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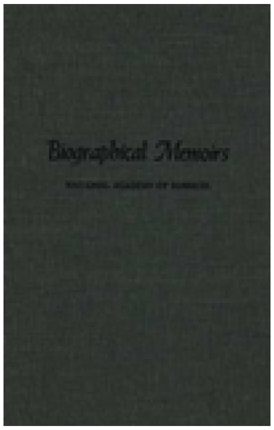

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THE NATIONAL ACADEMIES
*Advisers to the Nation on Science, Engineering, and Medicine*

# Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA

# Biographical Memoirs

## Volume XLVIII

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# Contents

# Preface

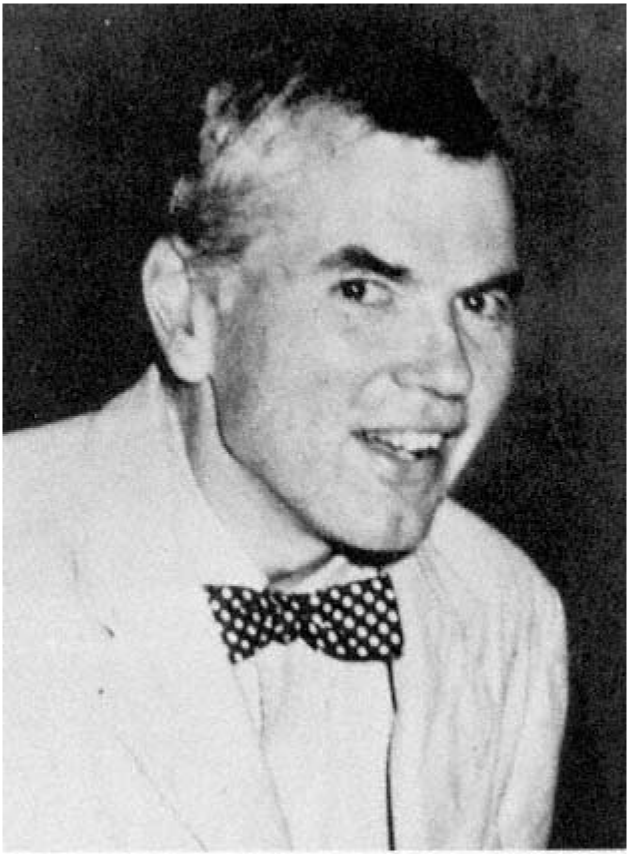
The *Biographical Memoirs* is a series of volumes, first published in 1877, containing the biographies of deceased members of the National Academy of Sciences and bibliographies of their published scientific contributions. The goal of the Academy is to have these memoirs serve as a contribution toward the history of American science. Each biographical essay is written by an individual familiar with the discipline and the scientific career of the deceased. These volumes, therefore, provide a record of the lives and works of some of the most distinguished leaders of American science as witnessed and interpreted by their colleagues and peers. Though the primary concern is the members' professional lives and contributions, these memoirs should also include those aspects of their lives in their home, school, college, or later life that led them to their scientific career.

The National Academy of Sciences is a private, honorary organization of scientists and engineers elected on the basis of outstanding contributions to knowledge. Established by a Congressional Act of Incorporation on March 3, 1863, the Academy works to further science and its use for the general welfare by bringing together the most qualified individuals to deal with scientific and technological problems of broad significance.

DAVID R. GODDARD
HOME SECRETARY

# Biographical Memoirs

VOLUME XLVIII



Fuller Albright

# Fuller Albright

January 12, 1900-December 8, 1969

by A. Leaf

Fuller Albright was born in Buffalo, New York, just twelve days after the opening of the twentieth century. His childhood and youth were passed in that period of peace, prosperity, and general optimism that came to an end with the outbreak of World War I.

His father was an industrialist, art patron, and philanthropist. His mother, a Fuller from Lancaster, Massachusetts, embodied the finest traditions of the New England culture. It was a large, happy, and close-knit family in which parents and children shared much of their lives together—whether at the great house in Buffalo, the long summer vacations at the family camp in the Adirondacks, the winter holidays at Jekyll Island, or on the "Grand Tour" of Europe.

It was a family characterized by a strong sense of humor—and no child growing up in it was in danger of developing a sense of self-importance. Nor were the close family ties confining. It was a hospitable household with a constant flow of visitors. However, when he entered Harvard College at the age of seventeen, young Albright was possessed of a naiveté that was unusual even in those days and an appearance that was positively cherubic! That look of boyish innocence somehow stayed with him always. He also displayed a natural ebullience and gregariousness that allowed him to fit easily into the society of a

Harvard undergraduate in the Boston community. In the Delphic Club he established friendships that he cherished all his life.

He graduated from college *cum laude* in three years and entered medical school in the fall of 1920. It was when he started to see patients that his long-range goals began to take form. At first he was fascinated by obstetrics. Later in the medical course he had a brief infatuation with orthopedic surgery but came to the conclusion that he did not have the manual dexterity to make a good surgeon. At the same time striking advances in medical research were being reported. Professor James Howard Means returned from a medical meeting to announce the dramatic discovery of insulin. Biochemistry was beginning to furnish new insights into the functioning of the body. Albright's natural curiosity was stimulated by the possibilities of applying the new discoveries to the study of disease. It was whetted too by the emotional experience of observing firsthand what, for example, this new insulin could do for a patient at death's door from uncontrolled diabetes. Throughout his later career his investigations were apt to be linked to the puzzles his own patients presented to him rather than to abstract problems of biochemistry.

After an internship in medicine at the Massachusetts General Hospital, he spent a year of research there with Dr. Joseph C. Aub, whose studies in lead poisoning meshed closely with Albright's burgeoning interest in the metabolism of calcium. In this happy environment in the company of Aub, Means, and Bauer, his latent talent began to blossom and clearly indicated the career that he should follow. Then came a year as assistant resident at Johns Hopkins under Dr. Warfield Longcope. Here he struck up an acquaintance with John Eager Howard, who shared his interest in endocrinology. They became fast friends and for years were in almost constant communication trying out new ideas on each other. Often, when such ideas reached fruition, neither of them knew whose it was in the first place —nor cared. Before returning to Boston he spent a year in Vienna with the great pathologist, Professor Jacob Erdheim, who proved to be an inspiring preceptor.

The remainder of his professional life was spent in research, teaching, and practice at the Massachusetts General Hospital. It was an extraordinarily productive career, which brought forth new concepts in endocrinology and delineated a number of hitherto unrecognized diseases. During this period he had associated with him in his laboratory a succession of young investigators who became leaders in the field of endocrinology in this country and abroad.

In 1933 he was married to Claire Birge, of New York, in what proved to be a supremely happy match. Claire was a superb hostess, and their household provided warm hospitality to hosts of students and visitors from all parts of the world.

There are two sons: Birge, an attorney in Boston, and Read Ellsworth, who teaches at the Fenn School, in Concord, Massachusetts.

Dr. Albright's clinical investigations were highly original and far-reaching. His name is associated with the initial clinical description of hyperparathyroidism and the distinction between over-activity of all parathyroid tissue and the effect of adenoma of a single parathyroid gland. He called attention to the association of hyperparathyroidism with kidney stones; and, in fact, on the basis of an extensive study carried out in his Stone Clinic, he laid the basis for the modern diagnosis and treatment of this condition. In his laboratory was developed a method for measuring gonadotropins in the urine, which made it possible to characterize various types of amenorrhea as well as disorders of testicular functions. In 1928 he described a condition that has come to be known as Albright's syndrome, the distinguishing features of which are precocious puberty in girls, cystic bone disease, and brownish pigmentation of the skin. More than half a dozen other original descriptions of disease might with equal propriety have borne his name. He pointed out the role of steatorrhea in depleting the body of fat-soluble vitamins. He first described renal tubular acidosis and its effective treatment with alkali. He called attention to the occurrences of thinning of the bones in women following menopause. He was among the first to use estrogen to inhibit ovulation in women and progesterone to correct the metropathia caused by estrogens. He unraveled the pathogenesis of Cushing's syndrome and sounded the first warnings of the harmful side effects of steroids on the tissues.

A total of 118 scientific papers bear his name, and his book *The Parathyroid Glands and Metabolic Bone Disease,* published in 1948, is still a prime source of information on the subject.

Dr. Albright was the recipient of honors and awards from universities and learned societies all over the world. He was President of the American Society for Clinical Investigation in 1943-1944, the Association for the Study of Internal Secretions in 1945-1946, and the Endocrine Society in 1946-1947. He was elected to membership in the National Academy of Sciences in 1955.

In 1937, at the height of his productivity, the early signs of Parkinson's disease made their appearance and progressed very gradually but relentlessly for nearly two decades. This long period was one of almost feverish activity for him, as if he were trying to outstrip the relentless advance of his disease. He maintained, nevertheless, a sublime indifference to his disability and managed to communicate complex ideas with extraordinary lucidity. Finally, in 1956, at his own insistence, he went through the newly devised surgical treatment for Parkinson's disease, the indications and contraindications for which were not fully understood and which left him worse off than before. The remainder of his life was spent in helpless invalidism, mitigated only by a clouding of the sensorium and the devoted care of nurses and attendants at the Massachusetts General Hospital.

In 1955 Harvard awarded Fuller Albright the honorary degree of Doctor of Science with the following citation:

> "Brilliant investigator in the complex field of nutrition and metabolism, your keen mind and enormous courage are a credit to this University and to Medicine."

His tastes were simple. He was never so happy as when casting a trout fly in an Adirondack lake, unless it was when he was talking shop with a colleague. He loved a good game of bridge. He had a good eye for color and form, but no ear at all for music. He and his wife Claire were both fond of travel and did a good deal of it in this country, in Europe, and in South America.

His dress reflected his lack of self-consciousness. Who can forget the old tweed jacket, the baggy trousers, and the jaunty bow tie?

One of Dr. Albright's best remembered characteristics was the twinkle in his eye, which was a manifestation of his unconquerable *joie de vivre* and a slightly amused outlook on the human condition. He carried his sense of humor into his medical writings and even into his lectures, a rare accomplishment indeed, which added immensely to his effectiveness and popularity.

Everyone who knew him has a stock of warm and pleasant memories of their associations with him. One day he was joined by a young aggressive foreign visitor in attendance at one of his clinics. In the course of the discussion regarding one of the patients seen on that occasion, the visitor reprimanded Dr. Albright for not having read the visitor's writings on the subject. Whereupon Dr. Albright humbly apologized for his negligence but added, "I hardly have time to read my own."

At that time he was already seriously incapacitated physically, but not mentally, by Parkinson's disease. Even then his cheerful demeanor and unrelenting good nature had a highly psycho therapeutic effect upon many of the patients who flocked to him for help. Minor complaints usually evaporated instantaneously in the presence of this revered physician who refused to make any concessions to his own unavoidable physical infirmities.

