book and a 100-page article on North American Carices, his seventh article on *Carex*. The latter appeared in the *Journal of the American Academy of Arts and Sciences*. Soon he was in constant demand as a speaker all over the state of Michigan. Meanwhile, his research on the hybridizing of various plants was continuing actively. A highlight in his career was a visit to the college by Alfred Russell Wallace, who was left in Bailey's charge for the duration of his stay. Bailey had read all of Wallace's papers and the two enjoyed a profitable exchange of ideas as Bailey introduced him to the flora of Michigan.

Toward the end of 1887, Bailey was invited to give a series of lectures at Cornell University. This resulted in 1888 in an offer of a professorship of horticulture, with freedom to develop the field as he envisioned it and some support funds. He was also granted a trip to Europe to study various departments of horticulture and the important European herbaria prior to starting at Cornell.

On his return from Europe in early 1889 Bailey joined Dean Isaac P. Roberts to initiate an outstanding team for teaching, research, and dissemination of knowledge about agriculture. Bailey continued his cross-breeding experiments, his inspired teaching, and pioneering new experiments such as growing plants under electric lights because, while in Cambridge with Gray, he had observed differences in the behavior of plants that were growing near gas lamps on the streets. His paper, "Some Preliminary Studies of the Influence of the Electric Arc Lamp upon Greenhouse Plants," published in 1901, has been selected as a classic paper in horticultural science. He also worked on the effect of enhanced levels of CO_2 around greenhouse plants and on the physiology of seed germination. In 1892 he published the first American book on controlled experimental breeding *Cross Breeding and Hybridizing*. In this book he cited Gregor Mendel's paper. Later, Hugo DeVries wrote Bailey, giving credit to this citation for his memorable rediscovery of Mendel's work on peas. In 1893 he published the first detailed study of the growth of plants under artificial light.

Between 1889 and 1896 half of the Cornell University Experiment Station bulletins were written by Bailey. Bailey's writing skill so impressed George P. Brett, president of Macmillan and Co., that he told Bailey to send along the title whenever he had a book under way because Macmillan would publish anything he wrote. Books from his pen kept appearing, eleven of them between 1896 and 1901. All of Bailey's books sold well. Some went through twenty editions and were still selling thirty years after their first publication. It is noteworthy that all of Bailey's writing was done in longhand and that only rarely were any changes in the first draft required. It is also said that he could be interrupted in midsentence and two days later pick up his pen and effortlessly finish the sentence.

In teaching, one of Bailey's most popular courses was the evolution of cultivated plants. He summarized his views in *The Survival of the Unlike* (1896), in which he pointed out that modifications by horticulturists support the theory of evolution. He felt that all life stemmed from one beginning and that an evolutionist can believe in God because in the beginning there was only God. Evolution, he felt, does not attempt to explain the origin of time, space, matter, or force. As for landscape gardening, he taught from the viewpoint of creating a picture, and for him that meant natural form, not heavily pruned, formal shapes. His efforts along these lines resulted in the award of the Royal Horticultural Society of London's Veitch Memorial Silver Medal in 1897.

As early as 1893 Bailey began making impassioned pleas for state-supported agricultural education at the university, where, among the faculty, it was regarded as "cow college stuff." Among some farmers as well it was still scorned as the ideas of those "smart college boys." However, Bailey and Roberts built up support for the new concept by providing bulletins, lectures, demonstrations, farming institutes, and even visits to farmers' homes. They listened to farmers' problems and provided valuable solutions. This gradually endeared them to their constituents on whom later they could rely for help when trying to initiate a state college of agriculture. All of this activity came eventually to be known as the Extension Program as more and more farmers realized the value of the help that was being provided. In 1894 some fruit growers pushed through the legislature a bill directing the state of New York to provide the Cornell University Experiment Station with $8,000 to conduct research on orchards in western New York. Thus, the principle of state aid was begun. By 1897 the appropriation reached $25,000. This broadening of the influence of the college had convinced the university trustees by 1896 to officially change the name of the university's Department of Agriculture to the College of Agriculture.

At some early stage, Bailey conceived the idea of a series of graded texts dealing with plants and nature, books that would attract and hold the interest of people of any age. His *Lessons with Plants* (1898) was followed by an elementary school book in 1890, a beginner's book in 1908, and a secondary school text in 1913. But Bailey wanted more than good books. He was concerned with the attitude and training of teachers. He wanted to see the whole broad concept of nature study presented in a way that would bring students into harmony with nature. (See *The Nature Study Idea* [1903], where, on p. 159, Bailey answered the query, Should I take up nature study teaching, by saying, "Yes, if you feel the 'call' to it; otherwise no. I would not have every teacher teach nature study any more than I would have everyone teach grammar.") Bailey wanted students to be inspired by teachers who were overflowing with enthusiasm for the subject. During the first decade of the 1900s, he developed an extensive rural nature study program, guided by himself and a group of dedicated collaborators. Their rural school leaflets reached several thousand teachers and 30,000 actively participating students. For many years this program exerted a massive impact on teaching in the state.

During the late 1890s, Bailey also had been working on the compilation of a four-volume *Cyclopedia of American Horticulture*. This massive work appeared in 1900, to be followed in 1909 by the *Cyclopedia of American Agriculture*. As if all this were not enough, Bailey had become the first editor of the highly successful journal, *Country Life in America*, and of two book series for Macmillan—the Garden Craft Series and the Rural Science Series.

By 1900 it was clear that the College of Agriculture must have state support to erect the necessary buildings. Early in 1903 Roberts sent Bailey to Albany to secure support for a large agriculture building. This effort failed in 1903, but by rallying support from all over the state Bailey succeeded in 1904. Roberts had retired in 1903, to be replaced by Bailey, so it was as dean that Bailey's lobbying skills produced the necessary votes to win over the legislature. And he won not merely the new building but, over strong opposition from other schools, the establishment of the State College of Agriculture at Cornell University as well. This meant a new state policy of ownership and maintenance by the state but administration solely by the university. Following dedication of the new college and building by then-governor Charles Evans Hughes in April 1907, Bailey set about expanding the faculty, adding new departments, and stimulating aggressive research and inspired teaching.

At his twenty-fifth wedding anniversary on June 6, 1908, the entire faculty gave Bailey a surprise party for which they produced a scroll reading: "Sympathetic, open-minded, always fair, you have ever been keen as an investigator, inspiring as a teacher, lecturer, and author, resourceful as an editor, masterful as an administrator."[2] They also presented the Baileys with a candelabrum of five lights, one for his literary accomplishment, one for his skill as an educator, one representing his administrative ability, one for his investigative skill, and one for the warmth with which he received faculty and students into his home.

In 1907 Michigan Agricultural College celebrated its semicentennial with Bailey as its major speaker. In his address Bailey gave his views on agricultural and country life. Agriculture at that time was suffering from an economic crisis, and he suggested ways to alleviate the situation. President Theodore Roosevelt was one of the eager listeners. A year later, in 1908, he asked Bailey to chair a national commission on country life to survey the whole field of rural life in America and to suggest cures for its deficiencies. The commission sent out thousands of questionnaires and then made a giant swing around the country holding open hearings and meetings with rural leaders. Bailey wrote the report that President Roosevelt submitted to Congress in February 1909. Sometime after 1909 Roosevelt wrote, "I doubt if I should have undertaken to appoint the commission if I had not been able to get Director Bailey for its head, and no man in our country did better work for the country than he did on that commission."[3] The work of the commission led to legislation establishing a U.S. Parcel Post system, a nationwide federal extension service, and the federally supported rural electrification program.

Bailey's wide-ranging activities did not obscure his technical, botanical, and horticultural skills. He became the acknowledged authority on the genus *Carex* by publishing twenty taxonomic papers between 1886 and 1905, one of them covering all species from Greenland to Alaska and south to Panama. He, more than any other, elevated horticulture from a craft to a science. He made botany the basis of sound horticultural research, teaching, and practice. His leadership extended farther. In 1892 he urged the foundation of a national society for the study of the pure science of botany. His genius for organization and his wise and tactful leadership energized the movement by which, in 1893, the first Botanical Society of America was founded. This society was enlarged in 1906 by the addition of two other botanical groups, and 1906 is now given as the date of the founding of the modern prestigious Botanical Society of America. In 1903 Bailey and S. A. Beach founded the American Society for Horticultural Science. Bailey served as its president during its first four years. His work and its influence were rewarded in 1900 by his election to the American Academy of Arts and Sciences.

In 1910 and again in 1912 Bailey was urged to run for governor of New York. But his mind was on retirement and he stated so at a party in his honor at the Cornell Club in New York City. So many persons present paid him tribute that he remarked, "I know now how the pancake feels when the molasses is poured over it."[4] In spring 1913 he submitted his formal resignation as dean, to be effective July 31. His ten years as dean had seen his department heads become leaders in their fields, the faculty increase from 11 to over 100, the student body from 100 to 1,400, the state appropriation from $50,000 for one building to millions for a dozen buildings, and the annual budget from $35,000 to $500,000.

Following retirement Bailey converted the carriage house at his home to a herbarium, where he worked vigorously for another thirty-five years. A first project, revision of the *Cyclopedia of American Horticulture*, caused him to make several trips to the American tropics to observe native palms. He soon realized that here was a vast field in need of work. Thus, he came to devote a large fraction of his time to the systematics of palms. The search took him to Mexico, Brazil, New Zealand, China, Southern Asia, the West Indies, Florida, and Southern California. (When he fell and broke his hip in New York City in 1949, he had a ticket to Africa in his pocket. He had intended to study oil palms, a project that never came to pass.) The revised *Cyclopedia* came out between 1914 and 1917 as the *Standard Cyclopedia of Horticulture*; as for the palms, Bailey increased the 700 species known when he began the study to several thousand by 1947. This work produced wholly new techniques for the collection of palm specimens, one of the best herbaria of palms anywhere, and some forty-five papers between 1930 and 1947. Along with the palms, Bailey produced a 1,000-page monograph of *Rubus* and extensive revisions of *Brassica*, *Vitis*, and *Curcurbita*. To these works he added valuable contributions to the taxonomy of cultivated plants such as *Hortus*, *Manual of Cultivated Plants*, *The Garden of Pinks*, *The Garden of Bellflowers*, and other books.

During a trip to New Zealand in 1914 to deliver a series of lectures, Bailey wrote *The Holy Earth*, a book about man's debt to the earth and earth's goodness to man. Another trip took him to China in 1917. One of his projects on this trip was the search for prototypes of cabbages and their relatives.

In 1935, at age seventy-five, in order to provide continuity for his life's work, Bailey gave his herbarium (125,000 sheets), the building housing it, and his library (some 3,000 books) to Cornell University, specifying that the complex be called the Liberty Hyde Bailey Hortorium. The university trustees constituted the hortorium a department of the university, with Bailey as its unsalaried director, daughter Ethel as curator, and Dr. R. T. Clausen as a research taxonomist. Bailey coined the name hortorium to refer to a place for the scientific study of cultivated plants. His objective was to bring order to the nomenclature of agricultural and horticultural plants. This involved growing novelties as soon as they were introduced into the trade and adding specimens to the herbarium as well. Bailey regularly grew between 2 and 800 such a year. One example of his procedure is found in his first paper in *Gentes Herbarum*, a journal initiated and supported financially solely by him. In this paper, *Plantae Chinenses* (1920), he described twenty new species in thirteen genera and fifteen new varieties and forms, some wild, some cultivated. This orderly treatment of the names of cultivated plants was regarded by Bailey as perhaps his most significant contribution to plant sciences. Bailey continued as director of the hortorium until 1951, when George H. M. Lawrence took over and saw the move of the hortorium from Bailey's home to the then newly built Agricultural College Library, where it now resides as a department of the Division of Biological Sciences supported by the New York State College of Agriculture and Life Sciences. Its present faculty and staff number eighteen (1990), exclusive of graduate students, postdoctoral persons, and visitors.

Bailey was recognized throughout his career by the world scientific community, as his extensive list of honors shows. He was elected to the National Academy of Sciences in 1917, but the year 1926 can be considered the pinnacle of his recognition. Already president or honorary fellow of many horticultural organizations and cofounder of others, Society of America and the American Association for the Advancement of Science. To those high honors the Fourth International Botanical Congress, held in Ithaca in August 1926, added another. He was made president and presiding chairman of the congress. The only other American who had received this worldwide recognition was his early mentor Asa Gray. A year later, in 1927, the renowned publication of Kew Gardens and the Royal Horticultural Society of England, Curtis's *Botanical Magazine*, dedicated volume 153 to Bailey "in recognition of his long devotion to the scientific training of workers in horticulture and agriculture and to the increase and spread of knowledge in these branches of science."[5]

Bailey's long list of honors, however, is remarkable less for its great length than for the breadth of the fields of study represented in it. Few scientists have equaled this record.

Bailey's writing, which spanned eighty-one years, is almost incredible. Between 1890 and 1940 he edited for Macmillan 117 titles by 99 authors covering subjects in agronomy, economics, botany, pomology, animal husbandry, dairy industry, soils and fertilizers, plant pathology, commercial floriculture, and home economics. Carol Woodward, editor of Outdoor Books for Macmillan, wrote that no English writer "has had that refined combination of botanical and horticultural knowledge that Bailey made his own."[6] Bailey himself wrote some sixty-five books and a large number of the individual items in the several large encyclopedias that he edited. His successor as director of the Bailey Hortorium, George H. M. Lawrence, estimated that he wrote at least 1,300 articles published in the world's periodical literature and over 100 papers in pure taxonomy. Curtis Page, husband of Bailey's granddaughter, has written that

Bailey may have been most proud of a series of books referred to as the Background Books. Bailey called these books his budget of opinions. The first was *The Holy Earth*, referred to earlier. Others were *Wind and Weather* (a collection of his poems), *Universal Service*, *What Is Democracy ?*, and *The Seven Stars*. They illustrate his humanism, his search for social good, his respect for others, his philosophy of life. He wrote in *The Holy Earth*: "It is good to live. We talk of death and of lifelessness, but we know only of life. Even our prophecies of death are prophecies of more life. We know no better world: whatever else there may be is of things hoped for, not of things seen."[7] That Bailey treated the plant sciences as a means for the betterment of mankind is shown by a piece that he wrote when still a student at Michigan Agricultural College: "It was not until scientific education began to manifest itself that agriculture began its ascent from the slough of contempt in which it lay."[8]

Surely it is a remarkable tribute to a man that words he penned seventy-five years ago in *The Holy Earth* should at last be creeping into the thoughts and actions of a steadily increasing number of environmentally concerned citizens in North America in the 1980s. He wrote: "If the earth is holy, then the things that grow out of the earth are also holy. They do not belong to man to do with them as he will. Dominion does not carry personal ownership. There are many generations of folk yet to come after us, who will have equal right with us to the products of the globe. It would seem that a divine obligation rests on every soul. Are we to make righteous use of the vast accumulation of knowledge of the planet? If so, we must have a new formulation. The partition of the earth among the millions who live on it is necessarily a question of morals; and a society that is founded on an unmoral partition and use cannot itself be righteous and whole."[9]

On March 15, 1958, the Bailey Hortorium staff sponsored a Bailey centennial. Nine distinguished speakers, whose talks were published in *Baileya* (vol. 6), paid tribute to his accomplishments in various disciplines. In addition, the U.S. Postal Service issued a three-cent commemorative stamp inscribed with the words "Gardening Horticulture." The first-day covers carried a photograph of Bailey with his signature and the words "Educator—Horticulturist—Author 1858-1958."

Olaf Larson spoke at the centennial on Bailey's impact on rural sociology, using an excerpt from one of Bailey's early talks to illustrate his thoughtful concern for people: "While the College of Agriculture is concerned directly with increasing the producing power of land, its activities cannot be limited narrowly to this field. It must stand bravely for rural civilization. It must include within its activities such a range of subjects as will enable it to develop an entire philosophy or scheme of rural life . . . the colleges of agriculture have three proper lines of work: the regular, or ordinary, teaching; the discovery of truth, or research; the extending of their work to all the people."[10] Larson indicated that historians of rural sociology credit the Country Life Commission, chaired by Bailey, as a major influence in securing support for the "development of rural sociology as a discipline worthy of public support."[11] Larson also included Bailey among the foundation builders of agricultural economics because, very early, he advocated the survey method to obtain necessary facts about economic and social conditions. Another speaker, H. B. Tukey, lauded Bailey's breadth of influence. He wrote, "Taxonomic botanists thought of him as an authority on classification. Nurserymen considered him one of their own—an authority on ornamental plants and plant propagation. Fruit culture thought of him as a fruit man. Olericulture considered him a vegetable man. Amateur horticulturists thought of him as their special leader. Agricultural administrators thought of him as a dean. People in South Haven, Michigan, think of him as their son. People in East Lansing, Michigan, name schools and streets after him and his portrait is kept flood-lighted day and night in the Horticulture building at Michigan State University. He was, besides this, an editor, rural sociologist, poet, and philosopher. In short, his interests were so great and his coverage so broad that he stood as a dozen men—helpful, interested, and a marvelous friend to all."[12]

Bailey was an indefatigable public speaker. It should be obvious that any man invited so constantly to give talks must have been eloquent. The following, related by H. B. Tukey, should prove the point. Bailey arrived late at the annual banquet of the American Society for Horticultural Science in Cleveland in 1930. "The main course had just been served. As he entered, everyone rose. He was called to the speakers' table to make a few remarks. He began gently, but grew more eloquent and meaningful as he proceeded. All sat entranced. The talk continued for upwards of an hour. No one objected-food for the mind was more important than food for the body. The dinner got cold. The waiters stood by in amazement. The management fumed. But there were no interruptions. When it was all over, one waiter, shaking his head in disbelief, was heard to mutter to himself, 'Never, never, I see nothing like this before.'"[13]

One often hears the question, "How could Bailey accomplish all that he did?" Of course, his own enormous capacity for work, his unflagging enthusiasm for all that relates to plants and for its transmission to others, his constant flood of new ideas, his steadfast adherence to his ideals, and his orderly handling of each idea from its conception to its final presentation in written form were crucial. But

Gould Colman, Cornell University archivist, suggested to me that equally valuable, particularly when he was dean, was Bailey's masterly delegation of authority and, more important, his ability to instruct the helpers in the precise manner in which the chore should be performed. The remaining members of the hortorium staff who worked under him testify to his prodigious memory and to his habit of constantly recording ideas, short notes, or longer paragraphs on whatever scrap of paper was available so that nothing was lost. Would that more of us were so gifted.

Liberty Hyde Bailey died on December 25, 1954. Mrs. Bailey had died in 1938. The Baileys had two children, Sara Bailey Sailor and Ethel Zoe Bailey. Ethel's career was as co-worker with her father whom she accompanied on numerous collecting expeditions. She played a substantial role in the production of the *Standard Cyclopedia of Horticulture* and the *Manual of Cultivated Plants*. She coauthored *Hortus*, edited the first eight volumes of *Gentes Herbarum*, and served as curator of the hortorium from its inception in 1935 until retirement in 1957. Subsequently, she voluntarily continued her monumental index to the world's cultivated plants almost until her death in 1983.

NOTES

1. Harold B. Tukey, "Liberty Hyde Bailey's Impact on Plant Sciences," *Baileya*, 6(1958):59.
2. Philip Dorf, *Liberty Hyde Bailey*, *An Informal Biography* (Ithaca, N.Y.: Cornell University Press, 1956).
3. *The Cornell Countrymen*, 11(Dec. 1913):88.
4. Dorf, op. cit., p. 159.
5. Andrew Denny Rodgers III, *Liberty Hyde Bailey*: *A Story of American Plant Sciences* (New York: Hafner Publishing Co., 1965):462.
6. Carol H. Woodward, 'The Influence of the Horticultural Writings of Liberty Hyde Bailey," *Baileya*, 6(1958):201.
7. Liberty Hyde Bailey, *The Holy Earth* (Ithaca, N.Y.: Comstock Publishing Co., 1915):7.

8. Tukey, op. cit., p. 67.

9. Bailey, op. cit., p. 16.

10. Olaf F. Larson, "Liberty Hyde Bailey's Impact on Rural Life," *Baileya*, 6(1958):15.

11. Ibid., p. 21.

12. Tukey, op. cit., p. 60.

13. Tukey, op. cit., pp. 65, 66.

HONORS AND DISTINCTIONS

1882	Bachelor of Science, Michigan Agricultural College
1885	Marshall P. Wilder Bronze Medal, American Pomological Society
1886	Honorary Master of Science, Michigan Agricultural College
1893	One of five founding members of Botanical Society of America
1896	Veitch Silver Medal, Royal Horticultural Society, London
1898	Diploma of Honor, Royal Botanic Gardens, Denmark
1900	Member, American Academy of Arts and Sciences
1902	Honorary member, Rhode Island Horticultural Society; honorary member, American Scenic and Historic Preservation Society
1903	Founding member, American Society for Horticultural Science; president, 1903-07
1906	President, American Association of Agricultural Colleges and Experiment Stations
1907	Honorary LL.D., University of Wisconsin
1908	Honorary LL.D., Alfred University, New York
1909	Honorary member, Philadelphia College of Pharmacy
1910	Honorary member, Horticultural Society of Norway
1914	President, American Nature-Study Society. Reelected in 1915; honorary member, Horticultural Society, New Zealand
1917	Member, National Academy of Sciences; honorary member, Japan Agricultural Society; honorary member, Horticultural Society of Japan; honorary member, Horticultural Society of China; president, American Pomological Society, two-year term
1919	Honorary Litt.D., University of Vermont
1921	Marshall P. Wilder Silver Medal, American Pomological Society; honorary member, Phi Beta Kappa, Cornell University
1923	Diploma of honor, Reale Academia di Agricultura di Torino (Italy)
1924	Honorary member, Pi Alpha Xi honorary floriculture society
1925	Honorary life member, American Rose Society
1926	President, IVth International Botanical Congress, Ithaca; president, Botanical Society of America; president, American Association for the Advancement of Science
1927	Veitch Gold Medal, Royal Horticultural Society, London;

	George Robert White Gold Medal, Massachusetts Horticultural Society
1928	Grande Médaille d'Isidore Geoffroy Saint-Hilaire, Société Nationale d'Acclimation de France; honorary fellow, Royal Irish Academy of Dublin
1931	Gold Medal, Garden Club of America; Arthur Hoyt Scott Gold Medal and Award, Swarthmore College; Distinguished Service Award, National Home Planning Bureau of the American Association of Nurserymen; president, American Country Life Association
1932	Corresponding member, Academy of Natural Sciences of Philadelphia; honorary D.Sc., University of Puerto Rico
1933	Honor Certificate for Distinguished Service, Epsilon Sigma Phi, national honorary extension fraternity
1937	Distinguished Service Ruby, Epsilon Sigma Phi fraternity; honorary member, Société Lyonnaise d'Horticulture
1938	Silver Medal, National 4-H Club Congress
1939	President, American Society of Plant Taxonomists
1940	Fellow, Cactus and Succulent Society of America
1945	Honorary fellow, Botanical Society of Edinburgh; honorary member, Linnaean Society of London
1946	Award of Honor, Ministeria Agricultura y Cria, Caracas, Venezuela
1947	Gold Medal, "The L. H. Bailey Award," National Garden Institute, Chicago; Marshall P. Wilder Silver Medal, American Pomological Society; Gold Medal, National Institute of Social Sciences
1948	Johnny Appleseed Bronze Medal and Certificate of Recognition, Men's Garden Club of America; Silver Medal "Green Thumb Award," National Victory Garden Institute, Washington, D.C.; National Award Scroll, American Agricultural Editors' Association; Bronze Medal, Exposition of Women's Art and Industries
1949	Honorary member, Vegetable Growers' Association of America; Gold Medal, National Council of State Garden Clubs
1950	Illuminated Testimonial Certificate, for seventy-five years of continuous service and contribution to horticulture, American Association of Nurserymen
1951	Citation for Distinguished Service, Garden Club Federation

	of Pennsylvania; Gold Medal, Federated Garden Clubs of New York
1952	Honorary member, Long Island Horticultural Society, New York; Distinguished Service Award, New York Botanical Garden
1954	Bronze Centennary Medal, Société Botanique de France

The abundant literature on Liberty Hyde Bailey includes two books, that by Philip Dorf, which is more personal and intimate, and that by Andrew Rodgers, treating Bailey in the context of his contemporaries in science. The major sources used in the preparation of this biographical memoir are listed below. The writer especially wishes to thank Professor Emeritus John G. Seeley, Department of Floriculture and Ornamental Horticulture; Professor David M. Bates, Professor Emeritus William J. Dress, and Louella Sullivan of the Bailey Hortorium; and Gould Colman, Cornell University archivist, Department of Manuscripts and University Archives.

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Munz, Philip A. 1958. The influence of Liberty Hyde Bailey on botany. *Baileya* 6:85-89.

Darrow, George M. 1958. The influence of Liberty Hyde Bailey on horticulture. *Baileya* 6:101-6.

Page, Curtis C. 1958. Liberty Hyde Bailey, the humanist. *Baileya* 6:111-16.

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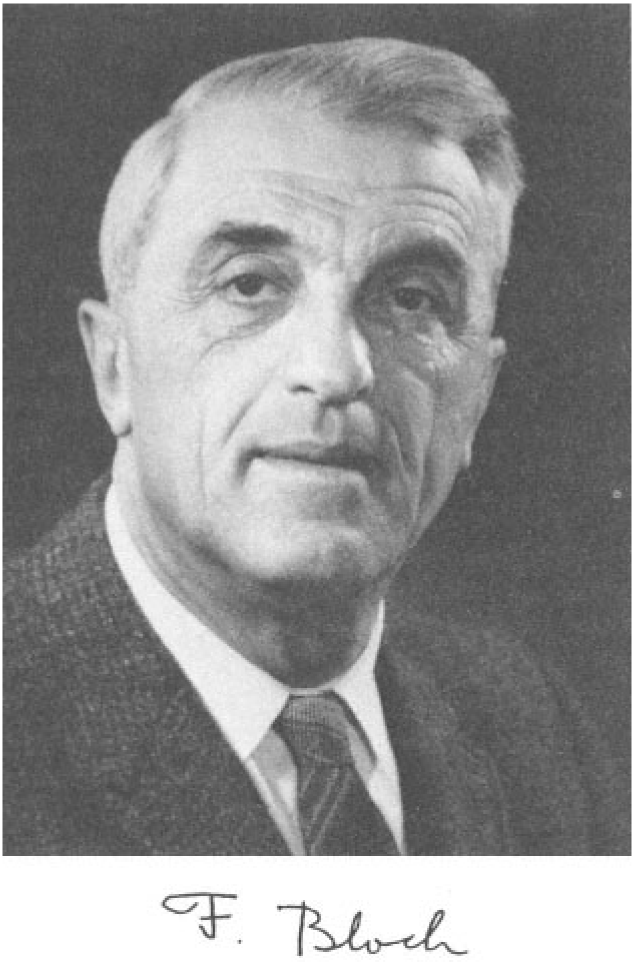

FELIX BLOCH

October 23, 1905-September 10, 1983

BY ROBERT HOFSTADTER

FELIX BLOCH was a historic figure in the development of physics in the twentieth century. He was one among the great innovators who first showed that quantum mechanics was a valid instrument for understanding many physical phenomena for which there had been no previous explanation. Among many contributions were his pioneering efforts in the quantum theory of metals and solids, which resulted in what are called "Bloch Waves" or "Bloch States" and, later, "Bloch Walls," which separate magnetic domains in ferromagnetic materials. His name is associated with the famous Bethe-Bloch formula, which describes the stopping of charged particles in matter. The theory of "Spin Waves" was also developed by Bloch. His early work on the magnetic scattering of neutrons led to his famous experiment with Alvarez that determined the magnetic moment of the neutron. In carrying out this resonance experiment, Bloch realized that magnetic moments of nuclei in general could be measured by resonance methods. This idea led to the discovery of nuclear magnetic resonance, which Bloch originally called nuclear induction. For this and the simultaneous and independent work of E. Purcell, Bloch and Purcell shared the Nobel prize in physics in 1952.

The aim of the physicist is to carry out and interpret experiments that yield new results. In this sense Bloch reached tremendous heights in both theory and experiment, and it can truly be said that he "made" physics in great leaps and discoveries.

In the detailed account of Felix's career which follows, I shall describe these and several other important advances made over the years. But I shall first speak about his background and early life, as he himself described it in a talk he gave at Stanford on January 20, 1970, entitled "How I Became a Physicist."

Felix Bloch was born in Zurich on October 23, 1905. This was the same year in which Albert Einstein made three transcendent discoveries in physics. His father was Gustav Bloch, a wholesale grain dealer in Zurich. His mother was Agnes Meyer Bloch, a cousin from Vienna. Gustav came from a large family living in western Bohemia and although he had strong interests in history and languages was unable to attend a university for financial reasons. He moved to Zurich in 1890 to take a position in his uncle's business and became a Swiss citizen. Gustav and Agnes had a daughter in 1902 and, as stated above, Felix was born in 1905. The name "Felix" means "lucky," and it was a propitious way to start out in life with this name.

The love of mountains that Felix acquired through vacations in the Alps remained a very deep part of his character all his life. He entered public elementary school when he was six years old. Experiences in school at that tender age were difficult for Felix, who spoke Swiss German with a somewhat different accent than most members of the class. He was also treated rather shabbily by his teacher. This led to a dislike for school, but his sister gave him strong support; when she died at the age of twelve, it was an extremely tragic event for him.

Felix led a depressed and isolated life in the years following the outbreak of World War I. But his feelings changed gradually after moving to a new school where education was governed by the benevolent Pestalozzi method. Arithmetic was a subject that had a special appeal because of its clarity and beauty. He also started music studies and played the piano when he was eight years old. He had a preference for Bach's harmonies. At twelve, Felix finished elementary school and began secondary school. At this time he and his parents made a decision to choose a six-year curriculum that would prepare him for a university. He attended the gymnasium run by the Canton of Zurich, entering in the spring of 1918. This was a very good choice because many of the professors were not only good teachers but were scholars at the same time who had previously earned the title of Ph.D. It was hard work in the gymnasium but his Latin studies were very enjoyable and stimulating. French, English, and Italian were taught, as well as Latin, mathematics, physics, and chemistry. Numbers were especially attractive to Felix and dealing with them instilled a deep respect for quantitative ideas. He applied elementary mathematics, which he had just learned, to astronomy and proved for himself that he could successfully calculate the length of daylight in Zurich at various times of the year.

At age fifteen, after three years in gymnasium, Felix started to study physics and continued in gymnasium with languages and mathematics until 1924. He entered the Federal Institute of Technology (ETH) in Zurich in the fall of 1924, having made a choice of engineering as a future profession. This early choice of career is similar to that of Dirac, Wigner, and von Neumann. Following the thorough program of that school, he took calculus and mechanics as well as a course in drafting that he didn't greatly appreciate but was necessary for an engineering degree. During summer vacation he worked in a small iron foundry on the lake of

Zurich. This experience provided him with grounds for the decision he needed, and he changed from engineering to physics. His father was rather skeptical of this choice because teaching in high school or working with physics in industry didn't seem to lead to a very promising career. Wishing advice, Felix went to see Professor Hermann Weyl who was the division head for mathematics and physics at the ETH and asked him whether he should study physics. Weyl said "no," but Bloch did not accept this advice because, as he indicated, he "couldn't help it."

In Peter Debye's class in introductory physics Felix found what he desired and felt later that he learned more from that class than from all his other courses together. Coming across Sommerfeld's famous book, *Atomic Structure and Spectral Lines*, Felix found that he needed to know what was meant by an "electromagnetic field." He did his own reading about that subject and many others in classical physics and made a brief foray into experimental physics that he never completed. On the other hand, he was absorbed by the lectures in the small colloquia held alternately in the departments of the University of Zurich and the ETH.

In 1926 an event occurred that had a great influence on his career. He described this in an article for *Physics Today* in December 1976. He writes:

> Once at the end of a colloquium I heard Debye saying something like: "Schrödinger, you are not working right now on very important problems anyway. Why don't you tell us some time about that thesis of de Broglie, which seems to have attracted some attention?"
>
> So in one of the next colloquia, Schrödinger gave a beautifully clear account of how de Broglie associated a wave with a particle and how he could obtain the quantization rules of Niels Bohr and Sommerfeld by demanding that an integer number of waves should be fitted along a stationary orbit. When he had finished, Debye casually remarked that this way of talking was rather childish. As a student of Sommerfeld he had learned that, to deal properly with waves, one had to have a wave equation. It

> sounded quite trivial and did not seem to make a great impression, but Schrödinger evidently thought a bit more about the idea afterwards.
>
> Just a few weeks later he gave another talk in the colloquium which he started by saying: "My colleague Debye suggested that one should have a wave equation; well I have found one!"
>
> And then he told us essentially what he was about to publish under the title "Quantization as Eigenvalue Problem" as the first paper of a series in the Annalen der Physik. I was still too green to really appreciate the significance of this talk, but from the general reaction of the audience I realized that something rather important had happened, and I need not tell you what the name of Schrödinger has meant from then on. Many years later, I reminded Debye of his remark about the wave equation; interestingly enough he claimed that he had forgotten about it and I am not quite sure whether this was not the subconscious suppression of his regret that he had not done it himself. In any event, he turned to me with a broad smile and said: "Well, wasn't I right?"

This quotation not only illustrates an important event in Felix's career but demonstrates as well how charmingly he could write and tell stories.

A little earlier Felix asked Debye for comments on an idea he had, since it concerned improving an older paper of Debye's on the Compton Effect. Instead Debye suggested that Felix should study Schrödinger's new wave mechanics. Many years later, Felix returned to the Compton Effect and wrote a paper on his original idea, this time using quantum mechanics.

Over the next eight years Felix's travels were complex and varied. Aside from short stays he spent extremely productive working periods successively in Leipzig, Zurich, Utrecht, Haarlem, Leipzig again, Copenhagen, Leipzig once more, and Rome before he went to Stanford in 1934. At almost every institution where he stayed, Felix made a major contribution to physics. Some of his achievements over these years are described below.

Debye left Zurich in 1927 and became professor at the University of Leipzig in Germany. Once more, taking Debye's advice, Bloch followed him to Leipzig to start graduate work there with Werner Heisenberg, who had just been appointed professor of theoretical physics at the university. Heisenberg was then twenty-six years old and Felix was twenty-two, but Heisenberg was already a famous man. This was a "happy" step, since Heisenberg was one of the discoverers of quantum mechanics, developing his own matrix mechanics approach, and was in a position to apply this new theory to many problems that until then had not been solved. While still in Zurich, Felix had studied Schrödinger's wave theory and learned that the Schrödinger approach and Heisenberg's matrix mechanics were equivalent.

Heisenberg had not yet arrived in Leipzig when Felix first went to the University and so he introduced himself to Gregor Wentzel, who was a young professor at that institution. Felix described to Wentzel a calculation he had made on radiation damping of a harmonic oscillator that he thought would moderate the spreading of electron wave packets that followed from the wave theory. When asked for advice, Wentzel suggested that he wasn't an expert and Felix should talk with Heisenberg directly about this calculation. Heisenberg pointed out that the wave would spread in any case, but he encouraged Bloch to complete the calculation for the general case, which he did promptly. This work resulted in Bloch's first paper and, as he later remarked, it was a forerunner of the paper by Weisskopf and Wigner on radiation damping and the natural line widths of spectral lines.

Heisenberg took Felix as his first graduate student and suggested that for his thesis Felix should study the conductivity of metals by applying the new quantum mechanical theory. This was a well-known problem in classical theory whose complete solution had baffled such accomplished physicists as Drude, Lorentz, Pauli, and Sommerfeld, even though they had made considerable progress and had explained experimental results concerning the specific heat in metals and the relationship between thermal conductivity and electrical conductivity. This last relationship was known as the Wiedemann-Franz ratio. Classical theory, with some quantum modifications, agreed with experiment, at least approximately, but in these semiclassical treatments, no one understood why the conduction electrons should be treated as an ideal gas of free electrons. By making the assumption of an ideal gas, Pauli had already explained the temperature independence of the paramagnetism of metals by applying Fermi statistics to the conduction electrons. Sommerfeld and Pauli had also produced the results mentioned above for the Wiedemann-Franz ratio as well as for specific heat, but the entire situation about an ideal gas in metals seemed very puzzling. Why the free electron approach worked for metals and why the electrons didn't contribute to the specific heat in solids also needed to be incorporated into any consistent explanation.

Felix wrote:

> When I started to think about it, I felt that the main problem was to explain how the electrons could sneak by all the ions in a metal so as to avoid a mean free path of the order of atomic distances. Such a distance was much too short to explain the observed resistances, which even demanded that the mean free path become longer and longer with decreasing temperature. But Heitler and London had already shown how electrons could jump between two atoms in a molecule to form a covalent bond, and the main difference between a molecule and a crystal was only that there were many more atoms in a periodic arrangement. To make my life easy, I began by considering wave functions in a one-dimensional periodic potential. By straight Fourier analysis I found to my delight that the wave differed from a plane wave of free electron only by a periodic modulation.
>
> This was so simple that I didn't think it could be much of a discovery, but when I showed it to Heisenberg he said right away, "That's it." Well, that wasn't quite it yet, and my calculations were only completed in the summer when I wrote my thesis on "The Quantum Mechanics of Electrons in Crystal Lattices."

Felix's thesis was published under the title "Uber die Quantenmechanik der Elektronen in Kristallgittern" in the *Zeitschrift für Physik* (1928). In this work he also calculates the specific heat and electrical resistance of metals. The importance of this paper can hardly be overstated for it provided the basis for the band theory of condensed matter. Out of it followed the formulation by A. H. Wilson of the difference between metals and insulators and the theory of semiconductors. Everyone knows now what this implies for the enormous strides made in our times in radio, television, computers, communications, space exploration, etc., by the replacement of vacuum tubes, with their limited lifetimes, by the long-lived and rugged simplicity of semiconductors.

The waves that Felix discovered have been called "Bloch Waves" or "Bloch States," and the concept of these waves turns up everywhere in the theory of condensed matter.

Incidentally, the wave solution that Felix discovered was a version of what was known in mathematics as Floquet's Theorem and had been used previously by physicists without realizing its full implications for the quantum mechanics of solids.

In 1928, as was customary in those days, Bloch wanted to gain experience in other centers of theoretical physics in Europe, and so he spent the academic year 1928-29 as assistant to Pauli in Zurich. Superconductivity was the main topic that concerned Pauli at the time, and he asked Felix to help in solving that problem which no one had done previously. Pauli was apparently anxious to clean up the subject of superconductivity and even worked on it a bit himself, but Bloch somehow felt that Pauli was not as deeply interested in this as he was in other current problems.

Bloch's thesis, in which he introduced waves known by his name, also contains a theory of electrical conductivity in normal metals. One of the results obtained concerned the resistance of metals at low temperature, and it could be observed from this theory that superconductivity could not result from an approach using single electrons. Thus, Bloch could see that one needed something new to explain superconductivity.

In the work on superconductivity Bloch contributed an important idea, though he never published it, and we know about it mainly from references by others, such as Bethe, London, Brillouin, and Pauli. Bloch and London pointed out that it was necessary, on thermodynamic grounds, that the superconducting state required a minimum of the energy below the critical temperature but that at temperatures above that point a zero current state is more probable. A theorem, known as Bloch's first theorem on superconductivity, stated that the minimum energy state carried *no* current, much less a supercurrent. On the other hand, he did realize that the flow of current in the superconducting state involves a correlation between the velocities of the free electrons. But he could make no progress in finding a solution and Bloch's second theorem on superconductivity was humorously stated as, "In the absence of external fields every theory of superconductivity can be disproved." This negative statement, never published by Bloch himself, influenced the work of many others in very constructive ways.

Since Bloch never felt he had a successful theory of superconductivity, he did not publish an original article in this field. However, as mentioned above, he had a great influence on the theoretical side of the field through his comments and criticisms of ideas of Bohr, Kronig, Brillouin, and others. Nevertheless, his interest in superconductivity never lagged, and he returned to the subject in the 1960s. His year as Pauli's assistant came to have a lasting influence on his career in physics. Bloch has commented on his early ideas in an article he wrote in 1980 for the *Proceedings of the Royal Society of London*. He describes his interaction with Pauli and in particular refers to a discussion in which he reminds Pauli that the problem was not as easy as Pauli thought when he gave it to one who had just completed his Ph.D. thesis. Pauli agreed. Felix later remarked that this was an indication that Pauli was "softening up."

While serving as Pauli's assistant Bloch also studied the magneto resistance of metals and shortly afterwards attacked the fundamental problems of ferromagnetism. Ferromagnetism had already been treated by Heisenberg, who showed that the basic explanation depended on the exchange interaction of electrons. Heitler and London showed previously that the hydrogen molecule's binding followed from the exchange interaction, and this mechanism offered, at least in principle, a basis for ferromagnetism. Bloch attempted to put Heisenberg's idea into a more rigorous framework. In doing this he showed how it was possible to calculate the exchange energy of a free electron gas and used John Slater's newly invented determinantal formulation of the wave function. His conclusion was that the zero point energy of the electrons figured importantly in determining whether a metal would be ferromagnetic. Slater extended Bloch's calculation at a later time and surmised that the 3d and 4s electrons, rather than the conduction electrons that Bloch studied, could explain ferromagnetism.

In the fall of 1929 Bloch went to Utrecht as a Lorentz Foundation fellow, where H. A. Kramers was his host. In November 1929 he published a relatively brief article on the electrical resistivity of metals at low temperature in which he reconsidered a calculation previously made that gave a T^3 dependence, where T is the absolute temperature. He included a small term omitted in the first calculation and obtained a T^5 law that agreed with experiment.

While he was visiting Kramers at the Physical Laboratory of the Rijks University, Felix came upon the idea of spin waves and their connection with ferromagnetism. The imaginative idea of spins flipping around the lattice was very novel at that time. Although J. C. Slater also had the same idea, he did not associate it with ferromagnetism. The discovery of spin waves proved to be an important precursor of quasi-particle theories. In this work Bloch derived the dependence of the magnetic moment on the absolute temperature in the low-temperature region.

He followed his visit to Utrecht by spending a few months in Haarlem with A. D. Fokker. There he wrote a short qualitative paper on the interactions between metallic electrons, summarizing his ideas on spin waves developed in Utrecht. Felix greatly enjoyed his stay in Holland and kept a painting of the Dutch landscape above his desk for many years. Both he and Kramers enjoyed the visit and became lifelong friends. In Utrecht Bloch and L. D. London met again after their brief acquaintance in Copenhagen, when Bloch and Kramers attended a meeting at Bohr's institute.

Late in 1930 Bloch returned to Leipzig, this time as Heisenberg's assistant. In May 1931 he wrote a paper, with B. Gentile, on the anisotropy of magnetization in single crystals of ferromagnetic materials. Returning to the developments that resulted from his thesis work, Bloch summarized in a small article the nature of the allowed and forbidden bands, especially in connection with electrical conduction and photoelectric phenomena. In this paper we see the modern theory of solids emerging because references are given to other participants who were developing the theory, such as R. E. Peierls and A. H. Wilson. Wilson had explored the nature of semiconductors and showed that the differences between insulators and conductors could be explained with bands generated by Bloch States.

After spending the academic year 1930-31 with Heisenberg in Leipzig, Bloch wrote his Leipziger Habilitationsschrift. This is a monumental paper on exchange interactions and remanence in ferromagnetism that includes much more than those two topics. It is interesting that this long article was written while Bloch was hospitalized due to an injury he received while climbing in the Alps with Egon Bretscher. A quotation from a historical paper on the solid state by Lillian Hoddeson, Gordon Baym, and Michael Eckert follows:

> Nominally devoted to exchange-interaction problems and residual magnetization in ferromagnets, the paper presents an exceptional wealth of formalism which has become part of the fabric of the modern theory of condensed matter physics and collective phenomena. Beyond its contribution to the theory of domain walls, this work serves as a bridge between the quantum theory of ferromagnetism in the 1930's and present theories of many-particle systems.

The process of magnetization in ferromagnets had been studied experimentally by R. Becker, who investigated domain structure and how it varied as magnetization proceeds. A vital step in understanding this process involved the boundary wall between domains and the manner in which it could move. Bloch worked out the thickness and structure of the boundary walls, and the wall structure became known as the "Bloch Wall." In a space of a few hundred angstroms the magnetization could reverse direction, and this proved to be energetically more favorable than a complete reversal at the boundary. Many years later the details of this progressive change at the boundary became observable experimentally.

In the fall of 1931 Bloch worked on an Oersted fellowship at Bohr's Institute in Copenhagen. Bohr had an enduring interest in stopping power, the loss of energy of a charged particle as it passes through matter. Ever since he wrote a famous article on this subject in 1913, Bohr wished to improve the theory to agree better with experiment. In many conversations with Bohr, Bloch was slowly led into making calculations of his own on stopping power. Bohr's classical calculation gave energy loss results larger than the observed losses of alpha and beta particles. H. A. Bethe developed a more accurate theoretical result in 1930 in a paper based entirely on quantum mechanics. Except for the treatment of close collision, it was not clear why Bohr's treatment and Bethe's results differed. Bloch explained this discrepancy, in a paper that appeared in 1933, by obtaining a result for the energy loss which showed that both the Bohr and Bethe calculations were opposite limiting approaches corresponding to the different ways in which the relative phase varied as the particle passed near an atom. This result pleased Bohr but did not end his interest in the stopping power phenomenon. Indeed fifteen years later Bohr returned to the problem and wrote a well-known monograph on this subject, which also included considerations of the energy loss of charged fission fragments.

Bloch returned to Leipzig in the spring of 1932 as a privatdocent. While there he completed the paper on stopping power that he submitted to the *Annalen der Physik* in the summer of 1932. At the end of 1932 he made an elegant modification of the stopping power calculation by employing the Thomas-Fermi atomic model in a particularly successful and practical way. The Bethe-Bloch formula that resulted from the work of Bethe and Bloch remained useful for many years and served as the basis of improved calculations that would include dielectric shielding and straggling effects not contained in the original works. Bloch's paper was published in the *Zeitschrift für Physik* (1933).

Nazism was beginning to spread in Europe, and in late 1932 Bloch could see what the future might hold. He obtained a Rockefeller fellowship with which he could leave

Leipzig and have support wherever he chose to go. In March 1933 Hitler came to power and Felix left Leipzig, never to return. The Rockefeller fellowship allowed him to go to Rome, where he worked in Fermi's institute at the University of Rome. But he spent about six months in Zurich before he went to Rome. This "free" period arose because Bloch left his position as a privatdocent in Leipzig and could not assume his fellowship until the fall. During this period he visited Paris for a short time, gave lectures, and stayed at the home of Langevin. After Paris Bloch visited Kramers once more in Utrecht. At that time it was known that, because of anti-Semitism and Nazism, Bloch's name, among others, was placed on a list of "displaced" scholars and he felt that this is how he came to the attention of Stanford. In the fall of 1933, actually while visiting Bohr in Copenhagen, Bloch received a telegram from David Webster at Stanford offering him a position in the Physics Department.

I quote from Bloch's interview with Charles Weiner in 1968:

> There's a rather amusing story there. Heisenberg was also in Copenhagen at that time and I went to him and asked him. I knew he had been around the world, so I asked him whether he knew something about Stanford, and he said he only remembered it vaguely. He said, "It's somewhere on the west coast and nearby is another university, the name of which I've forgotten," and he told me, "They steal each other's axe." Now you may not appreciate this, but this was a sort of a game with students. Before the big football game, Stanford has a symbol, an Indian axe, and the Berkeley team stole that. This incident was the only thing that Heisenberg remembered about Stanford. Also the name of Webster, I'm ashamed to say, didn't mean anything, either to me or to him.
>
> But then I went to Niels Bohr, and Niels Bohr did indeed know the place and he advised me. "It's a very fine place." He advised me strongly to accept it.

In Rome Bloch wrote a paper on X-ray scattering and a review paper on the molecular theory of magnetism, but, perhaps more importantly, he became familiar with Fermi's approach to physics. In fact, Fermi advised Bloch to do some experimental physics because "it was fun." In Rome Felix brought "quantized amplitudes" to the attention of Fermi, and shortly afterwards Fermi wrote his famous paper on beta decay.

At age twenty-eight Felix left Europe to go to Stanford. After a rough sea trip he arrived in New York where he was met by Gregory Breit. He then left New York by train for California, arriving in Stanford in early April 1934. His position at Stanford was as acting associate professor of physics. He was warmly welcomed at Stanford and felt that the people he met were very friendly.

Robert Oppenheimer was teaching at the time at Berkeley, and, since Bloch had already been acquainted with him, they saw each other constantly. They set up a joint seminar on theoretical physics, meeting alternately in Berkeley or Stanford and occasionally elsewhere on the West Coast. Because of Felix's reputation and presence at Stanford, prominent physicists visited him, most often in the summer, and stayed for a few weeks or longer. Among these many visitors were Gamow, Fermi, Rabi, Bethe, Weisskopf, Lamb, Nordsieck, Schein, and Bohr.

In his first research paper at Stanford Bloch treated the theory of the Compton line, which went back to his much earlier proposal to improve Debye's work. Soon afterwards he joined in the interests of his new Stanford colleagues and published papers on the "Radiative Auger Effect" with P. A. Ross, on "Double Electron Frontiers in X-Ray Spectra," and on the "Mechanism of Unimolecular Electron Capture" with N. E. Bradbury. These papers are still of interest in their respective fields.

Felix made a trip to Switzerland in the summer of 1935 that lasted about four months. In between he had spent some time in the summer school at the University of Michigan in Ann Arbor. This school attracted many of the then-rising group of American theoreticians and also prominent visitors from Europe, such as Fermi. On the trip to Europe Felix also visited Copenhagen, where once again he met Bohr. On this occasion he was encouraged by Bohr to think about doing neutron physics. The discovery of the neutron took place in 1932, and the physics of neutron interactions was very new. It was known that the neutron had a magnetic moment, and Felix's earlier knowledge of ferromagnetism made him think about polarizing neutrons in a ferromagnetic material.

In July 1936 Bloch submitted a "Letter to the Editor" of the *Physical Review* in which he first described the theory of magnetic scattering of neutrons. He also showed how the scattering could lead to a beam of polarized neutrons and how he could separate the atomic scattering from the nuclear scattering by temperature variations of the ferromagnet. From experiments on the scattering at small angles the magnetic moment of the neutron could be determined.

Bloch was thinking about neutron experiments in 1937 while he was visited by Arnold Nordsieck, who had been a visitor in Leipzig after Felix left. Nordsieck returned later to the United States, and together they worked on a problem of electrodynamics that had presented a great deal of difficulty to theorists. The problem appears in the scattering of an electron in a Coulomb field accompanied by the emission of a single light quantum. For low frequencies, if the results are taken seriously, there would be an infinity in the cross section. This infinity has been known as the "infrared catastrophe." The paper by Bloch and Nordsieck demonstrated that even though the mean total number of quanta emitted is infinite at low frequencies the mean radiated energy is finite. Thus, the infrared catastrophe was resolved.

Bloch returned to his considerations about neutrons and, together with Norris E. Bradbury and colleagues at Stanford who were experimentalists, built a low-voltage neutron source. The neutrons were produced by the deuteron-deuteron reaction and were used to find the scattering cross section of neutrons on cobalt. This work showed that the anomalously large cross sections for iron and nickel do not depend on their ferromagnetism, since cobalt, which is also ferromagnetic, has a normal cross section.

During Fermi's summer visit to Stanford in 1937 Bloch had a very important idea about neutron scattering that could permit measuring the magnetic moment of a free neutron. In a different context, Rabi had the same idea perhaps slightly earlier and used it in his celebrated molecular beam experiments to measure the magnetic moments of nuclei. But a beam of neutrons similar to the molecular beams that Rabi used would be very difficult to generate, and so Bloch applied his idea to a different sort of experiment. He thought of using a polarized beam that would pass through a region of constant magnetic field in which a radio frequency magnetic field could also interact with the neutron. He would look for a change in the transmission of the beam as the radio frequency was varied through the "resonant" Larmor frequency. Putting together his considerations about polarized neutron beams and the resonance idea, Bloch started to work experimentally on the idea. This work was carried out in 1938 when Rabi was visiting Stanford. The first experiment that was witnessed by Rabi gave a negative result because the neutron source was too weak. The second experiment was done at Berkeley where the 37-inch cyclotron produced a much more intense source of neutrons. At Berkeley Ernest Lawrence suggested that a young experimental physicist, Luis Alvarez, might work with Felix, and Luis and Felix started their famous experiment in the fall of 1938. They worked together through the spring and summer of 1939 on the cyclotron, which worked only sporadically at that time.

In this celebrated work Felix and Luis made a precise determination of the neutron magnetic moment equal to 1.935 ± 0.02 nuclear magnetons, and the sign was negative with respect to the proton's moment, which was known to be 2.785 ± 0.02 n.m. The deuteron moment was equal to 0.855 ± 0.066 n.m. Both the latter two values were determined by Rabi and his collaborators. The deuteron moment was thus seen to be the approximate sum of the proton and neutron magnetic moments, a result that seemed plausible since the proton and neutron were bound rather loosely in the deuteron. But more exact values of all the moments were obviously necessary to test whether there could be new physics in the binding of the deuteron, particularly since the deuteron had an electric quadrupole moment. The establishment of the resonance method by Bloch and Rabi was therefore important in more ways than one, since it involved the nucleon-nucleon interaction, which was the very heart of nuclear physics at that time.

The resonance method used by Bloch and Alvarez employed a beam of polarized neutrons obtained by passing the unpolarized neutron beam from the cyclotron through a very strongly saturated plate of magnetized iron. Fractional depolarization of the neutron beam could be measured by the passage of the beams through an analyzer plate of iron, also strongly magnetized. Between the two plates a constant strong magnetic field was placed, and, in addition, a weak oscillating magnetic field was introduced normal to the constant field and of variable frequency. As the frequency of the oscillating field was changed, the transmitted beam would pass through a resonance at the Larmor precessional frequency corresponding to the value of the magnetic moment in the constant magnetic field. At the value of the resonance, the polarization of the incident beam was changed, and the scattering of the beam in the second plate could be detected. At the observed value of the Larmor resonance, $v = 2\mu H/h$, the value of the magnetic moment μ could be determined, since the frequency v and the magnetic field H could be measured, and h was, of course, the known value of Planck's constant.

The technique was beautiful, and the only big problem was to obtain significant polarization of the neutron beam. This was accomplished since the neutron resonance dip was clearly observed by a change of intensity of the beam equal to about 2 percent.

Felix and Luis worked intensely on this problem for a long time, and the resulting observation made them extremely happy. This experiment remains to this day a celebrated example of a theorist turning experimental physicist, as Felix did. Thus, Fermi was right—"it was fun."

Of course, the experiment itself was very important, but in doing it Felix was bothered by having to measure the magnetic field with a flip coil. The flip coil method was a standard one, but it was not very accurate, and so Bloch tried a new approach whereby the accuracy could be greatly improved. This seemed possible since frequencies were employed and could be determined with great experimental accuracy. But a first step in getting accuracy was made by using two flip coils, one just as used previously, with the second inserted in the magnetic field of the cyclotron. The resonant orbital cyclotron frequency for protons, or a harmonic of it, could then be used to determine the proton moment. By taking the ratio of the two magnetic fields, that is, (a) that in the neutron beam and (b) that for protons in the cyclotron, the magnetic moment of the neutron could be measured absolutely in nucleon magnetons. Indeed the method could be applied to any nucleus, not just the neutron itself.

The success of this method inspired Bloch to think about how to measure the neutron moment with even higher precision and in absolute units. He decided that a small cyclotron should be built at Stanford that would provide the opportunity of making further measurements "at home" besides improving the method of polarizing neutrons. With this cyclotron and the collaboration of M. Hamermesh and H. Staub, a figure of 8 percent was established at saturation magnetic field values, a great improvement over the value observed in the Bloch-Alvarez experiment. Later 20 percent polarization effects were achieved. The cyclotron had a 20-inch diameter and was built by Bloch, Hans Staub, and William Stephens.

In 1939 while in New York and on their way to the spring meeting of the American Physical Society in Washington, D.C., Felix and Lore Misch met each other. Lore was a physicist in X-ray crystallography who had left Europe in 1938 and worked in G. Harrison's spectroscopy laboratory at MIT. She had obtained her Ph.D. degree with V. M. Goldschmidt in 1935 in Göttingen. In September 1939 Felix spent some time in the East, and he and Lore decided to get married. They were married on March 14, 1940, in Las Vegas. They had four children, three boys and a girl, who are now all happily married. There were eleven grandchildren, all devoted to Lore and Felix.

During World War II Bloch worked first at Stanford with the 20-inch cyclotron, measuring the energy distribution of neutrons emitted during fission. Interesting classified results were obtained that showed that the neutron spectrum extended well above the energy of 2 MeV that had been expected. After completing this work, Felix was invited to Los Alamos by Oppenheimer. At Los Alamos he was interested in the implosion method suggested by Seth Neddermeyer. Bloch left the Manhattan District Project after the implosion work and joined the Radio Research Laboratory at Harvard. He worked there in Van Vleck's group on reflectivity of materials to waves used in radar research. Later Felix wrote papers on his war work with Van Vleck and with L. Brillouin. At Cambridge he met with William W. Hansen, who came from Stanford and was an expert in electromagnetic radiation. The experience Felix obtained with microwaves was to serve him in good stead on his return to Stanford.

In 1946 Leonard Schiff was invited to Stanford. Shortly afterwards Leonard became chairman of the Physics Department and from that point he and Felix formed an appointments committee that gave the department international stature in just a few years.

The experiments with neutrons gradually led Bloch to new combinations of his previous experiences with ferromagnetism and magnetic moments. As a result he thought of the method of measuring atomic magnetic moments, which he called "nuclear induction." The idea is the following: If atomic nuclei are placed in a constant, say vertical (z direction) magnetic field, an alignment of their magnetic moments would take place that would be limited by thermal agitation. A weak oscillating magnetic field in a horizontal (x) direction perpendicular to the constant field could be superimposed on the constant field. When the Larmor frequency is approached, the original rotating polarization vector will be driven nearer the plane perpendicular to the constant magnetic field. The rotating horizontal component of the polarization vector will induce a signal in a pickup coil whose axis is in the y direction, that is, perpendicular to the weak oscillating field. The exact value of the frequency that gives the maximum signal can then be used, as in the Larmor resonance formula, to calculate the magnetic moment. It is clear that nuclear induction had a close filial relation with the Bloch-Alvarez experiment.

The nuclear induction idea was first applied experimentally to water, and the proton moment was measured to be in agreement with the value previously determined in the Rabi experiments with molecular beams. Bloch's collaborators at Stanford who carried out those first measurements were W. W. Hansen and graduate student Martin Packard. These results appeared nearly simultaneously with those of H. C. Torrey, E. M. Purcell, and R. V. Pound at Harvard who used a resonance method involving energy absorption of radiation in a cavity. The two methods that provided nuclear magnetic moments with relatively simple apparatus are now known as "nuclear magnetic resonance." For their invention of these techniques and the discoveries made with them Bloch and Purcell shared the Nobel prize in physics in 1952. For Stanford this was its first Nobel prize.

In the magnetic resonance technique there are two parameters, introduced by Bloch, known as T_1 (longitudinal) and T_2 (transverse) relaxation times, which relate to the interaction of the nuclear magnetic moment with the surrounding atomic or molecular environment. The behavior of these parameters is clearly related to chemical bonding or biological processes in the material examined. In a theoretical paper accompanying the experimental demonstration of nuclear induction, Bloch developed a phenomenological description of the nuclear inductive process including T_1 and T_2. The equations he developed have been known as the "Bloch Equations" since then.

The Bloch method of nuclear induction has had scientific and practical uses that no one could have foreseen. Obviously it allowed evaluation of many nuclear magnetic moments, for which it was originally designed. However, refinements of the technique and applications to chemistry, following from "chemical shift" experiments performed in Bloch's laboratory, have been so successful that magnetic resonance has become the most important spectroscopic tool used in structural and dynamic studies in chemistry. The practical value of research using refinements of the original method in chemistry and biology has been immense.

Moreover, a medical imaging method based on the Bloch technique has been developed by P. C. Lauterbur and others that uses the resonant frequencies in an inhomogenous magnetic field, thus connecting the frequencies with spatial coordinates. This medical tool is known as magnetic resonance imaging, or MRI. Although introduced only a few years ago, this method now rivals the traditional X-ray imaging method and even the very successful computer-assisted tomographic technique known as CT. Indeed, MRI probably represents the greatest advance in medical imaging since the discovery of X-rays by Wilhelm Röntgen in 1895. The MRI method is also complementary to X-ray studies since physiological and metabolic processes can be investigated through the parameters T_1 and T_2.

After a period of working with the novel nuclear induction measurements and obtaining interesting new results, Felix returned to a precise measurement of the magnetic moment of the neutron by the Bloch-Alvarez method. The small Stanford cyclotron was used as a neutron source, and the measurement was carried out by Bloch, Nicodemus, and Staub. The magnetic moment of the neutron could now be given in units of the proton's magnetic moment. To complete the accuracy needed for nuclear physics considerations, Bloch and Jeffries also measured the magnetic moment of the proton in nuclear magnetons. These measurements permitted a study of the additivity of the moments of proton and neutron as they are bound in the neutron, and a small difference was observed. This difference was a result to be compared with the theory of strong interaction physics and involves the value of the quadrupole moment of the deuteron.

In addition to measuring precise values of the magnetic moment of the neutron, proton, and deuteron, Bloch and his colleagues made a precise determination of the magnetic moment and spin of the triton.

Niels Bohr continued to have a strong influence on Felix and suggested that he should take on the job of director-general of CERN, which was just being organized. Although Felix had many doubts about the operation of big accelerators, he accepted the appointment in 1954, which, of course, was largely administrative. Even so, when he went to Geneva, he took along Stanford equipment, and he and two colleagues, Jim Arnold and Wes Anderson, continued nuclear induction experiments. As Felix had predicted, he didn't care for administrative work, and after a trial period of one year, he returned to Stanford, but not before leaving a great and positive influence at CERN.

When Bloch returned to Stanford in the fall of 1955 he joined four colleagues in proposing to build what is now called the Stanford Linear Accelerator Center. At that time it was called "Project M," where M stood for "Monster." The idea to build a very long linear accelerator (now two miles long) appeared first in mid-1954 after Felix left Stanford for CERN. Through correspondence he expressed caution about this idea but finally felt that it could be a very good thing if its administrative infrastructure did not come to dominate the Physics Department at Stanford. He also put it this way: "If we are going to build a Monster, let's make sure it is a Good Monster."

In a series of papers between 1950 and 1957, Felix, together with R. K. Wangsness, worked out a microscopic justification for the use of the phenomenological relation parameters T_1 and T_2 in nuclear induction. This work also established an understanding of the relative intensities and widths of the observed resonance lines.

In 1961 magnetic flux quantization was discovered by B. Deaver and W. M. Fairbank and by W. Doll and M. Näbauer. At that time, N. Byers and C. N. Yang investigated the superconducting properties that were needed for flux quantization. Considerations such as these demonstrated that pairing of electrons, as proposed by Bardeen, Cooper, and Schrieffer, was the right idea and was further confirmed by the observation of the flux quantum equal to one-half the London quantum. At this point, Felix returned to his old interest in superconductivity and, working with H. E. Rorschach, employed a model based on previous proposals of M. R. Schafroth for the superconductive state of a metal in which a charged condensed Einstein-Bose gas, made of di-electrons, reproduced many, but not all, of the main features of superconductivity. A long hollow cylinder was used to simplify the geometry of the model, and questions of stability of the superconductive state were investigated.

Felix had a great desire to simplify the theory of superconductivity in order to bring out the physics more clearly. In 1964 he again considered the long hollow cylinder but employed the off-diagonal long-range order introduced by C. N. Yang in 1962. He showed that Schafroth's model could be justified but generalized the theory to include either Bose or Fermi statistics.

Felix was elected president of the American Physical Society in 1965 and conscientiously attacked the many details required by such a position. Upon retiring from the presidency a year later he addressed the society with parting remarks. These remarks were really an attempt, once more, to develop a simplified physical theory of superconductivity. Although he made considerable progress, as outlined in his talk, he stated modestly:

> It seems to become of the many cherished traditions of the American Physical Society that every third retiring presidential address somehow refers to the theory of superconductivity. Thus in 1960, George E. Uhlenbeck (*Physics Today* 13, no. 7, 18, 1960) expressed in his address the thought that the theory of superconductivity "is still a bit controversial." In 1963, W. V. Houston (*Physics Today* 16, no. 9, 36, 1963) stated on the same occasion his belief that "a simple physical picture of superconductivity still remains to be carried out." Apparently it is indicated at this time to look again at the situation and to see what has happened along the lines desired by my predecessors. Although further progress has been made, I am afraid that I shall be unable to fulfill all their wishes; it is my hope, however, that the tradition will be maintained and that the retiring president will favor us in 1969 with a comprehensive account of the insights achieved.

I don't know for sure, but I have the feeling that Felix never did have the satisfaction of seeing his hopes realized.

Bloch's last original papers were connected with superconductivity and included discussions of the Josephson Effect, which he explained in a simple way.

In the ensuing years, particularly after his retirement, Felix started to write a book on statistical mechanics. He worked very conscientiously and carefully on it, and getting things just the way he wanted took a long time. In fact, he couldn't finish it before his death in 1983. J. D. Walecka took Felix's notes and organized them into a book which has recently been published with the title *Fundamentals of Statistical Mechanics*. The result is a lucid and elegant account of the subject. This book represents years of lecturing to students on thermodynamics and statistical mechanics and shows Felix's concern as a teacher for these often puzzling and hard-to-understand subjects. I think Felix would have been delighted with Walecka's presentation of his original material.

I won't discuss his many honors except to say that he was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the American Philosophical Society, and the extremely prestigious German honor society known as Pour le Mérite. Among the members of this society were Charles Darwin, Carl Friedrich Gauss, Otto Hahn, Werner Heisenberg, Max Planck, Otto Warburg, and Hideki Yukawa, to name just a few illustrious scientists.

I would now like to make a few personal remarks. Felix Bloch was a consummate physicist. He had a very deep love of physics, and he was working and thinking about physics up to the last day of his life in 1983. In choosing physics there could hardly have been a better time for him to enter the field, for during the years 1924-27 modern quantum theory emerged in great splendor and he was a witness to it. He rode the crest of the waves of this great new science, contributed to it, and showed how it could be applied to real unsolved problems, such as the conductivity of metals and ferromagnetism. It can truly be said that he was the father of solid-state physics and one of the great physicists of the twentieth century.

Felix was many faceted. Besides science he loved music, literature, nature, and particularly mountain climbing and skiing. Once in 1953 he, Leonard Schiff, and I hiked up a mountain in the Mono Recesses of the Sierra to a height of about 13,000 feet. We were all greatly pleased to get to the summit, and this climb remains one of the best memories of my life.

A few years ago my wife, Nancy, and I were visiting Lore and Felix in Zurich. On one beautiful day we took a téléférique to the top of a mountain called the Rigi, which could be viewed from their apartment. Families were walking there and brown Swiss cows were grazing, the sounds of their bells floating across the meadows. On the rim of the hill were some young sportsmen preparing hang gliders to take off down the valley. It was the first time any of the four of us had watched this procedure so closely. When they were ready there was a moment's pause. The human glider glanced around, and then took off into space, swooping down over the green valley and soaring in the wind. In the background were the jagged snowy peaks that characterize Switzerland and that Felix loved so much. It was a time to remember—a special time that we all shared.

Felix was full of slightly ironic humor. He was a raconteur with many reminiscences of some of the great men of science in our times. His stories transformed those legendary giants of science onto a human scale. Felix admired honesty, intelligence, originality, and kindness. He appreciated eccentricity and was usually tolerant of the idiosyncracies of others. One thing he did not like was an inflated sense of self-importance, and he was not above taking delight in the comeuppance experienced sometimes by those having such a tendency.

Although Felix was a convivial man, he sometimes liked solitude. When he was thinking about a difficult problem he would take long walks alone. He and Lore liked coming to our ranch located in a remote area of the Sacramento Valley. Sometimes in the early morning he would be up and out before anyone else, and he could count on not seeing anyone but curious cattle and birds. Later we would all four walk together enjoying the wildflowers and the running creeks in spring or the lush grass of winter. Felix helped in mending fences and bringing in firewood, and his hearty appetite made shared mealtimes a double pleasure. It is very hard right now to think of those times, but we will not forget them. When I came to California in 1949 to do summer work at Berkeley, on the Compton Effect at Ed McMillan's synchrotron, I came to Felix's attention because Leonard Schiff had suggested me as a candidate for a position in the Stanford Physics Department.

Felix traveled to the Radiation Laboratory one day to see what I was doing, and there is no doubt that he went there to check on me. I was fascinated to talk with him, and I guess I passed some of his tests, too. As a result I was privileged to know Felix for some thirty-four years. During that time I learned much from him, not only in physics but also about the best things in human companionship. When he worked at his Stanford desk his office door was open 100 percent of the time. I could, and did, see him practically every day and visited with him three or four times a week. I felt I had great rapport with Felix. I don't know if I ever disagreed with him on any matter of consequence, for his thoughts were very lucid and convincing. That is not to say that I was merely absorbing his ideas. Nor does it say that I could keep up with him all the time. But I always did have a fresh way of looking at things after a conversation with him. Felix laid out his thoughts very carefully and would have made a superlative lawyer.

When visiting in his office or his home I marveled at how few physics books or journals he had. This illustrates how he worked things out for himself and how his work always had a very personal viewpoint.

Felix had extraordinary gifts and he shared them with the world. He had a very honest appreciation of himself and his contributions. He was a man of strong principles and opinions and was direct and outspoken in expressing them. No one had any doubt about where he stood at any time. He had a knack of going to the core of any problem, whether in science, politics, or otherwise. As I have remarked earlier, he had a superb sense of humor and was often a bit sardonic. He saw through pretension and often enjoyed exposing it. He could see both sides of almost any issue and always tried to see the good side.

In 1950 Felix helped me start an electron scattering program at the High Energy Physics Laboratory. I needed the munificent sum of $5,000. Felix arranged a lunch with Ed Ginzton and me during which Felix asked Ed, as director of the laboratory, how big his science budget was for that year. Ed gave him the figure, and Felix remarked with a smile that the support I would need, and for which he asked, wouldn't even be noticed in the total. Ed agreed graciously. During the fifteen years or so I spent on the electron scattering program, I would often come to see Felix after I had worked through the night on a particularly successful run. I would tell him the results. Obviously enjoying what he heard, he would almost always ask a question or make a suggestion that had not occurred to me. This is an example of the exhilarating effect he had on others.

Once when Nancy and I visited Felix and Lore in their Zurich apartment, he and I started to talk about Einstein's views on chance, determinism, and quantum mechanics. I ventured the thought that Einstein's view would ultimately prevail. Brusquely he said to me, "Anyone who takes that view doesn't understand quantum mechanics." That sort of bowled me over, but he was right. We continued our discussion, since he never thought I was too dense to recant. I hope this shows how he sometimes expressed his mind.

There are some things that I greatly missed in our relationship. For example, I would have liked to have been a musician and to have been able to play music with him. It is a matter of deep regret to me that while he was alive I didn't read his papers with the thoroughness I gave them in preparing this biographical obituary. For I would have told him how much I appreciated and marveled at the depth, elegance, and beauty of his treatments of many fundamental problems in physics. I would have enjoyed listening to how he came upon his ideas and about his conversations with his colleagues on these subjects. I hope he would have enjoyed hearing about how I liked what he did.

My last conversation with Felix occurred the day before he and Lore left for Zurich in 1983. I telephoned him at his home from the small conference room in the Stanford Physics Department. There is a smiling picture of him on the wall across the room from the phone. He was looking forward to their trip, and he sounded happy to get my call. That cheery voice together with his smiling face is the way I want to remember Felix Bloch. I miss Felix a great deal. Many of us do. I was among the lucky ones to know him well. He was a friend, ally, mentor, and much more.

Felix Bloch died suddenly on September 10, 1983, after suffering a heart attack. He is buried on the side of a mountain that overlooks the city of Zurich.

THE AUTHOR WISHES TO express his appreciation to Mrs. Lore Bloch; the Stanford University Archives; and the Niels Bohr Library, Center for History of Physics, at the American Institute of Physics, for access to documentary source material. He wishes also to thank Ms. Lois Nisbet for her help in preparing the manuscript.

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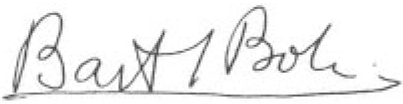

BART J. BOK

April 28, 1906-August 5, 1983

BY J. A. GRAHAM, C. M. WADE, AND R. M. PRICE

BART J. BOK WAS one of the movers and shakers in mid-twentieth century astronomy. He was a dreamer of dreams, but at the same time, a forceful and vital man who directly and indirectly influenced the lives of many people at all levels of society. A memoir writer has an abundance of material with which to work and it is a hard task to select those parts which are the most significant. An essential component in his life and work was the enduring love and devotion between himself and his wife Priscilla. Much of the story we tell took place between their first meeting in Leiden in 1928 and her death in 1975. They functioned as the most effective of collaborators through which their final bequest to the world was much greater than either could have accomplished alone.

In writing of Bart Bok's movements on the astronomical stage, it is appropriate that this chronicle divides rather naturally into three acts, each set in a place where he made a major mark on the development of astronomy. A prologue concerns his early years up until the time he met Priscilla Fairfield and set off to make his career in the United States. We end with an epilogue describing those concluding eight years when his life in no way slowed down but went off in new creative directions. His influence, particularly on young people, continues to be strong. He was, to make use of one of his own favorite words, "a phenomenon," a personality who combined great human warmth with a single-minded devotion to his chosen science. It was not possible to distinguish between Bok the man and Bok the astronomer. They were the same.

PROLOGUE: THE FORMATIVE YEARS (1906-29)

Bartholomeus Jan Bok was born on April 28, 1906, in Hoorn, Holland, the son of Sergeant Major Jan Bok and his wife Gesina Annetta van der Lee Bok. We are not sure what first turned Bart's interest to astronomy—he told conflicting stories about this. On several occasions he attributed it to a period in the Boy Scouts when the family had moved to The Hague shortly after 1918. By the time he was thirteen, it is clear that he knew he would be an astronomer. He attended high school at The Hague and was strongly influenced by a young physics teacher fresh out of the university. He graduated with high marks in 1924. In high school, he was an active amateur astronomer and it was during this time that he became an admirer of Harlow Shapley, certainly the second strongest influence on his life. He read widely, and he wrote about astronomy for one of The Hague's newspapers. It seems that much of Bart's style took form early: his fascination with the sky, his need to share and communicate, and his tendency to see people as heroes to be esteemed or as villains to be abhorred.

In 1924 Bart entered the Sterrewacht at Leiden with two other young Dutchmen who became outstanding astronomers, Gerard P. Kuiper and Pieter Th. Oosterhoff. His teachers at Leiden included several of the great figures of twentieth century physics and astronomy: W. de Sitter, E.J. Hertzsprung, J. J. Woltjer, and J. H. Oort; P. Ehrenfest and W. J. de Haas were especially influential. It must have been a heady atmosphere for a young scientist in those days. People like Schrödinger, Heisenberg, Dirac, and Madame Curie came to visit. Leiden gave Bart a thorough grounding in classical astronomy and a first-hand view of the excitement and frustration of research. In 1927 he went to Groningen to pursue his doctorate under P. J. van Rhijn, who probably influenced Bart more than any of his other teachers.

In the summer of 1928 the Third General Assembly of the International Astronomical Union was held in Leiden. Two events at this meeting profoundly affected Bart's life and career. First, he met Harlow Shapley, director of the Harvard College Observatory, whom he had long admired. Shapley was impressed with the young man, and Bart was duly invited to come to Harvard the following year. Second, he met an American astronomer, Priscilla Fairfield, with whom he promptly fell in love, even though she was ten years his senior. She did not immediately reciprocate, but Bart pursued her with characteristic vigor and ultimately was successful. They were married in Troy, New York, on September 9, 1929, two days after Bart's arrival in the United States from Holland.

Bart's marriage to Priscilla was without doubt the most important event of his life. They achieved a symbiosis that few couples approach, and, from the time of their marriage, the story of Bart is also the story of Priscilla. He was boisterous, unrestrained, easily swayed by the feelings of the moment. She was quiet, introspective, sensitive to others, inclined to take the long view. Her mind was clear and analytical, and she could reduce complex issues to simple terms. Bart rightly valued Priscilla's judgment and he never made an important decision without first consulting her.

With his marriage, Bart's foundations were in place. His formal training was finished, although he still had to complete his doctoral thesis. The Milky Way was now his main research interest. He had joined the Harvard College Observatory where he would stay for over a quarter of a century. Most importantly, he had a wife whose love and wisdom would be a strong anchor through the rest of his life.

I. THE HARVARD YEARS (1929-57)

In 1929, Harvard was an exciting place to be a young astronomer. This can be sensed from the many memoirs of the time. Bok started as the R. W. Willson Fellow. Harlow Shapley, the director of the observatory, was an energetic and enthusiastic man in his early forties who was a natural leader, and at this time was arguably at the peak of his long and distinguished career. He was one of the great communicators of his time and his skills were closely observed by the younger man. Bart admired Shapley enormously and from then on modeled his own career and administrative style on that of his hero. West Coast astronomy at the time tended to emphasize large telescopes and the pushing of observational techniques to their limits. At Harvard, the research style was more people-intensive, more oriented toward large-scale surveys in the tradition of the Henry Draper Catalog, more directed toward explorations from distant field stations, set up in an expeditionary style to collect data to be sent back home for detailed analysis.

Yet they were not easy years for a newly married couple. It was a time of turmoil and change. The crash of 1929 occurred shortly after Bok's arrival and the Roaring Twenties gave way to the Great Depression. The uncertainty of the times must have affected the atmosphere about him. However, life appears to have been happy and successful for the Boks. Their son, John Fairfield, was born in August 1930. Bart completed his dissertation and successfully defended it in Groningen on July 6, 1932. He became assistant professor in 1933, and daughter Joyce Annetta joined the family late that year.

Bart's doctoral thesis, "A Study of the Eta Carinae Region," is short and unusually lucid. The careful statement of the problem, the precise description of methods and results, and the clear enunciation of conclusions and their limitations are all vintage Bok. The final fifteen pages, which deal with the physics of emission nebulae, anticipate the classic work which Bengt Strömgren independently did some years later.

Bart's research during his first fifteen years at Harvard dealt mainly with the structure and kinematics of the Milky Way system. The observational basis consisted of massive star counts and large numbers of proper motions, radial velocities, and spectral classifications. The aim was to deduce the spatial distribution of the stars in the solar neighborhood and, as far as possible, throughout the galaxy. The analysis was complicated by uncertainties in the stellar luminosity function, by systematic errors in proper motions, by errors on photometry, and more than anything else by uncertainties of the interstellar absorption. His creative activity in stellar statistics culminated in the 1937 monograph, *The Distribution of the Stars in Space*. The labor involved in such studies is enormous; for sheer tedium, star counting remains unsurpassed. Bart organized a "star counting network" at a number of eastern colleges to increase the rate of data acquisition. It was a large project for its time; yet, looking back, it was not a great success, despite the huge effort by Bart and others.

Bart was actively interested in galactic dynamics. His 1934 paper on the stability of moving clusters in a rotating galaxy remains a classic ("The Apparent Clustering of External Galaxies," *Nature*, vol. 133, p. 578). It is probably the most mathematical of his papers. It includes the first demonstration that there is a critical density that a cluster must have in order to be stable against disruption. He concluded that twenty billion years was a reasonable upper limit to the age of the Milky Way system because observed stellar aggregations could not have survived much longer.

One of Bart's early papers provides an interesting footnote to the history of cosmology. Edwin P. Hubble had claimed that there is no great tendency to clustering among faint galaxies and that there is an ultimate uniformity in their distribution. In separate but complementary papers, Shapley and Bok showed that there is widespread clustering. We believe this to be the first demonstration of the fundamental large-scale clumping of matter in the universe.

Bok became an American citizen in 1938. Academic tenure followed in 1939 with appointment as associate professor. These developments made him more secure and more willing to speak out. In the May 1941 issue of the *Harvard Progressive*, he described his philosophy as a citizen and as a scholar. It is an unusual article, revealing an inner uncertainty that was rarely seen in his later writings. He voiced concern about what he perceived to be "the war-like imperialist sentiment" at Harvard and in the country. At this point, he was one of the minority of the faculty who actively opposed American involvement in the war. It is indicative of his character that he stood openly on the unpopular side of a highly emotional dispute. Bart described his feelings about teaching in the same article. To him, the teaching of elementary courses was enjoyable and rewarding. He worked hard on his lectures and delivered them in a lively style that caught the interest of every student. Believing that one bad lecture could alienate a promising student, he never gave bad lectures. He saw the popularization of astronomy among the general public as a natural extension of university teaching. He carried this philosophy to the end of his life.

The Boks spent some months in Mexico during 1941 helping to set up the new National Observatory at Tonantzintla. Bart supervised the installation of the main instrument, a 26-31-inch Schmidt telescope that was patterned closely on a similar instrument at the Harvard Observatory's Oak Ridge (later Agassiz) Station. He was in his element in such pioneering situations, leading the troops with his irresistible enthusiasm expressed in the local language (but always with a heavy Dutch accent). He returned to Tonantzintla for the formal dedication of the observatory early in 1942. He went back again for three months in 1944-45, this time to use the telescope he had helped put into service.

Another notable event of 1941 was the publication of *The Milky Way,* one of the semi-popular series *Harvard Books on Astronomy*. Bart and Priscilla had started work on it in 1937, rewriting the whole text several times before they thought it ready for publication. The spirit of the book is conveyed on the first pages where we are asked to "start off with a quiet evening at home where we shall get out our maps and photographs of the territory we are about to explore" and goes on to express the wish that "when you return, you will not regret having taken the time for such a long trip, and that you will still be curious about what lies beyond . . ." And in a concluding chapter, "It is essential for science that we realize the limitations of each special field of research, but it is also essential that we do not retreat as soon as there is some doubt about the foundations of our reasoning." The book went through five editions, the last appearing in 1981. It has attracted over the years many young, curious minds into the study of astronomy.

Bart took no part in war-related research during World War II. He was not physically qualified for military service, since a mild case of polio in the late 1930s had left his right hand withered and permanently stiff. He states in the A.I.P.

oral history that he would not have been a conscientious objector but had a strong antipathy to any participation that would involve the killing of fellow human beings. Bart's participation was aimed at saving lives through the teaching of navigation as Frances Wright has described elsewhere (*Astronomical Quarterly*, vol 5, pp. 151-56).

Harlow Shapley appointed Bok associate director of the Harvard College Observatory in 1946, and Bart continued in this capacity until Shapley's retirement in 1952. The two men had very similar views on running the observatory. It perhaps would have been wiser to appoint some other staff member who thought less like the director. It was especially unfortunate that Bart shared Shapley's relaxed attitude about the poor quality of Harvard's optical instrumentation, with the feeling that the skillful and determined observer can always overcome the deficiencies of the equipment. Bart achieved full professorship when he became a Robert Wheeler Willson Professor of Astronomy in 1947. He was forty-one years old. During the following years, he worked actively for UNESCO's program in the natural sciences through writing, personal contact with scientists, and behind-the-scenes lobbying with officials and politicians. This experience served him well in the later years in Australia and Arizona when he was endeavoring to radically improve local observing facilities.

In further service to the Harvard Observatory, Bok and his family set sail for South Africa in February 1950 to put into operation the 32-inch Schmidt telescope at the Harvard Boyden Station. The job was completed quickly and successfully. Bok stayed until September 1951, collecting large amounts of data from the southern sky. He was awed by the grandeur of the southern Milky Way. Given his liberal philosophy, he was repelled by apartheid but saw no realistic alternative.

At this point Shapley was preparing to retire. He proposed Bok as his successor, but this was not to be. Other senior members of the Harvard Observatory faculty, notably Donald Menzel and Fred Whipple, also anticipated Shapley's retirement and were suggesting changes in the observatory, the scope of its research programs, and its sources of funding. These positions, developed in Bok's absence, were largely at variance with his views. In particular, he was unwilling to seek funding from the Defense Department and was utterly opposed to classified research at universities. He was a strong supporter of the then-proposed National Science Foundation, which many knowledgeable people at the time thought to have doubtful prospects of success.

With Shapley's influence waning, and lacking the support of the senior members of the observatory, Bok's candidacy for the directorship failed. Donald Menzel became acting director in 1952 and was confirmed as director a year later. Disillusion with the developing changes soon set in. At this time, Bart immersed himself in the then-new science of radio astronomy, administering the construction of a 24-foot antenna and a group of new graduate students. This led to an increasing involvement in the advocacy for radio astronomy at the Washington level, which eventually led to the establishment of the National Radio Astronomy Observatory.

Late in 1955, Bart and Priscilla decided to leave Harvard. The precipitating factor was the imminent transfer of the Smithsonian Astrophysical Observatory to the Harvard Observatory grounds. Bok was strongly opposed, believing the transfer a threat to the integrity of the graduate program. Bok was also angry that he had not been consulted about the matter and felt frustrated that it was no longer possible to do research in the way that he wished. His resignation was announced officially on May 10, 1956, although it was widely known as early as January.

Two months before his announcement, Bok had been offered the directorship of the Mount Stromlo Observatory and the Chair of Astronomy at the Australian National University. This was largely the result of lobbying on Bart's behalf by Joseph Pawsey, then assistant chief of the CSIRO Division of Radiophysics and a powerful figure in the Australian scientific establishment. Pawsey's influence was decisive, for another strong contender had the support of Richard Woolley, the outgoing director. Although Pawsey held Bart in high esteem, personally and professionally, the main reason for his forceful advocacy was the belief that Bart would be far more supportive of radio astronomy in Australia than the other candidate.

When Bart and Priscilla set out for Australia in January of 1957, they left behind more than a professorship and a home. At Harvard, Bart's life had been that of a typical academic, with thoughts and activities centered mainly on his research, his students, and his family. The children had now grown up and Bok himself had begun to branch out with his efforts on behalf of UNESCO and his participation in the establishment of the National Radio Astronomy Observatory, though such things were not yet central to his daily pattern. This was to change. In Australia, he was to play on a much larger stage, and his style would grow to fit it.

II. THE MOUNT STROMLO YEARS (1957-66)

With characteristic enthusiasm, Bok began his thinking about the work to be done in Australia well before the official start of his appointment. In September 1956 he visited the Mount Stromlo Observatory to review first-hand the instrumentation programs and to discuss them with staff members. He spoke at some length with the administrators of the Australian National University (ANU) about his plans for the future of the department of astronomy and the equipment and programs of the observatory. Bok stressed the need to get the telescopes into first-rate operating condition, and then to outfit them with the most modern of operating instruments. In a detailed letter to the ANU he outlined details of new darkrooms to be built, dome repairs needed, refurbishment of staff quarters on the mountain, and a prompt paving of the mountain road to encourage visitors to Mount Stromlo. He further announced his plans to attract more graduate students and to tour the country widely, carrying the message of astronomy to young people at the schools and colleges. Finally, he pointed out the need to establish a field station at a better observing site in order to escape the prolonged spells of poor observing weather at Mount Stromlo. All this was done before he arrived! Bart and Priscilla took up residence at Mount Stromlo in February of 1957. For Bok this was a chance to further his galactic structure research, to work with the radio astronomers at CSIRO, and to build a major astronomy department for Australia. He was the right man, at the right place, at the right time.

Australia was still something of a scientific backwater in 1957 when the Boks arrived. An exception was the CSIRO Division of Radiophysics in which Pawsey had built one of the world's premier radio astronomy groups. The opportunity to work with these scientists was no doubt attractive, but it was the lure of the southern Milky Way and the opportunity to do galactic structure research in the way he wanted to do it that were irresistible. Under the direction of Woolley, the Mount Stromlo Observatory had become an active research center with a 74-inch telescope in competition only with an older telescope of similar size at the Radcliffe Observatory in South Africa. Woolley had brought the observatory into the recently formed Australian National University as part of the Research School of Physical Sciences. The school was headed by Mark Oliphant who had returned to Australia from a distinguished career at the Cavendish Laboratory and the University of Birmingham to help build a research department that would give Australia its own independent scientific capability and would keep talented Australian scientists at home. Oliphant and Bok worked well together and Bart received the support that enabled him to fulfill Woolley's plan to mould Mount Stromlo into an internationally respected institution.

Until jet air travel to Australia came into regular use, scientific visits to Australia still had somewhat of an expeditionary character. People did not come casually or easily. After 1958 one could reach Australia almost overnight from anywhere in the world and international contacts became much easier to establish. Bart came to Australia with his own extensive professional network and gave astronomy at Mount Stromlo stronger international links than it had enjoyed up until that time. Several well-known American astronomers, such as Gerry and Katherine Kron, Frank Bradshaw Wood, Paul Hodge, and Lawrence Aller came on leave; both Walter Baade and Harlow Shapley came on extended visits; others, like Th. Dunham, joined the staff. The Uppsala Schmidt telescope was set up and operated by a succession of Swedish observers, some of whom stayed to form long-term links with the country.

As Pawsey had anticipated, Bok's presence at Mount Stromlo was successful in fostering a strong relationship between Mount Stromlo and the CSIRO radiophysics group. Radio astronomy at this time was in the period of rapid growth and development that often follows the introduction of a new technique. Pawsey had brought together a group of young radio astronomers who were actively pushing both detection and resolution of radio sources to ever finer limits. Christiansen, Kerr, McGee, and Mills come immediately to mind. Bolton was shortly to return from the United States. Techniques of both optical and radio observation were dependent on each other and this was emphasized in Bok's research direction. Joint visits, colloquia, and meetings were arranged.

From the start, Bart and Priscilla had a deep interest in the education of young Australian astronomers. Bart lectured extensively at the state universities, astronomical clubs, and church groups in his inimitable style, which unveiled the excitement and exhilaration of astronomical research to impressionable minds. Many were swept up by his enthusiasm. He initiated a summer vacation program that attracted promising undergraduate students to Mount Stromlo in order to experience first-hand the nature of scientific research, and in turn to be watched as possible future graduate students. Even those who did not take up astronomy as a profession carried Bok's message out to the community at large. Arrangements were made to allow some research students to work at the Australian National Radio Astronomy Observatory, especially after the 210-foot radio telescope had been commissioned in 1961. He took the whole graduate program very seriously, with required courses presented at Mount Stromlo by the observatory research staff. His own lectures combined the clarity of his exposition with the rigor of blackboards full of equations and diagrams supplemented by extensive outside reading. He showed an intense, individual interest in the scientific development of every one of the students and was always available for advice and wise counsel. When things were not going well, as happens often in scientific research, a visit to the professor was all that was needed to get one's batteries recharged and enthusiasm generated anew.

Visitors' nights on Mount Stromlo were always brilliant and memorable occasions with the director acting as ringmaster for all the smaller shows going on around him. They were carefully planned with each staff member and student having specific duties to be carried out at specific times. Most importantly, they served to generate grass-roots support for astronomy in the community in general and among legislative leaders in particular. This activity proved to be exceedingly useful later on when new projects such as the development of a field station and the quest for the support of a major large telescope were initiated.

With the larger administrative load, Bok was less involved in large scientific projects than before. Priscilla remained an active collaborator and it is due to her that so much could be done. Her responsibility was the reduction of observations made at the telescope, often on the previous night. Priscilla had a critical eye for separating good from bad measurements and bears much of the credit for the enduring nature of their joint work. Galactic structure studies such as that of Selected Area 141 were accomplished with the aid of highly competent research assistants such as Jane Basinski. Other projects involved students. Then there was the occasional short paper such as the letter to *Observatory* entitled "The Spiral Structure of our Galaxy," which, in pointing out the existence of an extended spiral structure in the constellation of Carina, oriented the subject in a completely new direction. Following the Shapley tradition, the Magellanic Cloud always received good press from Bart. He and Priscilla did particularly valuable photometric work in a number of prominent clusters and associations, and in drawing people's attention to the unique place that the Magellanic Clouds occupy in galactic and extragalactic research.

Early in his directorship, Bart realized that the rapid growth of Canberra would soon threaten Mount Stromlo as a dark observatory site. Already a number of site surveys of the

Australian continent were under way and Bok supported these fully. The search was long and time consuming, partly because Australia had no obviously superior sites such as those that were quickly recognized in Chile and southern Africa. The Mount Stromlo participation in the project was placed in the charge of Arthur Hogg but Bok maintained an active interest in it, personally inspecting most of the sites that were proposed. The final selection resulted in the founding of Siding Spring Observatory. Bok's last major task was to oversee the construction of the first new telescope for the site, a 1-meter Ritchey-Chrétien reflector. Siding Spring has since become the location of the 3.9-meter Anglo-Australian telescope and is now Australia's major optical observing facility.

Bok dreamed of a 200-inch class telescope in Australia. He had worked toward this end, both in behind-the-scenes discussions in England and the United States, as well as with his exhortations to his scientific colleagues in Australia. He felt that he had made considerable progress, especially in convincing influential politicians in Australia. But his penchant for speaking out boldly and generating frank, open discussion was an unusual one in Australian scientific politics of the time, which was generally conducted very much behind closed doors by figures of the establishment. Today, it would hardly shock at all. He felt that in order to get things done, it was first necessary to get things moving. He was outspoken in his conviction and angry at what he considered unnecessary delays. It was unfortunate that the Anglo-Australian telescope never became a reality during his directorship. He did, however, do much of the preliminary work to make it possible and took great pride in later years when it was completed.

Bart and Priscilla's return to the United States in 1966 was no surprise. He had told his family that he would return at about this time. Even before the move to Australia, Bart had been an adviser to the University of Arizona on all astronomical matters. A position at the university had been talked about for many years and had actually been offered in 1964. Bok, however, wished to remain in Australia for two more years to try to get the large telescope project under way. It was also common knowledge that the Boks had long before planned to retire to the southwestern United States. Yet, Bart felt considerable uneasiness at having been out of the mainstream of U.S. astronomical thinking and action and was uncertain as to whether he could recover an influential position. But he was one of the natural activists of this world and was temperamentally incapable of occupying a passive position. His fears were essentially groundless.

III. THE STEWARD OBSERVATORY YEARS (1966-70)

In 1966, the Department of Astronomy (Steward Observatory) at the University of Arizona was still relatively small and very much in the shadow of the generously funded Kitt Peak National Observatory, just across the street, where development of the 150-inch telescope was in progress. Astronomy at the University of Arizona has grown enormously since then and it is ironic that, in the present time of tight funding for national facilities, the shadow now crosses the street in the opposite direction. Tucson in the 1960s was an exciting place to do astronomy. With the growing facility on Kitt Peak, the presence of the Lunar and Planetary Laboratory under G. P. Kuiper, and then, the arrival of B. J. Bok, it became a center through which astronomers were always visiting, whether for observing, for committees, or for scientific meetings.

During the previous several years, both the National Science Foundation and the University of Arizona sensed that, with the development of Kitt Peak, it would be advantageous to strengthen the department of astronomy at the university. Leland P. Hayworth, then director of the foundation and an old friend of Bok's, felt that the staff at the National Observatory should have access to young people who were part of a strong graduate student program. The University of Arizona administrators recognized that here was a chance to set up one of the major astronomy departments not just in the country but in the world. Bok's predecessor, Aden Meinel, had initiated a Science Development grant proposal to the NSF in which Bok was also closely involved. The grant came through shortly after Bok's arrival. Not only did this involve the construction of a major new instrument, a 90-inch telescope, for the observatory, but also a large increase in the size of the teaching staff. Bok was able to convince the university administration and the National Science Foundation that, for the department to develop as it should, the size of the staff should be doubled from six to twelve.

Bok always felt strongly that the bread and butter of an astronomy department, particularly in a state-funded university, lay in the teaching of young people outside astronomy. Beyond the training of graduate students, he believed it was one of the most important obligations and perhaps a one-time opportunity to make contact with people who would become leaders in the general community. He himself taught liberal arts Astronomy I courses until his retirement. Under his leadership, the graduate student program grew in size and prestige until, by 1970, it was ranked fifth in the nation.

Although not a practical instrumentalist, Bart showed repeatedly throughout his career an outstanding intuition of those techniques that were about to become important and was at his best in directing his energies into supporting people who could and did make them available for general users like himself. Already mentioned was his role in the early history of radio astronomy. The completion of the 90-inch telescope in 1970 coincided with the development of image tube intensifier detectors, making possible programs that until then had been beyond the light grasp of the largest telescopes in the world. He recognized early the coming importance of infrared detectors in astronomy. Observing continued to be an important part of his life, and he was always available to try out the newest experimental emulsion from Kodak. Trips to Cerro Tololo Inter-American Observatory in Chile were a major part of the activity. He and Priscilla delighted on these occasions in being back observing together their beloved southern sky. He went so far as to write on one occasion that, "Our visits to Cerro Tololo marked about the best days in the lives of Priscilla and Bart Bok."

Bart quickly became active in national astronomical organizations. He was delighted, and somewhat surprised, to be elected to the National Academy of Sciences in 1968, shortly after his return from Australia. He was a trustee of Associated Universities, Inc. (1968-71), vice-president (1970-71) and then president (1972-74) of the American Astronomical Society, and a board member of the Astronomical Society of the Pacific (1977-80). Although active on several NSF committees and panels, he was uneasy in his relations with the Association of Universities for Research in Astronomy (AURA) which operated the national optical observatories. While he was one of the strongest supporters of the Kitt Peak and Cerro Tololo observatories, he felt that the overall management techniques were not as good at that time than they could have been. His involvement with the National Radio Astronomy Observatory was both deeper and more satisfying.

Bok's research during the Steward Observatory years remained strongly directed towards the mapping of spiral structure in the galaxy. He was a strong propagandist for the density wave theory of galactic spiral structure as formulated by C. C. Lin and Frank Shu and urged that it be employed in mapping the spiral structure in our own neighborhood. Toward the end of his life, he expressed increasing doubts as to whether this theory was adequate to supply unique solutions to the problem. At about this time, his interest in dust globules and their relation to star formation was being renewed by the discoveries of molecular line radiation in the dark obscuring clouds in the Milky Way.

His activity on the national and international scene markedly increased following his resignation as director of the Steward Observatory in 1970 while still remaining professor of astronomy. With these outside interests, he was free from the temptation to interfere with the policies of his successor while at the same time he could continue his research on the wider stage. As one of the most active presidents of the American Astronomical Society, he took his duties very seriously, feeling a special responsibility toward the younger members. Each year there was an address to the membership on the current state of the profession and this was subsequently printed and widely distributed. He did not hesitate to speak out about unpopular subjects such as the tightening employment situation and inadequate funding by government agencies, often to the distress and discomfort of fellow council members. He urged colleagues to take a firm stand in the community about such subjects as astrology and pseudo-science. There was nothing passive about this man.

International recognitions came often. He became a vice-president of the International Astronomical Union in 1970 for a six-year term but felt the need to resign from this position in 1974 because of the declining health of Priscilla. It was touching to his friends to see the single-minded devotion to Priscilla in the last months of her life. When she died on November 19, 1975, he was desolate.

EPILOGUE: LAST YEARS

During the year following Priscilla's death, many of Bart's colleagues were seriously concerned about his health and well-being. Uncharacteristically, he withdrew into himself and was seen little outside of his home. The fact that he was so sadly missed at the many astronomy gatherings in the Tucson area was an indicator of the frequency and intensity of his participation in former years. It was at this time that he prepared a touching memoir of Priscilla that was distributed to their many friends. Towards the end of 1976, he began to involve himself again in the astronomy that he loved. Apparently, this was triggered by a peripheral involvement in the planning for the NASA Large Space Telescope. His interest was aroused and for the rest of his life he became a constant high-profile publicist for what is now the Hubble Space Telescope. Nobody was allowed to overlook that this was the instrument that was going to revolutionize astronomy. Not to participate meant to be left out of mainstream astronomy at the end of the twentieth century. Perhaps he somewhat underestimated the tremendous demands that this instrument will make on ground-based astronomy, but he made his point.

In his scientific interests, there was a strong break from his early interest in the overall spiral structure of the Milky Way. Realizing that observational errors smoothed out the mapping of its features at distances greater than several thousand light years, he believed that future progress in the area could best be made through high-resolution studies of external galaxies. His own work, prompted by Priscilla's advice shortly before she died, turned to the study of low-mass star formation in the small dust globules that he had studied at Harvard many years before. He was aware that in these places, molecular gas and dust was able to accumulate, shielded from the disrupting ultraviolet radiation that fills interstellar space. He developed a strong interest in the techniques for detecting this material. He was excited at the development of infrared observation that enable the embedded, newly formed stars to be precisely located and studied.

Travel became a big theme of his last years, in part to escape the loneliness of being at home, in part because of his naturally gregarious nature. He lectured frequently and it was amazing to his friends that, even as his physical health declined, his talent for communicating the excitement of astronomy remained at an incomparably high level. He could still fill a room and hold its audience spellbound. He was always a good synthesizer and this showed clearly in his lectures, review articles, and semipopular contributions. During these travels, many young people were attracted to astronomy through his encouragement and enthusiasm.

Further awards came in acknowledgement of his lifelong service to astronomy. The Bruce Medal of the Astronomical Society of the Pacific was awarded to him in 1977 and he was the Russell Lecturer of the American Astronomical Society in 1982. The Russell lecture itself was delivered in 1983 and published posthumously in the *Astrophysical Journal* ("Some Current Trends in Milky Way Research," *Astrophysical Journal*, vol. 273, pp. 411-20). It was an exuberant survey of the exciting parts of present day astronomy (as he saw them), delivered in classic Bok style.

Observing trips to Chile continued. One produced the famous "St. Valentine's Night" picture of the Gum nebula globule, which contains a beautiful demonstration of the isolated formation of a star of approximately solar mass. This widely circulated photograph of what is indeed a classic example, has had a strong influence in studies of star birth.

He continued to be active in National Academy affairs. Notably, he was an outspoken campaigner on human rights issues using to the full the international friendships developed over a lifetime to argue cases of less influential younger colleagues who were in difficulties.

On the morning of August 5, 1983, Bok passed away suddenly, still planning and enjoying life to the full. On the short term, a lively weekend with friends was on the agenda; on the longer term, an extensive European tour highlighted by a conference on the Magellanic Clouds. Up until the end, it was a full and generous life that led to the enrichment of many others. We owe him a tremendous debt, which, in gratitude, we can try to repay by following his example.

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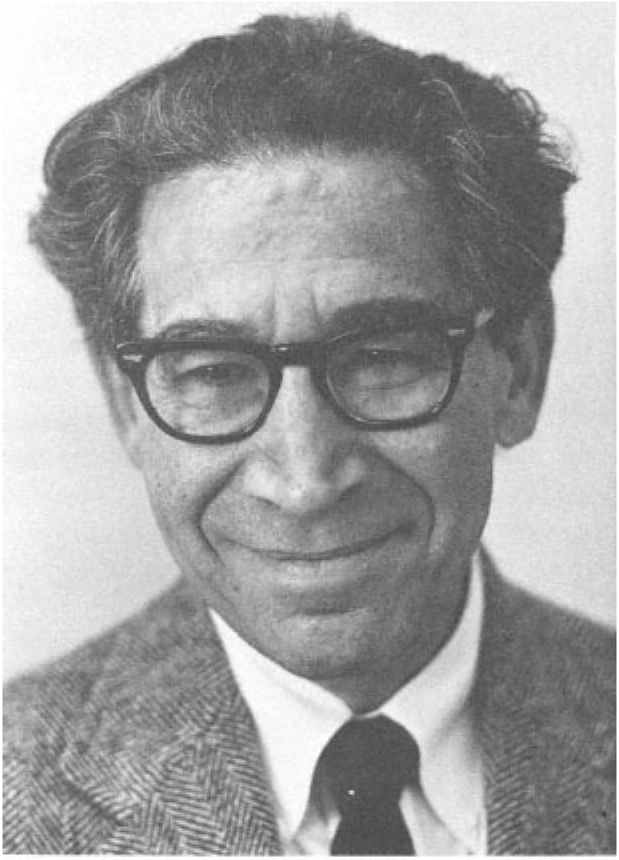

LEON FESTINGER

May 8, 1919-February 11, 1989

BY STANLEY SCHACHTER

ONE OF THE LAST TIMES Leon Festinger saw his father was in a nursing home in Brooklyn. The old man had been part of that great emigration of East European Jews in the years before the First World War. He left Russia a radical and an atheist and remained faithful to these views throughout his life. He was very sick at the time of Leon's visit, bedridden and virtually helpless. During this visit, he leaned toward his son and said, "You know Leon, I was wrong. All my life I was wrong—there is life after death." Puzzled, Festinger asked him what he meant and, pointing around the room, his father answered, "This—this is life after death."

In 1988 Festinger became ill with a cancer that had metastasized to the liver and the lungs. He dealt with his cancer as a research problem. He read the literature, spoke with the experts, weighed the possible side effects of treatment, calculated the odds, and decided, untreated, to die. And in a few months he was dead. The intervening months were relatively peaceful and, though toward the end he was wasting away, painless. He worked, he wrote, he saw his friends, and, when it became clear that he could no longer go on, he died.

The memorial service at the New School was, as such dour events go in academia, a remarkable occasion. Virtually all of his old students and many of his former colleagues and collaborators from all over the country, and indeed the world, flooded the auditorium. The eulogies were lavish and well deserved, for Leon Festinger was one of the most important psychologists of our time.

Festinger was born in Brooklyn, New York, on May 8, 1919, to Alex Festinger, an embroidery manufacturer, and Sara Solomon Festinger. He went to Boys' High School, City College, and, for graduate study, to the University of Iowa, where he worked with Kurt Lewin, a Gestalt and Field theorist who had fled the Nazis to arrive in an America where the psychological establishment, though hardly a dictatorship, was ruled by an even more dogmatic group, also convinced that it had the Truth, called Behaviorists.

Lewin and his students probably did more than any other group of scientists to mold psychology into an enterprise concerned with more than stimulus-response connections but with dynamic processes involving perception, motivation, and cognition. They did so quietly and without doing battle but largely by example—repeatedly demonstrating that it was possible to work with experimental and theoretical precision on problems of consuming human interest such as decision making, ambition, tension, level of aspiration, and the like.

Festinger honed his talents in his first work with Lewin. As an undergraduate working with Max Hertzman (Hertzman and Festinger, 1940), he had already demonstrated considerable skill working with Lewinian ideas. At Iowa, though Lewin's interests had shifted to social psychology or, as he called it, "group dynamics," Festinger, uninterested then in social psychology, continued to work on older Lewinian problems. He also turned his considerable mathematical talents to statistics and developed several of the earliest nonparametric tests (Festinger, 1946). On completing his degree, he worked for two years as a research associate at the University of Iowa and then, during the war, for two years as senior statistician for the Committee on Selection and Training of Aircraft Pilots at the University of Rochester.

In 1945 he rejoined the Lewinian group as an assistant professor at the newly formed Research Center for Group Dynamics at the Massachusetts Institute of Technology. To round out the way stations of his academic career, he moved with the Group Dynamics Center to the University of Michigan in 1948, then to the University of Minnesota in 1951, on to Stanford in 1955, and, finally, in 1968 to the New School for Social Research where he was the Else and Hans Staudinger Professor of Psychology. In New York he met and married Trudy Bradley. By an earlier marriage he had three children, Catherine, Richard, and Kurt.

It was at MIT that Festinger's interests turned to social psychology and he launched a series of studies of social influence and communication that became a turning point in the field, for they demonstrated that it was possible to work experimentally and with theoretical rigor, on nonbanal problems of considerable social and psychological importance. This work started as almost an accident. Festinger had been directing a study of housing satisfaction in MIT married-student housing projects commissioned by the university's Department of Architecture and City Planning. The study involved the conjoint use of interviews about attitudes to MIT housing and of sociometric questionnaires, that is, measures of the social relationships within the various projects by use of questions such as "Which people here do you see most often socially?" In addition to the material of interest to the housing people at MIT, several facts emerged powerfully from the data. First, it turned out that those groups of students who were sociometrically close tended to have highly similar attitudes on the various housing questions. Second, it appeared that those students who had deviant attitudes on the housing questions tended to be social isolates, that is, they were rarely named in answer to the sociometric questions.