In addition to his continuous and diverse clinical investigations, he managed a busy practice up to the end. Several special clinics that he conducted were an important part of his clinical practice. Thus, he established and presided over the Ovarian Dysfunction Clinic, the Stone Clinic, and his Saturday morning clinic. When asked what he saw at the unnamed Saturday clinic, he was wont to respond with a twinkle in his eye, "These are the patients I refer only to myself." In fact, it was from the often rare and esoteric problems that this group of patients had that many of his clinical investigations arose. He had an uncanny ability to capitalize upon Nature's experiments, to unravel complex disorders and provide clear physiologic understanding that often led directly to rational therapy for his suffering patients. His pleasure in unraveling some important physiological relationship was indeed great, but he always thought of his new findings in terms of how they would relate to improved treatment for some unfortunate patient. The theoretical and the practical were productively enhanced by his ever-active mind.

In a tribute to Dr. Albright published in 1962, one of his younger collaborators wrote as follows:

> "What about the personality of this remarkable investigator under whose luminous common sense so many knotty problems suddenly seemed simple? He never discussed personalities. His private life was uneventfully happy. He married Claire Birge and lived happily thereafter in a serene and comfortable home where friends from all over the world were received. What about his heroic battles with his tragic disease? Was it, after all. heroism which made him refuse to stop doing what he liked to do or was it just more of his famous common sense? His indif

> ference to pity was the indifference of a profoundly serene and happy man to public opinion of any kind. Perhaps Claire's role was more heroic; certainly it was brilliant. Charming, vivacious, and full of enthusiasm she appeared perfectly carefree as she added to her domestic duties the jobs of chauffeur, secretary, and finally nurse and shouldered all the burdens of the man of the house while appearing to depend on her husband. Although he never was made to feel dependent on his wife, Fuller could not have continued to work productively without her. Perhaps they are both heroes, but they are certainly not martyrs. Martyrs are never so widely loved and respected in their own time."

I wish to acknowledge others of Dr. Albright's friends, collaborators, and students who contributed to the preparation of this memoir: Frederic C. Bartter, John Browne, James M. Faulkner, Anne P. Forbes, Philip H. Henneman, and John E. Howard.

# DEGREES, APPOINTMENTS, AND HONORS

## Degrees

1921 A.B., Harvard University

1924 M.D., Harvard Medical School

1955 S.D., Harvard University (Honorary)

## Appointments

1924-192 West Medical House Officer, Massachusetts General Hospital

1926-1927 Research Fellow in Industrial Medicine, Harvard Medical School

1927-1928 Research Fellow, Massachusetts General Hospital

1928-1929 Moseley Traveling Fellow, Harvard Medical School

1929-1935 Henry Pickering Walcott Fellow in Clinical Medicine, Harvard Medical School

1929-1937 Assistant Physician in Medicine, Massachusetts General Hospital

1930-1935 Instructor in Medicine, Harvard Medical School

1935-1938 Associate in Medicine, Harvard Medical School

1937-1939 Associate Physician in Medicine, Massachusetts General Hospital

1938-1942 Assistant Professor of Medicine, Harvard Medical School
1939-1958 Physician in Medicine, Massachusetts General Hospital

1942-1961 Associate Professor of Medicine, Harvard Medical School

1958-1969 Board of Consultation, Massachusetts General Hospital

1961-1969 Professor of Medicine, Emeritus, Harvard Medical School

## Memberships

American College of Physicians
American Society of Clinical Investigation (President, 1943-1944)
Association of American Physicians
Association for the Study of Internal Secretions (President, 1945-1946)
National Academy of Sciences
Phi Beta Kappa
Alpha Omega Alpha

American Medical Association
Massachusetts Medical Society

## Honorary Memberships

1951 Royal Society of Medicine

1951 Swedish Endocrinology Society

1953 Columbia Endocrinology Society

1954 American Orthopaedic Association

## Awards

1947 Roche-Organnon Award in Endocrinology

1947 American College of Physicians Award for Achievement in Internal Medicine

1949 Borden Award, Association of American Medical Colleges, for "extraordinarily original and monumental contributions to the understanding of metabolism of bone and other tissues."

1951 The Joseph Goldberger Award of the American Medical Association's Council on Foods and Nutrition

1955 Doctorate of Science, Harvard University

1961 Citation, Massachusetts General Hospital, for being one of the 15 outstanding physicians who had received early training at the hospital

# Bibliography

*Key to Abbreviations*

Am. J. Med. Sci. = American Journal of Medical Science
Ann. Internal Med. = Annals of Internal Medicine
Arch. Intern. Med. = Archives of Internal Medicine
Bull. Johns Hopkins Hosp. = Bulletin of the Johns Hopkins Hospital
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Clin. Endocrinol. = Journal of Clinical Endocrinology
J. Clin. Endocrinol. Metab. = Journal of Clinical Endocrinology and Metabolism
J. Clin. Invest. = Journal of Clinical Investigation
J. Urol. = Journal of Urology
Metab. Clin. Exp. = Metabolism: Clinical and Experimental
N. Engl. J. Med. = New England Journal of Medicine
Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians

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G. v. Békésy

# Georg Von Békésy

June 3, 1899-June 13, 1972

by Floyd Ratliff

On the occasion of Georg von Békésy's election to the National Academy of Sciences in 1956, he gave the following replies to two items in a questionnaire sent to him by the Academy to obtain biographical information for its files:

"Major interest?" "Art."

"Major influences which determined the selection of your particular field of science?" "Pure accident."

These replies—"Art" and "Pure accident"—succinctly characterize much of the life and work of Georg von Békésy. His life was devoted almost as much to art as it was to science; and, although his love of art was carefully cultivated, the course of his scientific career was determined almost as much by chance as it was by design. He found his life work outside of his original chosen field during the economic depression in Hungary following World War I, and he was set on his wanderings half way around the face of the earth by the political turmoil there after World War II.

Professor Georg von Békésy died on June 13, 1972, at the age of seventy-three, in Honolulu. Born on June 3, 1899, in Budapest, he was the son of Alexander and Paula von Békésy. The Békésys were an old and distinguished family in Hungary, and Alexander von Békésy held a position in the Hungarian diplomatic service. Because of his father's various assignments, Georg von Békésy spent his early childhood in Budapest, Munich, and Constantinople. In 1916 he obtained his baccalaureate in chemistry at Berne, Switzerland, where his father was then a *charge d'affaires* in the Hungarian Embassy. Following World War I, Békésy returned to Hungary and in 1923 received his doctorate in physics at the University of Budapest. Békésy began his scientific career in the laboratory of the Hungarian Post, Telephone, and Telegraph. He spent the next several years there (except for the year 1926-1927, when he worked in Berlin with K. Küpfmiiller in the laboratories of Siemens and Halske). In 1932 he was appointed private docent in the University of Budapest, where in 1940 he became Professor of Experimental Physics. For some time Békésy had two laboratories; for, after his appointment at the University, he continued his research in the government laboratory at the Hungarian Post.

During the foreign occupation of Hungary after World War II, Békésy found conditions intolerable for scientific research and in 1946 accepted an invitation to go to the Karolinska Institutet in Stockholm to work with Y. Zotterman. A year later, under the aegis of Professor S. S. Stevens, he came to the United States and joined the faculty of Harvard University. In 1949 he was given a special appointment there as Senior Research Fellow in Psychophysics, which enabled him thereafter to devote his full attention to research. Békésy held this post for nineteen years. At the age of sixty-seven, with the unhappy prospect of retirement facing him, Békésy resigned and moved to a new laboratory built for him at the University of Hawaii, where he accepted an appointment as Professor of Sensory Sciences—an endowed chair provided by the Hawaiian Telephone Company. He continued his research at Hawaii for six years—almost until the day of his death.

In 1961 Georg von Békésy was awarded the Nobel Prize in Physiology or Medicine "for his discoveries concerning the physical mechanism of stimulation within the cochlea." The following excerpt from the Presentation Speech by C. G. Bernhard of the Karolinska Institutet provides a fine characterization of Békésy's research on the mechanics of the ear:

"According to the saga, Heimdal was able to hear the grass grow. Our hearing ability is perhaps not of that kind, but our ear is anyhow almost sensitive enough to record the bounce of an air molecule against the eardrum, while, on the other hand, it can withstand the pounding of sound waves strong enough to set the body vibrating. Moreover, the ear is capable of a selectivity which permits a close analysis of sounds the various qualities of which determine the characteristics of the spoken word and of instrumental and vocal expressions in the universe of music.

"A sound which hits the ear makes the eardrum vibrate. Within the air-filled middle ear the vibrations are transmitted via a subtle system of levers, the ossicle chain, to the fluid of the inner ear, the cochlea. The footplate of the stirrup which serves as the innermost link of the ossicle chain is movably mounted in the opening of the oval window of the inner ear which faces the middle ear. The vibrations of the fluid engage in their turn the so-called basilar membrane, an oblong partition which divides the spiral-shaped cavity of the cochlea in its longitudinal direction. Along its entire length the membrane carries sense cells, receptors, like fine tapering columns with hairy points reaching up to a covering membrane. The receptor cells, or hair cells, transform the mechanical energy, represented by the vibrations of the basilar membrane, into the specific form of energy which triggers the nerve impulses. The frequency of these impulses serves as the code to the information carried on to the higher nerve centers.

"Von Békesy has provided us with the knowledge of the physical events at all strategically important points in the transmission system of the ear. This does not mean that the properties of the oscillating systems of the ear have not been an object of study and theoretical considerations by scientists before von Békésy. The field of physiological acoustics has a noble ancestry, in which the theories of von Helmholtz hold an authoritative position.

"Von Békésy's distinction is, however, to have recorded the events in this fragile biological miniature system. Authorities in this field evaluate the elaborate technique which he developed for this purpose as being worthy of a genius. By microdissection he reaches anatomical structures difficult of access, uses advanced teletechniques for stimulation and recording, and employs high magnification stroboscopic microscopy for making apparent complex membrane movements, the amplitudes of which are measured in thousandths of the millimeter.

"Among von Békésy's important contributions to our knowledge of sound transmission in the middle ear should be mentioned the elucidation of the vibration patterns of the eardrum and of the interplay of the ossicle movements. His technical and theoretical mastery has reached its peak in those investigations which led to the fundamental discoveries concerning the dynamics of the inner ear. Experimental and clinical data had confirmed von Helmholtz's assumption that the frequency of the sound waves determines the location along the basilar membrane at which stimulation occurs. The physical characteristics of the pattern of the membrane vibrations and the conditions for its appearance had, however, previously only been the object of theoretical considerations. Von Békésy succeeded in unveiling the features of the vibration pattern. He found that movements of the stirrup footplate evoke a wave complex in the basilar membrane, which travels from the stiffer basal part to the more flexible part in the apex of the cochlea. The crest of the largest wave first increases, thereafter quickly decreases. The position of the maximal amplitude was found to be dependent on the frequency of the stimulating sound waves in such a way that the highest crest of the travelling wave appears near the apex of the cochlea at low-frequency tones and near its base at high frequencies. The conditions for the appearance of these specific vibration patterns were determined in model experiments.