These facts of the housing study were purely correlational. One could speculate endlessly, but one could say nothing about causal direction or about mechanism. Worrying through the meaning of these facts led Festinger and his students to the development of an experimental laboratory program of research that many consider the birth of systematic experimental social psychology. Their problems were many; they had to devise means of manipulating such ephemeral social variables as affection, social cohesion, group structure, deviancy, and the like; they had to devise controls to rule out alternative explanations; they had to invent means of unobtrusively measuring the effects of their manipulations on variables such as influence, exerted and accepted, and communication, its direction and intensity.

Along with Kurt Back, Harold Kelly, and John Thibaut, I was lucky enough to work with Festinger at this time, and I think of it as one of the high points of my scientific life. He was a wildly original and provocative scientist. It was a time of excitement, intense involvement, discovery, and fun. Working with Festinger was always fun. He was a great kibitzer, and he loved puzzles, problems, and games. He had little tolerance for banality or for tired ideas. We devised laboratory experiments for studying phenomena that, until then, no one had conceived of as manipulable or measurable. We discovered things no one had known before—virtually a sine qua non before Festinger thought an experiment worth doing. Festinger (1950) synthesized all of this work in his first theoretical paper in social psychology—a seminal paper concerned with informal social communication and the process, via social comparison, of establishing the correctness of one's beliefs.

Festinger's research career continued at Michigan and Minnesota, where, in a theoretical paper (Festinger, 1954) that was a tour de force, he extended his theorizing about beliefs, attitudes, and communication to the evaluation of abilities. With the support of several ingenious experiments, he demonstrated that, as with attitudes, and beliefs, the evaluation of one's abilities was also a socially determined process.

It was shortly after publication of this body of work in the 1950s that *Fortune* magazine nominated him as one of America's ten most promising young scientists, not psychologists, but scientists—an honor that, given its source and his political bent at the time, he managed to keep a well-hidden secret. No matter what his opinion of this particular honor, this was a prescient set of selections, for most of his fellow nominees went on to win a Nobel prize. It was this same work that led to Festinger's receiving the Distinguished Scientist Award of the American Psychological Association in 1959 and to his election to the American Academy of Arts and Sciences in that same year. He became a member of the National Academy of Sciences in 1972 and of the Society of Experimental Psychology in 1973. The honors continued throughout his career. In 1978 he received an honorary doctorate from the University of Mannheim, in 1980 he was named Einstein Visiting Fellow of the Israel Academy of Sciences and Humanities, and also in 1980 he received the Distinguished Senior Scientist Award of the Society of Experimental Social Psychology.

Festinger turned next to the development of a set of ideas for which he is perhaps best known in psychology— the theory of cognitive dissonance (Festinger, 1957). In a way the ideas of the dissonance work were a further and more basic development of his thinking about the social determinants of the evaluation of beliefs and abilities. The key to his earlier ideas was the hypothesis that, when there were discrepancies of opinion or ability among the members of a group, pressures arose to reduce such discrepancies. Dissonance theory was an attempt to determine, at a more basic, purely cognitive level, the origin of such pressures. In essence, dissonance theory was startlingly simple. The key hypothesis is that when incompatibilities exist between two or more ideas or cognitions, pressures will arise to reduce the discrepancy.

This was hardly a new idea and, in one form or another, had already been proposed by a number of psychologists now known as "balance" theorists. What Festinger did with the idea, however, is an illustration of his almost unique genius. He pushed this idea just about as far as it could go, examining and testing its implications for a breathtaking variety of phenomena. These included an experimental examination of the cognitive consequences of forced compliance; studies in both rats and humans of the effects of insufficient reward; a field study of the effects of being wrong on the proselyting efforts of a millenial group; and on and on in a body of work that Edward Jones (1976) described as "the most important development in social psychology to date."

It was marvelous work; however, Festinger moved on. Boredom was anathema, and the moment things got dull or he found that he was repeating himself, doing some trivial variation of a spent idea, he changed his interests. Starting about 1963, while at Stanford, he developed an interest in the visual system and perception. He worked during this period on a variety of problems related to eye movements, efference, and the conscious experience of perception as well as on neurophysiological coding for the perception of color. I confess that my expertise is such that I dare not fake an attempt to evaluate this research nor, in fact, am I able even to present a coherent synopsis of his work in these areas. I do note, however, that this work drew much attention, stirred much controversy, and attracted a talented group of students.

Finally, about 1978-79, some eleven years after he came to the New School, Festinger closed his laboratory and abandoned experimental psychology altogether. His explanation, in his own words, was (Festinger, 1983):

> Four years ago I closed my laboratory which, over time, had been devoted to studying ever narrowing aspects of how the human eye moves. It is natural for me to talk as if the laboratory was at fault, but a laboratory is only a collection of rooms and equipment. It was I who conceived of and worked on narrower and narrower technical problems.
>
> That is not a proper occupation for an aging man who resents that adjective. Young men and women should work on narrow problems. Young people become enthusiastic easily: any new finding is an exciting thing. Older people have too much perspective on the past and perhaps, too little patience with the future. Very few small discoveries turn out to be important over the years; things that would have sent me jumping and shouting in my youth now left me calm and judgmental and my lack of enthusiasm kept reminding me of that despised adjective, aging.
>
> Having a critical perspective on the recent past [was] debilitating in other ways also. I have been actively engaged in research in the field of psychology for more than 40 years . . . . Forty years in my own life seems like a long time to me and while some things have been learned about human beings and human behavior during this time, progress has not been rapid enough; nor has the new knowledge been impressive enough. And even worse, from the broader point of view we do not seem to have been working on many of the important problems.

And so, despite his marked success as an experimentalist, Festinger moved on. His first foray outside the laboratory involved an examination of what one might learn about the "nature of man" from archeological data. He visited a number of archeological digs with French and Israeli specialists and began a systematic examination of what one could deduce and infer about man and the structure of primitive society from archeological evidence. He published his speculations in 1983 in a book called *The Human Legacy*. It is an intriguing volume in which a first-rate mind trained in one discipline applies itself to the data and problems of another discipline and raises questions that, to my mind, provide one of the few nonbanal examples in the social sciences of the potential of cross-disciplinary work. For example, he notes that in some digs there is huge variability in the quality of workmanship of artifacts such as arrowheads, while in other digs such artifacts are all of similar high quality. This leads him into fascinating speculation about the development of the division of labor in primitive society. Similarly, other artifacts lead to speculation about the development of religious technology and of the role of play and of games in mankind's history. In its own way it is a marvelous book whose reception in Festinger's own professional circles bemused him no end for he was often asked by his fellow psychologists, "But what does this have to do with psychology?"

From what might be called psychosocial-archeology, Festinger moved on to a deep interest in the history of religion. He worked closely with a number of medieval and Byzantine church scholars, and eventually his interest focused on the differences between the Eastern and the Western or Roman church and the role such differences might have played in the differential development and acceptance of material technology in these two parts of the Roman empire. Festinger died before he could publish this material, but he made the same profound impression on the medieval historians as he had made earlier on the psychologists with whom he worked. Indeed, a recent book called *Papacy, Councils and Canon Law in the 11th-12th Centuries* is dedicated by its author Robert Somerville (1990) to the memory of Leon Festinger—surely the only time in intellectual history that a specialist in canon law dedicated a book to an unrelated social psychologist.

It was an astonishing intellectual career. Whatever area he touched, he enriched. He discovered things no one knew before; he made connections no one had made before, and he did it all with an éclat and an elegance that compel one to think of his work in aesthetic as well as scientific terms. Indeed, Zajonc (1990) has compared Festinger to Picasso, and Zukier (1989) has compared him to Van Gogh. The psychological world is a different place because he lived.

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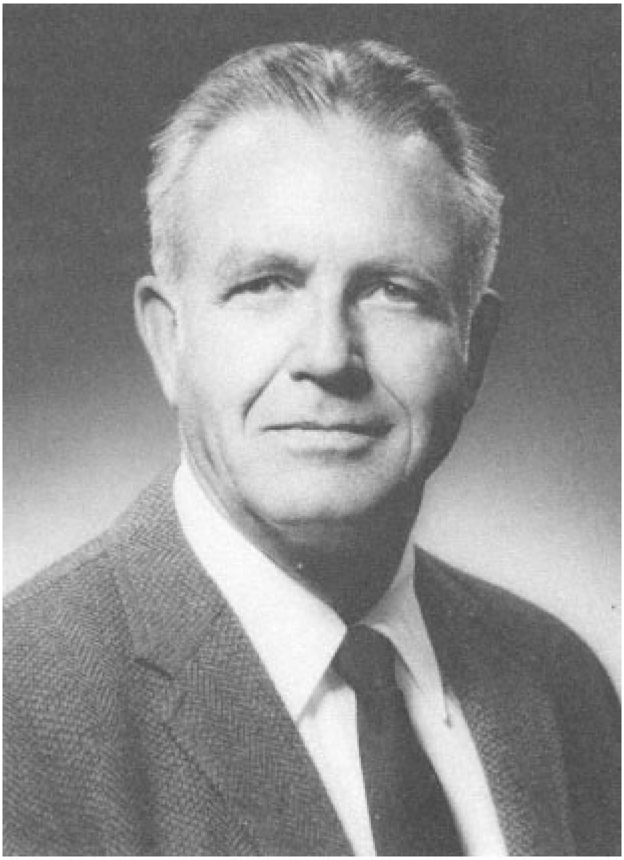

DAVID TRESSEL GRIGGS

October 6, 1911-December 31, 1974

BY IVAN A. GETTING AND JOHN M. CHRISTIE

LIKE MANY CONTEMPORARY scientists, David T. Griggs became enmeshed in a dual career: (1) scientific research and teaching in the field of geophysics and (2) the application of science to national defense.

David Griggs was born October 6, 1911, in Columbus, Ohio, the son of Robert Fiske Griggs and Laura Amelia Tressel Griggs. The ancestors of both parents derived from English immigrants who came to America prior to the American Revolution.[1] Robert stemmed from a long line of Congregationalists and Quakers; Laura was from a Methodist and Lutheran line—in fact, her father had been a minister. David had a younger brother and two sisters.[2] His parental home was a citadel of Christian values, based on love, fidelity, and truth.

While his early childhood was spent in Ohio, Dave's high school days were spent in Washington, D.C., where his father had become the first professor of botany at the George Washington University. In the early part of the twentieth century, botany in the United States had not reached a high scientific level, and Dave's father was instrumental in bringing it into the science realm. His research included expeditions to Puerto Rico, Guatemala, Texas, and Alaska. It was during one of these expeditions to Alaska, in 1916, supported by the National Geographic Society, that near Mount Katmai he discovered the Valley of Ten Thousand Smokes. The highest mountain in the area was subsequently officially named Mount Griggs.

It was natural for Dave to spend his first year of college (1928-29) at the George Washington University. His interests were in things mechanical and in physics. His next three years of undergraduate work were at Ohio State University (1929-32). In the summer of 1930 he persuaded his father to take him on one of his expeditions to the Valley of Ten Thousand Smokes. "This Alaskan experience and the encouragement he received from a gifted and enthusiastic teacher, Professor Edmund M. Spieker, led him to choose for his life work the application of physics to the problems of the earth."[3]

Before going on to a year of graduate work at Ohio State, Dave was elected to Phi Beta Kappa, the Mathematics Honor Society, and Sigma Xi. The next year (1933), he served as an assistant in the Geology Department at Harvard, and the following year he was appointed a junior fellow of the recently created Society of Fellows.

The Society of Fellows at Harvard University began operating in 1933, just one year before Dave's appointment. "The concept of the Society was largely the creation of two men: Lawrence Joseph Henderson, then professor of biological chemistry, and Abbott Lawrence Lowell of the old Boston family, president of Harvard. They sought a solution to the problem of advanced studies and research opportunities for young men at the university without the often debilitating requirements of formally proceeding to a doctor's degree."[4] Dave was reappointed after the first term of three years and served the following two years until he was called to support the war effort against Nazi Germany. Among his colleagues in the society were to be found many scholars who continued to distinguish themselves in later years: James Baker, renowned astronomer and optical designer; John Bardeen, twice Nobel prize winner in physics (transistors and theory of superconductivity); James Fisk, president of Bell Telephone Laboratories; Henry Guerlac, historian of science; Paul Samuelson, Nobel prize winner in economics; Stanislaw Ulam, mathematician and coinventor of the hydrogen bomb; Robert Woodward, chemist and Nobel prize winner (synthesis of quinine); Willard Van Quine, mathematical logician; Fred Skinner, psychologist; Garrett Birkhoff, mathematician; E. Bright Wilson, chemist; and others.[5] Dave Griggs's scientific contributions to geophysics during his tenure as a junior fellow are described in the next section-but here a few words about Dave as a person.

A single junior fellow was assigned to one of the houses where he lived with resident students and dined with distinguished faculty members who were also assigned to houses as tutors. Thus, Dave Griggs and one of the authors (I.A.G.) were assigned to Leverett House. There were four suites on each floor, and Dave and I found ourselves neighbors. While Dave's principal experimental research work was done at the Jefferson Physical Laboratory (his laboratory was flanked by Professor Bridgman's laboratory on one side and my laboratory on the other), his living room at Leverett House was always crammed with experiments: long-term deformation (creep) of rocks under applied loads, mountain-building models, and related experiments. Every opportunity that offered itself for mountain climbing (or skiing for that matter) was seized. Another resident on "our floor" was Agnew Bahnson, a well-to-do (by our standards) student who owned a Buick roadster. In the summer of 1936, Agnew and Dave set out to go mountain climbing in the Caucasus Mountains, a mountain chain linking the Caspian Sea and the Black Sea—a new range to climb and study. The trip is worth mentioning in this biography because it tells a lot about Dave and because the accompanying events had a profound impact on the rest of his life.

> The road stretches straight ahead, a long ribbon of concrete, the artery of the North Hungarian plain. We are heading eastward to Bucharest, Odessa, Tiflis, and the mysteries of the Caucasus. We stretch out to make up some of the time we have lost. The cyclist appears from nowhere, immediately ahead of us. Bon (Agnew) swings the wheel quick as light for the bare chance that we can swerve enough to miss him. We almost miss him and then I see the tree looming up. There is nothing more—all is quiet. Bon is no longer in the seat beside me. I see him stretched out in the middle of the road beside the car. Then I became conscious of a searing pain in my legs. They are caught between the seat and the radio as the body of the car telescoped. I can't even move my feet. Then I see the blood everywhere. No wonder I can't move them. My legs are like sacks of flour. Those legs that served so well going up the Matterhorn only a few weeks ago—are they no longer mine?[6]

After many delays and hardships, Dave and Agnew were transported to Budapest. Agnew had brain concussions and hemorrhages from which he fully recovered. Dave had compound fractures in both legs, the left knee was dislocated, and he had serious lacerations on both elbows. The Budapest surgeons recommended amputation. Dave would not accept the advice. He was moved to Vienna, where the same recommendations were repeated. Finally, his mother, Laura, brought him back to the United States on the *Queen Mary*. Dr. Smith-Peterson, at the Massachusetts General Hospital, performed innumerable operations, and much time elapsed. Dave's indomitable spirit prevailed, and in a few years he was not only walking but skiing in the mountains he loved so much. These characteristics of courage, faith, and endurance were his hallmark. To these must be added a disregard for personal risks. Nevertheless, when the United States entered World War II, Dave was found physically unfit to serve his country in uniform!

The automobile accident had another critical impact on Dave's career. Agnew's father had carried special insurance because of the hazardous nature of the contemplated trip to the Caucasus in the southern USSR. The insurance company awarded $15,000 to Dave as compensation for his suffering. With this money he purchased a Luscumbe airplane and became a pilot. So while at first he could not climb his mountains, now he could fly around and over them. And it was this situation that brought Dave, as pilot and owner of the Luscumbe, into the war effort.

As a junior fellow at Harvard, Dave adapted high-pressure techniques developed by Nobel laureate Percy Bridgman and began his systematic experimental studies of the mechanical properties of rocks and the effects of environmental factors, such as increased pressure and the presence of fluids, on the mechanics of deformation of mineral crystals and rocks. He designed and built new apparatus for long "creep" experiments at room temperature and pressure and in the presence of fluids. He improved apparatus for deformation experiments at high pressures, of the type pioneered by Th. von Karman (1911). He also devised ingenious scale models to investigate the possibility that mountain-building processes might be driven by thermal convection currents in the solid but deformable rocks of the earth's mantle ("substratum"), as was advocated by Arthur Holmes, C. L. Pekeris, F. A. Vening Meinesz, and H. H. Hess.

About a dozen important papers published between 1934 and 1941 established the relevance of experimental rock deformation and scale model studies in geology and geophysics. Most notable among these were Dave's papers on the creep of rocks, which provided a remarkably modern insight into the physics of slow deformations and of the effects of confining pressure, stress, time, and the presence of solutions on the solid-state flow of rocks. In his paper, "A Theory of Mountain-Building" (1939), Dave argued, on the basis of his experimental data and scale models, that thermal convection currents in the earth's mantle were responsible for the distribution, structure, and periodicity of mountain-building episodes in the earth's crust. This was a very controversial idea at the time, opposed vigorously by Harold Jeffreys and other geophysicists. The development of the unifying theory of plate tectonics in the 1960s has shown that Dave's visionary model was well conceived in principle, if not in detail, and has certainly vindicated his position in the controversy with advocates of a "strong" earth. This very fruitful period of research was terminated in 1941 when Dave left Harvard to apply his effort fully to national defense.

Dave Griggs's contributions to national defense in World War II were manifold: first at the MIT Radiation Laboratory and then as expert consultant to the Secretary of War. The Radiation Laboratory at MIT was established by the National Defense Research Committee (NDRC) in the fall of 1940 to develop microwave radar. One of the three startup programs was the demonstration of automatic radar tracking of airplanes. If such a capability could be demonstrated and then put into production, it would be possible to fire at enemy airplanes, day and night, in clouds or clear weather. The development of such a capability required the use of airborne targets. However, in 1940 the Army Air Corps had no planes to spare for experiments, and there were then no commercial flights of opportunity. In addition, attempts to use hydrogen-filled balloons had ended in near disaster. "I then remembered my best friend, Dave Griggs, from the Society of Fellows. So Dave was cleared for access to radar and for ten dollars an hour flew his plane over Boston and Cambridge as a target for our tracking experiments."[4]

Dave became immersed in the possible contributions of radar in air warfare, and in June 1941 took leave from Harvard to join the staff of the Radiation Laboratory. Automatic radar tracking turned out to be practical indeed. The first application was to ground-based anti-aircraft fire, and the resulting system became known as the SCR-584. Because of Dave's special interest in aircraft, he was appointed program manager of the airborne version, which went into production as the AGL-1. Dave's penetrating mind plus his insatiable drive impelled him to participate in the larger framework of the war as seen from the Office of the Secretary of War.

Having established a firm understanding of all the systems at the Radiation Laboratory, he transferred in July 1942 to Washington as an expert consultant to the Secretary of War—operating through a special office that had been established by Secretary Stimson under the leadership of Edward Bowles. Dave's assignment from Dr. Bowles was to do whatever was necessary to further the introduction and effect the most efficient use of radar in the war against the enemies of the United States.

He applied himself wholeheartedly to the end of the war, November 1945, starting in the European War and transferring to the Pacific as the tides of war demanded. Dave spent most of his time with the operational commands, at first with the Strategic Air Forces in Europe. While working directly with the commanders, such as General Pete Quesada, Jimmie Doolittle, and Tooey Spatz, he flew both training and combat missions introducing airborne bombing radar systems. In one flight over Bremen, the bomb-bay doors stuck. With usual disregard for his own safety, Dave kicked them open and in the process fell through the doors as they suddenly let go. He caught himself with one hand and was pulled back into the plane by the crew. The commanding general ordered him "grounded"; Dave was too valuable for such operational risks. Nevertheless, in a later tactical mission over northern Italy, he was hit by a 20-millimeter shell from an enemy plane; and for this he received the Purple Heart Medal—though a noncombatant.

In the words of General Doolittle, commander of the 8th Air Force, "Dave did the only thing that could have straightened out the equipment. He became one of the boys; he flew on combat missions and demonstrated the use of the equipment under combat conditions. Dave took some 'flak' in the leg, and I had to take him off flying. Dave had an unusual capability of dealing in an understandable way with operational people; and, when he left, the operators carried on in the image established by Dave." Dave then went to the 15th Air Force under General Nate Twining, where, as the general stated, "He did the same thing, changing from no success to full success."

In the 1944-45 period, following the invasion of the continent, Dave turned his attention also to the Tactical Air Forces, particularly the IXth, and in that connection became involved in the overall tactical use of radar and electronic control of toss bombing as well as radar control of fighters using the Radiation Laboratory-developed microwave ground radars: the MEW and SCR-584.

While he was in the European theater, Dave established the role of critical communications link and personal emissary between the theater commanders, such as Doolittle, Vandenberg, Twining, Spatz, and the leaders back at home such as General Arnold, Secretary Stimson, Dr. Bowles, Assistant Secretary for Air Bob Lovett, Dr. Vannevar Bush (head of the Office of Scientific Research and Development [OSRD]), and others. When the war in Europe came to an end, the war in the Pacific heated up. Here Dave played two major roles: (1) establishing working relations between

General MacArthur and his staff on the one hand and OSRD on the other (in this effort he was joined by Allan Waterman) and (2) making preparations for the A-bomb drop on Hiroshima.

For his working contributions, President Truman, on April 15, 1946, presented Dave Griggs with the Medal for Merit— the highest award of the nation given to a civilian for service during a declared war. The citation read:

> DR. DAVID GRIGGS, For exceptionally meritorious conduct in the performance of outstanding services as H2X Project Officer in the Eighth Air Force during the period 1 May 1943 to 1 April 1944. Dr. Griggs rendered invaluable service in connection with H2X equipment and instructing personnel to operate this equipment. Through his tireless efforts and outstanding leadership he made a substantial contribution to the heavy bombardment operations performed by the Eighth Air Force. The professional skill and the devotion to duty displayed by Dr. Griggs reflect the highest credit upon himself and the Armed Forces of the United States (signed HARRY TRUMAN, COMMANDER-IN-CHIEF).

In a more personal letter, Edward Bowles wrote to Dave (11 August 1947), "Your drive and determination, along with your brilliance, have been sources of both inspiration and joy to me. Your selflessness and your innate desire to give everything you had for the nation have given those who know you a great respect for your courage and idealism."

Dr. Bowles had a stable of scientific experts supporting his activities in the Office of the Secretary of War, including Julius Stratton, Louis Ridenour, William Shockley, Maurice E. Strieby, Norman Ramsey, Dale Corson, and Ivan A. Getting.[7] To support these operations required a staff of secretaries. One of these was a pretty young brunette from Iowa, Helen Avery. She and Dave fell in love, and on May 4, 1946, they were married.

Dave's postwar contributions to national defense continued to the day of his death, December 31, 1974. Although his principal interests were in support of the Air Force, he also contributed to the work of the Atomic Energy Commission, the Army, and to many other government agencies. In 1947 he was instrumental in setting up the RAND Corporation and became the first head of the Physics Department. As a member of the Air Force Scientific Advisory Board and as chief scientist of the Air Force (1951-52), he labored for a better understanding of the effects of nuclear weapons, development of the hydrogen bomb, and establishment of underground testing of nuclear weapons.

The early 1950s were characterized by the great debate as to whether the United States should develop thermonuclear weapons. President Truman had announced on January 31, 1950, that the United States would proceed with the thermonuclear (or fusion) bomb. However, the General Advisory Committee of the AEC, led by J. Robert Oppenheimer (October 29-30, 1949, et sequitur) opposed the development. Other scientists, such as Edward Teller and Luis Alvarez, supported it. The details of the debate are voluminous, but it is clear that Dave Griggs, while chief scientist of the Air Force, projected the official position of the Air Force in support of such a development, including the establishment of a second AEC laboratory, the Lawrence Radiation Laboratory at Livermore, California.

Oppie's opposition to the development of a hydrogen bomb, combined with his long record of association with liberals and communists, led Secretary Finletter by early 1951 to deny Air Force classified information to Oppie.[4] While Oppie's outward opposition to the hydrogen bomb decreased during Dave's tenure as chief scientist of the Air Force, Oppie became involved in an Army-contracted study, Project Vista, at the California Institute of Technology (1951-52). Here he advocated the development of small tactical nuclear weapons. The leadership of the Air Force looked upon this as an end run designed to delay the hydrogen bomb and reduce the availability of fissile material for use in hydrogen bombs. Secretary Finletter forbade the release of the section of the Project Vista report written by Oppie.[8]

From April 12, 1954, through May 6, 1954, a series of hearings were held in Washington under the auspices of the AEC entitled "In the Matter of J. Robert Oppenheimer." The issue at stake was whether Oppenheimer's clearance for access to classified AEC information should be reinstated. After some thousand pages of testimony, including testimony by Dave, the Personal Security Board voted to support the suspension of Oppenheimer's security clearance.

J. Robert Oppenheimer was a brilliant theoretical physicist. He led the successful wartime effort in the development of the atomic bomb. He was admired, if not worshiped, by many of his physicist colleagues. In the hearings, Dave accurately reported the official Air Force position; and though his evidence was not pivotal, Dave, in the minds of some physicists, became the Judas who had betrayed their god.

During the postwar period, efforts were made by a number of scientists, notably Dr. Eleanora B. Knopf and Professor Frank Turner, to persuade Griggs to resume his experimental studies of the mechanical properties of rocks. In 1948 he was induced by Professor Louis B. Slichter, director of the Institute of Geophysics at the University of California, Los Angeles, to accept an appointment as professor of geophysics at the institute, a position he held, except for relatively short leaves of absence, until his death.

At UCLA Dave Griggs established a new laboratory for experimental deformation of rocks that was productive in terms of both scientific papers and well-trained scientists. At UCLA he designed and supervised the building of improved high-pressure equipment for obtaining precise mechanical properties at high temperatures in gas pressure vessels. With Frank Turner and graduate students at Berkeley and associates and students at UCLA he undertook an exhaustive and valuable study of the mechanical properties of calcite crystals, limestone, marble, and other carbonate rocks. This research had immediate application to the interpretation of naturally deformed rock microstructures, as well as being at the cutting edge of materials science research on the development of "texture" or "preferred orientation" in crystal aggregates.

Application of the experimental data to the interpretation of geological processes was at least as important to Dave as acquisition of the data. One of his major interests was extrapolation of laboratory results to the conditions and time spans of deformation in the earth's crust and mantle and application of the data in modeling of dynamical processes in the earth by experiment and with the computer. His research had great scope and originality, encompassing fracture and seismicity, flow of rocks in mountain building, the global motions of the lithospheric plates, and, at the opposite extreme of the scale, the submicroscopic dislocation processes that are fundamentally responsible for the solid-state flow of rocks. In recent years he worked extensively on problems of earthquake mechanisms and played a significant role in establishing programs to investigate prediction and possible control of earthquakes.

Griggs's ability as an experimentalist was unique and deserves special mention. What he called his "gift for gadgets" was, in fact, an extraordinary genius for design and successful operation of apparatus of all kinds. In the difficult and sometimes dangerous practice of deformation at high temperatures and pressures, his inventions have had tremendous impact. The greatest range of experimental conditions consistent with safe operation of the equipment and the best attainable precision in measurement of stress and strain were the goals he aimed for and consistently achieved. The several generations of apparatus of his design that employed gaseous confining media have provided much of the basic mechanical data on the weaker rock types. Recent developments and improvements rely generally on the availability of superior engineering materials and improved ancillary instrumentation. In 1956 Griggs's introduction (with G. C. Kennedy) of the "simple squeezer" provided an extremely versatile exploration tool for both phase equilibrium and deformation studies over an extended range of experimental conditions and portended the development of a variety of modern anvil devices. In an effort to attain pressures and temperatures high enough to induce plasticity in the strongest silicates, Griggs designed (about 1960) the first "cubic apparatus" based on the principles of the tetrahedral presses then in operation, employing weak solids as confining media. This equipment achieved pressures up to 50 kilobars and temperatures up to the melting points of silicates, and its geometry permitted controlled deformation of prismatic samples. Extensive plastic flow of quartz crystals and many silicates was first obtained in this apparatus. Encouraged by the potential of this equipment for providing much-needed data on the flow laws of important crustal and mantle rocks, Griggs then developed devices with cylindrical geometry, also with solid confining media. They provided more reliable and continuous data on the temperatures and flow parameters of the samples over long periods of time (the longest tests lasted up to nine months). These devices, the DT apparatus and its larger successor, the GB apparatus, have provided much of the information currently available on the flow mechanisms and flow laws of minerals and rocks.

In 1965 Griggs discovered the phenomenon of "hydrolytic weakening" in quartz and silicate crystals: the addition of very small amounts (<<1 percent by weight) of water dissolved in quartz and silicate crystals has a spectacular effect on reducing their strength and promoting plasticity. The phenomenon was first observed in experiments on synthetic quartz crystals (with J. D. Blacic) and subsequently demonstrated in dry natural crystals (and silicates) into which small amounts of water were diffused. The recognition of this effect is probably the most significant step in our understanding of the mechanisms by which quartz and silicates deform at moderate to high pressures and temperatures present in the earth's crust and mantle. This may, in the future, be judged Griggs's most significant contribution to earth and materials science. The microscopic mechanism is still under vigorous study; it is still not fully understood.

As a teacher, Dave Griggs was tremendously effective. He was an intellectual father to a long line of students and implanted in them his high intellectual standards, scientific insight, and curiosity. He was impatient with incompetence and more so with carelessness or negligence, and his students quickly became aware of these traits. He had an uncanny insight and a critical ability that enabled him to detect flaws in a scientific argument or theory with ease; students and colleagues alike have seen their theories devastated under his critical examination. But this critical ability and insight were more frequently employed constructively with students in getting to the heart of a problem and suggesting a solution. Dave's relationship with his students was permeated by warmth, good humor, and mutual respect, and he always showed a deep concern for their personal and scientific welfare. Many of his students have made important contributions to geology and geophysics and are now in positions of professional eminence.

While continuing his research in geophysics, Dave continued to support the defense of his country; and in 1967, a quarter of a century after his departure from the Air Force as chief scientist, he again donned fatigues to help the fighting American soldier—this time in the national commitment in Vietnam. He made three extended trips to the theater. He helped General Westmoreland design an organizational structure to provide competent scientific support to the component commands with an information focal point at MACV Headquarters. During the Tet offensive he and an associate developed on-the-spot software that markedly improved the performance of the newly introduced sensor equipment system in use for the interdiction of enemy supply lines.

As many as have been Dave's technical contributions to military science and technology, perhaps his greatest contribution came from recognizing critical problem areas and calling upon his prodigious memory and his acquaintance with other scientists whom he marshaled in support. Throughout he was motivated by a strong feeling of patriotism and devotion to his country, always guided by a strict adherence to accuracy and truth.

On December 31, 1974, Dave had returned to his beloved mountains at Snowmass, Colorado, and to his favorite sport—skiing. He was with his friends Robert McNamara, former secretary of defense, and Richard Hodgson, a colleague from the MIT Radiation Laboratory. Dave was aware of a precedent heart attack, but with typical disregard for his own safety, he was not deterred from vigorous skiing. On the slopes he had a massive heart attack and died.

Years before, he had climbed Mt. Griggs in Alaska and at the peak buried the ashes of his parents and emplaced there a monument. His daughter, Nicola Andron, died in 1975 while giving birth to his granddaughter, Hilary. Now it was Stephen's, his son's, turn to climb the mountain with the ashes of Dave and Nicola.

NOTES

1. Robert F. Griggs, *We Two Together* (Pacific Grove, Calif.: Boxwood Press, 1961).

2. Brother and sister: Ruth Higbie, born June 25, 1909 (Ibid., p. 246), died 1990, author of *A Classful of Gods and Goddesses in Nepal* (Pacific Grove, Calif.: Boxwood Press, 1988); Julian Griggs, born April 21, 1917, died 1982, (Ibid., p. 260).

3. W. W. Rubey, "Foreword," in *Flow and Fracture of Rocks*, Monograph 16 (Washington, D.C.: Geophysical Union, 1972).

4. I. A. Getting, *All in a Lifetime* (New York: Vantage Press, 1989).

5. Crane Brinton, *The Society of Fellows* (Cambridge, Mass.: Harvard University Press, 1959).

6. Unpublished notes of David T. Griggs.

7. Unpublished activities of Edward L. Bowles, April 6, 1942, to August 13, 1947 (177 pp.).

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FELIX HAUROWITZ

March 1, 1896-December 2, 1987

BY FRANK W. PUTNAM

FELIX HAUROWITZ was born in Prague, then the capital of Bohemia and one of the provinces of the Austro-Hungarian empire. In his long life he saw many changes in the world, including the collapse of that empire at the end of World War I, in which he served, and the invasion of Prague by Hitler's army at the beginning of World War II. His flight with his family to Turkey was followed by a decade there before he moved to Indiana University at Bloomington, where he lived and worked the rest of his life. He survived successive political revolutions, and he participated in revolutions in science and medicine that have affected the condition of humankind. Felix and his wife, Gina, and family were displaced from the country and culture he cherished, but he never forgot either. In his last days he often talked about his early experiences in Prague. Yet he was fated to spend more than half his life in other countries, Turkey and America, which he also came to love. He was of a generation that will not be seen again. The product of centuries of European intellectual tradition-learned scholars, dedicated scientists, enlightened human beings—they were driven by barbaric intolerance to a new land to which they contributed so much. Their impact will be enduring, and Felix Haurowitz was one of the great ones among them.

Felix came from a family engaged in the textile trade but interested in literature, music, and foreign languages. German was his native language, but because Prague was predominantly Czech, he also spoke fluent Czech since his early childhood. His early education was at home with private teachers, then in a Catholic school run by a German order, and finally in a gymnasium. There for eight years he had Latin lessons daily and also studied Greek. No doubt this contributed to his linguistic fluency, for he later mastered Turkish well enough to lecture and write books in that language. Although the gymnasium taught a variety of subjects, including mathematics in which he excelled, it offered very little chemistry. He detested the required sports and gymnastic lessons but later became an enthusiastic mountaineer and skier. During the gymnasium years he also took private lessons in English, French, and Italian and also piano lessons. In later life he played the piano daily. He said he never practiced or played perfectly but that he played for personal satisfaction and relaxation.

On completing the gymnasium he enrolled in a textile school to please his father, although his mother wanted him to become a doctor. He enjoyed the textile school and said he learned much there of use for his later work in the laboratory. However, in November 1915 he was drafted for military service. He was assigned to an Austrian artillery battalion and sent to officers' school in Hungary. He made the highest grade in his class and was promoted to Fähnrich (ensign). He was made commander of the artillery battalion and assigned to the Austrian front in the southern Tirol. However, because they occupied an inaccessible ridge, there was little activity, so he began to prepare for medical school by reading textbooks of inorganic and organic chemistry by Holleman. These he found fascinating. In April 1918 he was given leave to enroll in the medical school of the German University in Prague. The war ended in October 1918 with the disintegration of the Austro-Hungarian empire, and Czechoslovakia became an independent republic.

In his first year in medical school Haurowitz worked as a volunteer assistant in the Department of Physiological Chemistry for Professor Richard von Zeynek, a hemoglobin chemist, and thereby began a life interest in this critical respiratory protein. His initial assignment was to work with Dr. Hedwig Langecker from whom he received his first introduction to the techniques of the biochemistry laboratory. Haurowitz analyzed the lipid-rich gonads of the jellyfish *Rhizostoma cuvieri*, which von Zeynek had collected. The latter was so pleased with the young student's report that he edited it and sent it off to the *Hoppe-Seyler's Zeitschrift für physiologische Chemie* with Haurowitz as the sole author. This was the latter's first publication. Von Zeynek gave him a half-time paid assistantship, which he held until he received his M.D. in 1922, when it was converted to a full-time position. He was asked to prepare crystalline horse hemoglobin and study it spectrophotometrically, and this led to his main research interest in the period from 1922 to 1936.

Haurowitz did well in all his courses in medical school; as was the custom, he spent a semester elsewhere, at the University of Würzburg in Bavaria. There he met the famous protein chemist Franz Hofmeister. After receiving his M.D. in 1922, Haurowitz was awarded the Dr. rer. nat. (D.Sc.) in 1923 for several papers published between 1920 and 1923. Already engaged in hemoglobin research, he visited the laboratory of Leonor Michaelis in Berlin to learn physicochemical methods from P. Rona, especially pH measurement, which was then new. A paper on ion measurements in blood serum resulted from this period. In the summer of 1924 he worked in the laboratory of the Nobelist F. Willstaetter in Munich on the purification of gastric lipase.

This also led to a publication. The young Haurowitz was fast developing a reputation. The same year the colloid chemist R. E. Liesegang visited him in Prague and invited him to write a book on the advances in biochemistry and pharmacology since the advent of World War I in 1914. Haurowitz declined to write on pharmacology but wrote his first book, *Biochemie des Menschen und der Tiere seit 1914*. This began a series of "progress" books with new ones appearing in German in 1932, 1938, and 1948 and then in English in 1950 and 1959 after he moved to America. These books gave him international recognition, and he later felt that the series in German did much to develop interest in and enhance the status of physiological chemistry and biochemistry in the German-speaking countries at a time when there were very few such departments in Germany. However, the immediate result of his first book and the concurrent research was his appointment as a docent. This rank was similar to that of an assistant professor, but it also entitled the recipient to announce any course he wanted to teach provided it did not duplicate another. He introduced courses in Prague on biophysical chemistry and advances in biochemistry. In 1930 he was appointed a tenured associate professor in the same department and finally achieved independence as a research scientist.

On becoming a docent, Felix Haurowitz married Gina Perutz in June 1925. She was a student of art history to whom he had been engaged for some time. She became his lifelong companion, supportive of him in everything he did. A few months after the wedding, they moved to Heidelberg for a research semester in the new Protein Research Institute of Albrecht Kossel, known for his fundamental work on protamines and histones for which he had received a Nobel prize in 1910. On their return to Prague he devoted the next five years almost entirely to research on hemoglobin. During this period he attended a series of international congresses where he gave papers on his studies of hemoglobin. He always recalled with pride the memories of the great biochemists he met at these early meetings, their gracious comments on his work, and the correspondence and personal contacts that ensued.

During the period of research on hemoglobin in Prague from 1925 through 1936, Haurowitz made a number of fundamental discoveries. Through spectroscopic studies of the combination of various oxidizing agents and other ligands with hemoglobin, he determined the absorption spectra of methemoglobin and other physical properties, and he crystallized several derivatives for the first time. He was the first to isolate fetal hemoglobin (Hb F) from fetal blood, and he crystallized it and determined its affinity for oxygen. He later pointed out that this was the first step in the search for further hemoglobins. However, his attempts to isolate abnormal hemoglobins from the blood of patients with anemias failed because there were no cases of thalassemia or sickle cell anemia in Prague. He also discovered the drastic change in the crystalline shape of deoxyhemoglobin from hexagonal plates to elongated prisms when oxygen was allowed to diffuse into the crystals. He showed this phenomenon to Max Perutz when the latter was in Prague visiting his cousin Gina while en route to Sir William Bragg's laboratory at Cambridge. Max later acknowledged that this surprising change gave him the idea to study the crystallographic structure of hemoglobin. Felix loved to tell of this meeting. He also claimed that this was the first observation of an allosteric reaction, and, in fact, it is sometimes cited as such. While at Prague he also published studies on other proteins and on methods of protein chemistry, but up to 1930 he was most noted for his work on hemoglobin.

A new and lifelong research interest began in 1930 and was stimulated by a phone call from a colleague, Fritz Breinl, who had just returned from a year at the Rockefeller Institute in New York. Breinl, a virologist, was excited by the experiments of Karl Landsteiner with synthetic haptens. He asked Haurowitz to read the papers and discuss with him what could be done to solve the mystery of antibody production. Thus began an exciting but short-lived collaboration that led to what was later called the template theory of antibody formation. Equally important, it committed Haurowitz to an experimental study of the role of antigen in antibody production for the rest of his research career. As might be expected, they used horse hemoglobin as an antigen in the work for their first paper. Unlike Landsteiner, who used only qualitative indices for the amount of antigen-antibody precipitate (- or +, ++, +++), Haurowitz and Breinl used quantitative methods to determine the amount of hemoglobin in the precipitate, and they also indirectly determined the amino acid content of the nonhemoglobin portion of the precipitate. In his autobiography for the National Academy, Haurowitz underlined the following statement for emphasis: "I concluded that the antibody must be serum globulin and suggested therefore that the antigen interferes with the process of globulin biosynthesis in such a way that globulins complementarily adjusted to the antigen are formed." Thus began the template theory, as it was later called, to which he adhered with some modifications for the rest of his life.

Shortly after this startling theory was announced, Haurowitz began experiments to determine the distribution of the antigen in the organism. Because radioactive isotopes were not yet known, he employed protein antigens to which various colored dyes were attached covalently, for example, arsenic-containing azoproteins. The chemical groups used to derivatize and label the proteins were known as haptens.

Haptens can convey antigenic specificity to a protein carrier to which they are linked but by themselves are not antigenic. The antibodies specific for the protein carrier can be removed by a process called absorption so that the remaining antibodies are specific for the chemical hapten. This ingenious procedure pioneered by Landsteiner was further developed by Felix Haurowitz who employed many different haptens and used it for quantitative determination of the composition of the antigen-antibody precipitate, calculation of dissociation constants, and other parameters of the antibody-antigen interaction. It became the primary experimental basis for his further immunochemical research until the introduction of radioactive isotopes. Although he never met Landsteiner, Haurowitz exchanged many reprints with him, and at the Landsteiner Centennial he said that he considered himself "as a kind of student or pupil of Landsteiner."

In the mid-1930s immunochemistry was developing rapidly as an exciting new field. In the United States Michael Heidelberger and his co-workers, notably Elvin Kabat, introduced a parallel quantitative approach by using the Kjeldahl method to determine the antibody nitrogen in precipitates of antigenic polysaccharides. The template theory was widely discussed and generally accepted, although Linus Pauling proposed a different concept. However, little was yet known about protein structure, and nothing was even surmised about the role of RNA and DNA in protein biosynthesis. Moreover, the end of the Prague era was at hand, for in Europe war clouds were on the horizon. Breinl returned to his first love, virology, and later died a tragic death from rickettsial fever.

In the summer of 1938 when Haurowitz was working at the Carlsberg Laboratory in Copenhagen at the invitation of Albert Fischer, his stay was abruptly terminated by the

Munich agreement. Because of Hitler's threat of a Nazi invasion of Czechoslovakia, he decided to return to Prague as quickly as possible to be with his wife and their two children, who had been born in 1929 and 1931. Not daring to cross Germany with a Czechoslovakian passport, he traveled by boat to Poland and continued by train to Prague. He was soon mobilized as an M.D. by the Czech army, but returned to civilian life when Czechoslovakia abandoned the Sudeten area of the country to Germany. The German University became an independent university of the German Reich, and Haurowitz was deprived of his right to teach. Just at that time he received an offer of the chair of biochemistry at the University of Istanbul. Though reluctant to leave Prague, he visited Istanbul, found the conditions favorable, and accepted the offer. Soon after, Hitler's troops invaded Prague. Two weeks later the Haurowitz family left for Istanbul to begin a new life. Most of their property except their furniture and library was seized by the Gestapo. They arrived in Istanbul with only 2,000 koruny (about $70). Toward the end of World War II, severe inflation forced them to sell some of their furniture to supplement his salary.

The Haurowitz family loved their life in Istanbul and considered it the most beautiful city in the world both for its natural beauty and for the Roman, Byzantine, and Islamic monuments. Felix and Gina often spoke with pleasure of their life there. They adapted well. Since lectures needed to be given in Turkish, his lectures were at first translated during class, but by the end of the second year he lectured and examined in Turkish. In fact, he was put on a committee to help modernize the Turkish language and rid it of Arabic terms. He published papers in Turkish and also a textbook on biochemistry that went through several editions. He developed a group of hard-working Turkish co-workers, some of whom later became professors in Istanbul and other Turkish universities. He maintained contact with them long after leaving Turkey, and in 1973 the University of Istanbul honored him by conferring the honorary degree of doctor of medicine.

At Istanbul Haurowitz's research was almost exclusively on problems of immunochemistry. Much of his time was spent in teaching large lecture and laboratory courses, and the budget was very low, especially during World War II. Yet in this period he published a number of papers in international journals, as well as in Turkish journals. He showed that the antibodies produced by the injection of a uniform antigen, such as an azoprotein, were always heterogeneous and could be fractionated by absorption with more or less modified azoproteins. He reported a method for purification of antibodies based on the dissociation of the antigen-antibody precipitate at low pH.

Despite the progress in research, the good life in Istanbul, and the satisfaction gained from the contribution he made to the development of medical education and extension of health care in Turkey, there was concern about the future of the children, who were reaching college age. In 1946 Gina and the two children, Martin and Alice, moved to the United States. However, Felix decided to stay in Turkey for two more years to fulfill his contract. Alice registered as a student at Indiana University in Bloomington and lived at the home of Harry G. Day, a professor of biochemistry in the Department of Chemistry. When Haurowitz visited the family in 1947 he gave a lecture at Dr. Day's invitation. This was followed by a reception where he met H.J. Muller, the geneticist and Nobel laureate whose wife was the daughter of a colleague in Istanbul. The next day Haurowitz was asked whether he would accept an appointment as professor of chemistry to teach biochemistry at Indiana University. He said he would be glad to do so but he had to return to Istanbul for one more year and also had been offered the chair of biochemistry in the Medical School at the University of Basel in Switzerland. While en route to Basel, he received the formal offer from Indiana University in London and accepted it. He moved to Bloomington in 1948 and spent the rest of his life there.

In 1948 Indiana University was expanding rapidly and was strong in chemistry and biology, particularly in genetics where it had a famous group: H.J. Muller, Tracy Sonneborn, and Ralph Cleland. Irwin Gunsalus and Salvador Luria were also there as assistant professors of microbiology. They asked Haurowitz to teach graduate courses in proteins and nucleic acids, which he gladly did. Among his students was Jim Watson, then a graduate student of Luria. Harry Day became a close friend and helped Haurowitz in many ways, as did his other colleagues in the Chemistry Department, particularly Frank Gucker, the chairman, who later became dean of arts and sciences. In 1950 while I was an assistant professor at the University of Chicago, Haurowitz invited me to give a seminar in the Department of Chemistry, my first such invitation. I was then a member of the Phage Group. I well remember the ferment and excitement in Bloomington at that time.

In Bloomington Haurowitz's research was almost entirely devoted to immunochemistry, but in addition to teaching courses in biochemistry and protein chemistry, he found time to write several books. Although he completed another book on the progress of biochemistry (from 1949 to 1959), he felt that the field had expanded too greatly to be covered any more by a single author, and he declined the publisher's request to become editor of a series of such volumes. In 1950 he published a book that had wide influence and the one of which he was most proud, *Chemistry* *and Biology of Proteins*. This had great success. The book was soon reprinted, and a second edition was published in 1963. A later book *Immunochemistry and Biosynthesis of Antibodies*, was published in 1968. Both books were translated into Russian, Japanese, and several other languages.

Haurowitz's early research at Indiana University focused on the fate of the injected antigen and its persistence in phagocytic cells. For this study he used diazotized aromatic amino acids labeled with radioactive isotopes ^{35}S or ^{14}C. Contrary to previous views Haurowitz showed that antigen is taken up by the phagocytic cells not the lymphoid cells and persists in the organism for a long time. He also showed that antigen was deposited in the cytoplasm not in the nuclei. Radioactive isotopes were also used to determine the dissociation constant of antigen-antibody complexes. Over the years, with the help of his graduate students and postdoctoral research associates, Haurowitz made many attempts to identify the specific combining sites of antibodies. One unique approach was to use antigens with two different well-defined chemical determinants and inject the doubly labeled protein into a single rabbit to eliminate complications caused by genetic differences. This work showed that specific combining sites of antibody molecules directed against a single antigenic determinant are heterogeneous even if produced in an individual rabbit. Thus, the combining site, although complementarily fitting the rigid antigenic determinant group, can be formed by different amino acid sequences. Later experiments with rabbits heterozygous for certain immunoglobulin allotypic markers showed preferential expression of antibodies of high affinity by one allotype.

After forty years of research and teaching, and publishing some 350 papers and eleven books, Felix Haurowitz retired in 1966 at age seventy. Actually the word retired is a misnomer, for he continued to go to the laboratory and his office every weekday and often on weekends, too, for almost twenty more years. For a time he continued immunochemical research with one assistant, and his work was still funded, for which he was very grateful. He attended seminars and asked penetrating questions. He and Gina attended scientific meetings where she watched over him carefully. She always had great difficulty persuading him to go to evening social affairs rather than work on his notes on the program of the day. He had major heart surgery at age seventy-seven, followed by complications from which he recovered, but except for that his health was good until almost his last year of life. He was never one to be interested in what he called "small talk" but always turned the conversation to some scientific theme. However, his own experiences had imbued him with a strong social conscience, and frequently he would raise issues related to world problems, especially those of Middle Europe.

International recognition came to Felix Haurowitz early in his scientific career, but major honors came rather late, perhaps because of the two mid-career moves resulting from the German invasion of Czechoslovakia. He was never active in professional societies though he did serve a term as chairman of the Division of Biological Chemistry of the American Chemical Society. He was a member of more than twenty scientific societies in half a dozen countries and was elected to honorary membership in several a decade after retirement. A rare honor was his election to the German Academy of Sciences (Leopoldina) in 1956. In 1960 he was awarded the prestigious Paul Ehrlich gold medal and prize, perhaps the highest honor in immunology and pathology. In 1970 he was elected to membership in the American Academy of Arts and Sciences. He was nearly eighty when elected to the National Academy of Sciences, but he greatly enjoyed the fellowship, and he and Gina went to every meeting while she was still alive. At age ninety he was lauded at a meeting on the history of immunology at the Congress of Immunology in Toronto where he gave his last paper.

A biographical memoir of Felix Haurowitz would be incomplete without a tribute to Gina Haurowitz, just as he would have been incomplete without her. Her death in June 1983 left him devastated. In his autobiography for the Academy he wrote, "Teaching, doing research, writing books and keeping myself up to date with biochemical research done elsewhere was a full-time job. I would not have been able to do all this without the help of my wife." Indeed, this was true, for she took care of all his needs and shielded him from all the daily demands of the world. More than that, she was his lifelong companion, a fellow hiker, an enthusiastic gardener, and a landscape painter. She was a gracious hostess and presided over afternoon teas for conversation among friends or evening receptions for campus visitors. At Bloomington we saw little of their children, for they had already established their own careers. Alice got her A.B. in chemistry at Indiana University. She and her husband, H. William Sievert, earned a Ph.D. degree in biochemistry at the University of Wisconsin and later worked at Abbott Laboratories. Their son Martin changed his name to Harwit. He received a B.A. at Oberlin College and a Ph.D. in physics at the Massachusetts Institute of Technology and was professor of astrophysics at Cornell and for five years was chairman of the Department of Astronomy there. He married Marianne Mark in 1957. Now he is director of the National Air and Space Museum in Washington, the most widely visited museum in the world.

Felix and Gina were very happy in Bloomington. They loved the university and the countryside, and all who knew them responded with affection and admiration. At Bloomington we honored him on his seventy-seventh birthday in 1973 with the Haurowitz Symposium. This drew several hundred distinguished immunologists from around the world. They told me they were drawn not just by his research but by their admiration for him as a person, for the ideas and encouragement he so freely gave to others, and for his acute insight and his thoughtful and helpful criticism.

His students knew him as a dedicated teacher, his colleagues as a leader of science. All who knew him will remember him as a fighter against intolerance who was also a wise, warm, and gentle human being.

IN PREPARING THIS BIBLIOGRAPHICAL MEMOIR I relied heavily on the autobiography and bibliography that Felix Haurowitz submitted to the National Academy in October 1975. Also, I had many conversations with him over a period of thirty years, especially in the last two years of his life. Harry Day supplied much valuable biographical material, including the transcript of a 1978 interview with Felix Haurowitz on his life and work. Many of the quotations and references to his personal recollections are from the autobiography and the transcript. I am indebted to family members and colleagues who reviewed this memoir.

ACADEMIC HISTORY AND HONORS

DEGREES

1922	M.D., German University of Prague
1923	D.Sc., German University of Prague

HONORARY DEGREES

1973	M.D., University of Istanbul
1975	Ph.D., Indiana University

UNIVERSITY APPOINTMENTS

1920-25 Assistant 1925-30 Docent 1930-39 Assistant Professor, Department of Physiological Chemistry, Medical School of the German University, Prague

1939-48 Professor and Head, Department of Biological and Medical Chemistry, Medical School, University of Istanbul, Turkey

1948-58 Professor of Chemistry 1958-66 Distinguished Professor, Department of Chemistry, Indiana University, Bloomington, Indiana

MEMBERSHIPS

1960	Deutsche Akademie der Naturforscher Leopoldina
1970	American Academy of Arts and Sciences
1975	National Academy of Sciences

AWARDS AND HONORARY MEMBERSHIPS

1960	Paul Ehrlich Medal and Prize, Paul Ehrlich Foundation, Frankfurt, West Germany
1971	Award for Distinguished Services to Immunology, First International Congress of Immunology, Washington, D. C.
1972	Société de Chimie Biologique, Paris
1973	Société d'Immunologie, Paris

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Courtesy of Woods Hole Oceanographic Institution

COLUMBUS O'DONNELL ISELIN

September 25, 1904-January 5, 1971

BY HENRY M. STOMMEL

FOR THREE ENCHANTED MONTHS in the summer of 1926, eight youths between the ages of twenty and twenty-three and a hired cook in the newly built 77-foot schooner "Chance" sailed the coasts of Newfoundland and Labrador as far north as Cape Chidley. Columbus Iselin, owner and skipper, was the son of a wealthy banking family much involved in the activities of the New York Yacht Club and a graduate of St. Marks and Harvard.

There was a gentlemanly tradition of oceanography at Harvard, starting with the world cruises of Alexander Agassiz (personally financed by Agassiz), and carried on vigorously by the ichthyologist Henry Bigelow through a long and productive life. Once, in 1962, when I encountered Dr. Bigelow in the library of the Museum of Comparative Zoology and told him that I had just returned from the Indian Ocean, he began to reminisce about his own adventures there in the Maldives with Agassiz in 1900. Bigelow could act effectively as an administrator and teacher. He never lost his love or ability to do abundant scientific research.