"Von Békésy then turned to the question of how the hair cells are stimulated. With a thin needle, the point of which touched the basilar membrane, different parts of the membrane could be set in vibrations in various directions. The point of the needle simultaneously served as an electrode for recording the electrical potentials from the receptor cells. It was found that a local pressure on the basilar membrane is transformed into strong shearing forces which act on the hair cells in various degrees.

"Thus, von Békésy has given us a clear picture of how the cochlea functions mechanically and his discoveries serve as a basis for our conception of the cochlea as a frequency analyzer."*

Fortunately for the student and researcher in the field of hearing, Békésy's writings on the main results of his many studies on hearing over the thirty-year period 1928-1958 were published in 1960 in a single volume, *Experiments in Hearing,* translated and edited by E. G. Wever. These papers are models of technical skill, elegance of experimental design, and clarity of presentation.

Békésy's physical measurements of the mechanical properties of the ear convinced him that some sort of neural "sharpening" mechanism must be required to account for the remarkable sharpness of pitch discrimination. The general form of Békésy's ideas was derived from the earlier work of Ernst Mach on

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* C. G. Bernhard, "Presentation Speech," in *Nobel Lectures, Including Presentation Speeches and laureate's Biographies. Physiology or Medicine, 1942-1962* (Amsterdam-London-New York: Elsevier Publishing Co. for the Nobel Foundation, 1964), pp. 719-21.

inhibitory interactions in the visual system. Békésy published a few preliminary observations on the possible role of inhibition in hearing as early as 1928; but extensive investigations (including work on skin senses, vision, and taste) were not carried out until much later, as he gradually became more and more interested in this aspect of his work. His researches in this field, prior to 1967, are well summarized in his book *Sensory Inhibition.*

This brief account of Békésy's life and work represents the sum and substance of the public record, excepting one autobiographical sketch published posthumously in 1974. Békésy generally left it to others to extol his virtues and proclaim his accomplishments.

Békésy was a solitary person. Although friendly, he was reserved; few people knew him intimately, and those who did respected his desire for privacy. Despite Békésy's usual solitary ways, however, when the occasion demanded he was outgoing and sociable. He had a keen sense of humor, and his lectures and conversations were always punctuated with some anecdote, wry comment, or little aphorism that went directly to the heart of the matter. For example: One day at the weekly colloquium in the Harvard Psychology Department (with which the Psycho-Acoustic Laboratory was affiliated), the guest speaker was at his worst and gave an extraordinarily long and dull lecture on a mathematical theory of behavior. Unfortunately, the audience responded in kind, and at equally great length, with pointless questions and irrelevant comments. When at long last it was all over, Békésy led me directly to the blackboard in his office, picked up a piece of chalk, and said, "This is the most important but least known equation in all of the social sciences. Always remember it, for as you have just seen, it completely describes a great deal of human behavior." And then—summing up the whole afternoon neatly—he wrote:

$$0 + 0 = 0.$$

Békésy often remarked that his was a lonely life, but he preferred it that way. His closest friends, from which he drew both solace and inspiration, were the art objects he had collected over the years. These filled his laboratory, secreted here and there in drawers and filing cabinets where one might ordinarily expect to find only tools, supplies, and records of data. But always at least one of these treasures was out on display on his work bench or desk where he might spend hours examining it and reflecting on its beauty. This was not a surprising aspect of Békésy's character, for he was truly creative himself, and his contributions to science were very close to art. Indeed, the private Békésy—known only to a few and even to them incompletely—can only properly be portrayed as the many-faceted person which he was: A true Renaissance man with very broad interests and great depth of knowledge in both the arts and the sciences.

Békésy's interest in art was undoubtedly fostered by the circle in which his family moved. During all of his early years he was surrounded by artists, sculptors, musicians, and other intellectuals who were friends and acquaintances of his parents. As a young man, Békésy studied music seriously; and it has always seemed strange to many people that in his later years the world's greatest authority on hearing was more interested in the visual arts than in the musical arts. The explanation is simple. Békésy found that he could not get music off his mind. After playing or listening to a good tune, he felt compelled to hum it or go over it in his mind for hours, or even days. This, he felt, interfered with sound, logical thinking. Because the perception of a work of visual art faded away at once—unless he made a conscious effort to recall it—he chose to make the study of art and archeology, rather than music, his avocation.

Békésy studied art not only for the great pleasure it gave him, but also for an effect that he believed it would have on his mind. Comparing one art object with another to determine quality and authenticity, he thought, greatly improved his ability to make judgments about the quality of scientific work too. Whether such transfer actually took place no one can say. But there is no question that art pervaded all of Békésy's science. The many superb instruments of his own design, the models and movies of various wave phenomena, the illustrations in the papers describing his experiments—indeed, even the experiments themselves—can all properly be called works of art. (Many of Békésy's private papers, films, and slides are now available for study in the Library of Congress.)

During his lifetime, Békésy collected a large number of works of art and rare books. Many were lost in World War I, others in World War II, and some were destroyed or damaged as a result of a fire in Memorial Hall at Harvard. In spite of all this, the collection gradually grew larger and more varied. But the collection was almost as private as was Békésy's personal life—few persons were ever privileged to see more than a mere fragment of it. Now, however, it is in the public domain. In his last will and testament, Békésy chose to honor the Nobel Foundation, which had earlier bestowed such great honor upon him, with a gift of that which was closest and dearest to him—the art objects that he had collected over the years and that had been both a source of inspiration in his work and a source of solace for a lonely man in times of need of comfort. The Georg von Békésy Collection of Art was placed on public exhibition for the first time, by the Nobel Foundation, on December 9, 1974.* His collection of books is now in the Library of the University of Hawaii.

As was mentioned above, Békésy's choice of a scientific career was, as he put it, "pure accident." But chance can only provide

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* A biographical sketch of Békésy, which focuses on his interest in art, is included in the catalog: F. Ratliff, "Georg von Békésy: His Life, His Work, and his Friends," in *The Georg von Békésy Collection,* ed. by J. Wirgin (Malmö, Sweden: AB Allhem for the Nobel Foundation, 1974).

opportunity; it remains for the individual to seize upon it and exploit it. And when he does so time and again, as Békésy did, that is the mark of genius.

For example, it was largely a matter of chance that a man trained in chemistry and physics became interested in the psychophysiology of hearing and in the role of inhibitory interactions in sensory processes. To begin with, it was Békésy's youthful idealism and patriotism that prompted him to leave Switzerland, following World War I, and to return to Hungary to help rebuild the country. His doctoral research in physics at Budapest had been in a branch of optics now known as interference microscopy, and he tried to find a position in the field of optics. But times were very hard then and there were no jobs at all for a physicist with his background and experience. Békésy finally decided to find the best-equipped laboratory in Hungary and work there for nothing, if necessary. The only laboratory still well equipped after the war was the Hungarian Post and Telegraph. It had support because the government was forced by postwar treaties to maintain the telephone and telegraph line that crisscrossed the country. Although the laboratory had no proper position for a physicist, they did employ Békésy and give him a small salary.

Every day brought a new experience. One day telephone lines would fail, on another there would be radio problems, and so on. As a result Békésy was drawn into the problems of the rapidly developing field of communication engineering—particularly the electromechanical means of the transfer and processing of information.

At that time the international telephone lines were tested over a loop made by closing the circuit in another city. The input voltage of a series of pure tones fed into the origin of the loop at Budapest would be compared with corresponding output voltage when it arrived back at Budapest. The complete measurement took many minutes—sometimes hours, if there was much trouble with the lines. Békésy developed a new method that would check the lines in about one second. To learn how to do this, he spent many hours at night in the room where the cable heads came in, listening to conversations and trying to match the systems properly. Békésy paid close attention to everything he heard over the lines, including the inevitable "clicks" when phones were connected and disconnected. These clicks seemed to change as the status of a line changed, so he started using them as the test signal, and within a few days he had perfected his new, more efficient method. As Békésy pointed out in his Nobel Lecture, the basic idea was similar to a musician making a quick check of the tuning of his violin by plucking a string rather than by the more time-consuming bowing. In effect, Békésy "plucked" the telephone line instead of "bowing" it. The key to the whole problem was that the clicks each contained a wide spectrum of frequencies—thus each click sent the equivalent of innumerable "pure tones" along the lines in a single short pulse. This click method provided the key to Békésy's future research and led him to the study of the sense of hearing and the mechanics of the ear. But this came about more or less by chance, too.

The Hungarian government wanted to make further improvements in the international telephone system and asked the laboratory for advice on how the limited funds available for research should be spent. Békésy's opinion was that the money should be allocated to improving the weakest part of the system. With the click method it was easy to determine that the telephone receiver was the worst part of all—including even the international cables themselves. But this focus on the receivers immediately raised the further question: Is the receiver more or less sensitive than the ear? For it would be futile to improve the receiver if it were already more sensitive than the ear itself. By making the click comparison it was evident that the eardrum was a much, much better instrument than the ordinary telephone receiver. It was therefore essential to improve the receiver first.

This study of the mechanics of the eardrum led naturally to the study of the middle ear and the investigation of the chain of bones—stirrup, hammer, and anvil—that conducts the sound from the eardrum to the inner ear. And likewise, this study led naturally to the inner ear itself and dealt with a very old problem-the form of the pattern of vibration produced on the basilar membrane in response to a pure tone. Others before Békésy, notably Helmholtz and Corti, had looked at the intact basilar membrane. Their technique was to chip away the surrounding bone. But the cochlea is imbedded in one of the hardest bones in the body; and by the time the cochlea was opened and the basilar membrane exposed to view, the membrane was usually displaced or disturbed in some way so that observations of its motions were inconclusive. Furthermore, the whole cochlea tended to dry out during the preparation, thus distorting the natural mechanical properties of the membrane.

With the elegant experimental techniques that were to characterize all of his later research, Békésy solved all of these technical problems at once. The solution was simple: do the dissection under fluids. The preparation was placed in a square bath with the fluid entering one side and flowing out the other. Then, by using a high-speed drill, it was possible to grind off very thin layers of bone. Each time the drill was used, a cloud of bone dust was formed in the bath; but, because the fluid was continuously flowing, the cloud cleared very quickly. With a special 200-power underwater microscope Békésy could observe the progress of the work closely and proceed very carefully. He was thus able to open nearly a full turn of the tip of the snailshaped cochlea and thereby to expose to view a substantial portion of the intact basilar membrane. In this way the various patterns of vibration of the membrane produced by various pitches of sound could be observed directly.