It was Bigelow who inspired the young Columbus to venture into oceanography and to make the Labrador cruise of the "Chance" into something of a scientific venture rather than a thing of pure sport. Botanical specimens were col lected, plankton tows were made, and two lines of hydrographic stations perpendicular to the coast were completed. Columbus wrote, and published privately, a charming account of the voyage under the title "The Log of the Schooner Chance." It is beautifully composed, evokes a sense of fun and adventure, and reveals a character sensitive to others and tolerant of pranks, a rare talent for leadership without the need to command, and a comfortable sense of mastery of the sea. For another forty years, through the shifting patterns of his career, people respected—yes, even loved— him. I cannot think of his directorship of the Woods Hole Oceanographic Institution as being similar in kind to that of other administrators whom I have known, there or elsewhere. Columbus seemed to assume the duties and role of an elder brother. He deserved loyalty and he got it. There was something magnetic about the man.

John Knowlton, one of the crew of the "Chance," kept notes on the cruise, from which he reconstructed an independent account in 1952. It confirms the early formation of Iselin's remarkable personality. "Columbus at twenty-one was not only an excellent sailor, he had a poise and fine judgement which few men acquire at any age. He had already achieved intellectual maturity whereas . . . we were . . . sometimes to be classed with the barbarians."

The Iselin log mentions engine troubles, but the Knowlton log tells how he (Knowlton) blundered into admitting seawater into the cylinders and how the Skipper patiently assisted for hours in removing the engine's head, cleaning it, and reassembling it, instead of being exasperated.

Or again, off the Straits of Belle Isle, where Iselin blithely notes the loss of half a drum of gasoline, with no hint of the immense danger of explosion, Knowlton informs us that it actually had leaked into the bilges, that the galley stove was burning as usual, "but there must have been too much air moving and it must have been too cold to form an explosive mixture" before it could be pumped out. We sense a vision of gilded youths striving to do something useful during a vacation to a barren land normally frequented only by the poverty-stricken Newfoundlander fishermen.

Columbus O'Donnell Iselin II was born in his family's summer home at New Rochelle, New York, on September 25, 1904. His ancestors were private bankers and philanthropists in New York City since the emigration of Isaac Iselin from Basel, Switzerland, in 1801. An uncle, Oliver Iselin, defended the America's Cup on four different racing yachts from 1893 to 1903.

In January 1929 he married a childhood girl friend, Eleanor ("Nora") Emmet Lapsley. Together, they had five children: three girls and two boys.

Columbus studied under Bigelow at Harvard at a time when the latter had just completed his compendious memoir on the oceanography of the Gulf of Maine. The Bureau of Fisheries had sold the "Albatross," and Bigelow was looking around for a means of getting to sea again. Due to the salesmanship of his friend Frank Lillie, the Rockefeller Foundation underwrote the formation of the Woods Hole Oceanographic Institution, the construction of a laboratory building, wharf, and the research vessel "Atlantis." Bigelow now had his ship and captain, twenty-five-year-old Columbus Iselin. He could plan to enlarge his previous studies of U.S. coastal waters to encompass a general exploration of the western North Atlantic. Perhaps he hoped that Iselin would emulate Captain Ault of the "Carnegie," who had been both captain and scientist before she exploded during fueling at Apia, Samoa, in November 1929. Columbus brought the "Atlantis" safely from Denmark in July 1931, doing some station work along the way under the tutelage of Franz Zorell of the Deutsche Seewarte. But

Columbus also had a family to raise, so command was turned over to Capt. Frederick McMurray, a career seaman, in less than a year. For another few cruises Columbus sailed as chief scientist to complete the hydrographic section along 30° W down to the equator, several crossings of the Gulf Stream from Bermuda to Halifax, from Bermuda to Chesapeake Bay, and across the Antilles and Florida currents. And then by the age of twenty-eight years, he more or less gave up going to sea—although still nominally "in charge of the 'Atlantis'" issuing sailing orders to McMurray, teaching a little at MIT, and working up the data he had obtained into a monograph entitled "A Study of the Circulation of the Western North Atlantic" (August 1936). It is a well-organized, discerning, scholarly work, written in the Bigelow qualitative descriptive style. It represents a lot of work at sea, a lot of thought, and still, even today, is worth study. At the time of publication he was one month short of his thirty-second birthday, an astonishing achievement for so young a man. It was also his best scientific work and the last that he could claim as deriving entirely from his own time at sea.

The five sections (1931-32) from Bermuda to Chesapeake Bay had exhibited a variability in dynamic topography that he thought might be related to changes in transport of the Gulf Stream. This great stream is certainly the major current of the western North Atlantic and for centuries geographers had speculated on how its variability might affect the climate (and fisheries) of Europe. In framing the plans for further use of the "Atlantis" for field study, it was therefore decided to accept the invitation of the Bermuda Oceanographic Committee of the Royal Society of London to join with their Bermuda-based "Culver" and its chief scientist E. F. Thompson in a projected five-year program to observe the variability systematically. From June 1937 to January

1940 (when the program was suspended because of the war), fifteen complete sections on a line between Montauk Pt., New York, and Bermuda were occupied by "Atlantis." The burden of the station work fell on Alfred H. Woodcock, at that time attached to the technical staff of "Atlantis." Iselin wrote this material up in a "Preliminary Report on Long-Period Variations in the Transport of the Gulf Stream System" (July 1940). In the acknowledgments he does not mention Woodcock by name, although he does mention others who contributed far less, such as the man who did the dynamic calculations for him. Was this oversight an unconscious denial of the fact that he no longer got his feet wet doing science? In the sailing orders he issued to Captain McMurray during these cruises, he was curiously reluctant to name Woodcock as chief scientist, even when Woodcock was the only scientific person on board. Or had it to do with a sense of fore and aft?

As a report of the data obtained, this paper is useful. By contrast, E. F. Thompson lost heart when contemplating a summary of the corresponding "Culver" data and never published it. However, the structure of these sections is complex, with evidence of multiple streams, or waves or eddies, and Iselin was able to offer no satisfactory objective criterion for defining "the transport" of the stream. He does not make clear why he decided to choose the extremes of dynamic height on two sides of the stream to define the instantaneous transport. This is especially surprising because he shows quite clearly that a previous overly optimistic suggestion that variations in transport could be determined by time series at only two stations—one in the slope water and the other near Bermuda—could not be used. And then he proceeds to choose two stations on each section in an arbitrary way. The mechanics of the schematic "lens" model he proposes is fantasy. One senses that as a scientist he was beyond his depth. Some obituaries mention that Iselin studied mathematics as an undergraduate, but there is no evidence of it in his work—in fact he had a distrust of any formal ideas and theoretical work that sometimes surfaced in the annual reports that he later wrote as director.

Iselin was now thirty-five years old and to succeed Bigelow as director of the institution. The coming of the war and the conversion of the institution's research to applied defense work gave him a new and fruitful opportunity for exercising his formidable gift of leadership.

In many ways the period of his first directorship (1940-50) was the high time of his career. It was now a frankly administrative role. His tolerant, humane, and kindly ability to persuade, influence, and support a motley mixture of scientists, yachtsmen, fishermen, and amateurs was uncanny. He could anticipate and understand the difficulties of doing work at sea that would confront the newly recruited academics. He had a certain knowledge about conditions at sea and the geographic distribution of various properties that proved important for those who were conducting the research. He was familiar with the customs and manners of the people of very widely different backgrounds who were assembled at Woods Hole during the war and able to maintain calm where misunderstandings could erupt. And he commanded the respect of the Navy. It is from this happy time that so many of the stories that illustrate his style of administration survive.

Two instances illustrating his human quality come to mind. The first concerns Fred Pingree, once a schoolmaster and of no research ability. It needs to be remembered that no one was ever fired during Columbus's time. We were driving back to Woods Hole from a martini lunch at the Little New Yorker restaurant in Falmouth. Puzzling over how to keep Fred usefully employed, Columbus sighed, "I just can't let him go—why if I did, he'd return to teaching and that would be a crime against American youth."

The second instance concerns Columbus's effort to calm the furor following a break-in at the carpenter's shop. In those days, with a host of amateur woodworkers about, the carpenter had to guard his tools with the temper of a she-bear. Thus, Stan Eldredge had honed an edge as sharp as that of his plane irons. One weekend the "Atlantis" departed on one of Maurice "Doc" Ewing's hastily organized cruises. Finding himself without tools, Joe Worzel, one of Ewing's most stalwart disciples, took down a fire axe, broke the panel in the shop door, and took Stan's favorite tools to sea.

When Monday morning rolled around Stan was in a proper rage. Storming upstairs to Columbus's office, he explained the situation in heated terms, ending with, "Mr. Iselin, what should I do?" To which Columbus smoothly replied, "Well, Stan, if someone had chopped down my door with an axe and I was as mad about it as you are, I'd get an axe and chop his door down."

Some idea of the lightheartedness of his youth can be gleaned from an excerpt from a talk he gave in later years about the character of his chief engineer, a Scotsman.

> . . . Harold Backus was the first person to go on the payroll of the Woods Hole Oceanographic Institution. He preceded me by at least four months. I hired him to be the chief engineer of the old "Atlantis" and I did not make a mistake.
>
> As we sailed on our first voyage together from Plymouth, England, to Woods Hole, we ran right away into nasty weather and head winds. The galley stove would not work when the vessel heeled over on one side. . . . It was a standard Danish steamship stove and the firebox required that the ship be on even keel. Thus on the first night out of Plymouth Harold and I had to rebuild the stove, if we were ever to get hot food.
>
> By six in the morning we had not quite finished the job, but were hungry. I went up on deck to see if anybody was at the wheel and if they

> were headed roughly in the right direction. I asked . . . the watch if they wanted some breakfast. They weren't interested. Upon returning to the wardroom I first saw an aspect of the Backus Factor at work.
>
> He had somehow found under the bunk in his cabin a very new looking blow torch, full of gasoline. I realized that I had not purchased a blow torch in Copenhagen and, of course, gasoline was not supposed to be on board below deck. Obviously the only available cooking equipment had somehow been left aboard the vessel by workmen from Burmeister and Wain, the builders.
>
> I said nothing to Harold and wedged myself in the downwind corner of the seat in the wardroom so as to hold the frying pan above my knees while he applied the torch in an expert manner to the underside of the pan.
>
> . . . each time the "Atlantis" returned from overhaul at the Electric Boat Company in Groton, Connecticut she always had a noticeable list to port. Harold Backus's cabin was on the port side. Knowing the stability of the vessel I could only conclude that he had close to a ton of tools under his berth. Since he could, of course, have shifted fuel or water to compensate for the list, he was obviously not at all ashamed of his latest acquisitions. It did not take the cost accountants at the Electric Boat Company long to catch on to this aspect of the Backus Factor. They simply added a small fraction to the bill for each day we were at the shipyard. When I pointed this out to Harold he was outraged. "They are already overcharging us. All I have been doing is to prevent the Institution from being robbed." The biggest piece of loot that he ever extracted from New London was a 5000 pound inclining weight which I still have as a mooring off my beach on the Vineyard. In order to get this aboard the "Atlantis" he had to start up and operate a large crane late in the night.

The story continues with an account of how Harold manipulated the revolutions-per-minute counter to covertly speed up homeward legs of cruises and of his skill in smuggling liquor during Prohibition.

One of Iselin's wartime concerns was the acoustic detection of submarines and the role of the refraction of underwater sound by the thermal stratification in the surface layers of the sea. Ewing's extensive experience in seismic surveys before the war was brought into play, and mathematical methods were developed by physicists to trace sound rays through various stratifications.

The bathythermograph played a central role in measurement of the near-surface stratification. This important instrument had its roots in one of C.-G. Rossby's devices called an oceanograph, used in 1934 to get as nearly continuous measurement of temperature against depth near the surface as possible. Rossby put A. F. Spilhaus on to the problem of improving it. In the summer of 1939, during one of the regular occupations of the Montauk Pt. to Bermuda, the hydrographic section Spilhaus (who was something of a bird of passage in oceanography) managed to obtain a detailed B.T. section over a short segment of the Gulf Stream front. For practical use by the Navy, Ewing and others had to make important improvements. The B.T. was manufactured by the thousands for use on naval vessels.

In 1942 the work of the institution turned toward practical application of oceanographic knowledge to warfare and for the first time was busy all year round. The "Atlantis" was sent for safety to the Gulf of Mexico, where Woodcock made a detailed study of diurnal temperature fluctuations during March and April—a phenomenon of importance in understanding the anomalous "afternoon effect" in the shallow propagation of sonar waves. She was then moved to a mooring in Lake Charles for better protection. In her place the institution procured a fleet of smaller craft for local field work: the "Physalia," "Anton Dohrn," "Reliance," and "Mytilus" were all used for detailed studies and experiments in the local waters of Massachusetts Bay and Vineyard Sound. Every week the environs of the institution reverberated to the sound of underwater explosives as workers of the Underwater Explosive Laboratory detonated various test devices off Naushon Island. Nonamesset Island was used as a depot for storing TNT, and a casting shed was set up there to manufacture shaped charges. This special group occupied the third floor of the institution's only building and was initially led by E. B. Wilson and Paul Cross.

Iselin was concerned that in the heat of the war-related activity not all concern for science itself should be lost. At least for 1942 he was able to continue support for biological research in plankton (George Clarke and Mary Sears), marine bacteria (Selman Waksman and Cornelia Carey), and metabolism of marine organisms (Lawrence Irving), using materials collected before the limitations on field work were imposed. Iselin evidently hoped that in addition to war-related activity he could keep a normal program of pure scientific research going and that Woods Hole would continue to be a place where, in his own words, "all qualified investigators who are interested in the sea are welcome." This became increasingly difficult as the staff increased from ninety-three to 335 during the four war years.

One of the most astonishing developments of the studies of underwater sound transmission was W. M. Ewing's discovery of the "sound channel," the level of minimum sound velocity that permits the sounds of small explosions to be heard across entire ocean basins. Studies were made to predict sea and surf conditions for forecasting conditions likely to be encountered during amphibious operations. Jeffries Wyman and Alfred Woodcock studied low-level meteorological phenomena pertinent to aircraft carrier operations and laying smoke screens. Alfred Redfield led a staff of about twenty persons in a study of antifouling paints and fouling organisms for the Bureau of Ships—a continuation of Waksman's earlier work. Some 60,000 bathythermograph records of shallow water temperature profiles obtained in the North Atlantic were reduced to monthly charts of temperature down to 200 meters depth (Frederick Fuglister).

Most scientists caught up in the excitement of war work would accord themselves a high score for doing as much as Iselin did as the institution's leader during these years. However, writing the director's report in 1946, Iselin was more circumspect. Viewing the results of these major investigations aimed at practical application from the perspective of a scientist, he wrote ingenuously: "The scientific advances may appear to be rather small, considering the size of the staff involved." Few laboratory directors would risk so candid a statement today.

The development of the B.T. was important for oceanography after the war because it meant that detailed data could be obtained from a ship under way. Of perhaps even more importance was the availability after the war of Loran, which improved navigation to the point where detailed surveys could be made meaningfully. The emphasis in Gulf Stream research shifted from the intractable problem of long-period variability of the Gulf Stream to studies of the detailed structure of its front. Frederick Fuglister was encouraged by Iselin to conduct this research. Fritz's training as an artist had accustomed him to visualizing form, and soon he was bringing back a series of wonderful surveys that showed us for the first time the meander and eddy structure of the Stream. Iselin's name was on their first joint paper, but Fritz did all the work and most of the thought. A close friend and colleague of mine during the late 1940s told me that he didn't think Columbus was interested in science anymore. But Columbus was clearly interested in helping others to do their work and must have gotten his satisfaction out of being a good boss.

The directorship of the institution passed to Admiral Edward H. "Iceberg" Smith, U.S. Coast Guard, in 1950. Smith had a very substantial background in practical physical oceanography dating back into the 1920s when he personally conducted the surveys made by the U.S. Coast Guard in the

Labrador Sea and Newfoundland Banks for the International Ice Patrol. No relation to the Captain Ed Smith who lost the "Titanic," he could proudly point to the likelihood that the Coast Guard's yearly surveys of the iceberg population had forestalled any further incident of the kind. He was likable, a little out of touch with the science, but a responsible administrator. During Iselin's days as director, the Woods Hole salaries were scandalously low. Smith raised them. Fuglister and I used to share coffee hour and endless discussion in those days. Iselin had a way of popping in quietly and unexpectedly, like a ghost, often catching us unaware in some private conversation. Once he came in while Fritz was happily waving around a slip of paper from Smith telling him of his first raise in salary. Columbus inquired about the merriment, and Fritz told him about the raise. "About time, too," commented Columbus—overlooking the ample opportunities he had had over the years. Columbus was made up of a strange mixture of friendly concern and aloofness. It was one of Fuglister's great sorrows that despite years of laboring on the Gulf Stream he was never once invited to Iselin's home on the Vineyard.

During the Great Depression, Columbus bought a farm on Martha's Vineyard. It was said that he bought it as a refuge from possible civil unrest, much as some academics of a later generation considered moving their families to New Zealand at the time of the Cuban missile crisis of 1962. As a result he had to commute regularly back and forth across Vineyard Sound in his small boat, "Risk." So we got accustomed to seeing him dressed in oilskins, and he himself maintained a taste of salt in his mouth long after he had finished doing actual work at sea. At exactly 5 p.m. each day Columbus would jump aboard the "Risk" and hurry home to the privacy of the Vineyard. He was thus a poor host to the various dignitaries who happened to be visiting

Woods Hole—and who found themselves stranded there for the evening or weekend. Actually this was a fine opportunity for us bachelors at the time because people like Deacon, Hidaka, Wüst, Revelle, Rossby, and Sverdrup suddenly found themselves dependent upon us for entertainment, and we relished the opportunity of discourse with such men. Columbus's social aloofness was not restricted to his employees.

An incident that illustrates Columbus's impulsiveness arose from some draft charts of the distribution of properties at 4,000 meters depth in the world ocean that Dick Stroup and I were making. The charts were on Fritz's table. We were talking about something private and were embarrassed and speechless by Columbus's sudden appearance in the room. To alleviate the awkwardness, I pointed out the sparsity of deep data in the Indian Ocean. There was a big meeting going on downstairs, I think of SCOR—the Scientific Committee on Oceanic Research, an international body. From what I can reconstruct, Columbus went directly down from our coffee break and proposed the International Indian Ocean Expedition. They must have thought he had been preparing the idea for months. On the other hand, he could play the opposite game like a master. I sat next to him once at a meeting where he spoke in an apparently offhand, tentative, free-association manner, as though the ideas were forming in his mind as the words came out. But they were all carefully written down on a pad in front of him, and he was reading them. He was an accomplished actor.

In many ways, during Smith's term in office (1950-56), Columbus remained a surrogate director. People automatically referred their ideas and plans to him as though Smith did not exist. And after Smith's departure, Columbus returned for two more years as director. One could hardly notice the difference except in salaries and that the halls had fresh coats of paint.

The secrecy that prevailed at Woods Hole during the war had disappeared by 1950. Nearly all of the work going on was open and free. Since most of our funding came from the Navy, the new openness must have seemed problematic to Iselin. By 1956 he devised a way to continue secret military advice for the Navy by means of a so-called Project Nobska, associated with the National Academy of Sciences, meeting in Woods Hole, but not at the institution.

During his directorship Columbus had been relatively free from direct control by the Board of Trustees—in fact, he rather airily regarded the annual trustees' meeting as a somewhat tiresome formality. The war had brought a new breed of professional, academic, and industrial administrator into the institution's Executive Committee.

In 1958 Iselin was replaced as director by Paul Fye, a chemist. The internal organization of the institution was examined by a consultant in business administration. A departmental structure and a rank system in some ways similar to civil service career "ladders" was installed. The unfamiliar and painful procedure of promotion reviews was imposed. Scientists were assigned "employee numbers." For the first time they were formally invited to criticize and evaluate each other. The days of the amateur, when, for example, Hilliard Barbour came to work at Woods Hole during the war and his main concern was not the salary, but whether he might lose his Corinthian status, were gone.

The aim was to move away from Iselin's informal paternal way of doing things to a more common professional standard. Iselin's role as invisible director (which he played during Admiral Smith's tenure) was accordingly ended. The "Atlantis" was sold to an Argentine school, and an important symbolic link between him and the institution was broken. As the institution grew, there were new employees who did not even know his name: once, about 1963, when I tried to phone him, the institution's telephone operator asked, "Iselin? How do you spell it?"

I think that he was hurt by the growing anonymity. He must have felt that Harvard was shaking him off as well, despite the fact that he had lectured there for years without compensation. He had an office at the Museum for Comparative Zoology. As you ascended the stairway you'd pass a door with a frosted glass panel enscribed "Woods Hole Oceanographic Institution." He must have spent very little time there, in fact so little that at the time of the incident that I want to recall the new museum director had not made his acquaintance after two years on the job.

The commonwealth of Massachusetts used to require a regular tuberculosis test (skin test or chest X-ray) for all teachers (even Harvard professors). Evidently, Iselin's was far overdue, and for over a year he let the registered letters from the secretary of the corporation apprising him of the fact pile up unopened on his desk at home. The secretary then turned to the museum director for help, and that official, irritated by never having met Iselin, offered no help. Accordingly, the Committee on Oceanography received a notice from the secretary barring Iselin from the classroom and instructing the sergeant at arms of the faculty (perhaps an elderly Latin professor) to enforce it. Francis Birch, an old friend, phoned Columbus to tell him what had transpired and accompanied him to the infirmary for the required test. But Iselin felt that he had been treated abruptly and without appreciation for his years of gratuitous service. He was now about sixty years of age. One bright event occurred: he was invited to sail as passenger on a hydrographic cruise of "Atlantis II" from Capetown. He spent much of the time enjoying the company of the bridge, telling some of his wonderfully perceptive stories, many about the scrapes he had been led into by his boyhood buddy Terry Keogh, perhaps about the time they established a boatyard in Lunenburg, built boats at a loss, and failed quickly because Keogh was such a good salesman.

I didn't see much of him in later years. The stress of age, of too many years on the Washington circuit, perhaps a growing sense of "being out of it," must have been at work. When he attended the dedication of the Iselin building—a large maintenance shed on the Woods Hole dock—he was heard to mutter: "They named the garage after me." Gordon Riley, who recalls seeing him at a meeting at the University of Rhode Island, says he was astonished by the transformation. To his eyes Iselin seemed thin and old, there were missing teeth, he had recently been hospitalized for alcoholism, and was unpleasantly critical of others—a thing totally uncharacteristic of the younger man.

Columbus died in Falmouth, Massachusetts, on January 5, 1971. His wife Nora died two weeks later. The funeral was on Martha's Vineyard.

What then can we say about Columbus Iselin? He was a good and generous man. He commanded respect and shouldered responsibility as a ship's captain should. He had a clear vision that oceanographic knowledge grows from measurement at sea. And he strove to build a scientific staff at Woods Hole that shared the enthusiasm he had for work on ships and the exuberance of an amateur. There was a sense of strength, selflessness, and integrity about him. He was indeed a great man, and he was overtaken by the times.

CURRICULUM VITAE

COLUMBUS O'DONNELL ISELIN

Born: September 25, 1904, New Rochelle, New York
Died: January 5, 1971, Falmouth, Massachusetts

EDUCATION

St. Marks School, 1917-22
A.B., Harvard University, 1926
A.M., Harvard University, 1928
D.Sc. (Honorary), Brown University, 1947

EXPERIENCE

Physical Oceanographer, Woods Hole Oceanographic Institution, 1932-40
Director, Woods Hole Oceanographic Institution, 1940-50 and 1956-58
Senior Physical Oceanographer, Woods Hole Oceanographic Institution, 1950-56
Henry Bryant Bigelow Oceanographer, Woods Hole Oceanographic Institution, 1958
Chairman, Department of Theoretical Oceanography and Meteorology, Woods Hole Oceanographic Institution, 1962-

HONORS

Alexander Agassiz Medal, 1943
Honorary Doctorate of Science from Brown University, 1947
Legion of Merit Medal, 1948
Henry Bryant Bigelow Chair in Oceanography, 1958
Henry Bryant Bigelow Medal, 1966

TEACHING EXPERIENCE

Assistant Curator of Oceanography, Museum of Comparative Zoology, Harvard University, 1929-48
Lecturer, Massachusetts Institute of Technology, 1936
Assistant Professor of Physical Oceanography, Harvard University, 1936-39

Associate Professor of Physical Oceanography, Harvard University, 1939-60

Professor of Physical Oceanography, Harvard University, 1960

Professor of Physical Oceanography, Massachusetts Institute of Technology, 1959-

OTHER

Member, American Geophysical Union, 1929-

Member of Corporation and Trustee, Woods Hole Oceanographic Institution, 1936-

Trustee, Bermuda Biological Station for Research, Inc., 1936-

Trustee, Marine Biological Laboratory, 1941-52

Fellow, New York Academy of Sciences, 1941

Member, American Academy of Arts and Sciences, 1944-

Member, Committee on Undersea Warfare, National Academy of Sciences, National Research Council, 1946-

Member, American Philosophical Society, 1950-

Trustee, American Museum of Natural History, 1951

Member, National Academy of Sciences, 1951-

Member, Committee on Oceanography, National Academy of Sciences, 1957-64

Member, Scientific Committee on Oceanic Research (SCOR), 1957-

Member, NATO Subcommittee on Oceanographic Research, 1959-

Member, NASCO Ocean-Wide Surveys Panel, 1962-

Member, Board of Directors, Scientific Advisory Committee, Travelers Research Center, 1963-

Board Member, American Geographical Society, 1963-

RESEARCH

Oceanic circulation, underwater acoustics, marine resources

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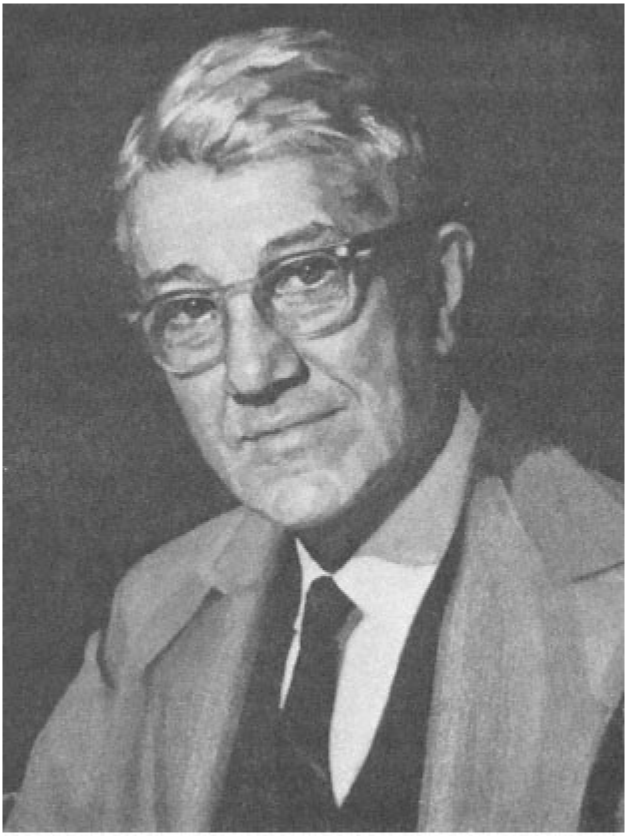

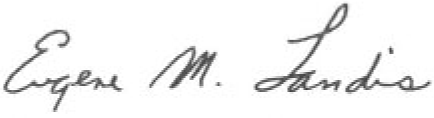
Eugene M. Landis

EUGENE MARKLEY LANDIS

April 4, 1901-February 14, 1987

BY JOHN R. PAPPENHEIMER

IN 1942 WALTER B. CANNON retired from the Harvard Medical School after serving more than thirty-five years as the George Higginson Professor of Physiology. Like his predecessor, Henry Pickering Bowditch, Cannon had brought extraordinary distinction to the Harvard Medical School and indeed to american medical sciences in general. His re-Burwell, a challenge that was magnified by the wartime conplacement presented a difficult challenge for Dean Sidney ditions of 1942. In November of 1941 Dean Burwell wrote as follows to James Bryant Conant, president of Harvard University:

> This to report to you the situation with regard to two important pending appointments at the Medical School. Of these, much the most important is the appointment of a successor to Dr. Cannon. I have reached a solution in my mind which I believe is the best one that can be made . . . . This paragon is Dr. Eugene Landis, now Professor of Medicine at the University of Virginia . . . . Lest you misjudge the kind of fellow he is from the fact that he is Professor of Medicine let me point out that he is a Doctor of Philosophy as well as of Medicine . . . . He is a naturalist who applies to the solution of problems of bodily function the principles of physics and chemistry. It does not seem to me to be a disadvantage that he has a knowledge of the changes in function which occur during the course of disease. Aside from distinction in research he is a person of broad knowledge and understanding.

Eugene Markley Landis was born on April 14th, 1901, in New Hope, Pennsylvania. His father was a biology teacher in a Philadelphia high school and young Gene frequently accompanied his father on weekend field trips to collect the animal materials needed for the laboratory course. Protozoology and microscopy captured Gene's interest at an early stage and his first paper, entitled "An Amicronucleate Race of *Paramecium caudatum*," was published in *The American Naturalist* in 1920 while Gene was a sophomore at the University of Pennsylvania. He became an undergraduate teaching assistant in zoology while he took advanced courses in protozoology, cytology, and comparative anatomy. However, it was not until he reached medical school that he began to think in terms of function. In a recent letter to a young physiologist at Oxford, he wrote: "My first year at medical school in 1922 was a thrilling one. Oxford-trained Cuthbert Bazett and Penn-trained Merkel Jacobs taught physiology in a way that correlated morphology and microscopy with function and brought it all alive. In 1924 I asked Dr. Jacobs whether I might use spare time and summers in his laboratory at Penn and at Woods Hole to learn something about research in physiology." Dr. Jacobs, in reply, suggested that Gene investigate the permeability of capillary walls using synthetic dyes. There were three reasons for this suggestion. First, August Krogh had just published his Nobel prize-winning book, *Anatomy and Physiology of the Capillaries*, in which he emphasized the quantitative histology of the capillary network and pointed out how little was known about the permeability of capillary walls. Second, World War I had laid open previously secret German patents on dyestuffs, and third, Robert Chambers had just described the micro-manipulator and the feasibility of micro-injections. Jacobs made his seminal suggestion in the spring of 1924 and on October 13th, 1925, Landis—by then a third year medical student—sent a long manuscript entitled "The Capillary Pressure in Frog Mesentery Determined by Microinjection Methods" to the editors of the *American Journal of Physiology*. It was the first of a series of five papers leading to quantitative characterization of the capillary wall in terms of transmural pressures on the one hand and rates of transcapillary fluid movement on the other. These papers also included the first measurements of the pressure drop along the vascular tree and localization of the separate components of the peripheral resistance to blood flow, in mammals as well as in frogs. The methods developed to measure pressures within microvessels and rates of fluid movement across capillary walls were truly elegant. With a micropipette attached to a pressure reservoir and a micrometer syringe, Landis found that he could insert the pipette into microvessels as small as 5 μM in diameter without serious injury or loss of fluid around the point of puncture. If the pressure within the tip of the pipette were slightly lower than that in the blood vessel, red cells moved into the pipette. If the pressure were then raised above intravascular pressure by means of the micrometer regulator, the red cells moved toward the vessel lumen. Using the movements of a single red cell as a guide and by skillful manipulation of the micrometer-regulator, Landis was able to determine the pressure within the pipette that exactly balanced the pressure within the microvessel. It was even possible to measure peak systolic and diastolic pressures. To measure net transcapillary fluid movement, the distal end of a single capillary was occluded by means of a blunt glass rod; transcapillary fluid movement in either direction was then estimated from the rate of movement of a single red cell towards or away from the point of occlusion. It was found that rate of net fluid movement across the capillary wall is proportional to the difference between capillary hydrostatic pressure and the osmotic pressure of the plasma proteins, thus providing experimental proof of Ernest Starling's hypothesis of fluid exchange. The constant of proportionality was the first quantitative measurement of the hydrodynamic conductance of the capillary wall (cubic micra of fluid per second per cm H_20 pressure difference per μ^2 of capillary wall). With these powerful methods, Landis was able to investigate capillary permeability under a variety of physiological and pathological conditions: by August 1929 Krogh could say in the new edition of his book, that "the situation has now been wholly changed by the brilliant work of E. M. Landis, whose methods open up the possibility of an intimate understanding of capillary permeability far beyond anything to be imagined before." This intimate understanding, of which Krogh spoke, is today the basis for everyday teaching in elementary physiology and the original series of papers, including a 1934 article in *Physiological Reviews*, remain as models of beautiful scientific writing. Landis was the sole author of all these papers and there are no acknowledgments of financial support for the simple reason that there was none. He made the original parts of his apparatus himself, washed his own dishes, drew his own illustrations and typed his own papers—all between classes at medical school. These papers were the most important ones of his entire research career, and the lonely, frugal environment in which he produced them had a profound effect on the philosophy of science which he offered to his colleagues under very different circumstances during his subsequent career.

In 1929, after completing an internship at the hospital of the University of Pennsylvania, Landis was awarded a Guggenheim Memorial Fellowship and for the next two years he worked with August Krogh in Copenhagen and with Sir Thomas Lewis in London. This association with the elite of experimental zoology and physiology on the one hand and clinical research on the other was no accident; indeed, it reflected most accurately the two poles of his career. With Lewis he wrote papers on Raynaud's disease and on acrocyanosis, and with Krogh he developed the pressure plethysmograph for physiological measurements of fluid exchange in human extremities.

After returning from Europe in 1931, Landis put on his clinical white coat for twelve years, first at the University of Pennsylvania and then as chairman of the Department of Medicine at the University of Virginia. During this period he made the first accurate measurements of the molecular weight of Inulin and the first renal clearance measurements of Diodrast in humans. He also verified unequivocally Tigerstedt and Bergman's discovery of renin in kidney extracts; this work was an important step in elucidating the role of renin in hypertension.

When Landis came to Harvard in 1943 to succeed Walter Cannon as the Higginson Professor of Physiology, he was faced with the difficult task of starting a new department with double-duty teaching under war-time conditions. At the same time he was president of the American Society for Clinical Investigation, and he had responsibilities for applied research on cardiovascular effects of acceleration for the Committee on Aviation Medicine in Washington. Under these circumstances academic research became a secondary consideration and almost all of his enormous energy and organizational skills went into the development of a super course in human physiology. In addition to more than sixty formal lectures, including two on Saturday mornings, there were thirty-two laboratory exercises, some of which lasted for two to four days. Many of the experiments were performed by the students on themselves—experiments on temperature regulation, syncope, renal clearances, plethysmography, special senses, acid-base disturbances, and metabolism. Most people who participated in this course, whether they were faculty or students, felt that it was an extremely rewarding experience, and for many students it was a crucial turning point that launched them into academic medicine. During the ten or more years that this course existed, Harvard students were always number one on the National Boards in physiology and by a wide margin. The conduct of such an elaborate course required a heavy load of teaching; during the war it was full-time and for a few years after the war it was half-time. Throughout this period, Professor Landis always made sure that he gave at least one more lecture than anyone else on his staff and he took personal charge of many of the laboratory exercises. He also did his share of correcting the several written examinations, all of which were in the form of essay questions.

The postwar period was an extraordinary one in the history of science. Academic research changed from being a joyous, spare-time privilege of a university teacher to a driving professional career. Explosive growth of government support for research enabled young investigators to create specialized research empires of their own without regard for departmental or other academic responsibilities. In this heady and inflationary atmosphere, Gene Landis retained the voice of reason, humility, and unassailable integrity. He was surrounded by prima donnas, both real and self-professed, but he was a master at controlling their inflationary tendencies while at the same time encouraging their creative ones. Indeed, he succeeded in creating an environment where prima donnas flourished without evading their responsibilities to the department or to the medical school as a whole. No less than six presidents of the American Physiological Society grew up as members of Landis's department. More important, perhaps, are the annual Bowditch Lectureships, which represent the highest honor the Physiological Society can confer on physiologists under the age of forty. Of the first fifteen Bowditch Lecturers, no less than eight were selected from the young people who had done highly original and independent work in Landis's entourage. Many chairmen of departments of physiology or medicine and at least four deans of medical schools carry with them some of the high standards of scientific excellence and unselfish ideals of service that they experienced during their apprenticeships with Gene Landis.

The changes of traditional academic customs and points of view that accompanied the rapid expansion of medical research often ran counter to Gene's sensitive and deeply ingrained ethical standards. He found it hard to adapt to the new professionalism in research with its pressures for multiple publications, rapid academic advancement, and competitive quest for money and fame. Perhaps it is for this reason that he withdrew personally from the research arena and concentrated on providing a departmental environment where talented young investigators could develop independently. Much of his time in later years was given to editorial work, including his role as editor-in-chief of Circulation Research. He himself wrote extremely well and he spent untold hours trying to bring the presentations in other people's manuscripts up to the standards he set for himself. He was indeed a superb editor—gentle and encouraging but always firm. He once remarked that "editorial advice is like snow; the softer it falls, the longer it dwells and the deeper it sinks into the mind."

Many honors came to Professor Landis, including memberships in foreign societies, the Phillips Medal of the American College of Physicians, the Distinguished Graduate Award of the University of Pennsylvania, and the Gold Heart Award of the American Heart Association. He was elected to the National Academy of Sciences in 1954.

Gene and his wife had strong roots in Pennsylvania and they spent many happy, fulfilled years there after retirement in 1967. Gene became adjunct professor of biology at Lehigh University and there he resumed his microinjection studies of single capillaries without feeling under pressure to do so. His last original paper, entitled "Fluid Movement Through Walls of Single Capillaries Exposed to Hypertonic Solutions," was published in the *American Journal of Physiology* in 1971. Ill-health forced a second retirement and the end came on February 14th, 1987.

Gene is survived by his wife, Elizabeth; his daughter, Barbara (Mrs. Jerry Amos); and three grandchildren.

SELECTED AWARDS AND DISTINCTIONS

1922	Phi Beta Kappa, University of Pennsylvania
1926-27	National Research Council Fellow
1929-31	Guggenheim Research Fellow (a) London (Sir Thomas Lewis), (b) Copenhagen (August Krogh)
1936	Phillips Medalist of the American College of Physicians
1936	Harvey Society Lecturer
1942-43	President, American Society for Clinical Investigation
1943-67	George Higginson Professor of Physiology, Harvard University
1944	American Academy of Arts and Sciences
1952	Honorary Member, Sociedad Argentina de Biologia
1952-53	President, American Physiological Society
1954	National Academy of Sciences
1966	Gold Heart Award, American Heart Association
1974	Foreign Member, Royal Danish Academy
1986	Distinguished Graduate Award, University of Pennsylvania

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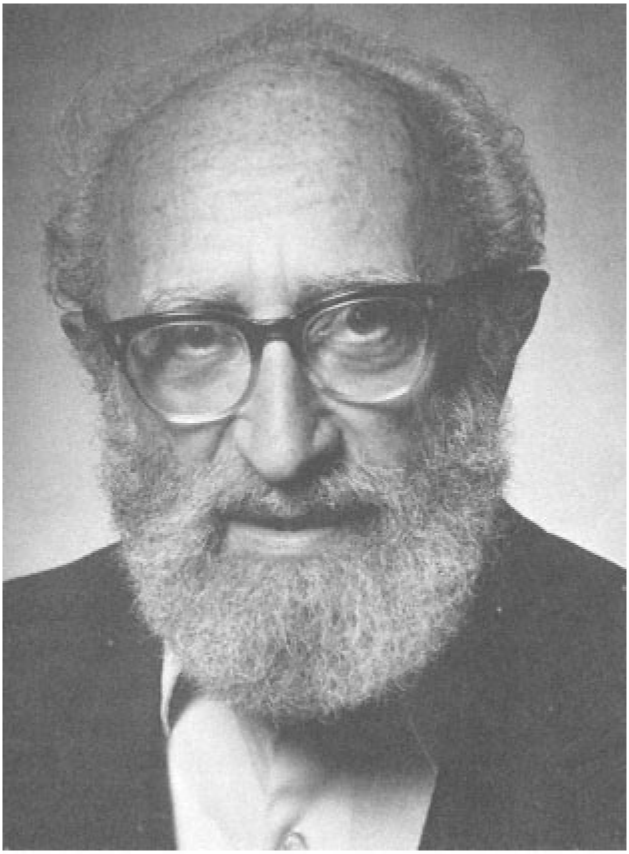

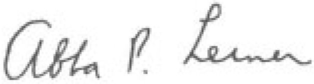

ABBA PTACHYA LERNER

October 28 1903-October 27, 1982

BY DAVID S. LANDES

Abba ptachya lerner was born in Bessarabia (then in Russia) in 1903 and came to England as a child of three, one more lucky escapee from endemic and epidemic persecution of Jews. We know little of his childhood, but the variety of his early work experience bespeaks the Outsider. No public or grammar school; no scholarships or fellowships. From the age of sixteen he worked as a machinist, a teacher in Hebrew schools (possibly the worst-paid job ever invented), and as a businessman. When he entered the London School of Economics in 1929, he brought with him the lessons of a variegated career and a maturity beyond that of his classmates, as well as an invaluable sense of what it was like out there in the Real World.

At LSE Lerner found a subject to his measure and a benign appreciative environment that gave full play to an extraordinary natural talent. In his first year he won the director's Essay Prize and a Tooke scholarship; these were only the first of a series of honors that crowned a run as first-place student in economics. He took his bachelor's degree in 1932 and went on to graduate study, first at LSE, then at Cambridge and Manchester. It was while still a graduate student that he persuaded Paul Sweezy and Ursula Webb (later Ursula Hicks) to join him in founding *The Re-*

view of Economic Studies. (Abba had been, in one of his avatars, a commercial printer and was not intimidated at the prospect of publishing a journal.) The intellectual justification, he recalled later, was that most journal articles were too long—too much verbiage—and that there ought to be a place for shorter notes that would make the sparks fly. Apparently there was no lack of material, but the Young Turk organizers needed to find money. Then, as later, the best-provided members of the discipline were the Americans; so the organizers put the bite on all visitors from across the sea, who couldn't have been more encouraging and gave five pounds to prove it. (Note that five pounds then was almost $25, and $25 then was like $500 today.) Only Jacob Viner was skeptical: he felt there was already a plethora of journals and he couldn't keep up; but he gave ten pounds.

With start-up funds in hand, Lerner and colleagues repaired to the major centers of teaching and research to drum up contributions. Oxford was splendid. The economists laid out a festive board, and Lerner never forgot the sliced grapes. But no one would talk about economics— only about such urbane topics as weather, politics, and people. In Cambridge, however, Joan Robinson took the provincials from the capital in hand and exposed them to the new macroeconomics. This proved so shocking to good students of Marshallian economics that a weekend meeting was held at Bishop's Stortford, a compromise site halfway between London and Cambridge. Joan Robinson ran the show, assisted from time to time by husband Austin, R. F. Kahn, James Meade, and others. As Lerner remembered the event, Joan did the talking. Lerner and company listened and expostulated. Joan told them that, yes, they were making progress; no, they were going backwards; and they parted ways agreeing to disagree. They did not seem to be able to understand one another—a clash of paradigms. But Lerner kept thinking and worrying about the issues and decided to spend a few weeks of a fellowship to Manchester clearing up things in Cambridge. So with wife and twin son and daughter, he moved to Cambridge and stayed six months.

At Cambridge he talked to everyone and attended the lectures of John Maynard Keynes. The text was the galley proofs of the soon-to-appear *General Theory*, which marked a revolution in economics and posed the same kind of paradigm shock that Joan Robinson had inflicted, only more so. Looking back on this encounter, Lerner had trouble understanding why everything had seemed so difficult. In 1936 he was asked by the International Labour Office to write a review essay on the *General Theory*; the resulting article (1936) remains one of the most limpid discussions of the Keynesian argument, clearer than that of the inventor himself. Lerner's puzzlement testifies to the pain of changing assumptions and parameters when one has mastered another system. Better to start from scratch, as Lerner was to find with his own students.

After his year in Cambridge and Manchester, where he went to learn about applied economics and statistics, Lerner taught as assistant lecturer at LSE, then came to America, where he was to spend the rest of his life teaching and writing at an extraordinary array of institutions. These included the University of California at Berkeley, Columbia University, the University of Kansas City, the New School for Social Research, Roosevelt University in Chicago, Michigan State University, and others too numerous to mention. Although he had started his teaching career late, he made up for it by offering instruction almost to the end of his life. After he retired from Berkeley in 1971, he served as distinguished professor of economics at Queens College of the City University of New York until 1978 (he was then seventy-five) and then took a chair at Florida State University, which he held until his death in 1982. In addition, he served as consultant or adviser at various times to the Rand Corporation (1949), the Economic Commission for Europe (1950-51), the Economic Advisory Staff in Jerusalem (1953-55), the Institute for Mediterranean Affairs (1958-59), and the Treasury and the Bank of Israel (1955-56).

The most productive years of Lerner's career came early, during that first exhilarating stage as undergraduate (yes!) and graduate student and the period that followed his first exposure to Keynesian economics. Reflecting on this precocity, Paul Samuelson later speculated that prodigies are not necessarily young. Lerner was twenty-six when John Hicks first discovered his quality in a class at LSE and called him to the attention of Lionel Robbins; and he was twenty-eight when his first paper appeared. Samuelson suggested that "it may be the number of years after you *enter* economics that counts and not the number of years after birth" (1964, p. 169).

Between 1933 and 1939 Lerner published twenty-nine articles and notes, some of which made a lasting mark on the discipline, on both its substance and folklore. In a country that was not unready to appoint to a professorship in Oxbridge on the basis of a single article, Lerner should have been professor many times over. But his achievements were counterbalanced in British eyes by religious origin, dress, and manners: Abba Lerner, Jew from Eastern Europe and then the brick and grit of the East End, bare feet in sandals (because, he said, his feet sweat), unpressed trousers hanging, shirt collar open, was a hippie before his time. In some things he could be difficult; in others he was too permissive. He had a disconcerting way of saying what he thought. The would-be genteel folk of academe could not see him twirling a sherry glass and making small talk in wood-paneled common rooms. The story is told, based on unpublished letters, that Professor Lionel Robbins of LSE consulted Keynes in this regard when a post opened at the London School. Lerner was an unavoidable candidate. Keynes Brit-wittily replied by referring to Lerner's origin as from the Continent. Maybe, he wrote, if they found a job for Lerner as a cobbler during the day, they might wear him out and have him teach in the evening. Lerner did not get the job. He probably continued to pay for his particularities when he moved to the other side of the Atlantic. At any rate, he did not receive a post at a major university until very late in his career.

But those articles! Take, for example, "The Concept of Monopoly and the Measurement of Monopoly Power," published in 1934, Lerner's first year as a graduate student. This revolutionary departure from the prevalent view of limiting cases of perfect competition and perfect monopoly was written before Edward Chamberlin and Joan Robinson's books on monopolistic competition became available. Not only did it introduce the notion of degrees of monopoly but it offered a measuring stick: the distance of prices from the social optimum reached in perfect competition, which Lerner defined as equal to marginal cost. Lerner showed that $P = MC$ is a necessary and sufficient condition of an optimum allocation of resources—hence that it is a necessary condition for maximizing welfare.

In addition, the article introduced "the first clear, rigorous and definitive statement of Pareto optimality" (Scitovsky, 1984, p. 1551). Pareto's statement of the principle had in effect been forgotten if not misunderstood. As Samuelson put it, Pareto was "obscure and a bit confused" (1964, p. 172); besides, the concept is a deep one. Lerner expressed it in the form that has since become a staple of training and thinking in economics:

> The social optimum relative to any distribution of resources (or income) . . . will be reached only if the resources which are to be devoted to satisfying the wants of each individual are so allocated . . . that his total satisfaction would not be increased by any transference of resources from the provision of any one of the things he gets to any other thing he wants. This would show itself in *the impossibility of any individual being put in a preferred position without putting another individual in a worse position*. We may adopt this as our criterion or test of the achievement of the relative optimum. (1934, p. 162; my italics)

Another theme of these early papers was international trade, and here too Lerner generated ideas in almost wasteful abundance. His readers—even the best of them—rarely caught all the implications. In preparing his sixtieth-birthday salute, Samuelson reread these pieces and was still making discoveries. In the above-cited piece on "The Concept of Monopoly," for example, Samuelson found "clear recognition that Marshall's dictum, that one should tax increasing cost industries to subsidize decreasing (or constant!) cost industries, simply represents an error" due to Marshall's failure to aggregate producers' with consumers' surplus. (Lerner was not himself always aware of his iconoclasm. Samuelson exclaims: "Lerner treats this as a well-known error!" Kenneth Arrow, however, recalls that Allyn Young, who taught at LSE until his death in 1930, had caught this as early as a review of Pigou's *Wealth and Welfare* in 1913, so that perhaps Marshall's remark was indeed a well-known error in some circles.) The same holds for the doctrine that the harm resulting from deviation from marginal cost is cumulative: Samuelson had always associated it with the postwar work of McKenzie and Scitovsky. But it's right there in Lerner's hypothetical calculations (1934, p. 172). And Samuelson sums up: "Papers like those of Lerner's are so packed with results that few readers have ever gleaned all their fruits" (1964, p. 171).

One wonders sometimes whether people were doing their reading. Lerner's very first article, "The Diagrammatical Representation of Cost Conditions in International Trade," published in 1932 when he was still an undergraduate, was the first to combine Haberler's concept of a production-possibility frontier (1930) with collective indifference curves to derive a two-country equilibrium of international trade. Who read it? Can it be that *Economica* was seen as a house organ of LSE and lacked the resonance of the older, more established journals? In any event, much the same ground had to be covered again, and to much more attention, in Leontief's 1934 essay in the *QJE* on "The Use of Indifference Curves in International Trade."

But one should not blame only Lerner's readers. He himself was not always aware of what he had done. There is the extraordinary example of his work on the equalization of factor prices across countries. Classical economists understood that free movement of labor and capital across frontiers would equalize their prices from one country to another, and the Swedish economists Eli Heckscher and Bertil Ohlin took the story further by showing that free movement of goods can substitute for factor mobility and reduce international differences in factor prices. Then in 1948 and 1949, Paul Samuelson offered first a geometric and then an algebraic proof that such movement would equalize prices, subject to specified constraints.

Samuelson's demonstration found its way to the desk of Lionel Robbins, who recalled that he had heard the same argument from a student in seminar some fifteen years earlier; and he still had a copy of that seminar paper, by Abba Lerner. At Robbins's urging, Lerner published the piece as originally written: "Factor Prices and International Trade," in *Economica* in 1952. The question remains, what had Lerner done with his copy of the paper during all those years?

The story, as told by Scitovsky, is that it had been lost; that Lerner had given the corrected typescript to a fellow student, who had volunteered to type it for submission to a periodical and then left it on a bus. It was never recovered, and Lerner was too busy working on other papers and perhaps too embarrassed by the circumstances to recover the text. At least that was the talk among Lerner's students in 1935, when Scitovsky was one of them.

These early pieces were written in the context of a gathering debate on the effectiveness of capitalism as an economic and social system, especially by comparison with a hypothetical socialist alternative. (It is always hard to argue against utopia.) Lerner was on the socialist side, but his economic analyses gave little comfort to his spiritual and intellectual comrades. His heart may have been in the right place, but he never let his heart rule his head. As a result, he preferred efficiency to orthodoxy, competition and freedom to state monopoly and dictation. Not that he thought private enterprise intrinsically superior: that had to be tested in the marketplace, and both private and public sectors should be free to prove their worth. (Ironically, Lenin had had fewer doubts on the subject. He thought that to let even one village grocer subsist would be to invite the return of capitalism.)

In anticipation of this contest between public and private, Lerner devoted a series of articles to those principles that should govern socialist planners and economic managers and enable them to duplicate the advantages of a free, competitive market. He then worked these into his first major book, *The Economics of Control—Principles of Welfare Economics* (1944). The book is written as a kind of owner's/ user's manual for a command economy, but it is much more than that, as the subtitle indicates. It is a study of the character and conditions of optimality, presented in clear, simple, nonmathematical prose. (That probably cost it with the experts, who were moving increasingly to mathematization of argument.) It begins with the exchange economy and moves on to production, with special reference to the problems posed by indivisibility of factors. From there it takes up such matters as efficient allocation in the short and long run, rent, economic surplus, taxation and fiscal policy, investment, international trade and finance, and—a gloss on Keynesian analysis—the thorny link between unemployment and inflation. Scitovsky's appreciation of this ambitious work will serve to situate it in the history of economic thought: "By comparing Lerner's book to Pigou's *Economics of Welfare* (1920), one realizes how narrow and one-sided was Pigou's interpretation of that term, and what enormous progress was made in one generation. Had Lerner written his *Economics of Control* fully footnoted with a complete set of references, one would also realize the magnitude of his own contribution to that progress" (1984, p. 1553).

The most controversial aspect of Lerner's book was his discussion of distributional optimality: What distribution of income would maximize happiness (aggregate satisfaction)? As Lerner had recognized but set aside in his article of 1934 ("We cannot here go into the problems connected with optimum distribution"), Pareto optimality was compatible with any and all distributions of income, however skewed. The principle seems intuitively unjust. In *The Economics of Control*, Lerner posed the question: What income distribution would maximize the sum of individual satisfactions *if* (1) the size of income were independent of its distribution; (2) the ability to experience satisfaction were independent of distribution; and (3) the ability to experience satisfaction were unknown, that is, if utility functions differed in ways unknown, so that ignorance was symmetric? His answer: if we assume there is diminishing marginal utility (that satisfaction decreases with growing consumption of any good or service) and that a move away from equality is as likely to increase as to diminish aggregate satisfaction, society's overall satisfaction will be highest if income is equal for all.

This argument, needless to say, became a subject of sharp debate, in which logical proofs alternated with comparisons of abstract values and psychological attitudes. Was the sum of individual utilities a proper measure of social welfare? Was satisfaction a function of absolute income or relative as well? Is equality of result inherently good or does it reward some more and some less than they deserve? And if it does the latter, that is, misallocate reward, does such misallocation reduce the social pie? Do incentives matter? And even if there are some distributions that are more productive than others, who is to say what they are and how to bring them about, much more convince people of their productivity and fairness? The kind of demonstration provided by Lerner is testimony to the power of economics to pose questions clearly, specify conditions, and generate answers within these constraints, but testimony also to the limitations of such reasoning as constraints are relaxed and complications introduced.