In 1928 Békésy published his first and probably most significant paper on the pattern of vibration in the membranes of the cochlea of the ear. This one paper provided the foundations for his whole career: The basic experimental observations in it set the course of practically all of his subsequent studies on the mechanics of the ear, and the ideas that he expressed in it on how Ernst Mach's laws of contrast in vision might also be significant in hearing and in other senses set the course of his work on lateral inhibition, which was to occupy most of his later years. Chance played a role here, too; Ernst Mach's work came to Békésy's attention by "pure accident" one day when he was searching for a paper by Mach's son, Ludwig, on what is now known as the Mach-Zender interferometer.

It was Békésy who pointed out to me, about 1950, the similarity between Mach's ideas on inhibition and the inhibitory interaction that H. K. Hartline (Nobel Laureate in Physiology or Medicine 1967) had just discovered in the compound eye of the horseshoe crab *Limulus.* This observation of Békésy's gave that phenomenon added significance and established a framework for its investigation and interpretation over the next quarter of a century. It also stimulated Békésy to devote more and more of his own time in later years to a comparative study of inhibition and the contrast effects it produces in practically all of the senses. One of his last papers was concerned with the study of some contour and contrast effects found mainly in Oriental art, which he had come to admire so much during his last years in Hawaii. Indeed, the search for truth and the love of beauty were never far apart in Békésy's life and work. Commenting on his first view of the organ of Corti he wrote:

"I found the inner ear so beautiful under a stereoscopic microscope that I decided I would just stay with that problem. It was the beauty and the pleasure of beauty that made me stick to the ear."

# HONORS

1931 Denker Prize, German Otological Society
1937 Leibnitz Medal, Akademie der Wissenschaften, Berlin
1939 Guyot Prize for Speech and Otology, Gronigen University
1946 Academy Award, Academy of Science, Budapest
1950 Shambaugh Prize in Otology, Collegium Oto-Rhino-Laryn-golicum
1954 Member, American Academy of Arts and Sciences
1955 Warren Medal, Society of Experimental Psychologists
1955 M.D. (*honoris causa*), Wilhelm University, Munster
1956 Member, U.S. National Academy of Sciences
1957 Gold Medal, American Otological Society
1959 M.D. (*honoris causa*), University of Berne
1961 Gold Medal, The Acoustical Society of America
1961 Achievement Award, Deafness Research Foundation
1961 Nobel Prize in Physiology or Medicine
1962 M.D. (*honoris causa*), University of Padua
1963 D.Sc., Gustavus Adolphus College
1965 D.Sc., University of Pennsylvania
1968 D.Sc., University of Buenos Aires
1968 D.Eng. (*honoris causa*), National University of Cordoba
1969 D.Sc., University of Hawaii
1969 M.D. (*honoris causa*), University of Budapest

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*Key to Abbreviations*

Acta Otolaryngol. = Acta Otolaryngolica
Akust. Z. = Akustische Zeitschrift
Ann. Otol. Rhinol. Laryngol. = Annals of Otology, Rhinology, g: Laryngology
Ann. Phys. = Annalen der Physik
Arch. Otolaryngol. = Archives of Otolaryngology
Elektr. Nachr.-Tech. = Elektrische Nachrichten-Technik
Forsch. Fortschr. = Forschungen und Fortschritte
J. Acoust. Soc. Am. = Journal of the Acoustical Society of America
J. Appl. Physiol. = Journal of Applied Physiology
J. Gen. Physiol. = Journal of General Physiology
J. Opt. Soc. = Am. Journal of the Optical Society of America
Percept. Psychophys. = Perception and Psychophysics
Phys. Z. = Physikalische Zeitschrift
Proc. Natl. Acad. Sci. USA = Proceedings of the National Academy of Sciences of the United States of America
Vision Res. = Vision Research
Z. Tech. Phys. = Zeitschrift fuir technische Physik

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Henry B Bigelow

# Henry Bryant Bigelow

October 3, 1879-December 11, 1967

by Alfred C. Redfield

Henry Bryant Bigelow was an accomplished systematic zoologist, being a recognized authority on both the coelenterates and fishes. His 1911 paper on the siphonophores was considered to be the most useful report on this group that had ever been written. In recognition of his later work on the fishes of the western North Atlantic he was awarded the Daniel Giraud Elliot Medal by the National Academy of Sciences in 1948. Of wider impact on the development of marine science was his recognition of the interdependence of the physics, chemistry, and biology of the sea, as exemplified by his studies of the Gulf of Maine and his part in the creation of the Woods Hole Oceanographic Institution, of which he was the first director.

Seventy-five years ago, when Alexander Agassiz visited the Maldive Islands with Henry Bigelow as his assistant, oceanography in America was an interest promoted from time to time through individual initiative and, when in line with their primary duties, by appropriate governmental agencies. Today it is a fully recognized division of science, complete with standard textbooks and special journals. Its work is implemented by many full-scale laboratories and research vessels, operated by university departments or independently. More important, it is a science in which a new viewpoint has developed. This has been the work of many men, but in the United States Henry Bigelow, more than any other, provided the wise leadership that has insured success.

Henry Bryant Bigelow was born in Boston on October 3, 1879. He died on December 11, 1967, in the 89th year of his life, at Concord, Massachusetts, where he had resided for many years. His father was Joseph Smith Bigelow, a banker. On his mother's side his grandfather, Henry Bryant, was a physician, as were two of his uncles and a cousin. Dr. Henry Bryant was also an amateur naturalist, whose extensive collections of hummingbirds and birds' eggs were deposited in the Boston Museum of Natural History. Henry Bigelow was married in 1906 to Elizabeth Perkins Shattuck, who survives. They were saddened by the death of two of their children, Henry Bryant Bigelow Jr., in a mountaineering accident in 1931, and Elizabeth Perkins Bigelow, from an embolism while horseback riding in 1934. Two surviving children are Mary Cleveland Bigelow (Mrs. Lamar Soutter) and Frederick Shattuck Bigelow, M.D.

By good fortune Henry Bigelow was born into a New England community in which the tradition of plain living and high thinking was graced by the fruits of Yankee enterprise. Young men were expected to receive the best of education, supplied in his case by Milton Academy and Harvard College. Intellectual ambitions were not frowned upon and natural tastes for outdoor life were encouraged. Summers at Cohasset, on Massachusetts Bay, gave Henry an instinctive knowledge of seamanship and things of the sea. Hunting in autumn took him to other parts of the coast and the uplands. In his earlier years the mountains were explored, in winter on snow shoes and, in later life, on skis; the mountains were in fact the true love of this oceanographer. And in the spring there were trout in the New England brooks. Thus he became the best-informed naturalist that one could wish to go afield with. His outdoor life was a routine, fixed by the seasons and followed with the same insistence on knowing all that was to be known about any subject that marked his more professional interests.

In *Memories of a Long and Active Life,*[*] written a few years before his death, he recounts in greater detail than space will allow here his experiences as a youth and in later life, including many amusing incidents that he says so often brightened his life. The impression given is that he had hunted a greater variety of game, both in North America and in Ceylon, had fished a greater variety of waters, and had climbed more mountains, from the Matterhorn on down, than is the lot of most sportsmen. The extent of these diversions from his scientific life, shared so far as could be with his wife and children, is indicated in the appended chronology.

Henry graduated from Harvard, A.B. *cum laude* in 1901. In the preceding summer he had gone on the Brown–Harvard expedition to Labrador in company with Reginald Daly and E. B. Delabarre. His first substantial publication, in 1902, was on the birds of the northeastern coast of Labrador. A later one, in 1907, was on hybrid ducks. A study under the guidance of G. H. Parker, published in 1904, on the sense of hearing in goldfish gave him acquaintance with experimental procedures. He received his A.M. in 1904 and Ph.D. in 1906, his doctoral thesis being on a study of the nuclear cycle of *Gonionemus vertens (murbachii* ), made under the supervision of E. L. Mark. He once told me that although he did not pursue cytological studies further this was a valued experience because he first learned from Mark the exacting requirements of scientific work. This was the source of the discipline to which his students were subjected, often to their immediate chagrin but ultimate profit.

It was inevitable that Henry should become a naturalist of some sort but it was not at all clear during his student days that he would become an oceanographer or even a marine biologist. The die was cast by the opportunity to accompany Alexander Agassiz to the Maldive Islands in 1901–1902 and later to the

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[*] Henry B. Bigelow, *Memories of a Long and Active Life* (Cambridge: Cosmos Press, 1964), p. 23.

eastern tropical Pacific and to the West Indies. His assignment was to care for the medusae and siphonophores collected on these expeditions. Thus he gained experience and competence in the classical disciplines of taxonomy which occupied the first decades of his mature career and made him an authority on the coelenterates. Perhaps more important was his introduction to the more general problems of oceanography and the detailed techniques of scientific research at sea.

According to his *Memories* the study of the Gulf of Maine, which established him as a foremost oceanographer, resulted from suggestions by Sir John Murray, who visited Harvard in 1910 and who had been a member of the Challenger Expedition. It followed that in 1912 the U.S. Bureau of Fisheries and the Museum of Comparative Zoology jointly undertook a general oceanographic exploration of the Gulf of Maine which continued under Bigelow's direction through 1924 when the fieldwork was terminated. These explorations resulted in the publication of three superb monographs: on the fishes, the plankton, and the hydrography of the Gulf. The preparation of the monograph on the fishes was far advanced when interrupted by the untimely death of W. W. Welsh, who had given special attention to this phase of the work, and was completed by Bigelow at the request of the Bureau. The other monographs are based entirely on his own work, not only in planning and direction but in the execution at sea, in fair weather and foul, in spite of seasickness and with ships and gear far from adequate.

It is difficult to appreciate today how primitive were the resources available for this work. Thus during 1912 and 1913 reversing thermometers were accurate to only ±0.15° C and the shortage of water bottles required repeated casts for all but the shoalest stations. Limited means were, however, more than compensated by the challenge of the unknown. He wrote:

"Few living zoologists have been as fortunately placed as were we on setting sail on the *Grampus* from Gloucester on our first oceanographic cruise in the Gulf of Maine on July 9, 1912, for a veritable *mare incognitum* lay before us, so far as its floating life was concerned, though the bottom fauna can be described as fairly well-known. Not but what an extensive list of pelagic crustaceans, coelenterates and other planktonic animals had been recorded thence, but everything was yet to be learned as to what groups or species would prove predominant in the pelagic fauna; their relative importance in the natural economy of the Gulf; their geographic and bathymetric variations; their seasonal successions, migrations, and annual fluctuations; their temperature affinities, whether arctic, boreal, or tropic; and whether they were oceanic or creatures of the coastal zone. We even had no idea (incredible though it may seem at this place and day) what we should probably catch when we first lowered our tow nets into deeper strata of Massachusetts Bay, for, so far as we could learn, tows had never previously been tried more than a few fathoms below its surface."*

The outcome was that the Gulf of Maine became perhaps the best known body of water of comparable size in the world, certainly the region most thoroughly explored by individual effort. Michael Graham has stated that the three monographs on the Gulf give a better and more coherent account than that done by many more hands in an area of comparable size. "For one man to have made such a clear and complete job of a relatively large area, . . . was a monumental job of which any man could be proud even if he had done nothing else in his whole life." Graham considered that Henry Bigelow might be called one of the founders of the *new* oceanography, that is "oceanography with an ecological aim, so that instead of the mere description of what there was in the sea there should be an explanation of the interconnections based on a full knowl

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* Henry B. Bigelow, "Plankton of the Offshore Waters of the Gulf of Maine," *Bulletin of the Bureau of Fisheries* 40 (1924):16.

edge and the applications of other branches of science."[*] His achievements as an oceanographer were recognized in 1931, when the National Academy of Sciences awarded him the Alexander Agassiz Medal.