For all these original and important contributions, any one of which might have been the making of a tenured career at a major university, Lerner will probably be remembered best for his clarification and extension of Keynesian theory and policy. Keynes himself was not always ready to keep up with him. In 1943 Lerner published an article, "Functional Finance and the Federal Debt," that announced a new approach to fiscal policy. (The subject was further developed in his *Economics of Control* and the *Economics of Employment*.) He noted that conventional fiscal wisdom was based on the principles and morals of good household management: don't spend what you don't have— a tacit reminder that the words "economy" and "economics" are etymologically derived from *oikos*, the Greek word for household.

Lerner, however, picking up on the summary Keynesian prescription of deficit spending, argued that governments should not be concerned with conventional morality but rather should consider only the results of their actions. The aim of government spending and taxing, he said, should be to hold the economy's total spending at a level compatible with and conducive to full employment at current prices— in other words, no unemployment and no inflation. In doing this the government should not be concerned with deficits or debt. Second, the government should borrow or repay only insofar as it wants to change the proportions in which the public holds securities or money. Changing this proportion will raise or lower interest rates and hence discourage or promote investment and credit purchasing. If the only question, then, was how to finance a deficit, Lerner advocated printing money. Third, the government should put money into circulation or withdraw (and destroy) it as needed to effect the results called for by the first two principles.

To those who objected to such a radical program (and Keynes, at least initially, was one), Lerner replied that it was not so radical as it seemed. If the government operated as he prescribed, nothing would go wrong. The natural, almost instinctive, concerns about inflation were denied if not allayed by the reminder that, so long as the government observed the first principle of good functional finance and increased the money supply as much as and no more than would hold effective demand at a level that would sustain full employment at current prices, there would be no inflation. (To this a cynical economic historian might reply that if politicians were economists they would cease being politicians, and conversely.)

By the same token, Lerner rejected the fear that servicing a large public debt would entail heavy taxes and thereby reduce the reward for risk taking and the incentive to invest. He pointed out that the same high income tax that reduces gain provides a deduction in the event of loss; net return may in fact be improved thanks to tax offsets. Scitovsky notes with surprise that "neither Lerner nor any of his critics . . . thought of another and possibly real danger of the high income-tax rates needed to service too large a public debt: the diminished incentive to work" (1984, p. 1560). To which I would add that entrepreneurs and investors do not ordinarily enter into ventures with the expectation of loss. They expect to make money, and the tax rate is necessarily a factor in their calculations of potential gain. To be sure, the rate at which tax deductions may be taken will affect (distort) normal incentives—hence the world of tax shelters and *Springtime for Hitler*. But investments made in anticipation of loss are surely less than optimal from a macroeconomic point of view.

Keynes jibbed at Lerner's logical policy development of his own macroeconomics. In a letter to James Meade in April 1943, Keynes noted that Lerner had written that, once the national debt built up big enough, it would no longer be necessary to borrow to enhance purchasing power—that the interest on existing debt would provide the necessary injection. (In effect, the government would be printing money.) "His argument," Keynes wrote, "is impeccable. But heaven help anyone who tries to put it across [to] the plain man at this stage of the evolution of our ideas" (cited in Colander, 1984, p. 1574). And much later, at a seminar at Boston University in 1972, Lerner recalled putting the matter to Keynes in Washington in 1946, at the time of Bretton

Woods: "Mr. Keynes, why don't we forget all this business of fiscal policy, public debt, and all those things and have some printing presses?" To which Keynes replied: "It's the art of statesmanship to tell lies, but they must be plausible lies" (*ibid*.). (Kenneth Arrow heard a somewhat different version from Paul Baran, who remembers Keynes's reply as, "Mr. Lerner, how many times do I have to remind you that you cannot run a government on *transparent* humbug?")

Keynes, like most people who think about government and serve it, was of two minds; Lerner of one. When confronted by the implications of his own reasoning, Keynes is said to have remarked, "I am no Keynesian." The quip is perhaps apocryphal, but *se non e vero*, *e ben trovato*. As Lerner himself put it later in "Keynesianism: Alive, If Not So Well" (1978), Keynes was timid: "He did not carry his conclusions all the way."

That was one charge one could not lay against Lerner: he was the quintessence of rationality if not practicality, and, once he established his premises, the rest followed inexorably. One of the best examples was his proposal in 1942, when the United States had just entered the war, that each unit commander be allocated funds and permitted to equip his unit as seemed best: so many tanks, perhaps some air support, a few engineers, and so on. In effect, Lerner the socialist was ready to turn that most socialized of institutions, the army, into a congeries of small enterprises. The very idea appalled those who heard it, and to the end of his life Lerner regretted that he had allowed some of his colleagues to talk him out of publishing the proposal. Tibor Scitovsky, who was one of those friendly counselors, writes that critics had more faith in the army's collective wisdom than in the good judgment of individual commanders. (In the light of generalized business incompetence in the military, at all levels, I would have placed more stress on problems of coordination and transaction costs.) Besides, they had Lerner's best interests at heart, fearing that "so fanciful an idea . . . would worsen rather than improve his chances for professional advancement" (Scitovsky, 1984, p. 1566).

Scitovsky subsequently had second thoughts about the substance of the proposal if not about Lerner's advancement: if the French had had such an arrangement in the 1930s, he writes, the young General de Gaulle might have had the opportunity to put into effect his revolutionary ideas of mechanized warfare, and the course of history might have been different. In fact, there is historical precedent for such an arrangement, although Lerner was probably unaware of it. In the days of mercenary armies, it was not uncommon for commanders to recruit their own force and arm it, as a kind of personal venture.

The problem, of course, is that he who invests (even other people's money) may expect a return. The result might be some ferociously energetic warfare accompanied by equally energetic rapine and pillage. The same for risk: one of the constants of coalition warfare is the effort of some commanders to transfer risk to others. A number of great battles have been lost because of foot dragging by allies. That's the market for you. To be sure, Lerner or another good marketeer would not be at a loss for remedies. Why couldn't one commander pay another for help? Of course, if the bargaining took too long, the supplicant might lose his ability to pay. That's war for you. (In Ottoman times there were no public firefighters in Constantinople: private companies ran to fires and negotiated competitively with owners the price of intervention. Time was of the essence, on both sides, for the willingness of the owner to pay fell with the value of the shriveling remainder.)

Lerner kept this ability to reason things out to their logical conclusion, however iconoclastic and revolutionary, to the end of his days. In 1979, vexed and troubled by monopolistic oil prices levied by OPEC, he proposed a tax on petroleum imports that would vary with the deviation of prices from some imputed market level. The aim would be to deter increases by multiplying their negative impact on demand. To make his point to his fellow economists, Lerner stood outside the entrance to the large hall where Robert Solow was about to deliver his presidential address to the American Economic Association and passed out flyers. The proposal was probably seen once again as politically impractical; and, in fact, the federal government has always been very chary of increasing the price of gasoline by levying higher taxes. But Lerner was surely right, as we have seen from reactions to unmultiplied price increases in the 1970s and 1980s. In the meantime, American motorists pay at the pump a third or less of the price paid by European or Japanese consumers and are correspondingly more wasteful.

In his last years one of Lerner's abiding concerns was inflation and its cousin stagflation (inflation with inadequate demand). As someone who believed that proper management could be allied with market incentives, he sought a way to discourage price increases while continuing to reward enterprise and growth and to hold costs while not increasing unemployment. Lerner was a manager at heart, but he understood that compulsion in the form of such traditional remedies as price controls simply did not work. He found his answer in MAP—an acronym for Market Anti-Inflation Plan, itself an abbreviation of Market Mechanism Anti-Inflation Accounting Plan, which he first presented to the Sixth Annual Atlantic Economic Conference in October 1978.

Stagflation, Lerner affirmed, was the result of an expectational equilibrium in which prices, wages, and total spending would keep rising and chasing one another in an inflationary zero-sum game. The cure, Lerner felt, had to come from continued spending at a level that would buy the output of full employment—but no more. Meanwhile one had to stabilize the average price level while allowing individual prices to vary to reflect changes in taste and productivity. The aim: an economy that cools down while retaining its dynamism. How to do this? Briefly, by setting for all firms a normal rate of increase in sales, say to 103 percent of the previous year. If a firm sells more, it incurs an "anti-inflation deficit"; if it sells less, it gets an anti-inflation credit. Successful firms that can and want to grow faster must buy credits from those that are growing slower. Meanwhile, to encourage hiring, each new employee entitles the firm to a free credit equal to his wage in previous employment multiplied by the new firm's ratio in the previous year between net sales and wage bill. This was later modified to avoid rewarding mere shifts to more labor-intensive techniques at the expense of efficiency. And so on.

The proposal was never put into effect, in part surely because it would have entailed the creation of a costly bureaucracy and generated a large (nightmarish?) accounting burden for individual firms. But it is testimony to Lerner's imagination and ingenuity and devotion to the common weal. He loved and wanted to do something about problems.

Of his many proposals, only two or three have actually found their way into common use. One is functional finance, which shocked at first but has proved congenial to governments. The second is government intervention to counter speculation by monopolistic manipulators, now standard procedure in money markets and foreign exchange. Lastly, there is Lerner's recommendation that socialist planners should choose between private and public enterprise on the basis of efficiency. We see more and more of this in eastern Europe. But can public enterprise survive in open and fair competition? And if it cannot, is this really socialism? And what about power?

Abba Lerner died in October 1982. He was survived by his wife, Daliah; a son, Lionel, and daughter, Marion, both by an earlier marriage; sisters Hannah (Banerji), Bella, and Dorothy; and brother Jack. Lerner was one of the greatest economists of this century. But perhaps because he worked with words and diagrams rather than equations—he was a master of limpid prose—he never got the recognition he deserved. That his name is not on the roster of Nobel prize winners in economics is cause for regret.

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HONORS AND DISTINCTIONS

DEGREES	
1932	B. Sc. (economics), London School of Economics
1943	Ph. D. (economics), London School of Economics

PROFESSIONAL RECORD	
1935-37	Assistant Lecturer, London School of Economics
1939-40	Lecturer in Economics, Columbia University (fall term)
1940-42	Assistant Professor, University of Kansas City
1942-46	Associate Professor, New School for Social Research
1946-47	Professor, New School for Social Research
1947-59	Professor, Roosevelt University
1959-65	Professor, Michigan State University
1965-71	Professor, University of California, Berkeley
1971-84	Professor, Florida State University

VISITING PROFESSOR	
1938 (spring);	
1958-59;	
1960, 1962,	
1963	University of California, Berkeley (summers)
1940	University of Virginia (spring)
1942-43	Amherst College (fall)
1947	Roosevelt College (spring, summer)
1948, 1950	New School for Social Research (summers)
1954-56	The Hebrew University, Jerusalem
1957	Columbia University (spring, summer)
1957-58	The Johns Hopkins University
1958, 1959	Michigan State University (summers)
1965	University of Hawaii (summer)
1965-66	University of Tel Aviv
1976, 1977	Florida State University (winter and spring)

CONSULTANT	
1949	The Rand Corporation (summer)
1950-51	The Economic Commission for Europe, Geneva

1953-55	Economic Advisory Staff, Government of Israel
1955-56	Treasury, Government of Israel, and the Bank of Israel (Adviser)
1958-59	Institute for Mediterranean Affairs, New York

HONORARY SOCIETIES

1971	Fellow, American Academy of Arts and Sciences
1974	Member, National Academy of Sciences

HONORS

1932	Gonner Memorial Prize, LSE
1932	Gladstone Memorial Prize, LSE
1932-34	LSE Research Fellowship
1934-35	Leon Fellowship, University of London
1938-39	Rockefeller Fellowship
1960-61	Fellow, Center for Advanced Study in the Behavioral Sciences
1963-64	Vice-President, American Economic Association
1964	Regents Lecturer, University of California, Santa Barbara
1966	Distinguished Fellow, American Economic Association
1970	Honorary Fellow, London School of Economics
1973	President, University Centers for National Alternatives
1978	D.Sc. (honorary), Northwestern University

PROFESSIONAL SOCIETIES

American Economic Association

Econometric Society

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CARL SHIPP MARVEL

September 11, 1894-January 4, 1988

BY NELSON J. LEONARD

CARL S. MARVEL had a spectacular career of seventy-two years in organic chemistry. It was during the same period that the chemical industry in the United States experienced its greatest growth. The two careers were synergistic. From 1920 to 1961, Dr. Marvel was on the staff of the University of Illinois in Urbana, and from the date of his first retirement through 1987 he was a faculty member at the University of Arizona. He consulted for nearly sixty years for the DuPont Experimental Station. He was a dominant figure in American organic chemistry and has been recognized as the "father" of synthetic polymer chemistry. The impact of his teaching, research, and consultation was matched by his important contributions to government, foundations, and the professional community. It was at the personal level, however, that his influence was most pervasive, reaching beyond the 176 Ph.D. students and 150 postdoctoral students whom he trained to thousands of chemists, friends, colleagues, and acquaintances with whom he shared common interests or goals. His personality was so memorable that his influence will continue to affect and guide the lives of all those with whom he came in contact.

Carl Marvel was born on a farm three miles south of Waynesville, Illinois, during the forenoon of September 11,

1894. His father, John Thomas Marvel, who was a farmer from a farm family, was of Norman, English, and Irish ancestry. The Marvel family migrated from Normandy to Cambridgeshire, England, in 1091 and eventually to Sussex County, Virginia (later Maryland), during the period 1630-1707. John Marvel had a limited grade school education with a very short period at the preparatory school of Illinois Wesleyan University. Carl's mother, Mary Lucy Wasson, who was also from a farm family, had grade school and high school training. She qualified for college, but her father disapproved of college training for women, so she taught country school until her marriage. She was of Danish, Scotch-Irish descent. The Wasson family is supposed to have moved from Denmark to Scotland during the Danish invasion and colonization period.

Carl grew up on the Illinois farm with three sisters and did the usual farm work during vacation periods. He did not, however, drop out of school to help on the farm during the spring and fall work seasons, which was contrary to the usual custom of the community. His father believed Carl's schooling came first. Carl was interested in flowers, birds, animals—in short, in all of nature. He spent much time hunting on winter weekends after he was six or seven years old. He also ran trap lines for muskrat, mink, and skunk to earn spending money. He related that after he had skinned one skunk the others were sold without preliminary treatment. He attended the West Hull District School in Barnett Township, Dewitt County, Illinois, through the eighth grade, which he completed in 1907. He remembered with special gratitude three teachers at that school, Mary Keys, Grace Barr, and Ida Jeffrey, who stressed proper study habits for their pupils and who insisted that only the best work possible was acceptable.

He was the last graduate of Waynesville Academy, which he attended from 1907 to 1911, when it was turned into a township high school. The school was four miles from the Marvel home, and Carl either drove or rode a horse back and forth every day. He singled out Professor W. H. Smith, a retired minister, as being a superb teacher of English, mathematics, history, Latin, Greek, and German (!), who also suggested books to read covering physics, zoology, and botany when he learned that Carl was ambitious enough to go to college. Carl learned to classify and identify the flowers of central Illinois and to know them thoroughly. He considered that this self-instruction method did much to improve his self-reliance.

At Illinois Wesleyan University, which he attended during 1911-15, receiving A.B. and M.S. degrees in 1915, he was introduced to chemistry as a freshman. An uncle, who had been a high school teacher, advised his nephew to take this subject if he expected to be a farmer, since the next generation of farmers would require scientific knowledge to get the most out of their work. During most summers while Carl was attending high school and college, he worked as a farmhand, but during one summer he was an auto mechanic and a driving teacher for another uncle who sold cars in Lincoln, Illinois. At Illinois Wesleyan, he took as much biology as possible under Professor F. E. Wood. Mr. Wood found, in botany class, that Carl was familiar with the flowers of the region and asked that he choose another family to study. The result was a semester spent in identifying the mosses of central Illinois and in the assembly of a collection of fifty to sixty specimens.

One of Carl's first successful scientific experiments was carried out during the summer vacation between his junior and senior years in college. His father was skeptical of the importance of bees in the fertilization of red clover blossoms. For his benefit, Carl selected a healthy red clover plant in the farm yard and carefully covered half of it with a fine screen shelter. The other half was allowed to grow exposed. He was pleased to be able to show his father a good crop of clover seed in the heads that grew outside the screen and the absence of seed in the sheltered heads. This experiment convinced his father that scientific study might be useful to a prospective farmer and persuaded him that perhaps Carl might even profit from graduate study when the opportunity developed in due course.

It was the excellent teaching and personal attention received from Professor Alfred W. Homberger at Illinois Wesleyan that was directly responsible for Carl's decision to specialize in chemistry. During his final year at Illinois Wesleyan, Carl was a teaching assistant and did a thesis problem with Professor Homberger that resulted in his first publication. The same professor succeeded in obtaining for his prize student a $250 scholarship in chemistry at the Graduate College of the University of Illinois to enable him to study further. Carl's graduate education started in 1915 with an overload of course work, including four lab courses, in order to "catch up." When he was not studying, he worked late at night in the laboratory. As a result, he slept as late as possible but still got to the breakfast table before the dining room door closed at 7:30 a.m. His student colleagues decided that was the only time he ever hurried, and they nicknamed him "Speed." A nickname was appropriate to his friendly spirit, but it causes us to smile in retrospect because it was really an accurate moniker indicative of his chemical thinking, his human insight, his fishing and bird-watching prowess, the way in which he took care of his correspondence, and the alacrity with which he helped a student or colleague who had a chemistry or personal problem.

As a chemistry graduate student, Speed Marvel delighted in the synthesis of organic compounds and earned an early and lasting reputation for his ability to classify and identify volatile organic compounds by odor alone. When he entered graduate school, Germany was the center of chemical manufacture and research, but the start of the war in Europe had interrupted the supply of chemicals to the United States. The shortage, which included research chemicals, led Professor Clarence Derick to organize a group of graduate students at Illinois to work during the summer of 1916 making chemicals to fill the needs of the departmental research programs. An A.M. degree was awarded to Speed in 1916. When the size of the Organic Chemical Manufactures group was increased by Roger Adams, who had arrived from Harvard to be head of the Department of Chemistry, Marvel joined the group in the summer of 1917 and stayed with it for two years. He went back to full-time graduate work in 1919, received a DuPont fellowship for his final year of graduate study, and in 1920 received his Ph.D. in chemistry under the direction of Professor William Albert Noyes with a thesis entitled "A Study of the Possible Asymmetry of Aliphatic Diazo Compounds," which was published in the same year in the *Journal of the American Chemical Society*. That study represented an initiation of his interest in organic stereo-chemistry.

He also received great stimulation from three teachers, Roger Adams, Oliver Kamm, and H. B. Lewis. Adams hired Marvel as an instructor in chemistry at the University of Illinois in 1920, a rank he held during 1920-22, followed by associate, 1922-25; assistant professor, 1925-27; associate professor, 1927-30; professor of organic chemistry, 1930-53; and research professor of organic chemistry, 1953-61. In his early teaching days, Wallace H. Carothers and John R. Johnson were also instructors at the University of Illinois. Marvel, Carothers, and Johnson, along with several graduate students, including Paul Salzberg and M. M.

Brubaker, organized a special seminar in organic chemistry that met weekly in Marvel's home to discuss controversial questions in organic chemistry. They called the seminar "Chemistry 398," since they were not quite at the "400" (graduate course) level of organic chemistry at the time.

Marvel gained wide-ranging experience in synthesis by submitting and checking preparations for *Organic Syntheses*. This publication, the first annual volume of which appeared in 1921, is a collection of preparations of organic compounds for which checked directions are provided in detail. Collective Volume I of *Organic Syntheses*, which contained the preparations that appeared in the first nine volumes, was published in 1932 and was reprinted in 1941. Nearly 20 percent of the 264 preps in that volume were either submitted by Marvel or checked by him, which represented a most energetic and devoted contribution to synthetic organic chemistry.

Marvel's early published papers reflected both his love of synthesis and the need of the chemical community for pure organic compounds—for example, the amino acids that were to form the basis of Professor William C. Rose's classical research on human nutritional requirements, in particular the essential amino acids. Marvel prepared dialkylmercury compounds and investigated their reactions, synthesized hexasubstituted ethanes and studied their dissociation, synthesized polyines and dienynes and determined their modes of rearrangement or cyclization, and made and studied the reactions of alkyllithium and Grignard reagents, quaternary phosphonium, and ammonium compounds. In addition, he perfected the synthesis of organic chemical reagents needed in the early years for characterization, identification, and analysis. His seminal research on intermolecular hydrogen bonding performed prior to infrared capability was based on solubilities and heats of mixing.

Trained as an organic chemist, Professor Marvel's interest in polymer chemistry emerged from studies of organic reactions. His approach to polymer chemistry was characterized by the same skills in synthesis, structure determination, and technique improvisation that marked his early contributions to organic chemistry. To him, polymers represented a logical extension of his research on simple organic molecules, and he thought of their synthesis, analysis, and characterization in a similar vein. Guided by this philosophy, he made major fundamental contributions to the polymer field.

In 1933 he determined that γ-halopropyldimethylamines yielded open-chain polymeric products and subsequently found that, by contrast, the diethyl, di-*n*-propyl, and di-*n*-butyl analogs yielded monomeric cyclic ammonium salts. He introduced high dilution to produce the dimethylammonium four-membered ring compound and delineated the conditions under which it converted to polymer.

His extended studies of olefin/sulfur dioxide polymers provided key chemical methodology for all subsequent investigations of addition polymerization. In 1934, Marvel and his students, following earlier reports of Solonia (1899), Matthews and Elder (1914), and Seyer and King (1933), and contemporaneously with Staudinger, found that a polymeric sulfone was produced by the reaction of sulfur dioxide and an olefin, cyclohexene, in the presence of a peroxide catalyst. Determinations of elemental analysis, average molecular weight, end-group analysis, and the products of alkaline fusion established the principal structural features of the high-molecular-weight polymer. Reaction of sulfur dioxide with monosubstituted olefins and acetylenes and with mixtures of olefins established the generality of the addition polymerization. Polypropylenesulfone was found to have a head-to-head, tail-to-tail orientation, while the polymer from vinyl chloride and sulfur dioxide was found to have a head-to-tail array of two units of olefin to one of SO_2. In an exploration of various initiators of olefin/SO_2 polymerization, Marvel demonstrated that identical structures were obtained from catalysis with peroxide, ultraviolet light, and amine oxides.

Marvel's success in elaborating the structures resulting from olefin/SO_2 polymerization led him, starting in 1938, to investigate the structures of other vinyl polymers, particularly homopolymers from monosubstituted olefins. He used the reactions of the carbonyl group in the polymers of methyl vinyl ketone and isopropenyl methyl ketone and of the hydroxyl group in polyvinyl alcohol to establish that these were head-to-tail polymers. He showed similarly that the copolymer of vinyl chloride/vinyl acetate had a head-to-tail structure. He supplemented end-group analysis with the finding that the kinetics of addition polymerization could be followed through the use of a vinyl monomer containing an optically active group. It should be noted that Marvel was among the first to recognize the significance of stereo-regular polymers, which he endeavored to prepare by the principle of asymmetric induction using optically active initiators and monomers.

Marvel began to receive recognition for his innovations in research. He gave the Foster Lecture at the University of Buffalo, Buffalo, New York, in 1937. He was elected to membership in the National Academy of Sciences in 1938 and served, during 1944-47, as chairman of the Section on Chemistry. He gave the Julius Stieglitz Memorial Lecture before the Chicago Section of the American Chemical Society in 1943 and received the Nichols Medal of the New York Section of the American Chemical Society in 1944.

With the onset of the second World War, Marvel became involved in U.S. Government service. In September 1940 he became associated with Section C-2, Synthetic Problems, of Division B of the National Defense Research Committee, performing various tasks until the reorganization of the NDRC in September 1942, when he served as chairman of Section B-3, Synthetic, Analytical, and Inorganic Problems, until a further reorganization of the section as Division 9 took place in December 1942. Then, at the request of the Office of the Rubber Director, he joined the Rubber Reserve Corporation program on synthetic rubber synthesis. The U.S. government launched a major program to alleviate the critical shortage of natural rubber, without which tires for automobiles, trucks, military vehicles, and aircraft could not be made. Speed Marvel organized a research group at the University of Illinois, with the aid of his colleagues in organic, physical, and analytical chemistry, that concentrated on the synthesis and polymerization of 2-substituted butadienes and styrene. He also helped coordinate work on the project that involved other universities, including MIT, Chicago, Minnesota, Cornell, and Case, and industrial laboratories, including Bell Laboratories, ESSO Research, DuPont, and Union Carbide. Within one year's time, this effort brought forth adequate formulation, additives, modifiers, and processes for synthetic rubber and thereby provided successful resolution, not broadly recognized, of a potentially catastrophic situation. There was, however, recognition for Dr. Marvel's role with the award of the President's Certificate of Merit for Civilians in World War II.

When hostilities ceased, Marvel went on a technical intelligence mission to Germany to learn of innovations that their rubber industry had made in butadiene-styrene copolymers. Their rapid polymerization technique, known as redox-polymerization, was adapted to low-temperature polymerization in the United States, thereby improving the synthetic product to the point where it became a most useful general-purpose rubber. Marvel's laboratory at the University of Illinois went on to prepare, in large number, unusual butadiene copolymers with vinylsulfonic acid derivatives, anthracene and other polynuclear hydrocarbons, aconitic esters, substituted styrenes, and methyl acrylate, the latter for lithium aluminum hydride reduction to a diene/allyl alcohol copolymer.

Another major contribution to the synthetic rubber program stemmed from Marvel's extensive studies of reactions of thiols with olefins. He was the first to demonstrate the preparation of high-molecular-weight polymers by adapting the reaction to bifunctional thiols and bifunctional olefins, with the goal of imparting rubbery character to the polymers. He extended the work further to include polythiol esters, polymercaptals, and polymercaptols. The advent of coordination catalysis provided opportunities for new kinds of polymers that Marvel was quick to perceive. He made polymers of the terpenes pinene, myrcene, and alloocimene and of linear compounds containing 1,6-diene functionality that resulted in ring-containing polymers.

About 1955 the Materials Laboratory of Wright Patterson Air Force Base approached Professor Marvel, asking him to study organic polymeric materials that would withstand high temperature without loss of strength. His association with the U.S. Air Force's high-temperature polymer research program as its principal contributor, which began toward the end of his first career at the University of Illinois, continued throughout his entire second career at the University of Arizona. His basic research on condensation polymers with aromatic and heteroaromatic recurring units led to a series of products with progressively better and better properties and finally to polybenzimidazole (PBI). This was commercialized due to its exceptional resistance to fire and retention of strength at high temperature. Fibers of this polymer are used to make reentry parachutes, the suits of astronauts and fire fighters, and for upholstery and other similar uses for aerospace application. Fibers and films fabricated from two-strand or ladder polymers were recognized as providing significant improvement over the usual linear type of polymer, combining properties of lightness and high tensile strength. The U.S. Air Force Materials Laboratory honored Marvel with its Distinguished Service Award (1966) and the Air Force Systems Command, with its Award for Outstanding Achievement (1966).

While Speed Marvel always felt, and frequently reminded his colleagues, that the essential product of academic research was the students, he also taught that the best graduate training was to be achieved, along with possible national prestige, by work on essential problems. He believed there was no such thing as a dead end to a worthwhile research problem—delays and detours and retracing of steps, indeed, but no dead end.

In an early University of Illinois tradition, a new instructor gained his first experience in directing research by working with senior undergraduates. When Marvel took up his duties as a teacher at Illinois in 1920, he had six seniors in research. In the first class that Marvel taught in organic qualitative analysis there were several impressive students, including Wallace Carothers, who later invented nylon at DuPont; Samuel McElvain, who became a professor of organic chemistry at the University of Wisconsin; and George Graves, who played an important role in the plutonium plant at Hanford, Washington, during World War II. Dozens of Marvel's students went on to outstanding careers, including those who directed research in major chemical industries such as Paul L. Salzberg, Glenn A. Nesty, Donald S. Frederick, William J. Sparks, Thomas W. Mastin, Robert

R. Chambers, Max T. Goebel, John R. Elliott, John W. Copenhaver, Charles L. Levesque, James H. Sample, Charles W. Hinman, and Lester E. Coleman, Jr. Among Marvel's students and postdoctorates who made their reputations in university positions were Vincent Du Vigneaud (Nobel laureate), Henry E. Baumgarten, William J. Bailey, Alfred T. Blomquist, Robert B. Carlin, Herbert E. Carter, Frans DeSchryver, Delos F. DeTar, Arjeh B. Galun, Chester M. Himel, Evan Horning, Michael M. Martin, Charles G. Overberger, Norman Rabjohn, and John K. Stille. Volume 8, issue number 8, of *The Journal of Polymer Science* (1970) was devoted to contributions from former students and research associates as a tribute to Dr. Marvel.

Speed Marvel was a founder of the High Polymer Forum that became the Division of Polymer Chemistry of the American Chemical Society, of which he was chairman in 1950-51. During his long membership in the ACS, begun in 1915, he held just about every elective office possible, up to and including the presidency in 1945. He held that office longer than any other ACS president in history because he stepped into the position when the previous president died in office. In 1980 Marvel received an award for forty years of service as an ACS Council Member. The ACS gave him the Priestley Medal (1956), the Witco Award in Polymer Chemistry (1964), and the Borden Award in Chemical Plastics and Coatings (1973). The New York Section of the ACS awarded him the Nichols Medal (1944, mentioned earlier); the Chicago Section, the Willard Gibbs Medal (1950); and the North Alabama Section, the Madison Marshall Award (1966); and he delivered the Edgar Fahs Smith Memorial Lecture before the Philadelphia Section (1948). The Division of Polymer Chemistry of the ACS gave him both its Divisional Award (1978) and its Educational Award (1984). The all-purpose meeting room of the ACS building in Washington, D.C., is designated "Marvel Hall" to indicate the esteem with which the ACS held Speed Marvel and the society's gratitude for the leadership he provided in raising the funds that made the building possible. Other societies joined in honoring Dr. Marvel: membership in the American Philosophical Society (1945); fellowship in the American Academy of Arts and Sciences (1960); the Society of Plastic Engineers International Award (1964); the American Section of the Society of Chemical Industry Perkin Medal (1965); the American Institute of Chemists Gold Medal (1955) and the Chemical Pioneer Award (1967); the Alpha Chi Sigma John R. Kuebler Award (1970); the University of Illinois Alumni Association Alumni Achievement Award (1976); the University of Arizona Foundation Creative Science Award (1978); and an American-Swiss Foundation Lectureship, Switzerland (1951).

During his long career Marvel gave unselfishly of his time on a variety of committees and editorial boards (*Organic Syntheses*, *Journal of Organic Chemistry*, *Journal of the American Chemical Society*, *Proceedings of the National Academy of Sciences*, and *Journal of Polymer Science*), as he did also for his two universities. The University of Arizona holds biennial Marvel Symposia, and the University of Illinois has annual Marvel Lectures. Both Arizona and Illinois award Marvel scholarships. In 1984 the University of Arizona renamed the chemistry laboratory building where he worked the Carl S. Marvel Laboratories of Chemistry. At the Memorial Tribute to Carl Shipp Marvel at the University of Arizona in 1988, Dr. Richard E. Heckert, chairman of the Board of Directors and chief executive officer, E. I. DuPont de Nemours and Company, announced that DuPont had commissioned the internationally acclaimed sculptor, Charles Parks, to create bronze busts of Speed Marvel. One now resides in Marvel

Hall of the American Chemical Society, one at the University of Arizona, and another at the University of Illinois.

In the spring of 1928 Speed was invited to become a consultant to the DuPont Experimental Station. His selection was due to the kindness of Roger Adams. DuPont originally wanted Adams as a consultant, to travel to Wilmington, Delaware, every month, but so much travel was not agreeable to Adams. He suggested that DuPont hire two Illinois chemists so that they could alternate visits, and he named Marvel as his consulting cohort. There followed a long and fruitful association, during which it is estimated that Speed gave some 19,000 individual consultations. He was adept at building confidence and in guiding the industrial scientists to make their own discoveries. One of his consulting abilities was to direct the attention of DuPont scientists to appropriate publications and reports by other DuPont scientists and to encourage and facilitate contact between people working in different parts of the company. Beyond the coordinating function, Speed also provided specific help with particular problems. For example, one practical and important contribution was the suggestion of amide solvents for polyacrylonitrile, which led to the process for the acrylic fiber orlon. He was also a valuable guide in the development of DuPont's heat-resistant polymers. One of Speed's most fruitful "assists" was his encouragement of Wallace Carothers, newly arrived at DuPont in 1928, in his plan to study polyesterification and polyamidation. Speed was able to offer Carothers one kilogram of adipic acid (price, $30), made in "summer preps" at the University of Illinois. This was the first sizeable quantity of a crucial intermediate that was to play an epic role in Carothers's discovery and DuPont's development of nylon.

There are so many facets to Speed Marvel's life, and so many attendant accomplishments, that it is difficult to record all of them. His government service, during and after World War II, also included chairmanship of the National Research Council's Panel on Synthesis of Antimalarial Drugs and membership on the Board for the Coordination of Malarial Studies, 1944-46; membership on the National Advisory Health Council, 1945-47; and membership on the National Science Foundation Advisory Panel for Chemistry, 1952-54. His contributions to various research foundations were capped by sixteen years, 1971-87, on the Scientific Advisory Board of the Robert A. Welch Foundation, the mission of which is to foster and encourage the growth of the chemical sciences, primarily through operating grants to faculty for research at universities and colleges in Texas. During these years, Marvel helped in the selection of the most worthwhile basic research grants and in evaluation of their progress and continuing quality. He also played an important role, according to Dr. Norman Hackerman, chairman of the Advisory Board, in decisions as to departmental grants, scholarships, fellowships, establishment of Welch chairs, visiting lectureships, and summer science training programs, as well as presiding over two of the annual Welch conferences on chemical research.

Marvel also presided over fishing expeditions to Canada, in which he had well-photographed success, and ornithological expeditions during which he succeeded in spotting and identifying practically all of the 630 species of birds in the United States and a good sampling of the bird life wherever else he traveled. He kept a "life list," an "American list," and a "foreign list" of birds he had seen, and he was very proud of his one publication, among over 500 papers and fifty-five patents, on ornithology. Speed occasionally organized poker games with his colleagues at Illinois and DuPont, an enterprise in which he claimed, as befits a professional, only modest success; however, he did not enjoy gambling for high stakes. He was a true connoisseur of wines and, during one period of his life, cigars. In short, he excelled at so many things that a person would have been ill advised to try to surpass him in areas nurtured by his experience and knowledge.

Over and above all of these pleasurable enterprises was the fun of doing chemistry, which Speed Marvel mentioned again and again. He had infectious enthusiasm for the making of new molecules. While he was willing to recognize other viewpoints, he held the opinion that anyone not engaged in synthesis was not making his living in the most positive manner. Another source of joy for Speed was in being able to do something for someone—usually immediately and by telephone! He had great influence and liked to use it to help people and projects. The warm friendliness of his approach made him most effective in this role. A one-man employment and reemployment agency, he sought out alumni at national ACS meetings, quickly recognized all former Illinois students, and helped them in every way possible. He was always in a hurry yet always had time for people; always busy, but never gave the impression that he was being imposed upon when asked for advice. His wife, Alberta Hughes, whom he married in 1933, created a relaxed home atmosphere for him. She was motherly, practical, and unsentimental, with a wry sense of humor and a generous balance of other interests. Speed was candid and critical but also considerate and indulgent with his children, John Thomas (Jack) and Mary Catherine (Mollie), and as they developed, so did his appreciation, pleasure, and pride in them. They provided special filial devotion and physical support for him during his final dependent years. Speed's extended family comprised the thousands of chemists with whom he came in contact, whether at Illinois, Arizona, DuPont, or in professional circles through out the country. The chemical community showed its appreciation for his research, teaching, and personal contributions with numerous recognitions in addition to those mentioned earlier and in honorary degrees from Illinois Wesleyan University; the University of Illinois; the University of Louvain, Belgium; and the Polytechnic Institute of New York.

In remembering Speed we may well recall a few of his favorite sayings, but some may become entwined with the aphorisms of Mark Twain, another man who crossed the Mississippi River. "Anyone is a fool to go into academic work. All good chemistry is done in industry. If chemistry isn't fun, it shouldn't be done. Insurance is useless due to inflation with the Democratic (or Republican) Party in power. It is wicked to gamble and lose. Membership in a scientific honor society is like a pair of pants—you don't get any credit for it but you would look funny without it." His advice to a department head (Herbert E. Carter) included the following messages: "Keep committees to a minimum. They seldom create new ideas and are too often swayed by the most aggressive talker. Never take a vote until you know you have a good majority on your side. Never ask the Provost for less than you need, but always supply documentation. Don't ask for funds to do something—start doing something, even at a sacrifice, and ask for funds to continue and expand a promising activity. Know your faculty and keep track especially of the young chemists." Speed could drink the hottest coffee and consume the largest amount of popcorn. He was fond of guiding his colleagues through the Greek alphabet and of interjecting Latin quotations. He teased us with statements of the wonderful chemistry they were doing at DuPont that he wished he could tell us about. In answer to our random complaints, he had lived through a bigger snowstorm, had had a worse graduate student (who improved dramatically) and a worse secretary (who responded to training), and always felt old (while doing the work of at least three young people). In remembrance of Speed Marvel we smile for someone we really cared for and who cared for all of us.

Carl Shipp Marvel loved people and loved chemistry. He contributed to basic methods for the synthesis of organic compounds and taught us that polymers could be treated as extensions of simple organic molecules. He made major fundamental contributions to the polymer field with innovative methodology in synthesis, analysis, and characterization. His ideas and his research led to substantial practical results; for example, the production in the United States of synthetic rubber, amide solvents for polyacrylonitrile, and polymers with exceptional resistance to fire and retention of strength at high temperature for aerospace applications. He was highly successful in teaching and in research guidance and training. He was the mainstay of organic chemistry first at the University of Illinois and then at the University of Arizona. Marvel was a valued and venerable consultant for the DuPont Company, and he was a magnet for chemists and for the profession of chemistry during almost three-quarters of a century.

I AM INDEBTED TO the other speakers at the Memorial Tribute to Carl Shipp Marvel at the University of Arizona in 1988 for information provided on that occasion. I have drawn heavily on Marvel's autobiographical material either published by him or available from the files of the National Academy of Sciences. I have also provided my own reading of this scientist and events of his life during the period (forty-six years) it was my privilege to be his colleague and friend.

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H W Menard

HENRY WILLIAM MENARD

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BY ROBERT L. FISHER AND EDWARD D. GOLDBERG

HENRY WILLIAM MENARD was a scientific revolutionary. His research and his hypotheses, along with those of a relatively small, relatively young band of colleagues in the United States and the United Kingdom, provided the observational foundation from which grew the new global tectonics of the mid-twentieth century. A few scientific revolutionaries are scholars and Bill was one. His interests in the earth sciences were broad, spanning sedimentology, geomorphology, tectonics, geophysics, and geostatistics. But he ranged far into other areas of learned endeavors. He was a student of history, fascinated by events in the United States and England over the past several hundred years. English literature was especially attractive to him. Four of his six books melded natural science and social science. Of these, *The Ocean of Truth*: *A Personal History of Global Tectonics*, published posthumously by the Princeton University Press in 1986, is one insider's scientific history of the seafloor-spreading concept and plate tectonics. Henry Frankel, the distinguished philosopher of science, said in a review (*Eos*, October 13, 1987), "I include Menard's work in the history of science within his professional legacy, since Menard did not approach the history of science as a retired scientist who decided to write, with memory as his only guide. . . ."

The Anatomy of an Expedition (McGraw-Hill, 1969) considers the problems, the excitement, and the serendipitous satisfactions of mounting an ocean-going, exploring expedition. In addition, Menard published over 100 scientific papers.

Menard was dedicated to an understanding of the history and sociology of science and brought together his observations in *Science*: *Growth and Change*, published by Harvard University Press in 1971. The volume was well received. The *Science* reviewer (C. Albritton, *Science* 176:639-41) noted that it was "an engaging and prophetic exposition of scientism at its operational best." Menard surveys the development of science in the United States, to a large extent based on his associations with the geological community. He develops many of the background problems of scientists—conflicts with administrators, absence of positions in specialties, the temporal ups and downs of scientific disciplines, salaries, and productivity. He utilizes elections to the highest-level scientific societies, such as the U.S. National Academy of Sciences and the Royal Society of London, and the receipt of prestigious awards, such as the Penrose Medal of the Geological Society of America, as more or less objective measures of scientific creativities, both in this volume and in *The Ocean of Truth*. Menard was well aware that prolific publication did not guarantee such recognition by peers and neither did notoriety in the media. Menard was a "lumper" as opposed to a "splitter" in his innovative ideas on the management of science. He proposed in *Science*: *Growth and Change* a coalescence of federal science departments into one superagency through which scientists could more effectively control their destinies.

The catholic interests of Menard can be illustrated by a rather tantalizing and unusual paper, "The Scientific Uses of Random Drilling Models" written with George Sharman (*Science* 190:337-43). They compare the expectable results of drilling randomly for oil in the contiguous United States with strategies based on geological and geophysical information. They conclude that success in exploration depends as much on chance as on social, economic, and technological factors. On the basis of the historical records, drilling has been successful at finding small reservoirs and less so for large ones. Menard and Sharman suggest that industry has aimed at small targets. Perhaps inadvertently, the exploration system has been formulated to search in the wrong places. They conclude, probably correctly, that most of the undiscovered oil of the future will be found in Alaska and off the continental shelf, as well as in imports.

Bill Menard (he rarely answered to Henry) was born on December 10, 1920, in Fresno, California, and attended Los Angeles High School. He obtained a B.S. in geology from the California Institute of Technology in 1942. He entered the U.S. Navy immediately after Pearl Harbor and served at sea as a photointerpreter and staff intelligence officer in the South Pacific theater. Following detachment from the service, he returned to Caltech to receive an M.S. In 1946 Bill married Gifford Merrill of New York, who survives him (three children were born to them: Andrew of New York City; Elizabeth of Encinitas, California; and Dorothy Merrill Crist of Silver Spring, Maryland). Bill then entered Harvard University and in 1949 received his doctorate under the guidance of Professor Henry Stetson, a pioneer marine geologist. His thesis project involved an experimental study of sediment transport in a flume at the Woods Hole Oceanographic Institution.

Bill then joined the Sea Floor Studies Section (led by Robert S. Dietz) of the Oceanographic Branch (headed by Eugene LaFond) of the U.S. Navy Electronics Laboratory in San Diego. This association was most fortunate for within a mission-oriented organization of the U.S. government these laboratory heads charted a path of basic marine geological research. Menard's proximity to the pioneer marine geologists Francis Shepard, of the Scripps Institution of Oceanography in La Jolla, and K. O. Emery, of the University of Southern California in Los Angeles, gave him access to cutting-edge research of the time. But more than this, " . . . all the publications I needed were convenient. They were few, which is one of the best reasons for entering a new field if it isn't too unpromising" (*Ocean of Truth*, p. 57).

This last sentence provides insight into the character of Bill Menard—he was a realist. He tackled only scientific problems of significance and substance that had a high probability of resolution. This mood was encouraged by his colleagues at the Navy Electronics Laboratory (especially Dietz and Edwin Hamilton), who with him undertook research following ongoing evaluations as to where to initiate potentially rewarding and productive scientific efforts. The Dietz/Menard collaborations resulted in five papers. The first author, according to Menard, wrote the first draft of the paper; the ideas were an amalgamation of their discussions. The trio participated in the exploration of the Mid-Pacific Mountains during the joint Naval Electronics Laboratory/Scripps Institution of Oceanography MIDPAC Expedition of 1950 led by Roger Revelle. This submarine range extends westward for nearly 5,000 kilometers from near Necker Island on the Hawaiian Ridge to Marcus Island northeast of Guam.

Revelle invited Bill to move to the nearby Scripps Institution of Oceanography in 1955, and he became a full professor in 1961. He remained there, with only two interruptions, until his death. In 1965-66 he was a technical advisor in the Office of Science and Technology during the term of President Johnson. He served as tenth director of the U.S. Geological Survey from 1978 to 1981 under President Carter.

Menard was elected a member of the National Academy of Sciences in 1968 and was awarded the Bowie Medal of the American Geophysical Union in 1985. He was a fellow of the Geological Society of America, the American Geophysical Union, the American Association for the Advancement of Science, the American Academy of Arts and Sciences, and the California Academy of Sciences and was a member of the American Association of Petroleum Geologists.

Another facet of Bill's activities was his involvement with practical problems. In the 1950s he was a consultant for the American Telephone and Telegraph Company for his knowledge of seafloor topography, crucially important in laying cable across the oceans. In 1953, he and his colleagues began to map the geology of the shallow seafloor by means of Scuba diving and soon after formed a corporation to interpret their prospecting observations for the oil companies. Early on Bill recognized, from numerous widely distributed bottom photographs taken on reconnaissance expeditions during the International Geophysical Year, 1957-58, the vast extent and potential economic value of manganese-iron nodules accreting on the deep seafloor. Such interests led him to join, and in 1967-69 to become the acting director of, the University of California's Institute of Marine Resources, headquartered at the Scripps Institution of Oceanography.

Bill Menard defined, in the strictest sense, several of the seafloor's topographic genera that became the key to the mechanisms of plate tectonics. In a classic paper in 1955 (*Geological Society of America Bulletin* 66:1149-98) he recognized and described the startlingly regular quartet of huge "fracture zones" that extend thousands of kilometers westward, almost as great circles on the spheroid, from the California and Baja California coastlines to the Central Pacific.

These four zones became the type localities for dozens of such linear features segmenting all the active midoceanic ridges around the world. Zones of disruption, they are the locales of the offsets in the "magnetic stripe" patterns first mapped in the mid-1950s off the western United States by SIO scientists aboard the U.S. Coast and Geodetic Survey vessel *Pioneer*. From additional observations that established the worldwide association of the fracture zones' topographic, seismic, petrological, and magnetic characteristics, Tuzo Wilson in 1964 proposed the "transform fault," an ingenious mechanism that unified and made compelling the accumulating observations that established plate tectonics.

Bill was a contributor and elucidator to all major themes in marine geology from the tumultuous turbidity currents of the early 1950s to the more subtle propagating rifts of the 1980s. His papers are models of succinct yet entertaining writing, gentlemanly attribution, and deft exposition. A generalist and a humanistic natural scientist, he read widely and could apply the principles or results of one field—say, plastic deformation in tested materials—to the observed shape of major geographic entities, in order to draw instructive, sometimes prescient, inferences. Ever the realist, Bill could judge how long, or far, to hold such views. As a collaborator he was generous, imaginative, and ever the scholar; as a shipmate Bill Menard was a "sea man" in the finest sense. No scientist, or adventurer, could ask for better.

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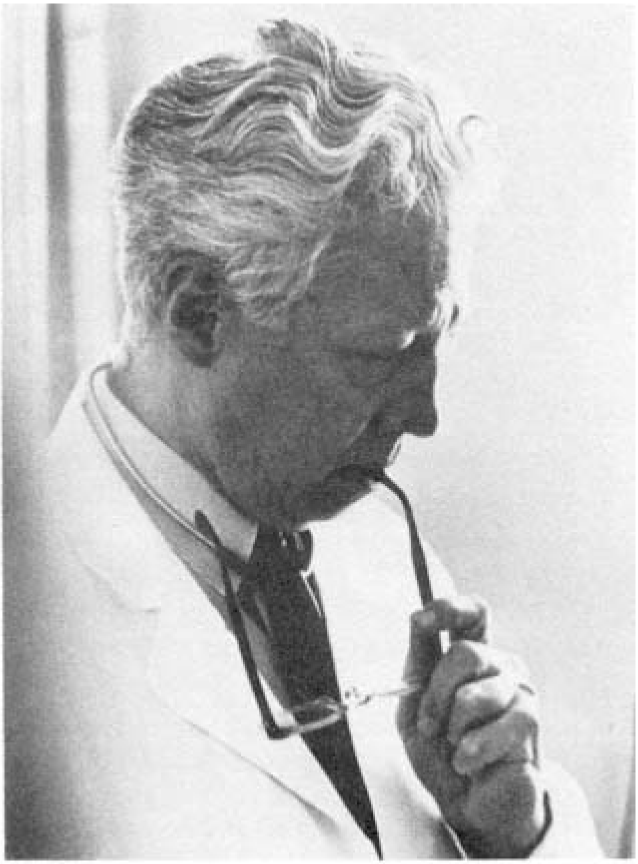

CARL VERNON MOORE

August 21, 1908-August 13, 1972

BY OLIVER H. LOWRY

CARL VERNON MOORE was a truly unique human being. This is said advisedly. I have known no other person who so combined exceptional ability with the most admirable personal traits, including leadership, teaching ability, skill in patient care, modesty, and, above all, consideration and service for others. He also belonged to a new breed of clinicians who thought that basic medical science is too important to be left to the preclinicians.

Many who knew Carl as the epitome of integrity, industriousness, unselfishness, and devotion to medicine may not have realized how human he was and that he may have had a few potential weaknesses like the rest of us, one of which he freely admitted.

Carl Moore was a St. Louisan all his life. He was born in St. Louis and was largely trained there, and there he spent all but four years of his career. His grandmother on his father's side was a relative of Henry Clay. She married a Missouri farmer and died while Carl's father (also Carl Moore) was still a boy. When the grandfather remarried, Carl Moore senior ran away from home and eventually became a St. Louis policeman with no greater ambition than to walk a beat.

Carl's maternal grandmother was married to a factory worker of German ancestry. They were very poor. The grandmother had to take in washing, and Carl's mother had to leave school after the third grade. At the time of her marriage, she was working in a laundry.

Carl had to work all during his schooling. His interest in medicine may have started at age ten when he began working after school for a drugstore delivering prescriptions on his bicycle. His sense of responsibility, so characteristic all his life, must have impressed the owner, who let him fill capsules and make up prescriptions. During this period Carl and his sister played a game in which he was the doctor and she the nurse.

Other part-time jobs while growing up were trimming trees, running elevators, taking children camping, and working in a box factory. Later in college and medical school, he cleaned houses and set up pins in a bowling alley and during the summers worked as a riveter in the steel mills.

There is no sign that Carl enjoyed having to work so hard. Although he never craved riches, he hated to be poor. However, it would be inaccurate to say he grew up in poverty. There was opportunity to take violin lessons, starting in grade school and continuing into college. He even eventually gave a concert, which he described as being "so terrible that I never played again."

There was also a chance to go to baseball games with his mother and time after school to chase baseballs for the St. Louis Browns during practice. As a result, Carl developed a lifelong love of baseball and knew almost as much about baseball statistics as he did about medicine. He was physically very strong, but to his sorrow had no great athletic ability.

One other extracurricular interest must be mentioned that may come as a surprise to those who knew Carl only on duty. He loved to gamble. His mother was a good card player, and he often played pinochle with his maternal grandfather. Carl became a very good poker player and would have liked to play every night if he had had the time. He would also have loved to play the stock market. Fortunately, his father convinced him that the odds were usually against the gambler. Because of this and because he recognized his gambling instinct, Carl made very few investments and these were all most conservative.

Three people were especially important in Carl's development: his father, his mother, and the Reverend Paul Press, a minister of the Evangelical Church. Although Carl did not admire his father's lack of ambition, he learned one most significant thing from him. Carl's father, because he was a policeman, often saw people at their worst. He became convinced that people are basically self-interested and greedy, and he persuaded Carl that this is true and that people are primarily motivated by a desire for power.

Consequently, although Carl himself can only be described as noble, and although those around him felt he expected you as well to try to be noble, the fact is that Carl apparently never really did expect people to act unselfishly. Instead, he was prepared for the worst and thereby avoided disappointment in the behavior of others, overlooked their defects, and took great pleasure when they did behave unselfishly.

In contrast to his attitude toward his father, Carl greatly admired his mother, Mary Moore. She had drive and energy, was up early and worked late. But she was gregarious and had a strong sense of humor, which she and Carl shared. She went to church three times a week, more for social than religious reasons. While Carl was still in secondary school, he helped his mother buy a confectionery store, which she ran with the aid of her stepdaughter, and Carl worked there after school (in addition to his other jobs).

It was the Reverend Press who introduced Carl to a liberal religious environment centered around the Eden Seminary, where Paul Press's brother was a professor, and this association led to Carl going into the ministry. Thus, when he graduated from high school (at age fifteen!), he enrolled in Elmhurst College, which is supervised by the Evangelical Church. This was probably against the wishes of Carl's father, who was afraid Carl would become a missionary. Instead, he wanted him to become a doctor!

Richard Niebuhr (brother of Reinhold) was the head of Elmhurst College. On several occasions Carl was stimulated by sitting in on long philosophical-religious arguments between the Niebuhr brothers. This or something else at Elmhurst changed his mind about the ministry. In fact, thereafter he was never a churchgoer; he "couldn't take the time." Having decided, after all, to go into medicine, he left Elmhurst at the end of three years and proceeded to Washington University, where he accomplished what the registrar said could not be done by completing all the medical school requirements in one year.

In medical school three staff members are believed to have been decisive in attracting Carl to medical research: Edward S. West, in the Biochemistry Department, who always kept the door to his laboratory open for students such as Carl; Leo Loeb professor of pathology; and Joseph Erlanger, professor of physiology and subsequent Nobel laureate.

Carl's house officer training consisted of a half year in pathology and a year and a half in medicine (six months of this while still in medical school). This was followed by an eighteen-month fellowship at Ohio State University with Charles A. Doan, who was one of the last two people to exert a decisive influence on Carl's career. From Doan and his group, Carl acquired his lifelong fascination with hematology and iron metabolism.

In Columbus he also met an even more influential person, his future wife, Dorothy Adams, daughter of a Presbyterian minister, granddaughter of missionaries, recipient of a master's of science in history from Clark University, and dedicated fighter for the rights of man. Having more faith than Carl in people's good intentions, she tended to counterbalance his skepticism on this score.

William Daughaday, a longtime colleague, describes Carl's subsequent career: Returning to Washington University in 1938, Carl proceeded to organize the hematology laboratories of the Department of Medicine. By the late forties, Carl's laboratory was recognized as a leading training center for hematologists, attracting talented fellows from around the world. Carl Moore encouraged his young associates to investigate the importance of immunological mechanisms in accelerated removal of blood cells. This resulted in the classic demonstration by Harrington of antiplatelet antibodies in thrombocytopenic purpura and of antileukocyte antibodies in transfusion reactions by Chaplin and Brittingham. To establish these disease mechanisms, a number of plasma-transfer experiments with high potential risk were undertaken. In characteristic fashion, Carl insisted that he should be a subject for one such experiment. This resulted in hospitalization with a frightening thrombocytopenia.

It was not long before Carl Moore's influence in the school extended beyond hematology. In 1948 Barry Wood asked him to share the departmental burdens as cochairman. They divided the heavy tasks of running the clinical service and charting the course of the department. After Barry Wood resigned the chairmanship of the Department of Medicine in 1955 to return to Johns Hopkins University, it was inevitable that Carl was selected to succeed him.

Carl Moore created a great Department of Medicine by selecting outstanding young men, equipped with experience in basic scientific disciplines, and providing them with the freedom to develop to their full potential. He recognized that application of the sophisticated techniques of modern science to medical research requires periods of uninterrupted time. He did everything in his power to free his faculty from unreasonable administrative chores and meaningless meetings. To provide the necessary balance, he encouraged respect for the activities of full-time and part-time men who devote most of their time to patient care and teaching.

Although Carl Moore's heart always remained with the Department of Medicine, he did heed the call for help from his medical school on two occasions. From 1953 to 1955 he served as dean, and from 1964 to 1965 he was vice-chancellor for medical affairs. During each period his wisdom and patience carried his school through difficult periods of growth.

Despite his contributions as an investigator and an administrator, Carl Moore was primarily a great teacher of medicine. By steady application and continuous study, he became an excellent general internist. He felt a personal obligation for all the patients on his service and always exhibited the greatest concern for their welfare and sympathy for their suffering. This quality was immediately sensed by house officers and students, and it created an unmatched environment for learning medicine. He was able to achieve the difficult task of building a department with strong investigative interests that at the same time excelled in teaching clinical medicine. He taught students and house officers more by listening and inspiration than by didactic brilliance. His students came to ward rounds primed with information derived from bedside and library study and prepared to contribute. They always found a professor equally well prepared. His "professor's rounds" were always conducted a free discussions, where the lowliest student commanded attention and respect.

Carl Moore's love of teaching never flagged. Even after seventeen years of directing his department, he still approached his teaching with the same enthusiasm and obvious pleasure that he had exhibited at the start of his tenure. In the brief periods when he relinquished "running the service" to his senior associates, he exhibited a restlessness that did not abate until he was back on the wards.

Carl Moore exerted a tremendous influence on many generations of house officers. He took a warm personal interest in their goals and accomplishments. He guided their training more by example than decree, and he had the capacity to inspire superior performance.

In Carl Moore's view, "One of the great privileges accorded the academic clinician is the satisfaction of being a physician and a teacher while he enjoys the luxury of being able to pursue his investigative interests."[1]

What kind of person was Carl Moore? Four of his close associates wrote: He was a truly compassionate physician, never abrupt or hasty, and always available. How moving was the response of his patients to his presence, though so many had fatal diseases.

Carl was uncomplicated. He was an expert on proved formulas for professional and academic achievement, and his department and school were never subjected to risky, untested ventures. He usually spoke last in group deliberations and typically with the best comprehension and analysis.

He was profoundly thoughtful and considerate. He knew that his department was made up of people—not himself, but other people: colleagues, house officers, fellows, students, and non-academic personnel. All had ready access to him, regardless of an overburdened schedule.

From the Department of Medicine under Carl Moore came one university president, three vice presidents, three deans, seven department chairmen, and twelve directors of hematology in American medical schools, as well as many men of stature in other fields of internal medicine and in other countries. There are scores of academic and practicing hematologists as well as hundreds of other physicians whose lives were meaningfully influenced by Carl.

In addition to all of these, the at-large indebtedness to Carl Moore is incalculable; the impact he had for twenty-five years on public and private health-related national agencies and organizations directly influenced the form and character of all of medicine, particularly the careers of its academic members.[2]

THE IDEAL DEPARTMENT OF MEDICINE

Carl Moore worried a good deal about how medical schools as a whole, and departments of medicine in particular, could operate without diminishing any of their three missions: teaching, exemplary patient care, and research. He devoted some time to this in his presidential address to the Association of American Physicians in 1964: "One of the great strengths of American medicine certainly is that research is concentrated in our medical schools, where students can be stimulated by creative minds rather than in a large collection of research institutes.

"We have held too long, I think, to the vision that the only desirable member of a department is so broad in his capabilities—a modern version of the renaissance man— that he is a polished clinician, a stirring teacher, and an investigator of great distinction. Many will regard as heresy the statement that this form of tripodal idolatry is in need of reformation. Within any department the responsibilities for patient care, teaching, and research must without question be kept in balance, but the time has come for the equilibrium to be provided by the department as a whole, rather than to insist that it be provided by each individual. Unless that is done, we are doomed to mediocrity, doomed to lose to other disciplines an increasing number of outstanding young men. I don't see why the capable, productive investigator should not be free to spend 75 or 80 percent of his time in the laboratory provided he is willing to devote his remaining efforts with enthusiasm to teaching and to working at the bedside—as an internist in general and not only within his special field of interest. He must identify himself enough with clinical medicine to become a competent physician, but he doesn't necessarily have to be the standard of reference for every unusual syndrome or to be equally informed in all subspecialty areas.

"If he learns from the house officers, if the resident must interpret the vectorcardiogram for him, there is no cause for embarrassment. He is in good company, as a matter of fact, and the best house officers will be quick to realize and appreciate that many things of importance can be learned from him. While a fair proportion of all research done in a clinical department should admittedly be disease oriented, not all needs to be. The increased understanding of disease that may come from the geneticist working with bacteria, the nephrologist studying cation transport across the toad bladder, the endocrinologist measuring the effect of insulin on the transport of glucose or amino acids across cell membranes, or the coagulationist working with fibrinolytic mechanisms has been demonstrated frequently enough; these activities must not be denied departments of medicine because the relationship to the patient at any given time seems remote. But if arrangements are made to provide this kind of freedom and a major fraction of time for research, the investigator takes on an obligation to use the time well-an obligation to his colleagues who assume a greater percentage of the teaching and patient care and to our society in general for providing the time, the tools, and the space for his creative efforts. I am constantly amazed by the attitude, evidenced fortunately by only a few, that opportunities such as these are an individual's God-given right rather than one of life's greatest privileges to be earned and safeguarded. The struggle to provide the investigator with 75 to 80 percent of his time for research is wasted effort if he elects to let himself be consumed by an interest in administrative matters or fritters life away by making three project site visits per week.

"The investigator must also be willing to respect the activities of those full-time men in the department who provide the balance for him, the men who devote three-fourths of their efforts to patient care and teaching, who express their intellectual curiosity by studying and describing the clinical manifestations of disease as their contribution to knowledge.

"All I am trying to say is that the clinical investigator closely allied to the basic sciences and the clinical investigator closely allied to the patient are both needed. Given both types of men, a department of medicine can fulfill its function, provide maximum opportunity for growth of each individual, achieve excellence in all three areas of responsibility, and increase steadily in scientific nature. Such departments will become increasingly different from those of the present or the past, but the change is inevitable if we are to participate as important partners in the scientific biological revolution that now surrounds rather than permeates us."[3]

CARL MOORE'S RESEARCH

Moore's scientific contributions were the result of clinical research of the highest order. The term "clinical research" has sometimes carried a slightly derogatory connotation, a step below "pure" research or "laboratory" research. To the contrary, Carl's clinical research could not have been more elegant or rigorous and because it dealt with human beings was an order of magnitude more difficult than the usual laboratory study. His subjects were human beings of all ages, with many inherited differences, and in which the specific disease of interest was often attended by other disease entities. Moore's classical studies proved that clinical research could be made as definitive as the best in any field.

IRON METABOLISM

Carl Moore began his lifelong study of iron transportation and metabolism in health and disease during his postdoctoral fellowship with Charles Doan at Ohio State University. He published altogether more than two dozen papers on the subject beginning in 1937 and ending with a posthumous paper in 1975.

The issue at the outset was how iron is absorbed from the diet and how it is transported around the body. It seemed probable that transport to the tissues is via the blood plasma, but it was hard to be sure because there is 500 times more iron in red blood cell hemoglobin than in the plasma, and it is difficult to separate the plasma without slight hemolysis with release of a significant trace of this hemoglobin. Carl's first paper was therefore devoted to developing an improved analytical method for measuring serum iron and to perfecting a procedure for collecting the necessary large volumes of plasma or serum with minimal red cell damage. His original measurements had to be made with the only device available, a visual colorimeter.

A companion paper describes what are probably the first time curves for serum iron changes after ingestion of oral doses of iron. Six or eight blood samples (each had to be 50 milliliters in volume!) were drawn for analysis at intervals over a twenty-four-hour period. The subject for the first experiment was Carl Moore. This was only one of many occasions on which he was the guinea pig. Habitually, he first tried out on himself every unpleasant or possibly dangerous diagnostic procedure or any potentially harmful substance.

These serum iron studies, combined with a dog experiment (1939) that ruled out the lymph as a significant means for iron transport, clearly showed that blood plasma is the primary avenue for transferring iron from the intestinal tract to the rest of the body. (This was independently established by Heilmeyer and Plotner,[4] a fact that Moore freely acknowledged.)

With his own analytical iron method, plus the use of radioactive iron, which had just been introduced by Hahn et al.,[5] Moore and his group now launched a series of classical studies of iron absorption and utilization in a wide variety of blood diseases as well as in normal men and women. The care and thoroughness with which each patient was studied was a model for clinical investigation in general.

This work established or confirmed the following: (1) Iron is absorbed chiefly from the duodenum, to a lesser extent from the jejunum, and not at all from the colon. (2) Low-serum iron levels are a reliable index of iron deficiency. (3) If anemia is due to iron deficiency, most of the initially absorbed iron is rapidly incorporated into the new red cells. If the anemia is due to a defect in hemoglobin synthesis, most of the absorbed iron enters the tissue stores but is available for hemoglobin synthesis if and when the defect is cured. (4) The absorption of iron from the diet is extremely variable but rarely amounts to more than 10 percent of that ingested and over long time periods is only sufficient to balance the very small losses from the body. (5) Absorption is affected by the chemical form of the iron and by other components of the diet as well as by the presence or absence of iron deficiency. Moore used built-in radioactive iron to test the availability of iron from different foodstuffs. For this purpose, he himself grew vegetables with radioactive iron in the soil and raised chickens and rabbits with radioactive iron in their diets.

THROMBOCYTOPENIC PURPURA

Thrombocytopenic purpura is a condition with hemorrhage caused by a deficiency of blood platelets (thrombocytes), which are required for normal blood coagulation. With Dr. William Harrington in the lead, Moore and his laboratory (1951, 1953) showed that this disease is caused by circulating antibodies that destroy the platelets. As part of the proof, a pint of a patient's plasma was injected into normal volunteers. Harrington and Moore were two of the first volunteers. Their platelet counts rapidly fell to dangerous levels, and both had to be hospitalized for a week.

As a follow-up with Drs. Steinkamp and Dubach (1955), it was shown that certain drug reactions (e.g., to quinine) were caused by a similar immunological reaction, but in this case the allergen (i.e., the drug) had to be present for the reaction with platelets to occur.

OTHER HEMATOLOGICAL STUDIES

There were many other hematological studies from Moore's laboratory. The following mentions some of them. His laboratory contributed much valuable information about the hematology of pernicious anemia, the nature and roles of intrinsic factor, extrinsic factor, and folic acid, which had erroneously been thought to be the extrinsic factor (now known to be vitamin B-12).

A study with over 100 patients treated with radioactive phosphorous (^{32}P) for a wide variety of blood diseases and blood-cell-related tumors showed that ^{32}P is of no value for some of these (e.g., lymphosarcomas); no better than X-ray for others (e.g., lymphatic leukemia); and is the treatment of choice for polycythemia (excessive numbers of red blood cells). (Since then, better means of treatment for polycythemia have been discovered.)

Important contributions were made to the mechanism of biosynthesis of heme and hemoglobin and the effect of oxygen on hemoglobin metabolism and red blood cell formation. Several genetic blood diseases were studied in great detail, particularly sickle cell anemia and thalassemia and their effects on hemoglobin metabolism.

EDITORSHIPS

Moore was in constant demand as an editor and contributor of textbook chapters and for major medical reviews. The preface of the fourteenth edition of *Cecil's Textbook of Medicine* says:

> When the planning for the present edition was just getting under way, Carl Moore died suddenly, to be mourned by friends and students everywhere. He had contributed powerfully to the development of his section on Hematologic and Hematopoietic Diseases, and because of his great experience and breadth of understanding had been able to write about one quarter of that section himself. We believe that his Introduction to Hematologic Diseases in the eleventh to thirteenth editions deserves to rank with another Cecil "classic," Fuller Albright's Introduction to the Endocrine section, which appeared in the seventh to tenth editions. We prized Moore's association with the book not only for the standard of his own section but also for his advice about organization of the entire work.

EPITAPH

We frequently hear that a strong research interest and compassion at the bedside are antithetical qualities not to be found in the same man. Carl was an outstanding example of the fallacy of this view. In the care of his own patients he communicated a degree of concern and sensitivity rarely encountered.[6]

Carl was one of the kindest and most gentle men I have known. He never forgot that a patient was a human being and not just an interesting case. He listened to everyone. He appeared to have as much time to talk about baseball with Will Anderson as he had to talk to a staff member about departmental affairs.[7]

Carl led his associates in a low-keyed manner, but lead them he did. He relied on his personal example to spur them to heights greater than they would otherwise have achieved. He appeared shy and humble to the casual observer, yet his was an intricate mixture of pride and humility.

Carl was an excellent poker player, and I often thought his talent served him well in creating a team out of the diverse personalities in his department. Only once I called his bluff and found him without a pat hand. When Carl was elected to the National Academy of Sciences in 1970, the members of the department of medicine held an impromptu champagne fest in the laboratory. After a few glasses of champagne we walked out of Wohl Hospital together. "It's a great honor for Washington University, but I wish it had gone to some younger man on the faculty rather than to me," Carl confided. In answer to my "incredulous profanity," he puffed his pipe, chuckled quietly, and walked away.[8]

Carl Moore was a man who looked on medicine and his daily work in the medical center as a privilege and a joy and as a means of attaining personal rewards far higher than fame, financial gain, or stature in society. For him the satisfaction of helping others led to his giving all his energy to the task before him, whether it was planning a complex new project, conducting student rounds, or helping a total stranger find his way in the hospital.

Carl was always willing to drive himself a bit harder, to go the second mile, to achieve the goals that he had decided were worthwhile. For those with whom he dealt whose ambition and maneuvering led to meanness and petty bickering, he had nothing but contempt in his heart but remarkably little rancor in his speech. He was a humble man. His humility was a source of wonder and even distrust to some of the powerful and arrogant people with whom his high offices brought him into contact.

Carl had a disarming simplicity and directness to his speech, with a knack for saying the proper thing; his writing reflected this simplicity with careful elimination of all that was wordy, flowery, or trite. Many people awed by his position, his international reputation, and his leonine visage were not privileged to see his pervading warmth, sensitivity, and attention to the small things that helped to righten the lives of friends he made wherever he went.

Carl Moore, despite his remarkable achievements, was not a genius whose every effort met with easy success. His achievements were the result of hard work. He recognized the superior intellectual endowments of a number of his associates, and without jealousy he encouraged them to excel and to reap their due rewards. His ambition was for his department, his school, and the fulfillment of the task at hand, not for his own personal aggrandizement.

I am left with the vision of a man whose life revolved around his desire to get on with the tasks he identified as being important, to tackle them with all the intensity he could muster without sparing himself in the effort, but along the way he radiated sensitivity, warmth, integrity, simplicity, and humility that struck a chord in the hearts of people who knew him, a chord that helped restore faith in man's basic humanity.[9]

I AM GREATLY INDEBTED to Dorothy Moore for the details about Carl Moore's family background and his own early years.

NOTES

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9. Ibid., Elmer B. Brown, p. 5.

HONORS AND DISTINCTIONS

DEGREES

1928	A.B., Washington University
1932	M.D. (cum laude), Washington University School of Medicine
1955	LL.D., Elmhurst College

UNIVERSITY AND HOSPITAL APPOINTMENTS

1932-34	Internship and residency, Department of Medicine, Barnes Hospital, St. Louis, Missouri
1934-35	National Research Council Fellow in Medicine, Ohio State University
1935-36	Instructor in Medical Research, Ohio State University
1936-38	Assistant Professor of Medicine, Ohio State University
1938-41	Assistant Professor of Medicine, Washington University
1941-46	Associate Professor of Medicine, Washington University
1946-55	Professor of Medicine, Washington University
1953-55	Dean of the School of Medicine, Washington University
1955-72	Busch Professor of Medicine and Head of Department, Washington University
1964-65	Vice Chancellor-in-Charge of Medical Affairs, Washington University

MEMBERSHIP IN AMERICAN ORGANIZATIONS AND SOCIETIES

American Association for the Advancement of Science, Fellow

American College of Physicians, Fellow (Governor for Missouri, 1953-56; Vice President, 1960-61; Regent, 1961-72)

American Institute of Nutrition

American Medical Association, Fellow (Secretary, Section on Experimental Medicine and Therapeutics, 1946-49; Vice Chairman, 1949-50; Chairman, 1950-51)

American Society for Clinical Investigation (Vice President, 1952-53; President, 1953-54)

American Society of Experimental Pathology

American Society of Hematology (President, 1959-60)

Association of American Physicians (Councillor, 1956-61; Vice President, 1962-63; President, 1963-64)

Central Interurban Club

Central Society for Clinical Research (Secretary-Treasurer, 1939-44; Vice President, 1945-46; President, 1946-47)

International Society of Hematology (Vice President, 1960-66; President, 1966-68)

St. Louis Society of Internal Medicine

Society for Experimental Biology and Medicine

COMMITTEES	
1946-49	Member, Hematology Panel, Division of Research Grants and Developments, U.S. Public Health Service
1951-56	Chairman, Hematology Panel, Division of Research Grants and Developments, U.S. Public Health Service
1952-56	Chairman, Hematology Study Section, National Institutes of Health
1953-60	Chairman, Blood and Blood Derivatives Committee, National Research Council
1955-72	Part-time Consultant, Research and Development Program, Department of Defense
1956-58	Member, Advisory Committee on Institutional Research Grants, American Cancer Society
1956-64	Consultant, Clinical Center, National Institutes of Health
1956-72	Member, U.S.P. XVI Panel on Hematology
1957-59	Member, National Cancer Institute Board of Scientific Counselors
1958-62	Member, National Advisory Arthritis and Metabolic Diseases Council, and National Institutes of Health
1959	Member, Advisory Committee, Burroughs Wellcome Fund
1959-60	Special Advisor, Committee on Clinical Research Centers, National Advisory Health Council, National Institutes of Health
1960-66	Member, Advisory Committee for Biology and Medicine, Atomic Energy Commission
1961-62	Member, Council on Foods and Nutrition, American Medical Association
1967-72	Member, American Board on Nutrition
1967-72	Member, Norman Jolliffe Medical Student Fellowship

	Awards Committee, American Society for Clinical Nutrition
1967-72	Consultant, Hematology Advisory Committee, Food and Drug Administration
1967-72	Chairman, Advisory Council, Life Insurance Medical Research Fund
1967-72	Member, Panel of Expert Consultants to Assist the Technical Committee, Pakistan-SEATO Cholera Research Laboratory
1968-72	Member, National Advisory Arthritis and Metabolic Diseases Council, and National Institutes of Health

EDITORSHIPS

1942-44	Assistant Editor, *Nutrition Reviews*
1944-49	Editor, *Journal of Laboratory and Clinical Medicine*
1949-53	Editorial Board, *Journal of Laboratory and Clinical Medicine*
1944-72	Editorial Board, *Blood*
1950-53	Editorial Board, *Journal of Nutrition*
1955-72	Editorial Board, *American Journal of Medicine*
1955-72	Editorial Board, *Journal of Chronic Diseases*
1956-66	Editorial Board, *Modern Medical Monographs*
1964-71	Coeditor, *Progress in Hematology*
1967-71	Coeditor, *Cecil's Textbook of Medicine*

HONORS

1955	Modern Medicine Award for Distinguished Achievement
1958	Elected as Affiliate of the Royal Society of Medicine, London
1959	Joseph Goldberger Award in Clinical Nutrition, American Medical Association
1962	William McIlrath Guest Professor of Medicine, Royal Prince Alfred Hospital, Sydney, Australia
1962	Honorary Life Member, Haematology Society of Australia
1962	Alumni Award, Washington University
1964	Stratton Medal, International Society of Hematology
1967	Elected as Fellow of the American Academy of Arts and Sciences
1968	Corresponding Member, German Society of Hematology

1970	Elected Member of the National Academy of Sciences
1970	John Phillips Memorial Award for Distinguished Contributions in Internal Medicine, American College of Physicians
1970	Centennial Achievement Award of Ohio State University
1971	Flexner Award of the Association of American Medical Colleges
1972	Master of the America College of Physicians (Posthumous)

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1939 With W. Arrowsmith, J. Welch, and V. Minnich. Studies in iron transportation and metabolism. IV. Observations on the absorption of iron from the gastro-intestinal tract. *J. Clin. Invest.* 18:553-80.

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1943 With V. Minnich, R. W. Vilter, and T. D. Spies. Hypochromic anemia in patients with deficiency of the vitamin B complex: Response to iron therapy with and without yeast. *JAMA* 121:245-49.

1944 With E. H. Reinhard, R. Dubach, and L. J. Wade. Depressant effects of high concentrations of inspired oxygen on erythrocytogenesis. Observations on patients with sickle cell anemia with a description of the observed toxic manifestations of oxygen. *J. Clin. Invest.* 23:682-98.

With L. A. Hempelmann, Jr., E. H. Reinhard, O. S. Bierbaum, and

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Photograph by Ben Spiegel

GEORGE PETER MURDOCK

May 11, 1897-March 29, 1985

BY WARD H. GOODENOUGH

GEORGE PETER MURDOCK played a peculiarly important role in the history of anthropology, to whose development he was a major contributor in the middle years of the twentieth century.[1] He laid the foundations for systematic cross-cultural research and the cross-cultural testing of generalizations about human society and culture, and in his own work exemplified the value of such research and testing. He remained active until shortly before his death, publishing his last two monographs in 1980 and 1981, the latter when he was eighty-three years old. His work was not readily accepted by many of his fellow anthropologists and was the object of hostile criticism from some while being praised by others. But his critics had to reckon with his work, and many of them found, in the end, that their own contributions grew significantly out of it, thereby underscoring the importance of his role in the growth of his discipline. He contributed not only intellectually but also as an organizer, providing leadership in the creation of the Human Relations Area Files, Inc., and in the promotion and funding of anthropological research in the Pacific region.

Murdock grew up on a prosperous farm in Meriden, Connecticut, the eldest of three children of George Bronson Murdock and Harriet Elizabeth Graves. He was a seventh generation descendant of Peter Murdock, who came to Long Island from Scotland in 1690. Murdock's parents were politically Democratic, individualistic, and agnostic in religion, an outlook that remained with him all his life. They regarded education and the cultivation of knowledge as the proper pathway to personal and social fulfillment and fostered Murdock's education accordingly, sending him to Phillips Academy, Andover, and then to Yale University, where he graduated with honors in history in 1919. He took leave from Yale to serve as a member of the National Guard in the Mexican border incident of 1916 and, again, as a second lieutenant of artillery in World War I. A devoted tennis player most of his life, he competed in the national tournament at Forest Hills in 1919.

After graduating from Yale, Murdock went to study law at Harvard. He found it unrewarding and in his second year, dropped out of law and spent more than a year traveling around the world. By the time his travels were over, he had decided to pursue anthropology, and enrolled in a combined program of anthropology and sociology under Albert G. Keller at Yale, where he received his Ph.D. in 1925. In that year, also, he married Carmen Emily Swanson, with whom he had a son, Robert. Encouraged by Keller, Murdock did a critical translation of Julius Lippert's *Kulturgeschichten der Menschheit in ihrem organischen Aufbau* in abridged form for his doctoral dissertation, which he later published as *Evolution of Culture* (1931).

After teaching sociology and anthropology at the University of Maryland for two years, Murdock returned to Yale in 1928 as an assistant professor in Keller's department, which later became the department of sociology. He received a joint appointment in 1931 in Yale's newly formed anthropology department and became fully affiliated with it in 1938, when he became its chairman. He was promoted to full professor in 1939 and remained at Yale for another twenty-one years. During World War II, he took leave to serve as lieutenant commander (1943-45) and commander (1945-46) in the U.S. Naval Reserve. In 1960 he accepted appointment as Andrew Mellon Professor of social anthropology at the University of Pittsburgh. After retiring from there in 1973 at age seventy-five, he and Carmen moved to Wynnewood, Pennsylvania, in suburban Philadelphia, to be near their only child, Robert, and his family. They remained there until Carmen's death, after which Murdock moved to a retirement home in nearby Devon, where he died.

Murdock's scientific career was marked by a remarkable consistency. From his boyhood love of geography came a lasting interest in ethnography on a world-wide basis, in the knowledge of which he was without equal. He did not accept the theories of Sumner and Keller in regard to social evolution, but he shared their conviction that a scientific approach to the study of society and culture required systematic comparative, cross-cultural study. Much of his career was devoted to creating organized data archives intended to establish a solid foundation for such study.

In this, as in everything else he did, Murdock sought to bring better order to the enormous mass of ethnographic information and to the many conflicting and competing generalizations, hypotheses, and typologies that anthropologists were continually generating but failing to test empirically in a rigorous way. He seemed compelled to bring tidiness to things so that the problems could be clarified, and what needed to be done could be more clearly seen and progress made in the accumulation and improvement of knowledge.

Changing fashions in intellectual posture did not appeal to Murdock except as they demonstrably led to new kinds of verifiable knowledge. He did not try to create new theory on a grand scale. Rather, he critically evaluated and selected from the theoretical contributions of others in anthropology and related disciplines with an eye to their utility in advancing anthropology as a scientific enterprise. He evaluated that utility against the evidence provided by linguistics, archaeology, and clinical and experimental psychology, and, especially, ethnography. How to organize and use that evidence effectively seemed to be a major preoccupation. For his multi-disciplinary perspective he owed much to fellow members of Yale's Institute of Human Relations: John Dollard (personality theory and psychoanalysis), Clark Hull (psychology), and Neal Miller (psychology). Others to whose work he gave credit as significantly influencing his own included the linguists Edward Sapir and Joseph Greenberg, and the anthropologists W. H. R. Rivers, Robert Lowie, Leslie Spier, Ralph Linton, and Fred Eggan.[2]

He did not make advances, himself, in descriptive ethnography, but he valued developments by others in ethnographic method and data analysis that advanced knowledge of culture and society and that improved the quality of the data base on which comparative study relied. Murdock held his students to the highest standards in the conduct of their own ethnographic research and inspired in them a concern for research method. At the same time, he did not ask his students to be clones of himself. He sought to find what it was that interested them and what ideas they had about it. If he thought it worthwhile, he then encouraged them to pursue it. He asked only that they convince him of its value. Murdock's broad interest in the behavioral and social sciences generally allowed him to see value in a wide range of subjects. He hoped to learn from his students. The one stricture was that whatever it was they did, they must do it to the best of their capability. He assessed their respective capabilities and judged their performances accordingly.

In keeping with his interests, Murdock's first major work after completing his doctorate was *Our Primitive Contemporaries* (1934), a book of ethnographic summaries that was widely used for many years as a teaching text. The eighteen societies covered constituted a small, representative sample of the world's cultures. Murdock intended that it should be used in the classroom as a basis for evaluating generalizations about human societies that students were encountering in their readings.

The schedule of information Murdock created for extracting information for the summaries from the ethnographic sources became the basis for what he soon elaborated as *Outline of Cultural Materials* (1938), which in its several editions has served as a standard reference for indexing cultural materials and as a topical guide in ethnographic research. This later work was developed in connection with the ambitious project called the Cross-Cultural Survey, which Murdock set up at Yale's Institute for Human Relations in the mid-1930s. The social and behavioral scientists at the institute were committed to developing a general, unified theory of behavior. They concurred with Murdock that the creation of an organized body of data on human societies and cultures was an essential resource for constructing and critically evaluating such a theory, and strongly supported the project.

The Cross-Cultural Survey was reorganized after World War II by a consortium of universities, under Murdock's leadership, as the Human Relations Area Files, Incorporated (HRAF), for expanding, up-grading, and making more generally available for research this data archive. Over forty years later, HRAF continues to serve the scientific community from its offices in New Haven.[3]

The effort to create and use the "Files," as the archive came to be known, was extraordinarily revealing of uneven-ness both in ethnographic coverage geographically and topically, and in the quality of information relating to the topics that were commonly covered. Good comparability sought by the Files' users was conspicuously lacking, except for a few subjects relating to social organization. Even here, the quality of description was very uneven. The Files thus exposed to critical examination the quality of what anthropologists had been doing as ethnographers. Resulting concern to improve the quality of the ethnographic data base led to major advances in ethnographic method, and to considerable rethinking and refining of the categories in terms of which data are reported and generalized. This concern also contributed to the development of protocols for enhancing comparability of ethnographic data.[4]

The comparative studies the HRAF files facilitated also contributed significantly to rethinking and refining concepts. A major contribution in this regard was Murdock's landmark work *Social Structure* (1949). Using a sample of 250 cultures, he formulated and tested what continues to be the most carefully worked out theory of the determinants of the culturally different modes of kinship classification and descent reckoning.

Social Structure laid to rest a number of issues that had been debated by students of family and kinship in the first half of the century. At the same time, Murdock's refinement of concepts and the increased order he brought to the subject led to recognition of new problems. It led colleagues to see a need for further refinement of concepts and to recognize hitherto overlooked forms of social organization, such as those involving non-unilinear (or cognatic) descent groups. Murdock, himself, joined actively in this development, editing the important volume *Social Structure in Southeast Asia* (1960), in which he sought to update his earlier formulations in an introductory essay. His contribution to this critical re-examination of concepts is also illustrated by his paper "The Kindred" (1964). The rethinking and refining in this area that Murdock's work so signally stimulated continues forty years later.[5]

The lack of good text and reference materials to introduce students to the cultures of major regions of the world with attention to their differences and similarities also concerned Murdock. In his ethnography courses he would take a region, such as North America or Africa, and with his students try to compile a comprehensive picture and appraise the quality of information available. His book *Africa* (1959) grew out of such a course. It drew heavy fire from Africanists for errors of detail and interpretation, but they could not ignore it. Their efforts at correction led not to dismissal of the framework of order he sought to bring to African ethnology, but to its modification and improvement. Indeed, it placed the anthropological view of Africa's peoples and cultures in a distinctly new perspective that has significantly influenced the work of Africanists since.[6]

Murdock abandoned his plan to write similar comprehensive books on North and South America, saying that he found the complexities there too great. But preliminary work to this end led to the *Ethnographic Bibliography of North America* (1941), the *Outline of South American Cultures* (1951), and papers on South American culture areas (1951) and social organization in North America (1955). The bibliography of North America, subsequently co-authored and greatly expanded by Timothy O'Leary into a five-volume fourth edition (1975), remains the most important reference for students and researchers, as do Murdock's codings of a large number of cultural variables for 218 North American societies.[7]

Finding his cross-cultural research demands running ahead of the resources being made available through the Human

Relations Area Files, Murdock undertook to code a much larger sample of the world's cultures in regard to matters of interest to cultural and behavioral theorists. To this end the Cross-Cultural Cumulative Coding Center was established at the University of Pittsburgh with Murdock as its director from 1968 to 1973. Results of this work appeared in final form, after several preliminary versions, as *Atlas of World Cultures* (1981), his last publication. The further development of data bases for comparative research is being continued by former students.[8]

Creating tools for cross-cultural research was not enough. Murdock felt it essential to demonstrate their usefulness. To this end, in addition to *Social Structure*, mentioned above, he published the results of a number of other comparative studies, ranging over such topics as the social regulation of sexual behavior, family stability, parental attitudes, parental kin terms, the distribution of kin term patterns, cross-sex kin behavior, and the division of labor by sex. His final comparative study was a global survey of theories of illness and their regional distributions (1980). A lasting legacy of his work that is invaluable to social and behavioral scientists is the growing accumulation he set in motion of tested cross-cultural findings.[9]

Murdock's comparative studies stimulated much debate about the methodological and conceptual issues they highlighted. One set of issues had to do with sampling and the units to be sampled. Another had to do with the problem of how to relate concepts appropriate to the description of particular cultures, involving cultural relativity, with concepts appropriate to the comparison of cultures, which had to remain constantly applicable across cultures. This consequence of Murdock's work is prominent in current debates in anthropology.[10]

Murdock also put much energy into stimulating research to fill major gaps in ethnographic information and into publication of the results of such research for their value as data and not just as a context for theoretical argument. To the latter end, he helped establish *Ethnology* at the University of Pittsburgh in 1962, a journal that quickly acquired international acclaim and that he edited for the next ten years. Out of his wartime work with the Navy he saw a need for a concerted effort to update and improve information on the peoples of Micronesia in the Western Pacific. With Harold Coolidge, he organized the Pacific Science Board within the National Research Council of the National Academy of Sciences and obtained for the Board funding from the Office of Naval Research for the Coordinated Investigation of Micronesian Anthropology (CIMA), a project that took forty-two anthropologists and linguists from more than twenty institutions to do field research in 1947-48. Murdock, himself, led the research group that went to Truk.

There were important sequels to CIMA. For almost a decade thereafter anthropologists were included on the administrative staff of the U.S. Trust Territory. The researches CIMA began have continued unabated to a point where Micronesia is now one of the best described areas of the Pacific. Another sequel of CIMA was the program of ecological studies of atolls, including human and cultural ecology, by the Pacific Science Board while Murdock served actively on it and while he was its chairman from 1953 to 1957. At the same time he promoted formation of a consortium of the Bishop Museum and the anthropology departments of Yale and the University of Hawaii in the Tri-Institutional Pacific Program (TRIPP). From 1953 until 1964 it supported ethnographers, linguists, and archaeologists in what were judged to be critically needed and hitherto neglected areas of study. This program, too, has a legacy of continuing research.

In addition to these activities, Murdock served as president in 1947 of the Society of Applied Anthropology, a society he helped found. He was president of the American Ethnological Society in 1952-53 and of the American Anthropological Association in 1955. He played a major role in creating the Society for Cross-Cultural Research in 1972. After his election to the National Academy of Sciences in 1964, he chaired the Division of Behavioral Sciences of the National Research Council until 1968. He was also influential in bringing linguists and social scientists into the Academy, where they had not been represented before.

In recognition of his contributions he was awarded the Viking Fund Medal in 1949, the Herbert E. Gregory Medal of the Pacific Science Association in 1966, the Wilbur Lucius Cross Medal in 1967, and the Huxley Medal of the Royal Anthropological Institute in 1971. He was also honored in 1964 with a large Festchrift volume containing major papers by twenty-four former students.[11]

In an obituary article Alexander Spoehr observed, "Murdock's long career spanned the coming of age of American anthropology. In the fifty years between his first professional publication in 1931 and his last in 1981, he played a leading role in anthropology's growth, development, and maturity."[12]

So, indeed, he did.

NOTES

1. This memoir draws heavily on two other biographical pieces by the author: "George Peter Murdock," in *International Encyclopedia of the Social Sciences*, vol. 18, *Biographical Supplement* (New York: The Free Press, 1979):554-59, and "George Peter Murdock's Contributions to Anthropology: An Overview," in *Behavior Science Research*, 22(1988):1-9. See also Alexander Spoehr, "George Peter

Murdock (1897-1985)," in *Ethnology*, 24(1985):307-17, and John W. M. Whiting, "George Peter Murdock (1897-8195)," in *American Anthropologist*, 88(1986):682-86. The first of my two pieces and that by Spoehr provide fuller bibliographies of Murdock's work. Much more information on Murdock's family background and early years is contained in his "Autobiographical Sketch" in a collection of his papers entitled *Culture & Society* (1965).

2. See Murdock's "Autobiographical Sketch" in *Culture & Society* (1965). In an unpublished biographical statement he also listed as influential former students: Harold Conklin, William Davenport, C. S. Ford, Charles Frake, Ward H. Goodenough, Allen Holmberg, Raymond Kennedy, William E. Lawrence, Floyd G. Lounsbury, Leopold Pospisil, and John W. M. Whiting.

3. Melvin Ember, "The Human Relations Area Files: Past and Future," in *Behavior Science Research*, 22(1988):97-104.

4. See, for example, Beatrice B. Whiting, ed., *Six Cultures*: *Studies on Child Rearing* (New York: Wiley, 1963).

5. See, for example, William H. Davenport, "George Peter Murdock's Classification of 'Consanguineal Kin Groups'," in *Behavior Science Research*, 22(1988):10-22, and John W. M. Whiting, M. L. Burton, A. K. Romney, C. C. Moore, and D. R. White, "A Reanalysis of Murdock's Model for *Social Structure* Based on Optimal Scaling," in *Behavior Science Research*, 22 (1988) :23-40.

6. See the assessment by Igor Kopytoff, "George Peter Murdock's Contributions to African Studies," in *Behavior Science Research*, 22(1988) :41-49.

7. Joseph C. Jorgensen, "George Peter Murdock's Contributions to the Ethnology of Native North America," in *Behavior Science Research*, 22(1988):50-58.

8. Douglas R. White and Lilyan A. Brudner-White, "The Murdock Legacy: The Ethnographic Atlas and the Search for a Method," in *Behavior Science Research*, 22 (1988) :59-81.

9. See the compilation of David Levinson, ed., *A Guide to Social Theory*: *Worldwide Cross-Cultural Tests*, 5 vols. (New Haven: Human Relations Area Files, 1977), and the important review of it by Guy E. Swanson, "In the Tradition of Murdock," in *Contemporary Sociology*, 9(1980):376-80.

10. As illustrated by Pegy Sanday, "Toward Thick Comparison and a Theory of Self-Awareness," *Behavior Science Research*, 22 (1988) :82-96.

11. Ward H. Goodenough, ed., *Explorations in Cultural Anthropology*: *Essays in Honor of George Peter Murdock* (New York: McGraw-Hill, 1964). Of the twenty-four former students who contributed to this volume, nine have been elected to the National Academy of Sciences.

12. Alexander Spoehr, "George Peter Murdock (1897-1985)," in *Ethnology*, 24(1985):313.

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THEODORE MEAD NEWCOMB

July 24, 1903-December 28, 1984

BY PHILIP E. CONVERSE

THEODORE M. NEWCOMB was one of the principal pioneers in the establishment of social psychology as a fertile area for study at the boundary between the traditional disciplines of psychology and sociology. During five decades of research he sought to enrich individualistic treatments of human motivation, learning, and perception with a keener understanding of the social processes that shape them. He was author or co-author of three widely used textbooks that gave systematic definition to this emergent field, and he directed the influential doctoral program in social psychology at the University of Michigan for twenty-six years.

Newcomb's personal research made an impressive series of conceptual and empirical contributions to the new area of social psychology. These included work on autism and social communication; the first ambitious tracing of the evolution of political attitudes over the adult life course; a careful elucidation of the basic principles of cognitive balance; studies of the primary forces shaping interpersonal attraction; and, in a closely related way, the growth of mainstream and deviant subcultures. Throughout his life he was also interested in the dynamics of the education process. More than a small portion of his basic research was focused on this interest, and he successfully applied much of what he learned to practical problems in higher education.

PERSONAL BACKGROUND

Theodore M. Newcomb was born July 24, 1903, in Rock Creek, a rural village in Ashtabula County at the northeastern tip of Ohio. His father was a Congregational minister. In an engaging autobiographical note entitled "The Love of Ideas" (1980), Newcomb recalled that his father had "a passion for the 'rural church,"' and as a consequence the family moved frequently from one small town to another. This meant in turn that his family provided the only continuity in his development, and he gave it uncommon loyalty.

The Newcomb ancestors had been early settlers in Connecticut and Vermont who had followed the westward movement to New York state, and then on into what was once the Western Reserve in Ohio. Family traditions were persistently Congregational, "not untouched by Calvinism." They had also been early abolitionists and, until a defection by Newcomb's parents in World War I, unshakably Republican.

Newcomb grew up feeling that the family was, however, sharply set apart from its rural setting. Both his parents were college-educated and held advanced degrees. They subscribed to "serious" magazines otherwise unknown in the community, and the family spent much time reading books, often aloud. There were even moments of ostracism for the family as the father took up locally unpopular positions from the pulpit, attacking the Ku Klux Klan or supporting pacifism. Family solidarity was thereby enhanced, and young Ted received his first lessons in the invigoration of departing from the herd politically.

Given the rural setting, Newcomb's early education was spent in one-room schools. The big transition came as he began ninth grade in a large high school in Cleveland, Ohio. He weathered the change well, however, and four years later was class valedictorian. His valedictory address heaped scorn on the New York State Legislature for having denied seats to two legally elected members on grounds that they were "bolshevistic" socialists. He was pleased that his parents liked the talk, and that the principal did not.

All of his social world, including Ted, took for granted that he would enroll at Oberlin College, where all good Congregationalists went. He and his closest high school friend had made a joint decision to become Christian missionaries, and his undergraduate days at Oberlin, spent as a faithful student volunteer, were heavily dominated by this goal. He graduated *summa cum laude* in 1924, but in retrospect found any serious learning from the college years a near blank.

To pay off college debts he put in a year teaching high school, and then entered Union Theological Seminary in New York City. With Columbia University across the street and a tradition of cross-enrollments in courses, the pace of his intellectual growth began to quicken. He was excited by fellow students, his professors, and the issues that were gripping them all. He recalled particular fascination with exegesis of the Old Testament from Julius Bewer; progressive education, as taught by William Kilpatrick at Columbia Teachers College; educational psychology with Goodwin Watson, also at Teachers College; general psychology with Gardner Murphy at Columbia, with whom he would later develop a most fruitful collaboration; and ethics with Harry Ward at the Seminary, who dealt more in current issues than in Bible studies.

Ironically, Newcomb's two years at Union convinced him that he might prefer to be a psychologist in academe rather than a Christian missionary. One small reinforcement for this decision was exposure to a seminary professor who lectured frequently on the total superiority of Christianity to all other religions, at a time when Newcomb was reading anthropology about cultural differences and the illusions of ethnocentrism. Therefore, he switched his registration, continued his graduate work for another two years, and received his Ph.D. in psychology from Columbia in 1929.

PROFESSIONAL, CAREER

Newcomb took his first academic position in the psychology department at Lehigh University in the fall of 1929. In part because of his exposure to educational issues at Teachers College, the fledgling instructor was asked to prepare an analysis of some of the university's operating policies. Newcomb's report to the dean was apparently not flattering, and officials requested that he make a number of changes in his findings to put the university in a more favorable light. Newcomb refused, and despite the growing national gloom following the collapse of the stock market, he felt obliged to look elsewhere for employment.

His next position was at Cleveland College, Western Reserve University, where he spent four years. On August 27, 1931, he married Mary E. Shipherd of an old Oberlin family. The relationship was an uncommonly devoted one for the remaining half-century, and soon the family grew to include three children: Esther, Suzanne, and Theodore, Jr. Meanwhile the Depression continued to gain force, and Newcomb was deeply affected. He recalls that he "learned as much from suffering students and their families as from newspapers," and during the darkest days of the early thirties he joined the Socialist Party.

1934 brought a remarkable job change, which influenced the rest of Newcomb's career. He received an offer to join the faculty at the new Bennington College in Vermont, and he accepted with what he called "indecent haste." His Western

Reserve colleagues were baffled by his choice of a tiny and nearly unknown women's college in the Vermont hills over his Cleveland position. Newcomb was, however, thoroughly committed to experiments in progressive education and was aware of the new college's philosophies. He found the prospect of dispensing wisdom in a more intimate setting an exciting one, and it was not until he had moved to Vermont that the sympathizer with the underdog came to realize that the Bennington student body was unrelievedly "upper crust."

The next seven years at Bennington were heady ones. Despite the implausible setting, Newcomb plunged into an array of mildly radical political activities, including the organization of student political groups on the one hand, and a teachers' union at the college on the other.

These considerable distractions notwithstanding, he saved his main energies for his research and writing, and in this period he became a nationally established scholar. He was invited by his former mentor Gardner Murphy at Columbia to participate in a thorough revision of a popular text, *Experimental Social Psychology* (1937), that Murphy and his wife had first published in 1931. Newcomb was to add a major new section systematizing the flood of studies of social attitudes, attitude change, and personality measurement that had appeared since the first edition. His statement increased the bulk of the volume by one quarter and was widely admired. The contribution put him in the habit of using skillful textbook writing to introduce fresh synthetic theory to his peers.

The collaboration with the Murphys dovetailed nicely with a rapid expansion of Newcomb's own research agenda. Although the Murphy book was originally focused on social psychological experiments, Newcomb's part dealt more with measurement of attitudes and personality in natural settings. He appreciated the inferential power of the experimental method, but worried about the potential artificiality of the behavior of self-conscious human beings in plainly contrived laboratory situations. Moreover, he had picked up from Kurt Lewin, his "principal social-psychological hero" as he once described him, a fascination with the "whys and hows of change" in groups over more extended periods of time than fitted the normal confines of the tightly controlled laboratory experiment.

Given these predilections, it is not surprising that Newcomb quickly came to see Bennington in the mid-1930s as a promising "natural experiment" in large-scale attitude change. Young women from genteel and often deeply conservative political backgrounds were gathered together at a time of national economic distress so overwhelming as to stimulate widespread interest in radical reform. It was clear that cohorts graduating from Bennington had on average arrived at views of the political world fundamentally different from the ones they had when they entered, and that the details of this change—who changed, who resisted, and under what circumstances—were shaped in no small degree by internal group processes. Issues pivotal to basic theory in social psychology were at stake here, and Newcomb wanted to subject this changing scene to systematic study.

Therefore, he designed an ambitious longitudinal project to monitor the timing and extent of these changes during the undergraduate career, and to specify the circumstances under which they were most likely to occur. Among a rich array of results reported in his monograph *Personality and Social Change* (1943), some of the clearest findings tied change closely to the college experience itself. Not only did movement toward more liberal views cumulate steadily over the undergraduate years, but the change was greatest among those students most involved in college affairs and most respected among their peers.

The "Bennington Study," as this research effort came to be called, had a major impact on the nascent field of social psychology. Not only was the longitudinal design novel for the period, but the study contained numerous innovative ways of capturing the interplay between individual dispositions and group process. And, of course, the careful demonstration of the potent shaping of individual responses by factors of group context gave empirical reassurance for some of the most central assumptions motivating the study of social psychology.

The Bennington Study also shaped Newcomb's own intellectual development in important ways. The long-term longitudinal study became something of a trademark, despite Newcomb's wry comment that waiting for such studies to ripen on the vine had notably shortened his list of publications relative to peers who completed a number of experiments or shorter studies per year. Some of the substantive findings about the effects of college also influenced some major institutional experiments involving the harnessing of group process to deepen the educational experience, which Newcomb put to real-world application at the University of Michigan thirty years later.

Finally, he was to carry on the Bennington study itself with further longitudinal extensions. About 1960 he returned to the same students, now middle-aged, whom he had studied at Bennington in order to assess the persistence of the attitude change associated with the college experience. The stability was impressive, and as clear for college converts as for those who had come to college already liberal in their views. At the time of his death in 1984, Newcomb and the young social psychologist Duane Alwin were in the late stages of yet another interview with the Bennington students, after more than forty years had passed.

In his seventh year at Bennington, Newcomb was invited to join the faculty at the University of Michigan. He accepted the new position with alacrity, as he had come to miss the stimulation of contact with graduate and pre-professional students. He took up his work in Ann Arbor a few months before Pearl Harbor plunged the United States into World War II. Soon thereafter he was on his way to Washington, D.C., to join a growing cadre of middle-aged social scientists drawn into various forms of research associated with the war effort. He served with the Foreign Broadcast Intelligence Service and the Office of Strategic Services, and, late in the war, spent time in Europe helping to carry out the U.S. Strategic Bombing Survey.

At the war's end he returned to Ann Arbor to resume serious effort on the task for which he had originally been hired: to help the University of Michigan develop strength in the growing area of social psychology. This he did in the grand manner; he proposed to the university the establishment of a joint doctoral program, which he would chair, shared by his two departments of sociology and psychology.

The new program, which had but one loose parallel at Harvard, turned into an instant success. It accepted students only beyond the master's level, but soon attracted a deluge of applications and was able to set a high threshold for entrance. Newcomb assembled a distinguished faculty to help define and teach in the program, including most centrally Daniel Katz and Dorwin Cartright, as well as other social psychologists from his Washington days who had come to Michigan to form the Institute for Social Research. In short order, the program was awarding Ph.D.'s at a rate that made it one of the largest in the university. Under Newcomb's leadership over the next two decades, program graduates went to all corners of the land, colonizing both sociology and psychology departments in the name of the joint discipline.

In the early period of the program Newcomb published his basic text *Social Psychology* (1950). This was a gentle but thoughtful work that enjoyed great popularity. Several good texts in this new area had appeared over the preceding fifteen years, including the 1937 Murphy revision to which Newcomb had contributed. Most of these were, however, written by psychologists from a distinctly psychological point of view. Although his own primary background had also been in psychology, Newcomb's definition of the field welded in a richer complement of basic sociological concepts, including social roles, social norms, and other essentials of social structure.

In this same general period Newcomb extended his Bennington studies by joining with W. W. Charters in experimental work manipulating the situational salience of reference groups to gauge variation in their impact (1952, 2).

He also began work on a set of theoretical notions that would permeate his own research agenda for at least another decade. He had become intrigued with the work of Fritz Heider in the 1940s, developing propositions about cognitive "balance," or a hypothesized tendency toward symmetry in feelings about objects that the actor cognitively associates with one another in "unit relations." One obvious special case of such an association might be the linking of a message with its source. If, for example, the actor listens to a source, for whom he has feelings, express some attitudes, for which he also has feelings, it is of course possible that at any given time these feelings are opposite in sign: the source is liked but the message is disliked, or vice versa. Such configurations represent a form of disequilibrium, which has predictable consequences for both the actor's attitudes and behavior, the most general of these being the longer-run expectation of a move toward greater symmetry of these linked preferences.

Newcomb took these ideas of structural equilibria out of the heads of individual actors and worked with them as a set of basic terms for the understanding of two-person or dyadic interactions. The relationship between persons A and B could be described as an attitude of liking or disliking, and each could like or dislike a shared object of orientation X. These terms formed an ABX "system" because there was some interdependence or "strain toward symmetry" between the valences present. The more detailed exposition of the nuances of the ABX system appeared in his influential article, "An Approach to the Study of Communicative Acts" (1953, 2).

Much of Newcomb's work over the ensuing decade was addressed to the phenomena surrounding interpersonal perception and liking. He was eager to test the predictions of his ABX theorizing in a more natural setting, and gravitated toward a field-experimental design that bore no small resemblance to the earlier Bennington Study. He felt he needed to capture the formative beginnings of such systems, catching "A's" and "B's" before they were aware of one another's existence, and hence before either knew the other's orientation toward various X's. Therefore, he arranged an intensive two-year study of students' attitudes and interpersonal attractions based in their living units from the time of arrival at the university, when dorm mates met for the first time. The resulting monograph, *The Acquaintance Process* (1961), stands as another milestone contribution to social psychology.

In his later years Newcomb turned his lifelong fascination with the useful integration of "living and learning" to some practical institutional consequence at the University of Michigan. He was one of the central figures in the design and creation of a small, informal residential college within the university. In an early role as associate director of this unit he devoted a great deal of time to the formation of its institutions and particularly to the creation of its community governance in a mode that involved a remarkable level of student participation. He frequently served as a consultant to similar developments elsewhere in the country, and summarized a good deal of informal experience, personal research, and a copious external literature in a two-volume work entitled *Impact of College on Students* (1968).

Over the course of his career, Newcomb served his two parent disciplines of psychology and sociology in a variety of roles. He was president of the Society for the Psychological Study of Social Issues in 1945-46 and president of the Division of Personality and Social Psychology of the American Psychological Association in 1948-49. He edited the summit journal, the *Psychological Review*, from 1954 to 1959. In 1955-56 he was president of the full American Psychological Association.

In a different context he served on the Board of Trustees of Antioch College for twelve years from 1959 to 1972, chairing the board for the last six years. From 1961 to 1966 he spent periods of each year at the Western Behavioral Sciences Institute as a Visiting Fellow. He also completed terms as Visiting Professor at both San Diego and Santa Cruz in the University of California system and as Distinguished Visiting Professor at York University (Toronto) and Amherst College. In his final years at Michigan he was the Mary Ann and Charles R. Walgreen, Jr., Professor for the Study of Human Understanding.

Newcomb's intellectual contributions brought him many other honors and awards in addition to his election to the National Academy of Sciences in 1974. He was a Fulbright Scholar in London in 1951-52, a fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford in 1956-57, and a Guggenheim Fellow in 1960. He was elected to the American Academy of Arts and Sciences in 1957. Other awards included the Kurt Lewin Memorial Award from the Society for the Psychological Study of Social Issues (1962); the first annual Research Award from the American Educational Research Association (1972); the Distinguished Scientific Contributions Award of the American Psychological Association (1976); and the Cooley-Mead Award of the American Sociological Association (1980).

TED NEWCOMB, THE PERSON

It remains to us to capture the essence of Ted Newcomb as a most lively colleague, mentor, and friend.

In his autobiographical statement, Ted recalled that when he was a boy in his early teens, his father announced to the family that he would present them with phrases that would best distinguish each of the children. For Ted, the phrase was "Isn't it going to be *wonderful!"* Seventy years later, as a group of Ted's closest colleagues and former students presented remembrances of him in a memorial service at the University of Michigan, one of several traits mentioned by all was his bursting *joie de vivre*, a zest for life that simply seemed irrepressible.