The study of the Gulf of Maine naturally led to intimate contact with Canadians working in adjacent and often overlapping waters. One fruit of this was a close and continuing friendship with Professor A. G. Huntsman, for many years chairman of the Biological Board of Canada; another was Bigelow's association with the North American Council on Fisheries Investigations, in which Canada, Newfoundland, France, and the United States were associated. He attended the meetings of the committee regularly between 1921 and 1933 and served as chairman at all but a few of them.

During this period Henry Bigelow formed associations with the European leaders in oceanography, marine biology, and fisheries; such men as Johannes Schmidt, B. Helland-Hansen, Johan Hjort, Martin Knudsen, Paul Kramp, A. Vedel Taning, Edouard Le Danois, D'Arcy Thompson, Stanley Gardiner, Michael Graham, E. S. Russell, F. S. Russell, Henry Maurice, C. T. Regan, and others. The esteem and affection that he won from these colleagues is shown by the records of the meeting of the International Council for the Exploration of the Sea, which he attended in March 1931, as a representative of the North American Council on Fisheries Investigations and where he reported on the newly founded Woods Hole Oceanographic Institution. They state that:

"The president . . . wished to take opportunity of his being actually present to express to him the satisfaction which his visit had caused to the Council. Dr. Bigelow . . . had attended many council meetings and had so impressed his personality on the

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[*] Michael Graham, "Obituary of Henry Bryant Bigelow," *Deep-Sea Research* 15 (1968):125 (hereafter cited as "Obituary of Henry Bryant Bigelow").

members and experts that the Consultative Committee had passed a recommendation so important that it ought to be specially treated. In effect it contained a standing invitation to the representatives of the Woods Hole Oceanographic Institution and the North American Council on Fisheries Investigations and he might add to Dr. Bigelow personally, whatever his future might be, to attend all meetings of the Council. The Council hoped in future to have many opportunities to consult them, to learn from them and to link up its own investigations with the work done on the western side of the Atlantic."

Henry Bigelow not only served as advisor to the government on fisheries, but also as Special Expert to the U.S. Shipping Board in 1917-1919 and during World War I as an instructor in navigation and as navigation officer on the U.S. Army transport *Amphion.*

He was a member of the National Research Council's Committees on Oceanography (1919-1923) and on Submarine Configuration and Oceanic Configuration (1925-1930), being vice-chairman of the latter in 1930-1932. He served on the National Academy of Sciences' Committees on Oceanography, as secretary (1928-1934) and chairman (1934-1938), on Long Range Weather Forecasting (1931-1935), and for the Murray Fund (1950-1953).

He was special consultant to the Commandant of the Coast Guard for the work of a board comprised of the heads of the agencies interested in the prosecution of scientific studies related to the International Ice Patrol, established in 1913 as a result of the tragic loss of life and property due to the collision of the steamship *Titanic* with an iceberg. During the early years of the patrol observations on plankton, as well as surface temperatures and salinities, were used to trace the drift of water carrying icebergs into the shipping lanes; later the techniques of dynamic oceanography were introduced to estimate on the spot the velocity of the movement. A succession of officers—

of the Coast Guard—came to Cambridge to receive indoctrination in oceanography from him. Largely as a result of his wisdom in guiding the scientific studies on which the work of the ice patrol was based, the hydrography of the northern seas became well understood and the patrol was enabled to discharge its duties with intelligence and success.

During World War II the use of amphibious craft and other small vessels required detailed knowledge of wave conditions for the use of the Armed Services. A popular book entitled *Wind Waves at Sea, Breakers and Surf* by Bigelow (in collaboration with W. T. Edwardson) was written to meet in part this need. In the preface to this book it is stated:

"We wish it expressly understood that we have made no contributions to the theory of waves. But we would not have dared to undertake the task if we had not observed the behavior of waves at sea, from large craft and from small, in various parts of the world, under various conditions of wind and weather; or if we had not had an opportunity to watch the development of breakers—and cope with the smaller sizes—off beaches of various shapes, off rocky coastlines, and over submerged ledges." This insistence on personal experience as a prerequisite of scientific judgment (or any other judgment for that matter) was characteristic.

The establishment of an oceanographic institution on the east coast of the United States originated in conferences beginning in 1924 between Wickliffe Rose, then president of the General Education Board, and Frank R. Lillie, the director of the Marine Biological Laboratory at Woods Hole. The outcome was that the president of the National Academy of Sciences was requested to appoint a Committee on Oceanography to consider the share of the United States in a world-wide program of Oceanographic Research.[*] Dr. Lillie was the chairman of this

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[*] Frank R. Lillie, *The Woods Hole Marine Biological Laboratory* (Chicago: University of Chicago Press, 1944), p. 177.

committee and served as president of the Woods Hole Oceanographic Institution upon its establishment. Henry Bigelow was engaged by the Committee as its secretary to prepare its report. No one could have been found so well equipped by personal experience or general ability for the task. The greater part of the report, reviewing the scope, problems, and applications of oceanography, has been made public in a book entitled *Oceanography,* published under his name in 1931. It is in the unpublished sections of this report, however, in which are set forth the principles that should determine the type of organization which would best remedy the then-present handicaps to the development of oceanography, that his genius for striking directly at the heart of any question and his power of exposition are displayed. It is no wonder that this report was received with confidence, or that it led to the establishment of a new institution at Woods Hole and to substantial benefits to oceanography and marine biology through gifts to the Scripps Institution, the University of Washington, and the Bermuda Biological Station.

The principle of the ripeness of time, as applied to the appearance of prophets, is well illustrated by the history of oceanography during this period. Not only did a man emerge who had prepared himself, perhaps unwittingly, for leadership at a time when men of influence sensed that something should be done to improve the status of marine science in America, but new ideas were in the air, wafted across the ocean from a multitude of general scientific advances. Henry Bigelow, though trained in the classical tradition, was sensitive to these breezes, wise enough to grasp their implication, and bold enough to act on their meaning.

The following paragraphs express in his own words the creed that was to guide his thinking:

"Oceanography has of late entered a new intellectual phase, to explain which a word of retrospect is necessary. . . . Students of the history of science may well date the birth of modern oceanography from December 21, 1872, the day when the *Challenger* set sail from Portsmouth, England, on her memorable voyage. . . . One great deep-sea expedition led to another, and more was learned about the sea during the last thirty years of the nineteenth century than had been during the preceding three thousand. But after a time, as so often happens when some scientific discipline takes a sudden spurt, this fact-catching began to lose something of its freshness. . . .

"Students began, in short, to feel that the mere accumulation of facts from the sea, when there is an inexhaustible supply, may actually become a bit sterile, just as catching fish is to a sportsman where fish are too plentiful. . . . So it was natural that when persistence in the old methods no longer yielded startling discoveries, signs could be seen of the approach of a period of stagnation. . . . And oceanography would probably be in a moribund state in America today, just as the art of sailing a square-rigger is, but for the birth of the new idea that what is really interesting in sea science is the fitting of these facts together, and that enough facts had accumulated to make the time ripe for an attempt to lift the veil that had obscured (and still obscures) any real understanding of the marvelously complex and equally marvelously regulated cycle of events that takes place within the sea.

"The foundation for this conscious alteration in view-point, from the descriptive to the explanatory, was a growing realization . . . that in the further development of sea science the keynote must be physical, chemical and biological unity. . . .

"When one picks up a fish, one may be said, allegorically, to hold one of the knots in an endless web of netting of which the countless other knots represent other facts, whether of marine chemistry, physics or geology, or other animals or plants. And just as one can not make a fish-net until one has tied all the knots in their proper positions, so one can not hope to comprehend this web until one can see its internodes in their true relationship. This is today the conscious aim of oceanographers."*

Henry Bigelow became the first director of the Woods Hole Oceanographic Institution in 1930, a position he held for ten years. His task in assembling a staff for the new institution was not an easy one, for there was little raw material with which to work. There were a few young men with some experience at sea, and by combing the museums of the country doubtlessly he could have assembled a respectable group of experts on special groups of marine organisms. A primary objective, however, was to give impetus to oceanographic studies in the universities, and there was the "developing viewpoint" to be fostered. He chose the bolder course of recruiting from the universities a new generation of chemists, meteorologists, physiologists, bacteriologists-whoever could be persuaded that scope for their skills could be found in studies at sea. The practice grew that each should make at least one short voyage at sea each season. Daily the director made his rounds, instilling little by little something of his viewpoint and wisdom on the opportunities that lay beyond the tide line. Boldness was encouraged. We were told that an oceanographer, like a turtle, makes progress only by sticking his neck out.†

On retirement as director in 1939, Henry Bigelow became a regular member of the Board of Trustees, on which he had previously served *ex officio.* On reaching the statutory age of retirement in 1952 he became an honorary trustee, and in 1960, in recognition of his great services to the institution, was named Founder Chairman of the Board. A chair in oceanography was founded in his name by the Woods Hole Oceanographic Institution in 1958, to which his former student, C. O'D. Iselin, was appointed.

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* Henry B. Bigelow, "A Developing Viewpoint in Oceanography," *Science* 71 (1930):85-86.

† Personal impressions and recollections of Henry Bigelow by many of his associates are recorded in the July 1968 issue of *Oceanus.*

On the occasion of the twenty-fifth anniversary of the founding of the Woods Hole Oceanographic Institution, a festschrift entitled "Papers in Marine Biology and Oceanography" was dedicated to Henry Bryant Bigelow by his former students and associates. It was published as a supplement to volume 3 of *Deep-Sea Research* for 1955. In acknowledging my contribution and referring to the biographical foreword, he wrote "my only criticism of which is that it makes me out a more important personage than I really am."

Henry Bigelow's association with Harvard University was not interrupted by the interlude in which he was actively concerned with the Oceanographic Institution. He resided in Woods Hole only during the summers. It was his pride that he had been in active service to Harvard University for fifty-five years, a period he thought must have broken some sort of record. His *Memories* record that in recognition he was presented with a bottle of bourbon whiskey "with the compliments of the President and Fellows." He considered himself to be unique, for no one else had ever before been presented with a bottle of whiskey by Harvard University.