Closely related, and mentioned with equal frequency, was an infectious playfulness. He was a quick wit, often irreverent and what one speaker called "bratty," but never mean-spirited to others in his humor. He also expected work to be play. "He never confused seriousness with solemnity," said William Gamson at the memorial. "A class was a time to play with ideas—serious play."

He was also a profoundly generous person. He was known for giving "not just money for good causes, needy students, or embarrassed colleagues," Robert Kahn recalled, "but generosity of time and self." His pervasive generosity was nowhere more clear than with his students, whom Ted once called his "jewels." Just as he was a stellar role model for aspiring young people, his marriage with Mary was a notably loving one. Their home was remarkably open, and they were considered as spiritual godparents by virtually all of the hundreds of students who came through the social psychology program. Over the years Ted drew many students into personal collaboration on research projects and other professional writing, a priceless learning experience for the students, although not always the most efficient use of his own time.

Ted was also unusual in his response to situations of rules and authority, and this was equally true whether he came at them from the top side or the bottom. He was, of course, intensely egalitarian, but this value he shared with many others of the same time and place. He was uncomfortable around pomp and puffery and resisted it in others in a quiet and good-humored way.

A hasty and superficial observer might have coded Ted as deeply irreverent toward authority. But in the degree such a description calls to mind the hostile malcontent, nothing could be further from the mark, or less in tune with his sunny and trusting disposition. He was not negatively disposed toward human institutions; in fact, as noted, he created several very innovative institutions himself and led them with high distinction. But he did not have a bureaucratic bone in his body. He knew that rules and social organization could facilitate progress toward many enticing goals, but he also knew that they could become obstructions as well, an outcome he was not prepared to tolerate.

Indeed, in his memorial service remembrance of Ted, William Gamson credited him with "a certain deliberate obtuseness about obstacles." His instinct was always "Why not?", with "the burden of proof always on the resisters and foot draggers rather than on those who would create some thing new and different." Boundaries were there to be crossed and unspoken rules to be broken. "Why not? They were our boundaries and rules, weren't they?" His focus was always on the goals and the main vision. Decisions about the best means were left to come tumbling after.

He did have one major public confrontation with Higher Authority, and acquitted himself impressively. In the early 1950s the House Un-American Activities Committee (HUAC) descended on Ann Arbor to interrogate suspected Communist sympathizers on the University of Michigan faculty. Although Ted was himself on one of HUAC's lists because of his memberships of the 1930s, he was not one of the immediate quarry. Several younger faculty members refused to identify their associations for the committee, however, and an intimidated university administration agreed to expel or suspend them. This capitulation distressed a substantial fraction of the faculty. When the president came to a meeting of the university senate to defend his actions, Ted took the floor with a calm but forceful challenge to both HUAC and the administration on the fundamentals of academic freedom. It was a matter of widespread embarrassment to many sympathetic colleagues that Ted alone had stood up to be counted on the issue.

Ted Newcomb was a person of thorough-going convictions without a trace of sanctimony. He was a warm and playful companion at the same time that he was an inspiring colleague and mentor. He did as much as anyone to define and systematize social psychology as an area of specialization, and he helped adorn the subject with five decades of excitement. Above all, he was insatiably curious about the human condition and how it might best be understood. And to a generation of us, he conveyed with consummate skill "the love of ideas."

MUCH OF THIS MEMOIR is based on a diffuse range of informal sources, including my personal recollections as a student of and later collaborator with Ted Newcomb, and many conversations over the years with friends and colleagues from different periods and sectors of his life. Two "hard-copy" sources were, however, indispensable. One was the self-portrait "The Love of Ideas" (see bibliography). The other was "Remembering Ted Newcomb: A Biographical Sketch and Remembrances from a Memorial Service for Theodore M. Newcomb, Held January 25, 1985, at the University of Michigan."

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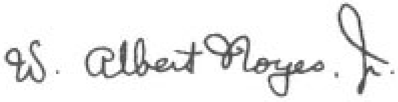

WILLIAM ALBERT NOYES, JR.

April 18, 1898-November 25, 1980

BY JOHN M. WHITE WITH PAMELA J. COOK

WILLIAM ALBERT NOYES saw himself, his memoirs reveal, as *A Victorian in the Twentieth Century*. A man of both discipline and imagination, he believed that his were especially interesting times, when science was advancing rapidly and the political face of the world was changing. He was not only an eminent photochemist, but a scholar with broad interests and a liberal philosophy. Albert Noyes considered himself, at bottom, a "university man," but his life encompassed numerous activities and enterprises outside the realm of academics. He died in Austin, Texas, on November 25, 1980.

The research career of Albert Noyes, pioneer of photochemistry, is well-documented in over 200 publications. His early work dealt with the identification of primary photochemical events and their connections to spectroscopic facts. The classic text with Philip Leighton, *The Photochemistry of Gases* (1941), describes much of this, and other work, up to 1941. Noyes's later work centered on the photochemistry and photophysics of ketones, particularly acetone, and simple aromatic compounds. Toward the end of his career, he became interested in the photochemistry of polymers, which he correctly thought would be at the heart of the next generation of photochemical research.

Beyond his laboratory work, Noyes's scientific career reflects constant attention to professional activities with the U.S. government, with the American Chemical Society, with universities, and with a number of industrial laboratories. His long-standing association with Argonne National Laboratory is, to this day, noted with a prominent photograph in the lobby of the Chemistry Division Building.

William Albert Noyes, Jr., was born April 18, 1898, in Terre Haute, Indiana, to William Albert Noyes and Flora Collier Noyes, their only child to survive beyond early childhood. His father was a chemist and, at the time of Albert Noyes's birth, a professor of chemistry at Rose Polytechnic Institute. Albert Noyes said that he inherited his love of music and his aptitude for foreign languages from his mother, who died in March 1900 after years of poor health. Albert's father was much involved with the burgeoning American Chemical Society and served as the editor of their *Journal* beginning in 1901. Also in that year he remarried, and Albert got a stepmother and, in 1904, a brother, Charles.

In 1903 Albert's father accepted a position as chief chemist with the new Bureau of Standards in Baltimore. As he waited for the buildings to be completed, he did research at Johns Hopkins University. He often brought his son to his laboratory, and young Albert began to meet prominent chemists and scientists. "To list the great scientists I met as a boy," he later wrote in his memoirs, "would be like giving a listing from Who's Who." His father was offered the chairmanship of the Department of Chemistry at the University of Illinois, and the family moved to Urbana in 1907.

In Baltimore Albert had been a poor student. But in Urbana, thanks to some good teachers and, he said, to the strict discipline imposed by his stepmother, he began to improve. He graduated from the eighth grade near the top of his class. In high school his first love was history, much of which he read on his own initiative. He played baseball and ran on the track team but claimed that he was never very good. During his last year of high school Albert's stepmother had a stroke, and she died in August 1914.

Albert had begun his senior year undecided about his future, torn between history and science. His physics teacher, Lloyd Howell, was a "truly inspiring teacher, one who permitted students to work problems in their own ways." This suited Albert, and he was convinced that he belonged in the sciences. In the fall of 1914 he went off to Grinnell College in Iowa to study chemistry. Always a lover of the railroads, he took a train trip from Iowa to California with his brother Charles in the summer of 1915. Later that year, his father married again; his new wife, Katharine Macy Noyes, was to be a good friend to Albert. She and Albert's father had two sons, Richard and Pierre, both of whom also followed their father into the sciences and academia.

At the end of his sophomore year, Albert was awarded a junior scholarship at Grinnell, but he had already decided to transfer to the University of Illinois. He spent the summer training as a commercial radio operator at the Marconi Company's school in New York City. Later that fall, during the election in which Woodrow Wilson won the presidency, Albert was called upon to help receive returns at the local radio station. At school he was taking a heavy load of chemistry coursework, and by the end of the year he was only twenty-one hours shy of the graduation requirement. World War I had intruded very little on his academic and social life, but by spring 1917 Albert had been asked to join the Signal Enlisted Reserve Corps of the U.S. Army, probably because he held a radio operator's license. In April, a week after the United States entered the war, Albert enlisted over his father's protests. June found him working as a radio operator on the *S.S. Warrior* along the Atlantic coast, and then on a series of passenger ships before being called to Camp Sherman for training with the 83rd Division in a Field Signal battalion. On June 12, 1918, the *S.S. Megantic*, with Albert Noyes aboard, sailed for Europe. In France he served as both interpreter (his French was very good) and radio operator, and he retained vivid memories of the time he spent near the front and the bombardments. In 1969, while a visiting professor at the University of Nancy, he revisited the Officers Training School in Langres, where he had been commissioned a Second Lieutenant on Armistice Day, November 11, 1918.

Albert was extremely fond of Europe, and he decided to stay in France and pursue advanced studies at the Sorbonne. Early in 1919 he began working in electrochemistry in Henry Le Chatelier's laboratory. In Paris he met Sabine Onillon, the "nice young lady" he would later marry, and settled down to his studies. He still found current events immensely interesting and followed closely the peace talks and the negotiations for the Treaty of Versailles. After two years at the Sorbonne, he received his doctorate and returned to the United States. He spent a year at Berkeley working as a teaching fellow under Joel Hildebrand and doing research with George Gibson on electric discharge through gases. In June 1921 he and Sabine were married at his father's home in Urbana. They returned to Berkeley, where Albert had been elevated to the position of instructor.

The University of Chicago lured Albert Noyes back to the Midwest in 1922. Here he had complete charge of the quantitative analysis course. He worked very hard during his seven years at Chicago, with little time for research, which was wedged into weekends and late nights. He made a number of friends there, including Robert Millikan, and was proud of the fine students he supervised, among them Louis Stevenson Kassel and William E. Vaughan. Then Brown

University offered him a faculty position, and he and Sabine moved to Providence in 1929. Their son Claude was born there in December. Albert found the department well run and the work satisfying. The Noyes family traveled extensively in Europe during summer vacations, and in 1937 they spent several months at Cambridge University where he worked with R. G. W. Norrish. Late in the decade, he and his friend Philip Leighton began writing a book on photochemistry; little was yet available in this interesting field, and Albert Noyes had acquired a reputation as a leading photochemist during his years at Brown. The book appeared in print, as Noyes noted in his memoirs, the week before the Japanese attacked Pearl Harbor.

The University of Rochester made him an excellent offer in the winter of 1938 and he accepted, becoming head of the department of chemistry and initiating a number of changes that would strengthen the graduate program. Then the Second World War began. Although the United States had not yet entered the fray, the country began to organize for war. Albert was asked to attend a meeting of the National Defense Research Committee (NDRC) in October 1940 to consider problems related to the war. As a result of this meeting he was to be active in defense work for many years.

It was inevitable that Noyes would become so deeply involved in government work: His interest in the world political scene was always intense and acute, and his scientific acumen was well established. Initially, he served as a liaison between the NDRC and universities doing defense research. Early in 1941 he visited the English office of the NDRC with a small group of scientists. He stayed at Salisbury and spent several weeks at the Experimental Station at Porton studying the use of gas and gas masks in combat, "learning the practical difficulties of bridging the gap between laboratory and production." The NRDC's Chemistry Division had many contracts with universities, and work was proceeding rapidly. Then came Pearl Harbor. William Albert Noyes, Sr., had died just several weeks earlier, and his son said that in some ways it was best that his father, a lifelong worker for world peace, had not had to witness the devastation of Pearl Harbor.

In 1942 the NDRC, the Committee on Medical Research, and the Office of Field Service merged into the Office of Scientific Research and Development, and Albert Noyes was named head of the Chemical Warfare and Smoke Division. His duties required a good deal of travel in the United States, Europe, and even Australia; when he was at home he worked six-and-a-half day weeks, still had duties at Rochester and, remarkably, served as editor of *Chemical Reviews*. His memories, however, were less of the drudgery than of the politics and personalities of the time, and vivid recollections fill several chapters of his memoirs. During and after the war, he was active in the International Council of Scientific Unions and, especially, UNESCO. Not until the war ended did his schedule ease up, and even then he remained active in government work until his move to Austin, Texas, in 1963.

When he returned to Rochester after the war, Albert had a number of offers from other universities but chose to stay on as head of the chemistry department. In 1952 he was named dean of the Graduate School and in 1956 dean of the College of Arts and Sciences.

During the spring of 1962, Noyes's good friend Professor George Watt of the University of Texas at Austin wrote asking whether he might be interested in a position there. Noyes was approaching his sixty-fifth birthday and, though his special professorship would have permitted him to stay on at Rochester, he felt strongly that "after retirement a person should not breathe down the necks of his successors." He and Sabine spent a semester in Austin, found the climate and the intellectual atmosphere to their liking, and moved to Texas permanently in the summer of 1963. There Noyes was active in recruiting new faculty in physical chemistry, as well as maintaining an active research program and teaching undergraduates. He retired to professor emeritus status in 1973.

Albert Noyes was elected to the National Academy of Sciences in 1943 and to the American Philosophical Society in 1947; he was awarded the Priestley Medal in 1954 and the Gibbs Medal in 1957, and in 1976 received the ACS Charles Lathrop Parsons Award. Clearly, his contributions to the field of photochemistry were widely recognized and appreciated. In addition to his editorship of *Chemical Reviews* from 1939 to 1949, he was editor of *the Journal of the American Chemical Society* from 1950 to 1962 and edited the *Journal of Physical Chemistry* from 1952 to 1964, where one of his goals, successfully attained, was to raise the theoretical level of the journal.

F. A. Matsen, who had been at Texas for some years when Noyes arrived there, considers him "the best thing that happened to physical chemistry at the University of Texas during my tenure. He brought in strong leadership and a real ensemble spirit." Noyes organized monthly physical chemistry dinners where faculty met to discuss problems of general interest. But what Matsen remembers best is that "if he met you in the hallway, he wouldn't ask, 'How are you?'—he'd say, 'Why is the intersystem crossover in benzene so rapid?' He was always thinking about science." Noyes was also a superb recruiter, Matsen recalls; he located potential faculty members while they were still graduate students, then kept in touch as they finished school and became available for faculty positions.

Norman Hackerman, former president of both Rice University and the University of Texas at Austin, and now professor emeritus at UT-Austin, met Noyes for regular Sunday morning talks. Noyes enjoyed conversation, and one topic of great concern to him was the war in Vietnam. The two shared many hours of discussion in the sixties and seventies, on the war and numerous other subjects, Hackerman remembers. His stress on discussion as a learning tool carried over into the classroom: Noyes's enormously popular course, "Science in Government and in International Affairs," required each student to meet with him individually for face-to-face discussion.

Early in 1966 Noyes, whom I had never met, dropped into my lab at Caltech, unannounced. Fortunately, I was at work, and he proceeded to quiz me about what I was doing and why. In less than five minutes, he sized up the situation and finished by asking me if I knew anything about Badger's Rule. A few weeks later, when I was invited to Texas to give a research seminar, I understood what he had been doing: As I learned, he recruited by roaming the halls and labs of institutions, and he had been pointed my way by someone at Caltech. Thankfully, I knew something about Badger's Rule!

This was not the end of the Noyes influence on my career. When I arrived in Austin in February 1967 to take up my faculty position, two excellent students, Gene Sturm and Gerry Wood, signed on with me within a few weeks. They had been encouraged to look into "that new young buck down the hall." Noyes's advocacy on my behalf with students and with the "powers" of the department set an excellent example that all of us would do well to follow. There were only a few things that Noyes would not tolerate—one of the more famous was the playing of radios during working hours.

I had the distinct pleasure of co-supervising Noyes's last Ph.D. student, Ardi Lee, and from that work we published a paper on one of Noyes's favorite topics—energy transfer in electronically excited benzene. Noyes's ghost is still around my own office: I have his desk and chair, and I hope that someone will take them from me with as much gratitude as I received them from William Albert Noyes, Jr.

THE AUTHORS THANK the following people for their help in compiling this memoir: at the University of Texas at Austin, Martha Boyd, Dorothy Frasch, and Professors William C. Gardiner, Norman Hackerman, and F. A. Matsen; and Julie Nowak of the University of Rochester. Much information was gleaned from Noyes's (unpublished) autobiography, *A Victorian in the Twentieth Century*, and from Professor Hackerman's memoir of Noyes written for the American Philosophical Society.

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CHARLES H. RAMMELKAMP, JR.

May 24, 1911-December 5, 1981

BY FREDERICK C. ROBBINS

CHARLES H. RAMMELKAMP, JR., died suddenly on December 5, 1981, of a ruptured abdominal aortic aneurysm at the age of seventy. On the night before his death he awakened with abdominal pain and suspected what the diagnosis might be. However, with his characteristic concern for others, he did not bother anyone and waited several hours until his wife awakened at 7:00 am, her usual time. His remark to her was, "I think you had better take me to the hospital, I believe I am going into shock." At the hospital he was indeed in shock, and although surgery was done promptly, he did not recover. At the time of his premature death he had just become emeritus professor and was looking forward to a new and exciting career of scholarly activities. During his career he made exceptional contributions to clinical research, teaching, and patient care. His scientific contributions were largely in the field of infectious disease, most notably early studies on the clinical application and mechanism of action of antimicrobials, i.e. sulfonamides and penicillin, and the epidemiology of streptococcal infections, the non-suppurative complications of streptococcal infections such as rheumatic fever and acute glomerulonephritis, and the prevention of rheumatic fever by treatment of the streptococcal infection with penicillin.

Dr. Rammelkamp, who was known to all his friends and acquaintances as Rammel, was born in Jacksonville, Illinois on May 24, 1911. He grew up in a family with a sister and two brothers. The environment was a scholarly one with a father who was president of Illinois College. Rammel obtained his A.B. degree (1933) at his father's college and upon graduation chose a medical career with the intention of becoming a general practitioner. He attended medical school at the University of Chicago (1937), after which he served as an intern in medicine at the Barnes Hospital in St. Louis (1937-38). After one year at Barnes he returned to Chicago as an intern in surgery at the Billings Memorial Hospitals of the University of Chicago (1938-39). His foray into surgery lasted only one year and he returned to Barnes as an assistant resident in medicine (1939). It was never quite clear why he chose to spend a year as a surgical resident because he had few of the attributes usually associated with a surgeon, but he seems to have enjoyed it and would often comment upon how valuable he found the experience. The course of his career was really determined in 1939 when he went to the Thorndike Memorial Laboratory of Boston City Hospital as resident physician. The Thorndike was an exciting place intellectually. At the time, it was populated by such luminaries as George Minot, hematologist and Nobel laureate noted for his work on pernicious anemia; Soma Weiss, teacher "extraordinaire" and cardiovascular investigator; Maxwell Finland and Chester Keefer, both of whom were distinguished infectious disease investigators. In addition there was a group of young physician-scientists who were to become leaders in academic medicine. Fortunately for the field of infectious diseases, Rammel accepted a position with Chester Keefer rather than Soma Weiss. Had he joined Weiss, cardiology undoubtedly would have benefited greatly.

Rammel's first task in Keefer's laboratory was to group and type beta hemolytic streptococci, a task that he found tedious and uninspiring. Nonetheless, this was the beginning of a lifelong fascination with the streptococcus and the diseases it causes. Next he chose to study an antibiotic, gramicidin, that had been discovered by Rene Dubos. Although effective in the test tube, gramicidin did not prove very useful *in vivo* because of its poor solubility. Along with these studies Rammel was involved in experiments with sulfonamides that were just beginning to be used clinically. His first publication dealt with sulfathiazole, the first sulfanilamide to be introduced in this country. Most of his early work concerned the pharmacology and clinical application of sulfanilamides and the bacterial products, gramicidin and tyrothricin.

In 1940 Dr. Keefer moved to the Evans Memorial Laboratory to establish a department of medicine at Boston University. Rammel joined him there and soon was engaged in exploring the use of the new and exciting antibiotic, penicillin. The studies done by Rammel, Keefer, and associates on the pharmacokinetics and clinical effectiveness of penicillin were critical in providing the basis for its rational clinical use. Penicillin was a precious commodity at that time and Keefer was given responsibility for its allocation nationally. Thus, Rammel was strategically placed to be kept informed about all studies being conducted throughout the country. One of Rammel's more significant contributions was to devise a procedure for quantitating penicillin levels in blood and other biological fluids, a method that became universally used.

Rammel's tenure in Keefer's department was highly productive but did not last long. The United States had entered World War II and the military was experiencing serious problems with acute respiratory diseases. The

Commission on Acute Respiratory Diseases had been established at Fort Bragg, North Carolina, with Dr. John Dingle as its head. Dr. Dingle recruited Rammel along with a fine group of clinical investigators and epidemiologists to conduct studies directed at elucidating the cause of acute respiratory diseases and to develop methods for their control. Rammel's special assignment was streptococcal disease but he was also involved with studies on primary atypical pneumonia (now known to be due to mycoplasma infection), influenza, and other respiratory diseases. During its five-year existence, the commission published extensively on these topics. One of the more significant papers described the relationship of epidemics of influenza with the frequency of pneumonia. This has provided the basis for the influenza surveillance by the Centers for Disease Control, which follows pneumonia prevalence as a surrogate for influenza. The commission adopted a policy of communal authorship of papers; the recorded author was the commission with only a listing of the members, so that a portion of Rammel's bibliography is not identified under his name.

With the war over, Dr. Dingle moved to Cleveland in 1946 along with several members of the commission, including Rammel, in order to establish the Department of Preventive Medicine at Western Reserve University School of Medicine. The department made many important contributions over the years but is best known for the landmark ten-year Family Study program. This careful study provided a gold mine of information about the common illnesses in families and the population at large. Although Rammel was very much involved in the design and conduct of the Family Study, he became intrigued with the high prevalence of streptococcal infection and rheumatic fever in troops serving in the Rocky Mountain area. In 1949 he became the director of the Streptococcal Diseases Laboratory at Warren Air Force Base in Wyoming. It was here, in a period of six years, that he and his associates conducted the classical studies on the epidemiology and clinical features of streptococcal infection that demonstrated that rheumatic fever could be prevented if the acute streptococcal infection was treated adequately with penicillin. These studies not only made it possible to prevent rheumatic fever but also provided the clinching evidence of the role of the streptococcus in its etiology. The Ft. Warren laboratory was highly productive scientifically but also served as a training ground for an unusually talented group of young physicians, among whom were Lewis Wannamaker, Richard Krause, Chandler Stetson, Harold Houser, and Floyd Denny, all of whom went on to distinguished careers. The significance of their work was recognized when, in 1954, the Albert Lasker Group Award was presented to the Streptococcal Disease Laboratory.

Rammel had been puzzled by the fact that only a single case of acute glomerulonephritis had occurred among the more than 1,000 cases of streptococcal infection that had been observed at the Warren Air Force Base. The organism recovered from that case was Type 12. This observation he put together with earlier findings from a family outbreak of Type 12 infection in which five members displayed evidence of acute kidney disease and proposed the hypothesis that Type 12, and possibly others, was a nephritogenic strain of streptococcus. This hypothesis on further study did indeed prove to be correct, and Type 12 and a few other types are now recognized as having the peculiar capability of producing acute nephritis. He and his co-workers were interested in why some strains were nephritogenic and others were not, but several lines of investigation did not yield the answer to this intriguing question.

In 1950 Rammel was asked by Dr. Joseph Wearn, then dean of the Western Reserve Medical School, to help develop an academic program at Cleveland City Hospital (later to become Cleveland Metropolitan General Hospital [CMGH], and now MetroHealth Medical Center). Dean Wearn had negotiated with the mayor of Cleveland a new agreement that gave the medical school appointment power for the staff and provided for new facilities including a research building. Rammel was given the titles of professor of medicine, associate director of medicine, and director of research laboratories at City Hospital. He was able to recruit a number of outstanding research-oriented staff and new directors of pediatrics and surgery. The model he had in mind was the Thorndike Memorial Laboratory at Boston City Hospital but, as it turned out, his influence and that of the new recruits affected all aspects of the hospital's functions. Largely through his efforts the City Hospital became the fine academic institution that it is today.

Rammel continued his interest in streptococcal disease and among other activities developed, with his associates, a mail-in system for the rapid diagnosis of streptococcal pharyngitis that allowed the physician to delay treatment until there was evidence that it was indicated, thus forestalling much unnecessary treatment. He also engaged in a series of studies in Chile on the use of penicillin treatment of acute rheumatic fever and valvulitis.

In addition to the streptococcus, Rammel was interested in the staphylococcus. Along with A. J. Gonzaga, Edward A. Mortimer, Jr., and Emmanuel Wolinsky, he conducted a series of classical studies on the epidemiology of staphylococcal infections in newborn nurseries. At the time, epidemics of staphylococcal infections were occurring frequently with considerable morbidity and mortality. Their studies showed, quite conclusively, that infection was transmitted on the hands of the caretakers and that simple handwashing was an effective control measure.

Although Rammel's principal scientific interests were in the field of infectious diseases and streptococcal and staphylococcal infections in particular, he brought the same degree of curiosity and scientific analysis to whatever he was concerned with. He was a strong advocate for the application of basic science to clinical problems and was concerned that basic principles were not being taught adequately in the ambulatory or outpatient setting. He devised and put into effect an ambulatory unit whereby students and their instructors could investigate their patients in more depth than was usual in the clinic. This became an effective and unique teaching unit in which ambulatory patients received exceptionally fine care.

Another example of Rammel's ability to combine a scientific approach with his great concern for teaching and patient care was the "Firm" system of organizing medical care. He had been impressed with the advantages of the "Firm" system as practiced in Great Britain. This consists of a group headed by a senior physician and includes registrars (the equivalent of residents and fellows in the U.S.) and students who are responsible for the total care, on a continuing basis, of a group of patients in the hospital and the ambulatory clinic. This provides for a degree of continuity of care and teaching that does not occur in the usual U.S. system. Although at first he was primarily interested in improving patient care and teaching, he immediately saw its research potential. One can introduce a certain procedure or behavior into one or more firms and not the others and compare the outcome. A key feature of the program is the random assignment of patients to the firms. Since Rammel's death, this unique technique of health care research has been continued at Metro by a cadre of young physicians. Their studies have evoked interest around the country.

Rammel had a profound interest in education. His inquiring mind was always searching for more effective ways to teach. He was a major force in the construction of the innovative curriculum at Western Reserve. His particular interests were better ways of integrating basic science with clinical teaching and the application of epidemiologic principles. He was one of the architects of the so-called "basic" clerkship which was a four-month period on either a medical or pediatric service and constituted the student's first intensive clinical experience. It was a long enough period so that the student could experience some continuity of patient care and contact with one group of faculty. It also provided the opportunity to do special projects and to give greater attention than is usually the case to biomedical and psychosocial processes underlying disease.

Rammel's concern for the education of his resident staff was great and, as already mentioned, in large part motivated the introduction of the firm system that proved so successful. He was always available to his house staff and fellows and was much concerned with their education and their personal welfare as well. As a result he was much admired and even loved by most of them.

Rammel was married to Helen Chisholm, who was Chester Keefer's secretary, and they had three children: Charles H., III, Colin C., and Anne K. Davies. Rammel was devoted to his family although he did not spend as much time with them as he might have because of his heavy schedule and tremendous devotion to his work. However, they had a cottage on the shore of Lake Michigan and he always found some time each year to spend there. The Rammelkamp family was close but not particularly interested in the social life of the community or faculty.

Rammel very much enjoyed association with his faculty, house staff and fellows and his colleagues throughout the country. Typically, with a cigarette in his mouth (in spite of many efforts, he never succeeded in kicking the habit), he would often be found deep in discourse with a group of colleagues that would continue to all hours. The topics discussed would deal with science, education, or intellectual subjects, and to a limited extent the usual academic gossip. Although Rammel was actively engaged in many professional societies and served as an officer including being president of many, he had little interest in professional politics. Indeed, among his outstanding attributes was a lack of personal ambition for power or prestige and an unselfish concern for his colleagues no matter what their rank or position. He was a warm, enthusiastic man who evoked respect and admiration from his colleagues and peers and exceptional loyalty from those who worked with or for him.

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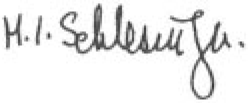

HERMANN IRVING SCHLESINGER

October 11, 1882-October 3, 1960

BY GRANT URRY

IT IS A CONSIDERABLE pleasure for me to have been asked to comment on the life and career of my mentor. There are among his more than fifty former graduate students more distinguished scientists, but it is unlikely that any among them remembers their tenure in the laboratories of Dr. Schlesinger with more gratitude or greater fondness than I. At the outset it should be said that my memory has been greatly aided by information furnished through the kind efforts of Ms. Joan Shiu of the Department of Chemistry at the University of Chicago and Dr. Elizabeth J. Sherman at the National Academy of Sciences.

One of the outstanding chemists of this century, Professor Schlesinger's greatest contribution to his chosen profession was the discovery of the borohydrides and the aluminohydrides. He devised simple, high-yield syntheses of these classes of compounds, making them available as uniquely valuable reducing agents, particularly for various functional groups in organic compounds. Without the ready availability of lithium borohydride and lithium aluminohydride the current state of medicinal chemistry and molecular biology would undoubtedly be severely retarded.

He would have viewed the students he trained as his greatest contribution. It is true that among this select group are several distinguished scientists who interpreted and extended his discoveries to make them commonplace utilities. A genuine humility made Professor Schlesinger a much-loved colleague; it also possibly accounts for the great disparity between his stature and the modesty of the reputation he was accorded during his lifetime.

Among the honors he enjoyed during his career, he prized most highly his election to the National Academy of Sciences in the spring of 1948. The other honors he felt worthy of mention were: honorary degrees from the University of Chicago and Bradley University; the honor scroll of the American Institute of Chemists; the George Fisher Baker lectureship at Cornell University; the Julius Stieglitz memorial lecture and the J. Willard Gibbs Medal of the Chicago section of the American Chemical Society; the Edgar Fahs Smith memorial lecture of the Pennsylvania section; and the William Albert Noyes memorial lecture at the University of Illinois. He was a member of the Bavarian Academy of Sciences and was awarded the Alfred Stock Memorial Prize of the German Chemical Society. The U.S. Navy presented him with its highest honor, the Distinguished Public Service Award. In 1959 he was awarded the Priestley Medal, the most prestigious award of the American Chemical Society. Near the end of his career he was much amused by being characterized in the popular press as "the Father of Rocket Fuel."

Hermann Irving Schlesinger was born October 11, 1882, in Milwaukee, Wisconsin. When his family moved to Chicago he was six years of age and enrolled in Schultz's School, a private grammar school established by the German-American community. The master's traditional German discipline had been tempered somewhat by the democratic principles of his adopted country. His school maintained typically Germanic attention to detail and rigor in all other respects.

Upon graduation from Schultz's School in 1896, the young Schlesinger entered Lake View High School of the Chicago Public School System. There he came under the tutelage of a science teacher, Mr. Linebarger, remarkable in his or any time. This high school teacher accomplished publishable research in his laboratory outside the usual heavy duties of teaching chemistry and physics. Under this benign influence Dr. Schlesinger's career choice was early and firm. When he entered the University of Chicago in 1900 it was to be educated as a chemist.

The faculty in chemistry included John Ulric Nef, Alexander Smith, and Julius Stieglitz. Albert Michelson and Robert Millikan were members of the department of physics. All of these, in addition to being eminent scientists, were exceptional teachers and their influence upon Dr. Schlesinger was evident throughout his long career. The ambience in this young university was easy going and informal. Relations between the faculty and the students were sociable and congenially close.

His undergraduate studies were completed with a laudatory record of achievement. In the overwhelming majority of his courses he earned the highest possible grade. His poorest performance earned average grades in two courses and in the recitation portion of a third.

After this stellar undergraduate performance he chose to complete a thesis under the direction of Julius Stieglitz. After an expected excellent graduate career, lasting just over two years, he was awarded the doctorate in 1905.

In September of that same year he journeyed to Berlin, there to work in the laboratory of Professor Nernst. The year he spent in Berlin was a greatly broadening intellectual experience for him. Again, he was in the midst of great movers of science. Landolt, though old and feeble, still came to the laboratory to conduct experiments that extended his classic proof of the law of conservation of mass applied to chemical reactions. Nernst had just announced the third law of thermodynamics; Planck had recently developed the quantum theory; Emil Fischer was engaged in his study of proteins and amino acids; and Van't Hoff was busily studying the origins of the great Stassfurt salt deposits. With the exception of Nernst, who gave no lecture course that year, Schlesinger attended lecture courses by all of these great men.

In spite of close personal relationships with Nernst and Van't Hoff that developed during the year at Berlin, fifty years after the event Dr. Schlesinger would compare unfavorably the rigid and remote formality of faculty-student relations at Berlin with his earlier experience at Chicago.

In August of 1906 he left Berlin to hike through the Bernese Oberland prior to joining Professor Thiele at Strassburg in September. Apparently this time with Thiele was not as rewarding as his time in the German capital, in spite of his work on the diazotization of dichlorostilbene. His later memories of this period were more of his journey through the mountains than his work in Thiele's laboratory. Of his experiences with Thiele the only one he told me nearly fifty years later was humorous but meaningful.

One of Thiele's students had gained a reputation of sorts among his peers as being capable of obtaining fantastically accurate analytical data and unbelievably complete material balances in all his experimental work. Thiele, while passing this student's work area one day, tipped the ash from an ever present cigar into one of the samples on the bench. The next day, after this paragon had reported the results for this particular sample, Thiele exclaimed, "My God! The cigar ash weighed nothing!"

The message of this story was not lost on Professor Schlesinger. Throughout his scientific lifetime he was care ful to have any results reported to him confirmed by one means or another before he would prepare them for publication. This was managed in such a graceful manner that no implication ever existed similar to that of the Thiele story.

In February of 1907 he returned to the United States and joined Professor Abel, one of the world's leading physiological chemists, at Johns Hopkins University. He had just begun to make progress in isolating the toxic principle from *Amanitas phalloides* when Professor Nef invited him to return as an associate in chemistry at the University of Chicago. His principal duty was to be the teaching of general chemistry. To make his research interests compatible with this task, he began research in a field that was novel for him. His breadth of training in all the other fields of chemistry prepared him for a lifetime career during which he made many outstanding contributions to his newly chosen field of inorganic chemistry.

In 1910, the year he was appointed instructor in chemistry, he married Edna Simpson, a member of the gifted family that included the distinguished paleontologist, George Gaylord Simpson. Throughout their long life together Mrs. Schlesinger's intelligence and gentle good humor eased many of her husband's burdens. My own acquaintance with her came as a consequence of the dinner parties she gave for the Schlesinger research group. We all called Professor Schlesinger "the Boss" outside his hearing. I was never to reach the exalted state of Anton Burg and Herb Brown in addressing him as "Hermann." Mrs. Schlesinger sensed that our perceptions of "the Boss" were sometimes unrealistically greater than life. Her stories of her experiences with "Hermann" would invariably display him in a loving, more human light.

It is possible for me now to recall only one of these. When their sons were quite young she had asked "the Boss" to take the children to the lakeshore at the 55th Street promontory for some time in the sun. Typically, he took the most recent journal to read while he overlooked their play. She was startled some hours later to see him returning, journal under arm, without the boys. His absorption in the chemistry he was reading had driven all thoughts of domestic responsibilities out of his mind! Dr. Schlesinger seemed always to enjoy the humor of these family legends as much as anyone. His contentment in life with his wife, "Teddy," was apparent to anyone seeing them together.

It was his lifetime habit to combine family and professional lives harmoniously. He lived for much of his life in an apartment building owned co-operatively with Professors Thorfin W. Hogness and Warren C. Johnson. He spent his summers with his family in a summer cottage compound, at Elk Lake near Traverse City, Michigan, shared with these and other university colleagues. Throughout his entire life he continued an active and important role in the family company, begun by his father. It was a fine woodworking factory specializing in architectural panelling and furnishings.

This melding of the personal with the professional had important scientific consequences in one case known to me. Both of his sons enjoyed very successful careers in advertising. Richard's career continued in Chicago, but Allen moved with his young family to Minneapolis. Dr. and Mrs. Schlesinger made many visits to the grandchildren there during which he would "touch base" at the University of Minnesota. During one of these academic visits he encountered a young assistant professor in chemistry who shared his interest in the structures of the boron hydrides. Over many years he was to carry carefully purified samples of all of the hydrides and related compounds prepared in his laboratory at Chicago for Professor Lipscomb's study by x-ray crystallography.

He was promoted to assistant professor in 1911, to associate professor in 1917, and to professor in 1922. Professor Nef had died in 1915 and Professor Stieglitz became chairman in that year. Upon becoming a full professor Schlesinger was appointed by Stieglitz as secretary of the Department of Chemistry. In 1933 Professor Stieglitz retired as chairman, but no new chairman was appointed and all of the administrative duties of the department were borne by the secretary. In 1946 Professor Warren Johnson was appointed chairman, relieving "the Boss" of the many duties he had discharged with such graceful equanimity throughout the period of great trials and triumphs prior to and during World War II.

My own university training was entirely at Chicago. I had no experience of other chemistry departments, but even I was perceptive enough to notice the unusual administrative structure in that chemistry department. The reluctance of the university administration, while Robert Maynard Hutchins was president, to appoint a chairman in chemistry must have had an interesting rationale. While I knew him, Professor Schlesinger never gave me any clue that would explain this unique situation.

Others have commented upon this matter so I feel free to add my speculation as to a possible reason for the patchwork administration in the chemistry department during these twelve years. Robert Maynard Hutchins arrived at the University of Chicago as president at the age of twenty-nine. He was full of himself and the glory of the logic in law. It is possible that he saw the huge costs of laboratory instruction, as have university presidents before and since, as an avoidable burden. He suggested and made as if to implement his plan to teach sciences without the necessity of costly laboratory instruction. Professor Schlesinger undertook the thankless task of educating this young administrator in the nature of science. In faculty meetings as well as in the published literature "the Boss" invested this battle. It is clear from history that Hutchins did not win this point. It is also clear that this disagreement did not prevent Professor Schlesinger from participating whole-heartedly in many science education projects dear to Hutchins's goals. His ground-breaking films for ERPI were among the best early audio-visual aids. These, along with Schlesinger's *General Chemistry* (1925), were used in the physical sciences survey courses at Chicago. I can only suggest that Hutchins was bruised in this exchange and was human enough to let his resentment express itself in this foolish way. Let me hasten to add that this is my own speculation and does not deserve the weight of fact.

A peripatetic academic career during the past thirty years has resulted in a perspective that allows me to assess Dr. Schlesinger's impact upon the Department of Chemistry at the University of Chicago with more confidence than I once could. As student and faculty member he lived in the department for most of the first sixty-five years of its existence. When I left Chicago to embark upon the first of several academic positions, I assumed that all departmental faculties were mutually supportive and that among such faculties the human conflicts and destructive competition I had encountered in the many non-academic jobs I held while financing my education would be ameliorated. Sadly, the Department of Chemistry at Chicago during Dr. Schlesinger's long career was unique in my experience. Since his death the department apparently has become more like all other departments. It is an inescapable conclusion, for me, that this temporal coincidence is a true measure of the idealizing influence that "the Boss" enjoyed during his time in this department.

The Schlesinger research group was active during the days of World War II with research that has had far-reaching consequences. As was common the work was always directed towards a war-related goal. One of the earliest efforts was prompted by the need to find volatile compounds of uranium suitable for use in the diffusion separation of the uranium isotopes. Uranium borohydride is, after uranium hexafluoride, the most volatile compound of uranium known. Uranium hexafluoride presented serious handling difficulties, which required the directors of this effort to seek alternatives. When the synthesis problems had been sufficiently well defined by the Schlesinger group, it was decided to see what kinds of other difficulties might arise in the use of a boron compound for uranium separation. Fermi's purported comment when he observed the slow neutron cross section for boron—"My God! It's as big as the side of a barn!"—not only gave a name to the unit of cross section, it put finish to thoughts of using uranium borohydride for the diffusion process.

Meanwhile, some officer in the U.S. Army Signal Corps had been having problems with farmers. It was necessary for field units of the corps to generate hydrogen gas in the field in order to loft their radio antennas to altitudes where they were useful. They had been using a mixture of ferrosilicon and sodium hydroxide for this purpose. This reaction produced a satisfactorily large volume of gas per unit weight but suffered from two serious problems. After the hydrogen had been generated the generator was filled with a rock-like mass of silicates that a GI had to chip out before the generator could be re-used. These product silicates had a salty taste much loved by farmer's cows. Unfortunately, the acid in the cow's stomachs converted the silicates to silica. The cows with their stomachs full of silica would slowly starve to death, presumably without ever being hungry! Someone in the signal corps had noted in the reports from the Schlesinger group the high yield of hydrogen in the hydrolysis of lithium borohydride. The Schlesinger group then began research under the auspices of the Signal Corps.

In the early days of the signal corp research program it was pointed out that this use of lithium borohydride would rapidly use up the world's known supply of lithium. Accordingly, the efforts of the Schlesinger group were directed towards the synthesis of its congeneric salt, sodium borohydride. This turned out to be a formidable task and it was in this effort that H. C. Brown made his greatest contributions to "the Boss's" research. Most of us will remember the monumental series of eleven papers co-authored by Brown and Schlesinger, along with many collaborators, that appeared after the war when they were free to publish.

What was never published was the great consternation at Chicago when the first carefully prepared sample of the new borohydride was "hydrolysed." It was to be an occasion of some importance, and the "brass" from the Signal Corps were invited. Everyone watched as the sample, which had been prepared and handled only in vacuum lines because of the remarkable reactivity of such hydrides, was dropped into water. This beautiful white crystalline solid merely dissolved! No hydrogen was evolved! The remainder of the Signal Corps project was devoted to a search for a catalyst for the hydrolysis of sodium borohydride. This search gave Brown the idea that later resulted in his hydrogenation chemistry, where a sodium borohydride solution in water is added to an olefin in which is suspended a platinum-charcoal catalyst.

Later experiences of the research group gave all of us the feeling that for defense purposes the boron hydrides would always be a bridesmaid but never a bride! There exists an overwhelming literature that documents the utility of these interesting compounds in all aspects of chemistry. We have all, I am sure, been able to overcome any disappointment the failure of borohydrides in the defense effort may have caused us.

Near the end of his life Professor Schlesinger, to my knowledge, was twice nominated for the Nobel Prize in Chemistry to honor his landmark work with boron hydrides. My opinion of his generous spirit makes me confident that, were he alive, he would have been made as happy by the awards to Lipscomb and Brown as if he himself had been accorded the honor.

Professor Schlesinger did not suffer from the pride of ambition but he was human enough on one occasion to display pleased vanity. During my graduate student days, my wife was reading Saul Bellow's most recent novel, *The Adventures of Augie Marsh*. She came across a passage where the protagonist was instructing a novice in the fine points of shoplifting books from the University of Chicago Bookstore. In his instructions the expert pointed out that you should only steal good books that were easily sold such as Fuller's *Botany* and Schlesinger's *General Chemistry*. "The Boss" was delighted with this when I pointed it out to him the next day in laboratory.

There were also times in his research when fortune did not smile upon him. Professor Nan V. Thornton, who enjoyed a long and distinguished academic career at Randolph-Macon College in Lynchburg, Virginia, was a graduate student in his laboratory during the late 1920s. Her thesis research involved the study of arc-induced reactions of various chlorofluoromethanes. Using the vacuum techniques that Dr. Schlesinger did so much to develop, she had characterized and measured the yields for all of the volatile products arising from this complex reaction. She was left with a pea-sized lump of a waxy white solid. It contained enough material to prevent her from obtaining a material balance that was satisfactory to her. All of her efforts to dissolve this solid in order to confirm the composition, which she could estimate from the material balance data, were in vain. It did not dissolve in fuming nitric or sulfuric acids even near their boiling points. At the end of her tether she went to "the Boss" with her problem. He pointed out that she had completed enough work for a thesis and should devote that thesis only to the volatile products. This was a kind decision since it accelerated Professor Thornton's doctorate considerably. If Dr. Schlesinger had been less kind they might have been credited with the discovery of Teflon some fifteen years earlier than the DuPont work!

The most important lesson he taught me was two years after my leaving Chicago. It was at the national spring meeting of the American Chemical Society in 1958, and with the exception of the meeting at which he was awarded the Priestley Medal, was the last meeting he was to attend before his death in October of 1960.

With Anton Burg he had accomplished an elegant experiment in the early 1930s. In that experiment they treated a solution of diborane in liquid ammonia with metallic sodium and obtained only one-half mole of molecular hydrogen per mole of diborane. It was perfectly reasonable for them to conclude that this proved an ammonium salt of the novel "diboranoamide" anion as the best formulation of the "diammoniate of diborane." Later, in the same laboratory, Schlesinger and Burg, working with R. T. Sanderson, another student of Schlesinger's, discovered aluminum borohydride. The next twenty years were spent synthesizing and characterizing many examples of this new and useful class of double hydrides. During the same twenty years Anton Burg had become a widely respected professor of inorganic chemistry at the University of Southern California.

Early in the 1950s, the then-young R. W. Parry, an assistant professor of inorganic chemistry at the University of Michigan, began a series of experiments giving results that suggested that the earlier formulation for the "diammoniate of diborane" could be improved. His experiments suggested a novel cation, the "diammonoboronium" ion with borohydride as the counter ion. For the next seven years the literature and the national meetings served as a forum where every piece of Parry's experimental evidence was contested by Burg.

Anton was justifiably proud of his formidable experimental talents and it was inconceivable to him that Parry could be correct. Burg had become so involved in this controversy that a large portion of his energies, intellectual and experimental, were devoted to supporting his position. It also occupied much of Parry's attention.

Professor Schlesinger ended this wasteful polemic in a fashion that demonstrated his stature. At San Francisco, in September of 1958, Anton was on the podium criticizing Parry's just-presented paper of further experimental proof. In the midst of Anton's detailed (not to say nit-picking) queries, Schlesinger stood and proceeded, with some physical difficulty, to make his way from his seat in the front row of the lecture hall to the podium. Parry and Burg, with the entire audience, watched silently until this distinguished, fragile old man reached the microphone.

His comment, brief, to the point, and made in a surprisingly youthful voice, was: "Anton, it really is not so bad for the two of us to have been wrong nearly thirty years ago. Let us join in congratulating Professor Parry for an excellent piece of work." Saying no more, he returned to his seat and the battle was over.

At a time in this country when reputations often were based on the number of a chemist's publications, where contemporary titans such as Henry Gilman and I. M. Kolthoff were in serious competition to be first to pass the magic number of one thousand publications, Professor Schlesinger's lifetime list is exceedingly short. The bibliography terminating this memoir includes all of Professor Schlesinger's publications. If, in his opinion, work was not sufficiently important or the results not susceptible to logical understanding, no manuscript was submitted for publication.

Usually, his decision not to publish an account of a given research effort was based upon the state of current knowledge. In the late 1930s one of his students (if memory serves it was Leo Horvitz) studied gas phase reactions of boron trimethyl with carbon monoxide. The results were difficult to represent by balanced equations. There was no suspicion that the experimental results were poorly observed or reported. It obviously was a complex system of reactions that resisted rationalization. After hydroboration was discovered and elegantly exploited by H. C. Brown, it became logical to study the boron trimethyl-carbon monoxide reaction in a solvent. Brown's boralkylation reaction with its powerful synthesis applications resulted from further study of the same reaction in an improved climate of knowledge.

Such examples of imperfect knowledge resided in "the Boss's" memory as poorly-digested meals. He often would describe his intellectual discomfort to later students, arousing serious interest on some occasions and extending knowledge as published work.

His insistence upon a lucid, if terse, style of exposition was impressed upon all of his students, usually during seemingly interminable sessions devoted to the rewriting of reports and theses. Each sentence was held up, not for the pleasure of criticism, but for recasting in a form requiring fewer, or more accurate, words. Many of his students' doctoral theses were so shortened that binding for deposit in the university library necessitated padding with blank fillers.

At the time of my own thesis preparation this process was a source of some growing pains. The end result was a manuscript consisting of twenty-three pages. It was a humbling experience, not intended by "the Boss," that four years of grinding effort produced so little of real substance. The memory of this experience has since evolved into a source of personal pleasure, partially as a result of the fact that the thesis was submitted, as written, and published essentially without change. This set a standard that I have found impossible to maintain throughout my own career.

Professor Schlesinger's standards of publication were made clear to me at the time I was serving as his research associate. At Chicago this position was informally known as a "Ph.D. pusher." He continued my education during this period to prepare me for my chosen career. He gradually increased my role in the day-to-day aspects of graduate education and research management.

An application for a postdoctoral research opportunity arrived from a student being trained at a prestigious university. It was an imposing application throughout, but the most impressive feature was a listing of some twenty publications that this young man had to his credit. After listening to my enthusiastic assessment of the candidate, Professor Schlesinger's terse comment, "It is unlikely that a scientist can produce twenty *good* publications while completing a thesis," made no mystery of his own judgment.

His judgment of the quality of a scientist's work always had more serious importance to Professor Schlesinger than the volume of production. Two of his junior colleagues at Chicago, during the period of his greatest influence, were promoted through the ranks of associate professor and professor without benefit of any formal publications. His wisdom in both of these instances was manifest by the illustrious careers enjoyed by both of these men. Such action is unheard of in the present academic world where university administrators apparently can count but cannot read.

Professor Schlesinger spent his summers with his family at Elk Lake throughout the period of my doctoral and postdoctoral education. Through every such summer neatly written discussions of the work at the lab, raising cogent questions and suggesting solutions to current problems, arrived periodically, postmarked Rapid City, Michigan. It is still possible to evoke powerful images of "the Boss" by rereading these.

During the last summer I spent in his laboratory his missives became more chatty and I was startled to learn that he was planting 25,000 northern pine seedlings on his summer property. A less likely activity for him could not have occurred to me at that time. Now, it is easy to conjure a salubrious image of the mature forest that has resulted from this mundane activity.

It also gives me great personal pleasure to contemplate the mature forest of ideas and practical applications that have grown from the seedlings he planted in the loam of his career. His great joy in producing something of lasting value was intrinsic to both of these widely disparate activities.

It is difficult to overstate the impact of his discoveries upon the course of modern chemistry. A large majority of published work involving organic synthesis, whether it be natural product synthesis, medicinal chemistry, bio-organic chemistry, or molecular biology, cites the use of reagents that have come from his discoveries. Borohydrides, aluminohydrides, and derivatives of these are so commonplace as reagents that querying the present users would reveal few who can name their discoverer. As previously mentioned, at least two Nobel prizes were awarded for subsequent work closely related to Professor Schlesinger's. It would be even more informative to count the Nobel awards where the honored work was enabled by the use of these remarkable reagents.

Professor Schlesinger's beloved wife, Teddy, died of a heart attack in 1957. One evening, in their apartment at 58th and Blackstone Avenue, she felt the onset of the attack. Unable to find her medicine, "the Boss" hurried to Sarnat's Drugstore a block away at Blackstone and 57th Street. She had died before he returned. Recounting this deeply personal experience to his many colleagues must have helped him come to terms with his loss.

The few years after Mrs. Schesinger's death were spent with little diminution in "the Boss's" activities. The day before he died, he worked at the laboratory, conducted a meeting of the cooperative apartment members, and indulged a lifetime interest by watching professional football on television. His death the next day, October 3rd, from pneumonia, was possibly a consequence of the absence of his wife's loving care.

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CALVIN PERRY STONE

February 28, 1892-December 28, 1954

BY ERNEST R. HILGARD

CALVIN PERRY STONE served as a professor of psychology at Stanford University for most of his adult career and achieved distinction as a comparative and physiological psychologist. His scientific contributions were recognized by his election to the presidency of the American Psychological Association in 1941 and to the National Academy of Sciences in 1943.

Calvin was born on a farm near Portland in Jay County, Indiana. He was the youngest son and the seventh of eight children, born to Ezekial and Emily Brinkerhoff Stone. His paternal great-grandparents were North Carolinians who moved to Ohio in 1830 and thence to Indiana two years later, where they occupied the first tract of land to be settled in Jay County. From his mother came Dutch blood. The Brinkerhoffs had first settled in New York, and, like the Stones, had gradually moved west, passing through Pennsylvania and Ohio to reach Indiana in the late nineteenth century.

The farming region in which Calvin grew up was sprinkled with relatives who, from time to time, took an active part in the affairs of his family. Significantly, the occasional teacher, minister, doctor, lawyer, or justice of the peace in the ancestral and immediate family presented hints of potentiality and offered models for emulation to each succeeding generation.

Calvin grew up in an atmosphere of cultural change. His birth coincided almost exactly with that which historians customarily use to mark the end of the old and the beginning of a new American frontier. His lifespan corresponds to that era of American development when society became more urban and uniform than regional and rural, but in which the pioneer patterns of family organization were still embodied in a living generation.

Two special situations provide a framework wherein developed the active, agreeable, and intelligent youngster that Calvin is remembered to have been. One of these is the sharp pressure of material insecurity which, though not uncommon among farm families of the region, took a particularly dramatic, and possibly particularly stimulating, form. When Calvin was five years old his father died of pneumonia. On a bitter winter afternoon, while all of the family but Calvin were at the funeral, the house caught fire and was utterly destroyed. Although neighbors offered shelter to the horror-stricken family, Calvin's mother insisted on an attempt to restore the home. Friends and relatives rallied to build a new house and provide temporary aid during the immediate period of the catastrophe. From then on, she and the children struggled successfully to defend their independence against adversity. Farming, managing home industries, sending its members to school and to work on neighboring farms, the family built up a way of life that demonstrated convincingly the unlimited possibilities inherent in unremitting effort, competence, and courage.

The second significant aspect of the early years was the steady thrust of Calvin and his brothers and sisters toward higher learning. The source of this tendency appears to be broad, residing rather in the family and social environment than in special pressures from Calvin's mother. The Bible constituted the whole of the family library, and her routine was too arduous to permit recreational reading. However, she seems to have provided that practical aid to her children's ambitions that permitted them to combine their duties to the family with their educational advancement. To this was added the efforts of the children themselves.

How strong was this motivation may be seen not only in its overt expression but in its results. The family disaster prevented the eldest three children from finishing their secondary education, although one of them later completed a course of nursing training. Among the remaining five, one obtained the degree of Bachelor of Arts, two the Master of Arts, and two, including Calvin, the Ph.D. The momentum demonstrated here, it may be added, has carried into the following generation. In all but two families (including one childless sister) there is at least one member who has taken a degree beyond the Bachelor of Arts; nearly each family has members who have taught or are teaching.

Calvin began school at the age of six, proceeded through the elementary school, skipped high school, and qualified for entrance into Valparaiso University in 1907, when he was fifteen years old. Combining summer work with his course of study, in order to pay his way, he obtained the degree of Bachelor of Science in 1910. This degree entitled him to teach in the secondary schools, and he spent the next three years as teacher, principal, and superintendent at Deer Creek High School. Summer school work led to the degree of Bachelor of Arts in Classics at Valparaiso in 1913. The following year he taught at the high school in Stilwell, Indiana, and then entered Indiana University. By the time he left Valparaiso his interests in advanced study were divided between medicine and the social sciences. Letters indicate that he was reading Veblen and Spencer, the latter with great approval. Work with Ernest H. Lindley and Melvin E. Haggerty in psychology and philosophy at Indiana University apparently determined the direction his career finally took. In 1916 Haggerty moved to the University of Minnesota and persuaded Calvin to join him there as a doctoral candidate. In view of the extended program of study lying ahead, Calvin sought means to enlarge his financial resources. An opportunity offered itself in the form of a position as director of research in psychology at the Indiana State Reformatory in Jeffersonville. This position he accepted in September 1916 and held it until the following summer, when he entered the U.S. Army. In the same year, on June 30, he married Minnie Ruth Kemper of Brook, Indiana, whom he had met at Valparaiso four years before.

As a student Calvin consciously sought the top of the class, and he apparently was particularly perceptive of ideas and possibilities that lay beyond the routine curriculum of the university program. Thus, he moved rapidly through the areas of teacher training, liberal arts, pre-medicine, and finally, psychology, not because of indecision but by reason of mastery and comprehension of situations and opportunities. In the usual competitive sports he showed no special talents, but as a gymnast he became so proficient as to work with professionals in his college years. His graduating class elected him as its vice-president, a member of the yearbook's board of editors, and class poet. The latter distinction—bestowed, according to the yearbook, because of "the unique unexpected combination of words which he always uses"—is memorialized in nine stanzas of heroic couplets. The final lines anticipate in a surprising way a characteristic of his later career, the capacity to be thorough:

> Oh yes, believe me, you must draw your pen,
> Not once or twice, but o'er and o'er again

Through what you've written, if you would entice
The man who read you once, to read you twice.

As a teacher in these early years, Calvin is recalled to have been effective in instruction, capable of generating enthusiasm in his classes, and gifted with the ability to import conceptions of a moral character. Of the latter, persistence, rational employment of one's capacities to reach increasingly high levels of attainment, and self-reliance were most notably recalled by his students. He appeared, to those who worked with him, to display a balance of interests and activities that gave due emphasis both to physical and intellectual effort. Teaching and learning were vocations of the highest seriousness to him. In his first year at Deer Creek at the age of eighteen, he prepared his first research paper, intending it for presentation at the County Institute. Entitled, "What a Pupil Should Know Before Being Permitted to Enter High School," it cost him "quite a large share of my spare time during the past two weeks. Much of my data, which illustrates my discussion, I obtained from special tests given Freshmen and Eighth Grade classmen. This of course required quite a lot of time. Statistic computations are not obtained without much expenditure of energy." It was with the same straightforward approach to learn that he participated actively in various county and school training programs while at Stillwel, and, in 1915, abandoned a cherished plan of going East for graduate study, in order to remain in contact with those teachers in whom he already had confidence and who were prepared to work enthusiastically with him to advance his education. By the time his education was interrupted by World War I, he had progressed to a level of professional achievement in a chosen—and, be it remembered, relatively new—field of psychology and had developed those traits of character that, then and thereafter, made success seem to him less the result of fortune and talent than of the employment of these gifts through diligence, serious and clear intent, and self-discipline.

In August 1917, Calvin entered Officers' Candidate School at Ft. Benjamin Harrison, Indianapolis. His qualifications in psychology were immediately noted, and he was officially discharged on September 15 in order to receive a new appointment as a psychological examiner. He was first sent to Camp Taylor, Louisville, Kentucky, where his name was called to the attention of Robert M. Yerkes, then in charge of psychological services for the Army. He was commissioned as first lieutenant, Sanitary Corps, and early in 1918 he went to Camp Greenleaf, Chattanooga, for training in testing. While there he became adjutant and assistant director of training. In the period from 1918 until he was discharged on September 21, 1919, he served at Camp Pike, Arkansas; Fort Sheridan, Illinois; Camp Custer, Michigan; General Hospital 38, at Eastview, New York; and Walter Reed General Hospital. He attained the rank of captain.

During the summer of 1919, Dr. Karl Lashley invited Calvin to come to the University of Minnesota as a teaching fellow and graduate student. In the autumn, he began a heavy program of advanced work in medicine, with an emphasis on anatomy and neurology, and psychology. The two years of preparation were hectic, as any graduate student knows. But he found time to attend university concerts and to provide assistance to his wife and growing family (James, born 1918, and Robert Kemper, born 1920). He received his Ph.D. degree in June 1921, and afterward was appointed an instructor at the university. During the academic year 1921-22, he taught introductory psychology and assisted in a laboratory course in neurology. Before the end of the year, Calvin received an offer of an appointment from Dr. Lewis

M. Terman, chairman of psychology at Stanford. Dr. Lashley's letter of recommendation may be quoted—in part—as an indication of the character, training, and professional interests that were then apparent:

> I can answer your inquiry concerning Dr. C. P. Stone most adequately by saying that if there were any chance for advancement for him here, we should make every effort to keep him. We have all been very favorably impressed with his work here and he bears an equally good reputation in the department of Anatomy where he took his work for a minor and where he is now doing part time teaching. In research he has shown a good bit of originality, and very great perseverance. His thesis subject included a descriptive study of the development of sexual behavior in the male rat and a correlation of this with anatomical and physiological development. He suggested the problem and planned most of it himself, showing a rather unusual independence. This year he has been continuing the same kind of work, is full of suggestions of new problems, and gives every indication that he will continue to be active in research. His interest is chiefly in the nervous and glandular mechanisms.
>
> He would like to teach either physiological psychology, animal behavior, or functional neurology and I believe that he is well qualified to teach any of these subjects. I understand that in his work in the army he showed good executive ability.
>
> Personally Stone is rather quiet and unassuming, slow in speech, and outwardly unemotional. For a time he showed a little tendency to overconfidence, but that has nearly cleared up and he is well-liked by students and teachers. I have always found him absolutely reliable and conscientious in detail. The responsibility of a wife and two children add to his stability.
>
> I believe that you would find Stone entirely satisfactory for any work in the biological side of psychology. We have decided that in case of my leaving, he should be appointed to succeed me.

Stone began teaching at Stanford in the autumn of 1922. Except for the several instances to be mentioned, he declined offers to work elsewhere, and on occasions when administrative positions were made available to him, he reaffirmed his devotion to teaching and research. Intermit tently, he took summer positions at other institutions. In 1928 he spent his sabbatical year at the Institute for Juvenile Research, Chicago, Illinois, where he conducted the research that was published in 1932 under the title, "Wildness and Savageness in Rats of Different Strains." In 1932 he made a trip to Europe in connection with the International Congress of Psychology held in Oxford. In 1945 he spent a year at the Psychiatric Institute, in New York, as associate research psychologist. Here he carried out studies on the effects of electroconvulsive shock.

Upon arrival at Stanford, Stone immediately entered upon an active research program. When he attacked a problem he was very thorough about it, and he and his student collaborators would publish related experiments over the years. For example, he early began studies of the sexual and maternal behavior in the albino rat. Starting with an early paper (1922), he and his associates published through 1942 a total of thirty-four reports on their research. The years of World War II interrupted the research program, and after the students began to return from war service he initiated another program concerned with the effects of electroconvulsive shock (ECS), beginning in 1946. A series of fourteen papers came out during the remainder of his career, including one that appeared posthumously (1956, 1).

While the studies of sexual and maternal behavior were going on there were a number of varied studies dealing with animal behavior in the typical kinds of experimentation current in the late 1920s and 1930s, dealing with learning, maturation, and incentive motivation, chiefly using the maze and special apparatus for studying discrimination. Such studies began in 1928 and were reported intermittently through 1941, when the war interrupted the laboratory studies.

In the meantime he had interspersed studies of other topics. One set departed from the animal experimentation and was devoted to physical and mental development in the human during puberty and early adolescence. The first of these reviewed reported cases of puberty praecox to determine the relation, if any, to mental development (1927, 2). The same topic was reviewed again some years later (1936, 5). During the years 1934 to 1939, Stone and Roger Barker published six papers on physical development, menarcheal age, and related psychological changes by studying large samples of college women.

World War II had its effect on universities, and it was a skeletal staff that remained to teach the few students enrolled in the classes to be taught, as most of the male students and many of the female students left to work in war-connected activities. Stone continued his teaching but his research suffered. He had been elected president of the American Psychological Association to serve for the year 1941-42. Transportation difficulties resulted in cancellation of the national meeting scheduled for New York in the fall of 1942, so that he had no opportunity to present his presidential address, although he was able to publish it in the usual manner (1943, 1). His scientific status was recognized by his election to the National Academy of Sciences in 1943.

Although continuing his duties at Stanford, Stone also played an important role at the national level by serving as one of a seven-member Subcommittee on Survey and Planning of the Emergency Committee at the invitation of and under the chairmanship of Robert M. Yerkes, with whom he had worked in World War I. The subcommittee had responsibility for looking toward the normalization of psychology after the war. As its most significant contribution, it proposed a unification of psychology at the national level, in view of rifts growing among the several psychological societies. The subcommittee also took the initiative in proposing an Intersociety Constitutional Convention, ultimately meeting in New York in May 1943, under the chairmanship of E. G. Boring. The convention decided to retain the American Psychological Association as the overall organization, but proposed a new set of bylaws for the APA, including a divisional structure to give a degree of autonomy to the various specialized topical or professional interests of the constituencies. Its recommendations were soon adopted.

As the war came to an end and the students returned to graduate study in the universities, Stone's research again flourished.

Despite returning to the laboratory he found time for other professional responsibilities, such as editing the *Journal of Comparative and Physiological Psychology*, in 1947-50. He also took on the editorship of the *Annual Review of Psychology*, and remained its editor for the first six volumes, 1950-55.

In the laboratory he studied the effects of ECS, a technique that he had observed in use with psychiatric patients, during his sabbatical year at the New York Psychiatric Institute in 1945. The first of his experimental studies, using white rats as subjects, appeared in 1946 (1946, 1), and altogether twelve studies were published, the last posthumously (1956, 1).

Despite a severe heart attack in December of 1948, Stone soon resumed teaching. He edited a book, *Comparative Psychology* (1951), to which he also contributed. His ECS research continued, and he presently undertook his final set of experiments on the effects of hypophysectomy, remaining deeply involved until his death by heart attack just after Christmas, 1954. Several papers were still in press.

As a teacher, Stone made his courses memorable for their richness of content and the thoroughness with which he treated the material. Most of his courses were of a specialized nature, but that on abnormal psychology was well attended as an elective course by students outside the field of psychology. His effectiveness as a lecturer arose from clarity in language and in organization. By constantly reviewing and revising his lectures and syllabi (as his files attest), he brought fresh material to his classes. It is said that his was the first course on Freudian psychology to be given in an American university; it is true that his last presentation of the course in abnormal psychology, in the quarter before his death, was as advanced, in terms of present knowledge, as the first one had been in 1922.

The standards he set for his students were invariably high. Intent observations of his students' academic progress were matched by constant interest in their personal welfare. His last graduate student has spoken of him as a second father as well as a distinguished teacher.

In his professional attitudes, Dr. Stone was fully committed to the ideals of scientific scholarship that he had received from his first teachers. He began his research before he reached Stanford; his experimental program was interrupted only for a brief period in 1932, when he was in Europe, and for a few months in 1949, while in ill health. In instances when he was urged to take time enough to write books, he expressed his conviction that a continuous series of monographic reports would better contribute to current knowledge in psychology than would extended treatises that could only present what was already known. It was his practice to carry forward his research with his hands as well as with his mind. His family and students recall that his schedule brought him to his animal laboratory at early hours each day. In the early period, he himself fed, watered, cleaned, and housed the colonies of rats, guinea pigs, canaries, pigeons, rabbits, chicks, cats, dogs, and mon-keys that, at one or more times, he used experimentally. Much of the apparatus used in his experiments he designed or built himself. Experiments that students carried on under his direction were watched at every stage. Publications that bore his name as co-author with his students were fully guaranteed in their authenticity.

Calvin's writing was never fluent, and he exerted much effort to develop the capacity to express his ideas and to report his data with directness, economy, and clarity. He gave the same attention to manuscripts that he received; those that graduate students prepared were more carefully reviewed from the literary as well as from the scientific standpoint. His reading was largely in his professional field; in belles lettres, he was fond of poetry, and all his life he could quote passages from nineteenth-century American poets.

His place in the university community was unostentatious but highly respected. Relationships with colleagues were always cordial and generous. He fitted well into all professional groups and met students easily and with an understanding of their interests.

Early in Stone's career, the wife of his department chairman once remarked that, unlike many, he never sacrificed his career to domestic affairs, nor family activities to professional advancement. He struck a balance in all that he undertook. Until late in life, he was regularly on the tennis court and always was active in the garden and in the routines of domestic economy. The education and recreation of his children were matters that always engaged his attention, and although he himself was proficient in none of the arts, he encouraged their practice in the family. Time was set aside on week-ends and during the summer for family activities; in the shorter periods, for sports and outings, and in the longer periods, for extended trips to points of interest in the West and across the United States. He visited his mother, brothers, and sisters on numerous occasions in the Middle West, and he wrote voluminously to colleagues, friends, and relatives. Stone often served as family counselor to friends and neighbors. Family relationships were characterized throughout by a serious respect for individuality and an affection that expressed itself by positive action as well as by words. A concern with the development, in his children, of a high sense of personal responsibility was accompanied by a sensitivity to their limitations as well as to their capacities.

In December 1948, while he was conducting a class on a field trip to an asylum, Calvin suffered a heart attack. Although the injury to the heart was extremely serious, he recovered sufficiently to resume teaching in March and took up laboratory activities again in the autumn. For the next six years he carried on a full teaching program and organized other professional activities in such a way that he continued with the most important of them. He was, in fact, never free from discomfort, but his death on December 28, 1954, came suddenly during the early evening after a particularly pleasant Christmas season. In the days immediately before his death, he had been reading the letters of Freud, and his comments on them formed the central topic of several holiday letters to old friends. Arising from his observations on Freud's career were certain significant implications about his own: Freud, he thought, characteristically failed to work long enough in any one area to "sift the seed from chaff and to discard [most? many?] of his 'hunches.'" That he considered that Freud "suffered" from this quality suggests his own admiration for steady and cautious pursuit of experimental knowledge.

The effects of this lifelong conception are epitomized in the comment of a colleague: It was not only that his loss would be irreplaceable in the special field of research that he had developed; more than that, "he knew so much."

As a scientist, Stone reveals himself in his comment on his own research, which, one correspondent noted, had turned in a surprising direction late in life. He wrote on the eve of his death:

"During the past two years fortune has smiled again in pointing out a relatively virgin field of animal research suited to my present limitations in time for laboratory studies. . . . I now intend to put my nose to the grindstone with hypophyseal research for a few years; at least, until my curiosity is satisfied."

I WISH TO ACKNOWLEDGE the great assistance provided to me in the preparation of his father's biography by James H. Stone, Ph.D., Emeritus Professor of Humanities at San Francisco State University.

The material he furnished me included a complete collection of the reprints of his father's scientific writings, case-bound in four large volumes, and a biographical sketch that he, Dr. James H. Stone, prepared, derived from family documents, including letters and personal reminiscences.

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RALPH H. WETMORE

April 27, 1892-April 28, 1989

BY JOHN G. TORREY

IN THE PERIOD immediately after World War II a renewed effort beganin the botanical world on the nature and causes of plant morphogenesis. In the Department of Biology at Harvard University were assembled faculty members especially qualified to pursue collaboratively the problems of understanding plant development, using structural-analytical and biochemical-biophysical tools in new and revealing ways. On the physiological-biochemical side was K. V. Thimann with his interests and insights into plant hormones; on the structural-biophysical side was I. W. Bailey with his wide experience in structural-functional aspects of secondary growth in plants. In the middle and providing both the bridge and the cement was R. H. Wetmore with his understanding of the anatomical-cytological basis for meristematic activity.

Ralph H. Wetmore was born in Yarmouth, Nova Scotia, on April 27, 1892, of loyalist forebears who had moved from New England to Canada at the time of the American Revolution. His father was foreman of a wood-working plant in this maritime community, his mother an enthusiastic gardener and homemaker for the four children of which Ralph was the eldest.

After finishing the public school preparatory course for college in Yarmouth, including the twelfth-year alternative to first-year college, Ralph at age sixteen began his teaching career at Pleasant Valley, near Yarmouth, in a one-room country school with twenty-six students of all grades, one through eleven. At age eighteen he took a year at Normal School to obtain his teacher's license, then taught in a public school in Milton, Nova Scotia, for four years.

During the war years 1914-18, Ralph was in and out of the army with physical disabilities. He enlisted on at least two different occasions but was rejected for overseas service with varicose veins on his legs and color-blindness. Most of the war he served in the divisional headquarters of the Canadian Army in Halifax, achieving the grade of sergeant.

Immediately after the armistice he entered Acadia University in Wolfville, Nova Scotia, late in the academic year of 1918, together with many other veterans anxious to pick up their life-work. He completed the undergraduate course in two and one-half years, receiving his B.Sc. with honors in biology in 1921. During this time his interests in botanical science were stimulated by the excitement created in the one-man botany department of Professor H. G. Perry, graduate of Harvard University. With Perry's encouragement Ralph applied for graduate study and entered Harvard in September 1921, working with Professor E. C. Jeffrey with whom Perry had taken his degree. In 1923 he married Marion G. Silver of Dayspring, Nova Scotia. He completed his Ph.D. in 1924, working in the field of evolutionary plant morphology and anatomy. His doctoral thesis was on the anatomy of dicotyledonous woody stems and the aerenchymatous system related to lenticels, published in 1926. During his graduate study he spent a summer in Labrador as botanist with a Canadian government expedition characterizing the flora around Lake Melville. After taking his doctorate, Ralph was awarded a National Research

Fellowship in the biological sciences at Harvard studying the cytogenetics and taxonomy of the genera *Aster* and *Solidago*, based largely on breeding experiments. In 1925 he returned to Acadia University to an appointment as assistant professor in biology, with the expectation that he might settle there and succeed Professor Perry who was nearing retirement.

In 1926 he received the offer of an assistant professor position in botany at Harvard by then-chairman, Professor Oakes Ames. Wetmore was persuaded by Professor Perry and the president of Acadia, Dr. Patterson, that his going to Harvard was not only an important and wise career choice but also that it would continue access of Acadia graduates to graduate study at Harvard. With some reluctance Ralph accepted the position. In 1926 he and his young family moved back to Cambridge, where he renewed what became a life-long association with the botanical sciences at Harvard University.

Immediately, Wetmore was drawn into full-time activity in teaching and departmental administration at which he was clearly adept. He taught the elementary course in botany with an enrollment of over a hundred students and was responsible for the laboratory of the botanical half of a large general course in biology offered for non-science concentrators. He was involved heavily in the planning of the Biological Laboratories building, at 16 Divinity Avenue, which was completed in 1930. He became chairman of botany in the new building and thereafter director of the Botanical Laboratories, serving until 1934 when the three separate departments of botany, zoology, and physiology were united into a single Department of Biology with one chairman and one director of the Biological Laboratories. Such a load diverted Wetmore's energies from research, except for summer studies in cytogenetics and cytotaxonomy, part of which was conducted at his summer cottage in Nova Scotia. Some aspects of the work were taken up by his graduate student Delisle and the results of the study were published in 1939.

Upon relief from administrative chores, Ralph turned increasingly in his research to plant anatomy, the major topic of his doctoral research. His interests centered on the evidence that plant structure, especially wood anatomy, could bring to taxonomic relationships and evolutionary origins. In this line of work, Wetmore joined in collaboration with Professor I. W. Bailey of the Arnold Arboretum. Together, they assembled the wood collections necessary for the research.

Authenticated wood samples collected from around the world were placed in the Harvard University Herbaria; sections were prepared on slides for microscopic study. Further collaboration with Professor S. C. Record of Yale University led to large collections of wood samples at both universities. The collection at Harvard grew to over 25,000 specimens of wood covering 300 families of gymnosperms and angiosperms and over 35,000 microscope slides of wood sections, in addition to permanent mounts of pollen samples and flower parts, which together form the Bailey-Wetmore Wood Collection housed today in the Harvard University Herbaria building at 22 Divinity Avenue.

Research based on these collections led to new insights into both in the relationship between plant anatomy and taxonomy and in ideas as to evolutionary origins of the angiosperms.

Influenced by the activities and success in understanding systematic relationships among the woody dicots achieved by I. W. Bailey and his students, Ralph undertook a comparative study of the developmental anatomy of the herbaceous angiosperms. He soon found that too little was known concerning the development of the primary body of herbaceous plants to permit extensive or comprehensive comparative studies. He therefore began a series of studies in collaboration with his graduate students of the developmental processes in early embryogenesis of several plants including the woody gymnosperm *Pinus strobus*, the herbaceous flowering plant *Phlox drummondii*, and several species of the ferns and lower vascular plants, including the bracken fern, *Pteridium aquilinum*, and the fern *Phlebodium aureum* as well as studies in the lycopsids, *Lycopodium* and *Selaginella*. This research was extended to the anatomical analysis of continuing embryogenesis in the vegetative shoot apices and flowering apices of a number of the vascular plants, studies that paralleled comprehensive efforts of other plant anatomists active around the world, including K. Esau at Davis, California; A. S. Foster in Berkeley; R. A. Popham at Columbus, Ohio; E. C. Abbe at the University of Minnesota; and, in Europe, R. Buvat and L. Plantefol in France, F. A. L. Clowes in England, and others involved in similar research in the 1930s-1950s.

Careful and detailed anatomical studies of the shoot apex and of the origin of the cauline or stem-component of the shoot versus the foliar appendages—the leaves of the vegetative shoot apex or the floral parts produced by flowering apices—led naturally to speculation concerning the inherent independence of the meristem derivatives. Surgical operations on the shoot apex provided a novel, if drastic, approach to the problem. Wetmore and his students joined this approach, one pioneered by C. W. Wardlaw in Manchester, England, over the years of World War II and available in published form in the early 1940s. Over the period beginning in 1945 Wetmore's laboratory became one of the centers in the United States for research in the newly evolving field of experimental plant morphogenesis.

Using developments from tissue culture research pioneered in France by R.J. Gautheret and evidence for the autonomy of cultured shoot and root apical meristems from work by P. R. White and others in the United States, Wetmore's group began to excise meristems and meristem parts and culture them in isolation in sterile nutrient culture. Close morphological and anatomical examinations of structures resulting from surgical manipulations led to new concepts of tissue and organ interaction. Attention focused on the role of the still incompletely defined plant hormones, especially auxin. In collaboration with Professor K. V. Thimann, a series of studies was made involving the role of auxins in leaf and shoot development of diverse taxonomic groups of plants.

Another related line of research was concerned with the physiological/biochemical control of vascular tissue differentiation in the shoot apex and its subtending tissues. Students in Wetmore's laboratory combined techniques of meristem culture and callus tissue culture deducing evidence for the chemical influences, especially hormonal, of the shoot on undifferentiated tissues. Further studies involved the development of isolated leaf primordia and the discovery of their early pluripotent capacity to form either buds or leaves and their later determination as leaves.

Wetmore served as an effective bridge between plant anatomy and plant physiology and was able, together with other faculty and with graduate students and postdoctoral fellows, to create a research atmosphere in which students were encouraged to reach across the gap and bridge it themselves.

Twelve years after his first marriage, Ralph's wife Marion died suddenly in 1935 from a hemolytic streptococcus infection. Ralph was left with two daughters, Katherine and Jean, then eight and five years of age. Ralph characteristically undertook the full responsibility for their upbringing.

In 1940 he married Olive (Hawkins) Smith, a tutor in English at Radcliffe College and together they raised the two girls and saw them each married and with children of their own, six grandchildren in all for the Wetmores. Olive continued in academic life at Radcliffe, serving in the department of English as tutor and later as acting dean of the college. Together Ralph and Olive created a home-away-from-home for the many students both undergraduate and graduate who had the good fortune to know them during their student lives in Cambridge. It was a long and happy marriage of mutual dedication to each other and to the academic life they both chose.

Ralph retired from teaching at Harvard and became professor emeritus in 1962 but continued active in research. He made the trips and visits to other research laboratories that he had foregone during his academic career. He traveled to Versailles in France to work with G. Morel and then for a period to Dartmouth in Hanover, New Hampshire, to conduct research with his former student A. DeMaggio. He also spent some time at the University of California at Davis, in the laboratory of E. Gifford. When back in Cambridge after retirement, Ralph devoted much of his time to the sorting and arranging of the large wood collection, spending half-days for a number of years, working on the wood collection, meeting colleagues and students, and keeping up with the world of plant development.

After giving up their home on Francis Avenue, just a block from the Biological Laboratories, Ralph and Olive moved to an apartment on Garden Street where they continued to host friends and students and colleagues. In 1982 Olive died and Ralph stayed on in Cambridge, active and independent and open to his many friends and family. At the ripe age of ninety-seven, he passed away at a nursing home near his daughter in Boxford. A memorial service was held June 13, 1989, at Memorial Church, Harvard University, as a service of thanksgiving for the lives of Ralph and Olive Wetmore who had together influenced the lives of so many at Harvard.

Ralph participated in the national life of botanical and biological organizations in the United States. He had membership in many societies. He was a fellow of the American Association for the Advancement of Science and of the New York Academy of Science. He was an active member of the Botanical Society of America and served as its president in 1953; he was president of the Society for the Study of Development and Growth in 1948-49; and president of the New England Botanical Club, 1948-51. He held membership in the American Society of Naturalists, the American Society of Plant Physiologists, the Scandinavian Society of Plant Physiology, the International Association of Wood Anatomists, and the Torrey Botanical Club.

In 1932 Ralph was elected a fellow of the American Academy of Arts and Sciences and in 1954 a fellow in the National Academy of Sciences. He was awarded an honorary D.Sc. degree by his alma mater, Acadia University, in 1948. He received a Certificate of Merit by the Botanical Society of America on the occasion of its Golden Jubilee in 1964.

Over more than forty years of research and teaching and nearly fifty years of active scientific publication Ralph played important roles as quiet innovator, interpreter, and arbitrator in his chosen fields of science. He was one of the first among the developmental botanists to see the importance of organ and tissue culture methods and to apply them as tools useful in dissecting the intricacies of developmental processes—embryogenesis, cytodifferentiation, and meristem expression. Tissue and organ culture were introduced as laboratory experiments in his undergraduate courses and served as experimental approaches for many of his students interested in plant morphogenesis. In a seminal review coauthored with C. W. Wardlaw of Manchester, England, Ralph fostered a better comprehension of the value and significance of surgical and *in vitro* cultural approaches to problems of plant development. His experiments with Rier on vascular tissue differentiation continue to provide the paradigm for studies on plant cytodifferentiation. Ralph's laboratory served as focal point in the United States for the development of experimental approaches to problems in plant morphology and anatomy.

His keen involvement in experimental research and interest in relationships between structure and function were carried on through his students, several generations of graduate students and postdoctoral associates, who went on to become distinguished teachers and researchers throughout the world.

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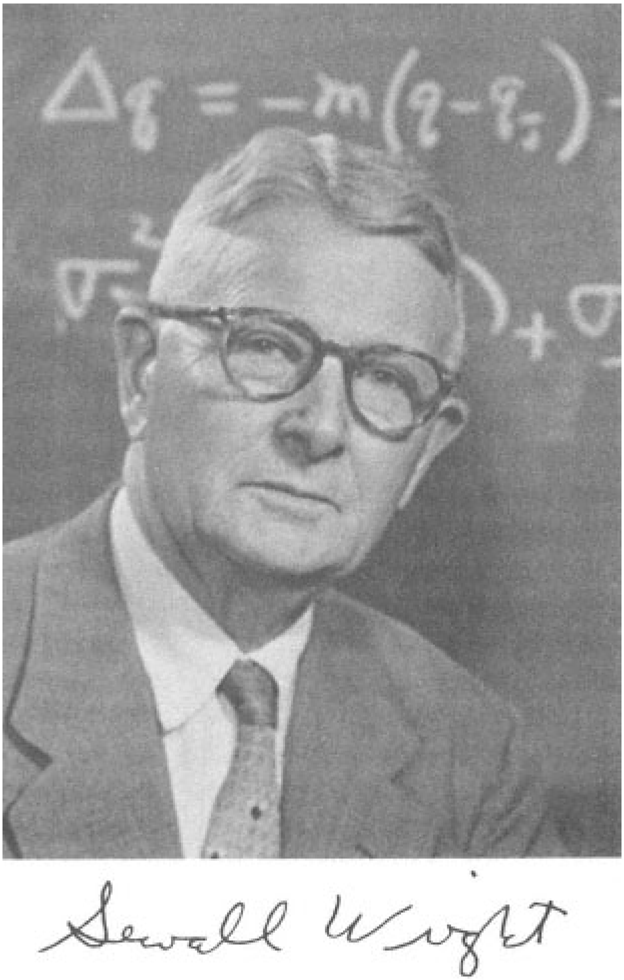

SEWALL WRIGHT

December 21, 1889-March 3, 1988

BY JAMES F. CROW

THE MATHEMATICAL THEORY of evolution and the science of population genetics began with, and for a generation was almost totally dominated by, three men: R. A. Fisher, J. B. S. Haldane, and Sewall Wright. Wright's unique contribution was his "shifting balance theory," which holds that the best opportunity for evolutionary progress is afforded by a large population comprising many partially isolated local groups. Within each group a certain amount of trial and error experimentation can take place, and successful combinations can spread throughout the population. Although the theory remains controversial, it has been very popular and influential in the biological community.

Wright also developed much of the theory of inbreeding (his coefficient of inbreeding is standard material in elementary textbooks) and the genetics of quantitative traits. In addition, he was a pioneer in physiological genetics and was uniquely responsible for the developmental and coat-color genetics of guinea pigs. Wright's method of path analysis, originally used mainly by animal breeders, has become a standard statistical technique in the social sciences.

Wright was elected to the National Academy of Sciences in 1934.

PERSONAL HISTORY

Sewall Green Wright (he later dropped the middle name) was born in Melrose, Massachusetts, December 21, 1889. His father, Philip Green Wright, was an economist who moved with his family to Galesburg, Illinois, in 1892 to join the faculty at Lombard College. There he taught an astonishing variety of subjects—economics, mathematics, astronomy, surveying, English composition—and was director of the gymnasium. He also printed the Lombard College bulletin on his own printing press. Later, he did research at the Brookings Institution and published several books; one, *The Tariff on Animal and Vegetable Oils*, included a statistical appendix by his son Sewall.

Sewall had two brothers. Both became distinguished, Quincy in international law and Theodore in aeronautical engineering. Quincy and Sewall regularly operated their father's printing press and were the first to publish the poetry of Carl Sandburg, then studying writing with their father at Lombard College. Philip Wright was indeed a polymath. Carl Sandburg called him the "Illinois Prairie Leonardo."

Sewall was a precocious child. He could read before starting school. At the age of seven he wrote a pamphlet—still preserved—on natural history, with chapters on marmosets, ants, dinosaurs, chicken gizzards, astronomy (he had seen the constellation Lyra through his father's telescope), and a wren that could not be discouraged from nesting in the family mailbox. He read his father's math books and learned to extract cube roots before entering school, a skill that he said brought him instant, lasting unpopularity with the other students. Later he became fascinated with analytical geometry and invented for himself a way of determining areas, somewhat like the integral calculus that he would learn later from his father at Lombard. His interests were clearly in science, and he never developed his father's passionate fondness for Greek and poetry, although he did enjoy Latin and became interested in sound changes and grammatical forms in the Indo-European languages. He found grade school a disappointment, having learned most of the material at home on his own. In high school he pursued his interests in natural history and took what science courses were offered; but, as with grade school, he did most of his learning outside. In his senior year he read Darwin's *Origin of Species* in its entirety.

Entering Lombard College Wright started to major in chemistry, but found much of analytical chemistry, at least the way it was taught, not to his liking. He took math courses from his father, going as far as differential and integral calculus. He never took any advanced mathematics and his later theoretical work in population genetics depended on methods that were learned on his own or were his own invention.

Philip Wright also taught a course in surveying and this led to Sewall's obtaining a job between his junior and senior years. At that time the Chicago, Milwaukee, and St. Paul Railroad was building a new spur through the Cheyenne and Standing Rock Indian Reservations in western South Dakota, and Sewall's knowledge of surveying was put to use. He also used his mathematical skills to calculate the rail curvature. The year was a rich experience in the old west tradition, with hardships, adventures, and Indians. In his nineties, Wright still remembered words from the Sioux language. These were the same local tribes that had destroyed General Custer's troops at the Little Big Horn thirty-three years earlier. In the latter part of the year Wright's work was cut short by an attack of pleurisy. During his illness he lived in a caboose and read about quaternions. I find it interesting that J. B. S. Haldane also read the same book (Tait's *Elementary Treatise on Quaternions*) while convalescing from war injuries. The book is still preserved, along with some of Wright's marginal notes, so it is possible to see that he got about half way through the book. This was the year of Halley's Comet, and Wright saw it from the roof of his caboose. Unfortunately, his failing eyesight prevented his seeing it again in the 1980s. As a result of his lung infection, Wright was refused standard life insurance, a fact that he found increasingly ironic as he continued to live into his late nineties.

Returning to Lombard for his senior year, Wright took a biology course for the first time. Wilhelmine Entemann Key, one of the first women to receive a Ph.D. from the University of Chicago, was an inspiring teacher and led a graduate—type seminar. Wright learned his first genetics by reading Punnett's article in the eleventh edition of the *Encyclopedia Britannica.* His professional interests were now clear. He obtained a $250 scholarship to the University of Illinois. (This was awarded automatically to the valedictorian of the class. Wright was second in a class of seven, but the woman who was first declined.) William E. Castle visited the University of Illinois during this year and, on meeting Wright, offered him a Harvard assistantship on the spot.

Castle was then the nation's leading mammalian geneticist. Each student had a species to study. C. C. Little worked on mice and later founded the Jackson Laboratory. E. C. MacDowell studied rabbits and Wright took over the guinea pig work. At the time, Castle was selecting hooded rats for greater and lesser amounts of white. Wright played a crucial role by suggesting the experiments to distinguish between the view, wrongly held by Castle, that the color changes were in the major gene itself, and the opposing (and correct) one, that there were many segregating modifiers. Wright did important size and coat-color experiments on guinea pigs, starting a program of research that he continued for more than forty years.

Upon receiving his doctorate from Harvard, Wright moved to Washington where he became senior animal husband-man in the U.S. Department of Agriculture (USDA). There he took over the analysis of a colony of guinea pigs, some of which had been sib-mated for many generations. Wright's analysis of the effects of inbreeding and hybridization are classic. At the same time he continued his studies of coat-color inheritance. This was the period in which Wright began to make major theoretical advances. He worked out the consequences of various mating systems, and his studies on quantitative inheritance, along with those of R. A. Fisher, became the foundation for scientific animal breeding. During this period Wright also developed what he later called the "shifting balance theory."

In 1926 he moved to the University of Chicago where he continued his theoretical work as well as his experiments with guinea pigs. He also took up the standard academic duties, teaching several courses and supervising graduate students. This continued until 1955 when he retired from Chicago at age sixty-five and moved to Wisconsin, which had a retirement age of seventy. Wright was not paid a full salary, only a supplement to his Chicago retirement annuity. This lasted for five years, after which Wright continued to work an additional quarter century. What a bargain Wisconsin got!

After his second retirement Wright completed the monumental set of four volumes, *Evolution and the Genetics of Populations* (1968, 2; 1969, 2; 1977; 1978, 1), in which he not only summarized his own work but reviewed and analyzed an enormous body of experimental and theoretical literature.

In his nineties Wright's eyesight became so poor that he could read only with the aid of an enlarging machine. He gradually gave up active research and scientific reading. Yet he continued to write. His last paper was published in 1988 and reprints came only a few days before his death. My last conversation with him was concerned with his asking me to mail reprints to his friends and with his wondering how he could handle his income tax from a hospital bed.

Wright was in excellent health until the end. It was on one of his customary long walks that he slipped on an icy spot. He died suddenly and unexpectedly a few days later, March 3, 1988, from a pulmonary embolism, the consequence of a pelvic fracture. He had passed his ninety-eighth birthday anniversary three months earlier.

In 1921 Wright married Louise Williams, a genetics teacher at Smith College. She died in 1975. This left him very lonely, but he didn't complain; this was not his nature. He just went on working.

Wright was survived by three children, Richard (dec. 1993), Robert, and Elizabeth (Mrs. John Rose).

SCIENTIFIC WORK

Wright's first scientific paper was published in 1912. It was a morphological study of a fish parasite, a trematode, done while he was at the University of Illinois. His first genetic paper (1914) was a suggestion that one could make a distinction between auto- and allo-polyploidy by the frequency of homozygosis for recessive genes.

Three of Wright's major areas of interest were apparent in the next few years, at Harvard and USDA. These were: correlation analysis, animal breeding, and mammalian physiological genetics. His evolutionary ideas followed soon after. Although the major papers were published after reaching Chicago, the main idea was already formulated while he was still in Washington.

Statistics. Wright's first statistical paper (1917, 1) corrected Raymond Pearl on the use of probable error to test Mendelian ratios. In the same year (1917, 2) he used the additivity of variances and covariances to separate guinea pig weights into within-and between-strains components. This was actually analysis of covariance, though he was unaware of Fisher's work and the words were to come later. Wright (1920, 1; 1926, 1) also found a transformation to linearize cumulative percentage data, now called the probit transformation.

Wright's most important contribution to statistics is his method of path analysis (1921, 1; 1934, 10; 1983; 1984, 2). He always wanted to use statistics interpretatively rather than for description and prediction. Although the mathematics are those of partial regression, the point of view is original. A simple and useful Wrightian device is to diagram causal sequences so that paths of direct causation are indicated by arrows, while correlations between anterior, unanalyzed causes are represented by double-headed arrows. Each causal step is associated with a path coefficient, a partial regression coefficient standardized by being measured in standard deviation units. These coefficients measure the relative importance of the different paths. From such a diagram Wright found simple rules by which one can easily write all the appropriate equations. The method has the virtue of making immediately obvious whether there are enough data and relationships to permit a solution.

In addition to using the method for genetic problems, Wright applied it to such diverse situations as growth and transpiration of plants, respiratory physiology, prey-predator relations, and the relative importance of heredity and environment in human IQ. The most impressive analysis is that of the production and prices of hogs and corn. Wright had 510 correlations, and did the calculations himself, a time-consuming job in those days before computers. He was able to account for 80 percent of the variance of hog production and prices by fluctuations in the corn crop, various intercorrelations, and cleverly adjusted time lags. This paper (1925, 1) was not published immediately; it was deemed improper for an animal husbandman to write a paper in economics. It required the help of Henry Wallace, who prevailed on his father, then secretary of agriculture, to intervene and see that the paper was published.

From 1920 to 1960 the method was seldom used outside of animal breeding circles. Scientists in general and biologists in particular made almost no use of it. Why? One reason is that the method cannot be applied routinely; it doesn't lend itself to "canned" programs. The user must have a hypothesis and diagram it. Biologists have made a great deal of use of correlation and regression analysis, but the emphasis has been on prediction and significance tests, for which Fisherian methods are more appropriate. At the same time psychologists preferred to use factor analysis, which uses much of the same algebra but has a different conceptual basis.

Recently, however, path analysis has become popular in the social sciences. New methods of formulation, and particularly the use of computers, have greatly increased the power of Wright's methods. Yet, he was not always pleased with the uses, or with mathematical criticisms of it. One of his last papers (1983) was a spirited defense of his methods.

Animal Breeding. Wright's (1922, 2-4) studies on inbreeding and crossbreeding of guinea pigs, utilizing the accumulated USDA records and data of his own, were masterful. The meticulously-kept records included not only pedigree information, but many kinds of measurements—litter size, individual and litter weight at various stages, and viability. The husbandry conditions were often miserable, including wartime shortages and the Washington summer heat. It is a testimony to Wright's analytical skills that he could extract so much consistent and useful information. He documented the usual, but not invariable, decline on inbreeding; the recovery on crossbreeding; and the *quantitative* predictability of decline when these hybrids were inbred. He showed that all this was entirely consistent with Mendelian inheritance and dominance.

At the same time Wright developed his widely used algorithm for computing the inbreeding coefficient for any pedigree, however complex (1922, 1), and wrote a series of papers on the consequences of different mating systems (1921, 3-7). He later (1925, 3; 1926, 4; 1943, 1) showed how to separate the effects of nonrandom mating from those of reduction in population size, and showed that in Short-horn cattle the small size of the breeding population was by far the most important.

For many years animal breeding was dominated by a single figure, Jay L. Lush, of Iowa State University. A Wright disciple, he carried the gospel. He wrote a book that became the standard, and his numerous students came from all over the globe. As a result, Wright's path analysis, inbreeding theory, and prediction formula for selection of quantitative traits spread widely and rapidly. Animal breeding changed from an art to a quantitative science. In recent years, with computerized records and artificial insemination, the methods have become very sophisticated. The steady improvement of milk production testifies to the effectiveness of a well-organized, cooperative selection program. The current methods superficially look quite different from path analysis, but they trace back to the Wright-Lush influence.

Mammalian Genetics. Wright's work on physiological and developmental genetics is much less well known than his work on animal breeding and evolution. Yet for many years Wright devoted the major share of his research time to guinea pig studies. He did his own mating and record keeping; the guinea pig colony was often the best place to find him. He continued this work throughout the Chicago years and stopped on moving to Madison only because the University of Wisconsin could not furnish guinea pig facilities. I believe this was fortunate, for it gave Wright the chance to complete his long-contemplated project, writing his four-volume monument (1968, 1; 1969, 2; 1977; 1978, 1). As it was, he spent his first five years at Wisconsin writing up his guinea pig studies, some done years before.

Early in his Washington years Wright wrote a series of eleven papers on color inheritance in various mammals (1917, 2-9; 1918, 1-4). These papers are noteworthy in two regards. First, Wright interpreted the color interactions in terms of the latest knowledge of pigment chemistry and enzyme kinetics. Second, he discovered extensive similarities among the mammals and inferred that the causative genes had a common ancestry, facts that are now being definitively confirmed by DNA similarity.

Throughout his guinea pig studies Wright went as far toward a chemical explanation as knowledge of the time would permit; he wanted to explain dominance and epistasis in chemical terms. His quantitative bent led him to formulate the relationships in path diagrams and to express the kinetics as differential equations, assuming flux equilibrium kinetics. Wright's major analyses (1941, 1, 3) appeared the same year as the work of George Beadle and Edward Tatum on biochemical mutants in *Neurospora*. This started a new direction in genetic research, and molecular biology and microorganisms took over. Wright continued his guinea pig studies for another fifteen years, but these later works—masterful as they were in extracting maximum information from difficult material—attracted little attention.

Wright's early work was ahead of its time in other regards. One of these was in the correlation of size of various body parts (Wright 1918, 6). He analyzed the phenotypic variance into components associated with general size, limb-specific factors, fore-and hind-limb specific factors, and factors associated with the upper and lower limb (whether fore or hind). This kind of work has had a recent resurgence of interest.

Population Genetics and Evolution. In this area the name of Wright is regularly associated with those of Haldane and Fisher. Each had his own style and made distinctive contributions. Haldane wrote a series of papers exploring selection under a variety of genetic conditions; usually, but not always, these involved single factors. Fisher dealt with many problems, but his best-known was showing how to deal with gene interactions for quantitative traits, in particular for fitness itself. He showed in his "Fundamental Theorem of Natural Selection" that, regardless of gene interactions, selection acts on the additive (least squares linear) component of the genetic variance. To Fisher, gene interactions and random gene-frequency fluctuations were impediments to efficient selection, much like noise in a physical system. To Wright, these provided an opportunity for evolutionary creativity.

Wright's shifting balance theory is a way of taking advantage of gene interactions. He had long been concerned with cases in which genes interacted in ways not predictable from their individual effects. He believed that evolutionary creativity often depended on putting together favorable combinations of genes that were individually deleterious. But selection will not ordinarily incorporate such genes in a large, sexually reproducing population. So he argued that the best chance for the evolution of harmonious gene combinations lies in the population structure. In a population divided into many local populations between which there is limited interchange, the gene frequencies will vary randomly in each of them (provided the size is small enough). Among the local populations, one or more may drift into a happy gene combination. This local population will then be at a selective advantage relative to the others and can be expected to reproduce faster. It will then increase or, more likely, send out migrants to adjacent local populations upgrading them to the level of the immigrants. These in turn become more fit and send migrants to still other populations until eventually the whole population attains the favorable gene combination.

This theory has found a great deal of favor with biologists who are impressed by interactions and see this as a way for a sexual population to have some of the benefits of asexuality (i.e., the ability to select for the entire genotype rather than individual genes) and still retain the advantages of Mendelian segregation and recombination. The theory has been criticized on three grounds: (1) The theory requires rather specific relations among the magnitude of selection, migration, and local population size—conditions that may not often be met; (2) The theory may not be needed. It may be that a population hardly ever, if ever, finds itself in the position that *no* allele frequency change can increase fitness. A Fisherian process may suffice. (3) The theory is very difficult to test, mathematically, experimentally, or observationally.

The different viewpoints of Wright and Fisher led to a bitter controversy that lasted from around 1930 until Fisher's death. It produced two opposing camps that to some extent still exist. It is quite possible that both were correct; that evolution usually proceeds by a Fisherian process but that some innovative changes take place by the Wright model. In his last paper, written at age ninety-seven, Wright (1988) was more conciliatory:

> Kimura's "neutral" theory dealt with the exceedingly slow accumulation of neutral biochemical changes from accidents of sampling in the species as a whole. Fisher's "fundamental theorem of natural selection" is concerned with the total combined effects of alleles at multiple loci under the assumption of panmixia in the species as a whole. He recognized that it was an exceedingly slow process. Haldane gave the most exhaustive mathematical treatment of the case in which the effects of a pair of alleles are independent of the rest of the genome. He included the important case of "altruistic" genes, ones contributing to the fitness of the group at the expense of the individual. I attempted to account for the occasional exceedingly rapid evolution on the basis of intergroup selection (differential diffusion) among small local populations that have differentiated at random, mainly by accidents of sampling (i.e., by local inbreeding), exceptions to the panmixia postulated by Fisher. All four are valid.

Wright made many contributions to the mathematical theory of population genetics. As mentioned before, he developed the F-statistics. These extend the inbreeding coefficient to include hierarchical population structure. They now form the basis for analysis of natural population structure. With the coming of molecular polymorphisms, this theory has found a much wider use.

The stochastic theory of population genetics comes mainly from Wright and Fisher. Although Fisher first worked out a quantitative theory, he largely dismissed it as not likely to be very important. Wright, in contrast, regarded random processes as central and spent much of his life working out more and more general forms of his basic stochastic equation. I enjoyed reading Wright's papers sequentially, seeing that as the methods became more general and sophisticated they became easier to understand. Wright (1945, 4) finally realized that his equations were solutions of the Kolmogorov forward equation. These equations represent the high point of Wright's mathematical work.

Wright's work in population genetics was almost entirely theoretical, but he had an important collaboration with Th. Dobzhansky. Dobzhansky played the same role in evolutionary circles that Lush did in animal breeding. By his combination of lucidity, forceful personality, and indefatigable experimentation, Dobzhansky did more than anyone else to bring Wright's work to the biological public. Their collaboration (1941, 2; 1942, 1; 1943, 3; 1947, 2) was the beginning of a burst of activity studying natural populations of *Drosophila*, a subject that is enjoying a renewed interest because of molecular techniques.

WRIGHT AS PHILOSOPHER

Wright was one of a small number of biologists who had a serious, personal, original philosophy. Early in his life he arrived at what is now called "panpsychic dualism." Wright rejected any notion of emergence. He saw no clear borders between living and nonliving, or between thinking and nonthinking. There is no place at which one can say that mind exists after this point but not before. Emergence of mind from no mind is, in his words, "sheer magic." He thus arrived at the view that mind is everywhere. Mind and matter are both universal. Science can produce a statistical description, but not the deeper reality. "Science is a limited venture, concerned with the external and statistical aspects of events and incapable of dealing with the unique creative aspect of each individual event," he wrote (1964, 1).

Most biologists have either disagreed with, or more often have ignored, Wright's philosophy. Some regard the mind-body problem as something best left to philosophers. Others think of mind as a consequence of a sufficiently complicated and appropriately organized state of matter. Wright did, however, find a sympathetic companion in his philosopher friend, Charles Hartshorne, who was for many years Wright's colleague at the University of Chicago. Wright was quick to say that his philosophical views had little relevance to the day-to-day practice of science, and philosophy hardly ever entered his conversations with biological colleagues.

WRIGHT'S IMPACT

Wright made lasting contributions in statistics, mammalian genetics, animal breeding, population genetics, and the theory of evolution. He would rank as an important contributor in any of these areas. Collectively they place him among the greatest of twentieth-century biologists. I'll cite one example of his remaining influence: *The 1988 Science Citation Index* lists some 500 articles that refer to his papers.

WRIGHT AS A PERSON

Socially, Wright was shy and retiring. He had no small talk and was hard to engage in conversation. But, paradoxically, when he did start to talk about something of interest—his childhood, his experience on the railroad surveying team, his ancestors, guinea pigs, evolution, genetics, politics—he could, and would, talk at length. His lectures invariably ran far over the allotted time. He was always gentle, yet he defended his views forcefully and he stated them fully.

Wright was extremely generous with his time. He spent an inordinate amount of time helping others with their papers and data analysis, and often this involved extensive calculations. Likewise, he was an extremely careful reviewer of manuscripts, often providing the author with substantial improvements. He was a conscientious teacher, and spent many hours in the classroom and in the laboratory, which he ran himself. In regard to his time, he was generous to a fault. He published 211 scientific papers, most of them alone. What would he have done had he followed the not uncommon practice of selfishly concentrating on his own work?

Wright has received virtually every honor that is open to evolutionary biologists. He received ten honorary doctorates, far fewer, he used to say, than Herbert Hoover. I have appended a list of honors.

I should like to repeat an anecdote that I have frequently cited before, but which epitomizes this modest, unselfish man and his self-deprecating wit. In his late eighties while writing his four books he received a small stipend from the National Science Foundation. When I brought him the news that the foundation had offered to provide an inflationary adjustment he demurred. According to his calculations, he said, his productivity was declining at the same rate as the value of the dollar and he therefore didn't deserve any increase. He never accepted it.

Wright died at the age of ninety-eight. It is perhaps wrong to regard a death at this age as premature, but I do. Wright was in good health, enjoying life, and intellectually alert. He knew that a centennial celebration was being planned and looked forward to it. But for encountering an icy spot on the sidewalk, he would surely have been in attendance.

MY MAIN SOURCE of information has been a regular association with Wright for more than three decades. I obtained much information from a full-length personal and scientific biography by Will Provine (1986), based on hundreds of hours of taped interviews with Wright and a study of his extensive correspondence. It is a treasure of information for those who would like to know more than can be presented in this short article. I have also drawn freely on my own earlier writings, listed in the references below.

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CHRONOLOGY

1889	Born December 21, in Melrose, Massachusetts, to Philip Green Wright and Elizabeth Quincy Sewall Wright.
1892	Moved to Galesburg, Illinois, where his father taught at Lombard College.
1902	Entered high school in Galesburg.
1906	Enrolled in Lombard College.
1909-10	Instrument man on an engineering party for a new line of the Chicago, Milwaukee, and St. Paul Railroad in South Dakota.
1911	Received B.S. from Lombard College, which later merged with Knox College.
1911-12	Graduate student at the University of Illinois, receiving M.S. in 1912.
1912-15	Graduate student with W. E. Castle at Harvard University, receiving Sc. D. in 1915.
1915-25	Senior Animal Husbandman, U. S. Department of Agriculture, Washington.
1921	Married Louise Smith, February 21, in Granville, Ohio.
1926-29	Associate Professor of Zoology, University of Chicago.
1930-37	Professor of Zoology.
1938-54	Ernest D. Burton Distinguished Service Professor.
1943	Hitchcock Professor, University of California, Berkeley.
1949-50	Fulbright Professor, University of Edinburgh.
1955-60	Leon J. Cole Professor of Genetics, University of Wisconsin, Madison.
1960-88	Professor Emeritus.
1988	Died in Madison, Wisconsin, March 3.

HONORARY DOCTORATES

1942	University of Rochester
1948	Yale University
1951	Harvard University
1955	Michigan State University
1957	Knox College
1958	Case Western Reserve University
1959	University of Chicago
1961	University of Illinois

1965	University of Wisconsin
1984	State University of New York-Stony Brook

AWARDS AND HONORS

1944	President, American Society of Zoologists
1947	Elliot Medal, National Academy of Sciences
1947	Weldon Memorial Medal, Oxford University
1949	Lewis Prize, American Philosophical Society
1952	President, Genetics Society of America
1952	President, American Society of Naturalists
1955	President, Society for the Study of Evolution
1956	Kimber Award, National Academy of Sciences
1958	President, Tenth International Congress of Genetics
1966	National Medal of Science
1980	Darwin Medal, Royal Society of London
1982	T. H. Morgan Award, Genetics Society of America
1982	J. F. Meckel Prize, American Society of Medical Genetics
1984	Balzan Prize, Balzan Foundation, Milano

MEMBERSHIPS

National Academy of Sciences
American Philosophical Society
American Academy of Arts and Sciences
American Association for the Advancement of Science
American Genetics Association
American Statistical Association
Genetics Society of America
Biometric Society
Royal Society of London (Foreign Member)
Royal Danish Academy of Science and Letters (Foreign Member)
Royal Society of Edinburgh (Honorary Fellow)
Genetical Society (Honorary Fellow)
Econometric Society
Phi Beta Kappa (Honorary Member)

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