His Harvard association was primarily with the Museum of Comparative Zoology, first as an assistant to Alexander Agassiz, then as curator of coelenterates (1913-1925), research curator (1925-1927), and curator of oceanography (1927-1950). He did not formally relinquish the last of these responsibilities until 1962 when he retired from the Museum faculty. He continued to work at the museum until his death. He was appointed Lecturer in Harvard University in 1921, Associate Professor of Zoology in 1927, Professor of Zoology in 1931, and Alexander Agassiz Professor of Zoology in 1944. He retired from the Harvard faculty in 1950, at which time he became emeritus.

During his service as assistant in the course in elementary zoology, Henry encountered a student who had drawn a tunicate fully equipped with a set of mammalian viscera, which the student accounted for as derived from "natural logic" rather than observation. Legend has it that Henry was so enraged by this heresy that he told off the unfortunate student in expletives so unambiguous that Henry was told he never again would be allowed contact with the students of Harvard College. Perhaps this explains why many years elapsed before he was to present a formal course of instruction in the university. Be that as it may, he gave a course in oceanic biology beginning in 1931, followed some years later by one in invertebrate zoology.

Among his advanced students were Columbus O'D. Iselin, who succeeded him as director of the Woods Hole Oceanographic Institution; Edward H. Smith, its third director; Mary Sears, for many years clerk of its corporation and an editor of *Deep-Sea Research;* and Oscar E. Sette, Robert A. Nesbit, William C. Schroeder, William C. Herrington, and Lionel A. Walford-all distinguished in the fisheries service.

In 1939 the Sears Foundation For Marine Research of Yale University sponsored an ambitious cooperative publication on the fishes of the western North Atlantic for which participation by ichthyologists from throughout the United States was invited. Henry Bigelow served as Editor in Chief and with his close associate, William C. Schroeder, contributed extensively. The first volume of the publication elicited the following comment by Carl Hubbs:

"The first volume of Fishes of the Western North Atlantic sets a very high standard—perhaps so high a standard as to render difficult the completion of subsequent volumes by authors less well equipped than Henry Bigelow and associates in courage, energy, time, meticulousness, experience, library facilities and willingness to sacrifice much else for this one grand task. . . . In several ways this volume has been successfully adapted, in line with the policy set for the series, for the use and interest of sportsmen and general naturalists as well as ichthyologists. Features that lead to this desirable end . . . include the excellent summaries of natural history information, the limited treatment of internal and particularly microscopical anatomy, the simplified keys, the complete coverage of the species, in the clear-cut illustrations."*

Henry Bigelow's achievements were recognized not only by the award of the Alexander Agassiz and the Daniel Giraud Elliot Medals of the National Academy of Sciences, but also by the award of the Bowie Medal of the American Geophysical Union (1944), the Johannes Schmidt Medal of the Carlsburg Foundation, Copenhagen (1947), and the Monaco Medal of the Institut OcCanographique, Paris (1950). He was also the first recipient of the Henry Bryant Bigelow Medal established in his honor by the Woods Hole Oceanographic Institution in 1960.

He was elected to the National Academy of Sciences on April 28, 1931. He was also a member of the Norske Videnskaps Academi, the Royal Geographical Society of London, the Zoological Society of London, the Marine Biological Association of the United Kingdom, the Russian State Geographical Society, the American Academy of Arts and Sciences, the American Philosophical Society, and the Academy of Natural Sciences, Philadelphia. He held honorary degrees from the Universities of Oslo, Yale, Harvard, and Rhode Island.

While it is easy to recount the achievements for which Henry Bigelow was honored, it is impossible to give an adequate picture of the personality that made them possible. Michael Graham has written: "I feel that one of his great qualities was that he had a definite effect on everybody who worked near him or dealt with him in any way." Referring to meetings of the International Council, Graham continued: "Throughout the proceedings the conference was richer whenever he was present ... One had the feeling that he was a man

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* Carl L. Hubbs, "Fishes of the Western North Atlantic (a review)," *Copeia* (1949):155-57.

of such excellence and such exceeding pleasantness that not for a moment would one relax in the effort to do one's very best in order to support him as far as possible."* Professor Huntsman once said of him: "Like all of us, he has his own peculiar personality, but his is more peculiar than that of anyone else I know—a queer combination of lively humor and deadly seriousness—his way is inimitable."

He was an utter realist in his respect for apparent facts. In considering any question he had a unique ability to strike through to the pertinent point. He appeared to be completely impersonal in his judgments and to never make a foolish decision or give unwise advice. If he were ever mistaken, it was because some needed facts were missing. Though his excitement was in the discovery of new facts, he was not one to be concerned primarily with learning more and more about less and less. He wanted to know more about the facts and to comprehend the relations between the primary considerations. Thus, he encouraged new fields of inquiry and broadened our conception of what marine science could be.

Among the facts he recognized was the diversity and frequently the folly of his fellow men. This was the theme of the humor with which he confronted the world. "Thinking fellows" and "silly clucks" were among the categories into which he put us. His daily rounds of the laboratory at Woods Hole, in which he assessed our capabilities and kindly corrected our follies, made him a great director. The proverbial uncle is an elder member of a family, detached from its petty turmoil, but kind and wise, to whom the youngsters turn for understanding and counsel. As such, he is remembered with affection and gratitude by his associates at the Museum of Comparative Zoology and the Woods Hole Oceanographic Institution, who knew him as Uncle Henry.

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* Graham, "Obituary of Henry Bryant Bigelow," p. 125.

# CHRONOLOGY*

1879 Born October 3

1895 Graduated from Milton Academy

1896 Studied at Boston Museum of Natural History

1897 Entered Harvard College

1898 Hunting in New Brunswick and Prince Edward Island

1900 Member, Brown-Harvard Expedition to Labrador

1901 A.B. *cum laude* Harvard College

Published note on the American eider in Virginia

1901-1902 Member, expedition to Maldive Islands with A. Agassiz

1902 Hunting in Ceylon

Published on the birds of Labrador

1903 Studied beach sands at Bermuda

1904 Hunting mountain sheep and goats in British Columbia

1901-1905 Member, expedition to Eastern Pacific with A. Agassiz

1905 Hunting moose in Quebec

1906 Ph.D. in Zoology, Harvard

Appointed Assistant, Museum of Comparative Zoology

Married Elizabeth Perkins Shattuck

Canoeing with Mrs. Bigelow in Newfoundland

1907 Elected Fellow, Royal Geographical Society, London

Member, expedition to West Indies with A. Agassiz

1908

Camping in New Brunswick

Moved to Concord, Massachusetts

1910-1913 Duck shooting yearly in North Carolina, Virginia and on Lake Erie

1911 Elected member, :American Academy of Arts and Sciences

Salmon fishing in Quebec

1912-1924 Cruises in Gulf of Maine and adjacent waters

1913 Appointed Curator of Coelenterates, Museum of Comparative Zoology

Appointed Consultant on International Ice Patrol

Visited coast of Louisiana for Rockefeller Foundation to advise on land use for game preserves

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* The dates of recreational activities, taken from Dr. Bigelow's *Memories of a Long and Active Life,* may be subject to some error in recollection.

1913-1964 Trout fishing yearly at Wareham, Massachusetts

1916 Hunting in Montana with Mrs. Bigelow

1917-1919 Served as Special Expert to U.S. Shipping Board

1918 Served as Acting Navigator, U.S. Army Transport, *Amphion*

1919-1923 Member, National Research Council Committee on Oceanography

1921 Appointed Lecturer, Harvard University

1921-1932 Member, North American Council on Fisheries Investigations

1925 Appointed Research Curator, Museum of Comparative Zoology

Published "Fishes of Gulf of Maine"

1925-1932 Member, National Research Council Committee on Submarine Configuration and Oceanic Configuration

1926 Published "Plankton of Gulf of Maine"

1927 Published "Physical Oceanography of Gulf of Maine"

Appointed Associate Professor of Zoology, Harvard University

Appointed Curator of Oceanography, Museum of Comparative Zoology

1928-1938 Member, Committee on Oceanography of National Academy of Sciences

1928 Mountaineering in Canadian Rockies

1930 Appointed Director of Woods Hole Oceanographic Institution

Mountaineering in Switzerland, climbed Matterhorn

1931 Elected member of National Academy of Sciences

Awarded Agassiz Medal of National Academy of Sciences

Appointed Professor of Zoology, Harvard University

1931-1935 Member, National Academy Committee on Long Range

Weather Forecasting

1932 Elected corresponding member, Academy of Natural Sciences, Philadelphia

Fishing in Nova Scotia

1933 Hunting in Nova Scotia

Skiing at Innsbruck

1937 Elected foreign member, Norski Videnskaps Academi

Elected honorary member, Marine Biological Association of the United Kingdom

Elected member, American Philosophical Society

Fishing in southern Florida

1939 Resigned as Director of Woods Hole Oceanographic Institution and elected member and President of Board of Trustees

1939-1963 Became Editor in Chief and contributor to "Fishes of Western North Atlantic"

1941 Given Honorary Sc.D., Yale

1944 Awarded Bowie Medal of American Geophysical Union

1946 Given Honorary Ph.D., Oslo

Given Honorary Sc.D., Harvard

1947 Awarded Johannes Schmidt Medal, Copenhagen

Published "Wind Waves at Sea, Breakers and Surf"

1948 Published chapters of "Fishes of Western North Atlantic" on cyclostomes and sharks

1949 Awarded Elliot Medal of National Academy of Sciences

1950 Appointed Professor of Zoology Emeritus, Harvard University

Awarded Monaco Medal of Institut Oceanographique, Paris

1950-1953 Member, Murray Fund Committee of National Academy of Sciences

1952 Retired as Trustee, Woods Hole Oceanographic Institution and elected Honorary Trustee

1953 Published chapters "Fishes of Western North Atlantic" on sawfishes, guitarfishes, skates, rays and chimaeroids

1955 Presented festschrift by former students and associates

1956 Visited Northern Ontario with family

1958 Bigelow Professorship in Oceanography established by Woods Hole Oceanographic Institution

1960 Given Honorary Sc.D., University of Rhode Island

Awarded Henry Bryant Bigelow Medal of Woods Hole

Oceanographic Institution

Appointed Founder Chairman of the Board of Trustees, Woods Hole Oceanographic Institution

1962 Retired from Faculty of Museum of Comparative Zoology

Visited Puerto Rico with Mrs. Bigelow

1963 Visited Jamaica with Mrs. Bigelow

Publication of "Fishes of Western North Atlantic" completed

1966 Skiing in New Hampshire with daughter Mary

1967 Died December 11

1968 Last papers on fishes published

# Bibliography

*Key to Abbreviations*

Biol. Bull. = Biological Bulletin

Bull. U.S. Bur. Fish. = Bulletin of the United States Bureau of Fisheries

Bull. Mus. Comp. Zool. = Bulletin of the Museum of Comparative Zoology, Harvard College

Bull. Natl. Res. Counc. = Bulletin of the National Research Council

Harv. Alumni Bull. - Harvard Alumni Bulletin

J. Wash. Acad. Sci. = Journal of the Washington Academy of Sciences

Mem. Mus. Comp. Zool. = Memoirs of the Museum of Comparative Zoology, Harvard College

Mem. Sears Found. Mar. Res. = Memoirs of the Sears Foundation of Marine Research

Pap. Phys. Oceanogr. Meteorol. = Papers in Physical Oceanography and Meteorology

Proc. Boston Soc. Nat. Hist. = Proceedings of the Boston Society of Natural History

Proc. New Engl. Zool. Club = Proceedings of the New England Zoological Club

Proc. U.S. Natl. Mus. = Proceedings of the United States National Museum

1901 A Virginia record for the American eider (*Somateria dresseri*). Auk, 16:189.

1902 Birds of the northeastern coast of Labrador. Auk, 19:24-31.

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Eliot Blackwelder

# Eliot Blackwelder

June 4, 1880-January 14, 1969

by Konrad B. Krauskopf

In 1963 Eliot Blackwelder filled out a questionnaire sent to him by the Eugenics Record Office of the Carnegie Institution and by the Eugenics Society of America. The questionnaire was a long one, requesting data about his family back to his grandparents, especially regarding occupations, diseases, longevity, "special gifts or peculiarities of mind or body," and "character, favorite studies, and amusements." The fact that he would fill out such a form with scrupulous care betrays much about his character—his attention to detail, his willingness to give time and effort to a project that he thought might ultimately benefit society, and his conviction that social problems needed the application of science for their solution.

The document gives an intimate glimpse of a stalwart American family. Eliot's paternal grandparents came from North Carolina, but spent most of their lives on a farm in central Illinois. His father, leaving the farm early in life to take a position as county clerk, ultimately became a company executive in Chicago. His mother, daughter of a minister in upstate New York, went west to Kansas as a university student. All the family, according to the questionnaire, were people of substance—successful farmers, schoolteachers, ministers, businessmen, clubwomen. Most lived to a ripe old age, had large families, and suffered little from disease. They are described as kindly, generous, optimistic, and strong in their sense of duty and in the enforcement of discipline. They had lively intellectual interests, many of them especially in history and biography, and they took an active part in community affairs. Altogether it is an enviable eugenic background. When one finds that Eliot has put down after his own name in the questionnaire, "studious rather than sociable . . . interested in social and economic affairs, especially history," the influence of his forebears is clear.

The one trait that does not seem to fit this heritage is his fascination with science. He describes his father specifically as "not scientific," and of his mother he says only that she had "an early interest in natural history." Otherwise there is no mention of science in the document. Yet somehow this city-born son of a business executive and a prominent Chicago clubwoman developed at an early age what he described as "a liking for the out-of-doors, tennis, hunting, and an interest in sciences." The interest was so strong, even in boyhood, that he once assembled a collection of more than 6000 specimens of beetles and butterflies. By the age of fifteen his accomplishments in ornithology gained him membership in the American Ornithological Union. These interests in birds and insects remained strong throughout his life.

When he attended the University of Chicago at the turn of the century, however, the long-time family concern with history reasserted itself. From many courses in Latin and Greek came a great love of classical antiquity, and his first choice of a major subject was in this field. The interest in ancient Greece and Rome persisted in later life, giving him a reputation as an amateur classicist and providing him with a wealth of apt quotations from classical authors.

In his senior year at the university, he was wooed away from the classics by the inspired teaching of R. D. Salisbury, and his dedication to geology as a career was confirmed by two summer-long expeditions with Professor Salisbury to the Rocky Moun tains shortly after graduation in 1901. These trips introduced him to the mountains and deserts of the American West, which were to become the focus of most of his professional work.

After two years of teaching geology at the University of Chicago, Blackwelder accepted an exciting invitation to accompany Bailey Willis on an expedition to China under the auspices of the Carnegie Institution of Washington. Together with a topographer, Harvey Sargent, he and Willis made their way to the Orient via Europe and Russia. During the numerous stopovers in Europe, including attendance at the International Geological Congress in Vienna, the young man established personal contact with many noted geologists. These contacts, several of which were to ripen into firm friendships, were the beginning of the wide circle of acquaintances that he maintained all over the world. After the sojourn in Europe, the three-man expedition had a long and memorable ride to China on the newly built Trans-Siberian Railroad. The route in China went from Peking to Tsingtao, then west to the ancient capital of Hsian in Shensi Province, south across the high Tsinling Range to Ichang on the Yangtze River, and down the Yangtze by boat to Shanghai. The expedition logged some 3000 miles by foot, pony, boat, and train. It made an indelible impression on young Blackwelder's mind, providing him with a rich background of personal and professional experience. Not only the geology, but also the lives and social institutions of the Chinese people, excited his sympathetic interest; and he followed the later political developments in China with sorrow and misgivings.

On his return to America, Blackwelder married Jean Bowersock, daughter of a prominent Kansas family, whom he had known from childhood. For several years he was on the faculty of the University of Wisconsin, becoming a full professor at the age of thirty, before obtaining his Ph.D. degree from Chicago in 1914. From 1916 to 1919 he served as head of the geology department at the University of Illinois. Then followed a brief period of apparent indecision. He went to Stanford University as a visiting professor, but left the same year to accept a position with the Argus Oil Company in Denver. After less than two years as a petroleum geologist, he returned to an academic post at Harvard, then in 1922 accepted an invitation from Stanford to succeed Bailey Willis as head of its geology department. This position he held for twenty-three years, until his retirement in 1945.

The move to Stanford was an important turning point in his career. Before that time he had spent most summers in fieldwork with the U.S. Geological Survey in the western states and Alaska, work that resulted in many descriptive papers on the mountains of Wyoming and the glacial geology of the Alaskan coast. From this long field experience, he also gained the background for more general papers on the geologic history of the continent and for some memorable shorter papers on specific geological topics. These latter papers, continually pointed out to students as models of clear geologic reasoning and exposition, include a variety of subjects: the geologic role of phosphorus; the interpretation of unconformities, the breaks in a sedimentary sequence that indicate times when the land was undergoing erosion; the origin of the Bighorn dolomite; and the effect of climate on the characteristics of continental sediments. In each article the author describes in simple, straightforward language the status of current thinking about subjects that at the time were highly controversial, carefully analyzes the semantic confusion that so often obscured the actual argument, marshals his observations from the field to support his preferred hypothesis, and then points out candidly the places where his ideas are weakest. The papers are not profound, but they represent a clarification of basic geologic thinking at a time when such clarification was sorely needed.

Blackwelder's breadth of interest, as indicated by titles of papers from this early period, seems extraordinary in our modern age of specialization. Geomorphology and sedimentation, then as later, were his major concerns, but he writes easily and well about fossils, climate, structural geology, Precambrian rocks, and phosphate deposits. In the early 1920s, when he evidently thought fleetingly of becoming a petroleum geologist, he wrote a paper on oil domes and another on oil movement. At the early age of thirty-one, he coauthored an elementary textbook, which combined physical and historical aspects of geology—a combination that is touted by publishers of some modern texts as a radical innovation in geology teaching. From a modern viewpoint the book seems curious, because it limits the discussion of igneous rocks, metamorphic rocks, volcanoes, and earthquakes to a few pages in the first chapter and concentrates on geomorphology and sedimentary rocks. The skewed emphasis suggests the direction of Blackwelder's interests, and the simple unadorned style with many carefully phrased definitions is revealing of his ideas about pedagogy.

The new base at Stanford after 1922 provided an opportunity for research in two directions that over the years became major lines of activity: the history of glaciation in the Sierra Nevada and the development of desert landscapes. At first in frequent brief abstracts, then in longer definitive papers, his bibliography records the progress of his thinking on these subjects.

In the Sierra Nevada, especially on its semiarid eastern side where the evidence of glaciation is not obscured by forests, he could distinguish the debris and erosional forms produced by three major advances of ice tongues down the steep valleys and the still older debris left on a few ridge crests from an earlier advance. A similar periodic waxing and waning of glacial activity during the Pleistocene had been established for the ice cap that once covered the central part of the continent, but Blackwelder faced the necessity of working out criteria for age assign ments that would be applicable in the very different environment of mountain glaciers. Once the criteria had been developed and tested in the Sierra Nevada, he found them useful for studying glaciation in other ranges of the Great Basin and in the Rocky Mountains. Thus he could correlate glacial events over most of the West and, more tentatively, could suggest correlations with the better-known chronology of the midcontinent. The glacial story of the Sierra Nevada has been much refined since Blackwelder's work, but the major divisions he established remain the framework for more recent research.

The peculiarities of erosional and depositional processes in deserts have long fascinated geomorphologists, but Blackwelder's special gifts of keen observation and critical analysis proved particularly valuable for their study. Hardly an aspect of desert topography escaped his attention, but he is best known for four contributions: demonstrating the fallacy of the then-prevalent notion that much desert weathering is accomplished by diurnal temperature changes, clarifying the role of wind in shaping desert features, noting the importance of mudflow deposits in desert sediments, and emphasizing the distinction between broad erosional surfaces (pediments) and depositional surfaces (bajadas) as parts of desert plains.

Blackwelder's wide-ranging interests, although emphasizing mountain glaciation and desert processes, touched many other subjects during his years at Stanford. He pointed out the abundant, but far from obvious, evidence for Pleistocene lakes in many of the now-dry valleys of the Great Basin; demonstrated the recency of formation of the valley that the Colorado River has carved across Utah and Arizona; marshaled evidence for an impact origin of Meteor Crater; and showed that certain peculiar sedimentary rocks in many of the western mountains are best explained as formed from debris left by glaciers far back in geologic time. He wrote notes about earthquakes, landslides, and finds of vertebrate fossils. Occasionally, too, he contributed short papers summarizing and clarifying specific topics: exfoliation in rock weathering, the distinction between fault scarps and fault-line scarps, the anatomy of desert plains, and the insolation hypothesis of rock weathering. Like his earlier papers of this type, these have become classics in geologic literature, less for their originality or profundity than for the crystal clarity with which they illuminate subjects that previously had seemed bogged down in controversy.

Involvement in scientific work never completely overshadowed Blackwelder's deep concern for the welfare of society. During the 1930s, the long drawn-out depression and the increasingly ominous events in Europe occupied more and more of his thinking. When in 1940 he faced the problem of choosing a subject for his presidential address to the Geological Society of America, he abandoned geological topics for a subject that would enable him to express some of his social concerns, "Science and Human Prospects."

The address is an interesting document. One theme is a forceful expression of the old idea that education is the only "sure cure" for the ills of democracy. This is coupled with worry about the quality of teachers produced by schools of education, particularly about their meager training in science. The scientific method, he points out, is difficult to learn, and an appreciation for it is woefully lacking among the general populace. A prime goal of education should be to supply this lack. A widespread knowledge of science, or at least an understanding of its capabilities, is particularly important because science will increasingly be called on to solve social and political problems. Without the popular support for science that comes from recognition of its accomplishments, further social progress will be impossible. Many other species before *Homo sapiens,* he reminds his audience, have reached a limit in their development and then declined toward extinction; perhaps mankind, without an adequate understanding of science, has reached that sort of turning point. It is only in the last paragraphs, with a suggestion that humanity should take warning from the fate of its predecessors, that a geological theme emerges.

One hesitates to criticize an effort of this sort. There are many germs of truth in it, and much noble sentiment. If it sounds in places naive and simplistic, in view of the disillusioning experiences of the past thirty years, perhaps this is only the inevitable reaction of any generation to an expression of ideals and aspirations by the generation that came before. A scientist seldom has the courage to express his thoughts on such subjects to a large audience, and when he does he should be applauded rather than exposed to the captious comments of those with the great advantage of hindsight.

So disturbed was Dr. Blackwelder by the war and the uncertainties of the postwar world that after retirement, in 1945, he renounced geology in order to give his entire attention to efforts toward establishing a durable peace. For many years he played an active role in Atlantic Union, an organization whose goal was a political union of the principal democracies. During these years his only geologic activities were occasional attendance at meetings, publication of a few abstracts based on earlier work, and preparation of articles for guidebooks and symposium volumes. Among the latter, the two papers he contributed to the volume *Geology of Southern California* are notable as lucid summaries of his ideas on geomorphic processes in the desert and on Pleistocene lakes of southeastern California.

The last years of his life were made difficult by Parkinson's disease, a debilitating illness that slowly sapped his physical strength, while his mind remained alert and active. To the very end he retained an interest in national and international politics, in affairs of the university, in geologic organizations, and in the many kinds of birds that came to the feeder outside his bedroom window. For one so active in earlier life, it must have been galling in the extreme to lie helpless in bed while the months stretched into years. But there was never a word of complaint—only now and then a touch of rueful humor about his progressive incapacities. The Stoic philosophers, whose writings he had once admired, would have found him an apt disciple.

Dr. Blackwelder regarded himself as primarily a field geologist, and it was indeed by long and careful observation in the field that he made his great contributions to science. His special talents were those of a disciplined observer—the ability to see at once, almost intuitively, the essentials of a situation, to sort out the important from the unimportant, to relate one observation to another, to sense what additional observations would be needed in the field or laboratory. Beyond this was his capacity for clear, direct thinking and for expressing complex ideas in simple words. His genius was not to generate spectacular or revolutionary concepts, but patiently, unobtrusively, always with a keen eye for his own inadequacies, to bring order and good sense into disputed areas of science. Many of his discoveries seemed so difficult to make—then so simple and obvious once they were pointed out—the sort of discoveries that are taken up at once into the body of a science and quickly become textbook dogma, with their discoverer all but forgotten.

In teaching as well as research, Blackwelder's emphasis was always on accurate observation. Students fortunate enough to have worked with him will long remember his gentle insistence and their frustration at being told to examine a specimen repeatedly, even after they were sure they had seen all there was to see. And especially, they will remember the glow of satisfaction that came when they had finally spotted the missing detail and the dawning realization that they were being trained to see as they had never seen before.

To casual acquaintances Dr. Blackwelder often seemed reserved and a little austere, but at heart he was a kindly person, patient and helpful to students and attentive to the needs and wishes of colleagues. He was a man of principle, well disciplined himself and unhappy but not critical when those around him showed lack of discipline. Like his forebears, he believed firmly in the virtues of hard work and of striving to make the world a better place.

He was an active member of many societies and served as an officer in three: President (1921) of the Geology and Geography Section of the American Association for the Advancement of Science; Vice-President (1934 and 1939) and President (1940) of the Geological Society of America; and Vice-President (1945-1946) and President (1947-1949) of the Seismological Society. He was a member of the American Association of Geographers, the American Association of Petroleum Geologists, the Washington Academy of Science, and the California Academy of Science.

Honors came to him in abundance. He was a member of the National Academy of Sciences (elected in 1936) and an honorary member of the American Philosophical Society, the Geological Society of London, the Geological Society of Belgium, the German Geological Association, and the Geological Society of China.

Dr. Blackwelder survived his wife by nearly three years. The couple had celebrated their sixtieth wedding anniversary in 1964. Surviving Dr. Blackwelder at the time of his death were his brother Paul, of St. Louis, Missouri; seven children; sixteen grandchildren; and thirteen great-grandchildren. The children are: Dr. Richard Blackwelder, Professor of Zoology at Southern Illinois University; Justin Blackwelder of Washington, D.C.; Mrs. Margery Alden, Mrs. Trude Ball, Mrs. Lois Fuller, and Mrs. Ruth Lanz, all of Palo Alto, California; and Mrs. Martha Merk of Portola Valley, California.

Eliot Blackwelder will be remembered by all who knew him as a dedicated scientist and as a true "gentleman of the old school"—dignified, courteous, generous, always considerate of the views of others, never losing his basic humility despite the recognition and honors that came to him during a long and distinguished career.

In compiling this memoir I have had the assistance of Mrs. Martha Merk and my colleagues Arthur D. Howard and Ben M. Page.

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*Key to Abbreviations*

Am. J. Sci. = American Journal of Science

Bull. Am. Assoc. Pet. Geol. = Bulletin of the American Association of Petroleum Geologists

Bull. Geol. Soc. Am. = Bulletin of the Geological Society of America

Bull. Seismol. Soc. Am. = Bulletin of the Seismological Society of America

Bull. Utah Geol. Mineral. Surv. = Bulletin of the Utah Geological and Mineralogical Survey

Econ. Geol. = Economic Geology

Int. Geol. Congr. = International Geological Congress

J. Geol. = Journal of Geology

J. Wash. Acad. Sci. = Journal of the Washington Academy of Science

Pan-Am. Geol. = Pan-American Geologist

Proc. Geol. Soc. Am. = Proceedings of the Geological Society of America

U.S. Geol. Surv. Bull. = U.S. Geological Survey Bulletin

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H A Borthwick

# Harry Alfred Borthwick

January 7, 1898-May 21, 1974

by Sterling B. Hendricks

Harry Borthwick was born in Otsego, Minnesota, a small village about thirty miles from Minneapolis. His mother, Frances, was the aunt of Hubert Humphrey, who became Vice-President of the United States, and the sister of Harry B. Humphrey, a leading phytopathologist. It was the latter who influenced Harry to enter the School of Agriculture of the University of Minnesota in 1917. When his parents moved to San Jose, California, in 1919, Harry transferred to Stanford University, where he majored in botany. After receiving a B.A. degree in 1921, he continued in graduate school with research interest in plant morphology, leading in 1924 to an M.S. degree. Harry became a research assistant in the Division of Botany of the Agriculture School of the University of California at Davis in 1922, shortly before his marriage to Myrtis Hall.

At Davis, Harry was first an assistant to and later a close associate of E. C. Robbins, a botany teacher and author of a well-known textbook in that field, who was engaged in research on crop plants. Harry continued working toward a doctorate at Stanford, devoting his attention mainly to the reproduction and development of both higher and lower plants. The position at Davis also required attention to basic and applied aspects of vegetable crop plants. These endeavors fashioned the pattern of his later scientific efforts. The main themes were: A wide knowledge of the functioning of plants of all types, with close attention to minute details of form and development and concern for ways to turn the more basic work to practical use in agriculture. Harry and Katherine Esau divided the work at Davis on the development of the vegetable crops of California—asparagus, beans, sugar beets, and carrots, among others.

One of the first undertakings at Davis was the study of the development of lettuce from ovum fertilization to seed maturity. This classic work, often referred to over the course of the last fifty years, also served as a background for later work on seed germination. In the early 1930s, Harry worked on thresher injury to beans and on carrot seed development. The latter study was undertaken with L. T. Emsweller, who left Davis in 1935 to take charge of work in floriculture at the U.S. Department of Agriculture station in Beltsville, Maryland.

Shortly after Emsweller went to Beltsville, the Congress passed what was known as the Bankhead-Jones Act, providing for research in depth in several aspects of agriculture. One of the most fundamental discoveries in biology had been made in 1920 in the Department of Agriculture by H. A. Allard and W. W. Garner. This was photoperiodism, or dependence of plants and animals on the length of day. In 1936 a decision was made to establish under the Bankhead-Jones Act a small group to look further into the nature of photoperiodism and its significance in agriculture. Because the photoperiodic response in plants partly regulates flowering, it was thought that progress might best result from attention to the morphological aspects involved in plants changing from vegetative to reproductive growth and to the underlying physiology. Emsweller recommended Harry Borthwick to undertake the work. Marion W. Parker, who was then teaching plant physiology at the nearby University of Maryland, was invited to join the effort.

Borthwick and Parker foresaw that an understanding of photoperiodism would probably require growing plants under closely controlled environmental conditions in order not to confound responses to light with temperature and other changes. Such controlled conditions, which are now commonplace, had not previously been obtained except in a very limited way at the Boyce-Thompson Institute. They decided that a.c. carbon arc lamps, developed for treatment of tuberculosis, were the only available type giving the required high light intensities. Growth rooms were constructed with such lamps as light sources, but the plants grown in them were poorly developed compared to those from the field. Because the rare-earth loaded carbons gave radiation that was relatively more intense in the blue parts of the spectrum than in the red, Borthwick and Parker experimented with supplementary radiation from incandescent filament lamps to enhance the red. The resulting growth of the selected plants was entirely satisfactory and of low variability. The high requirement for red light anticipated what later became known as the high energy reaction for plant growth, the exact nature of which is still much debated.

On H. A. Allard's recommendation, a soybean and cocklebur variety sensitive to light and requiring short days for flowering was selected. Barley *var. Wintex* was chosen as a long-day plant. The findings by others that the leaf was the receptor organ for the effective light and that transport of the stimulus to the terminal of the plant required phloem continuity were soon verified. A major discovery at this juncture was the effectiveness of short irradiations near the middle of long nights in preventing flowering response. In the 1940s, the tendency of those interested in control of flowering was to attempt detection of florigen, a hypothetical hormone. Parker and Borthwick considered this a poor approach, offering little opportunity for examination by experiment. They were more inclined to conduct quantitative studies of the involvement of light in the flowering process. This would require measurement of action spectra. Wanting advice on methods and instrumentation for such an undertaking, they sought me out as one who might be interested. We soon began an informal cooperation that would endure for the next twenty-five years and unfold a whole new area of knowledge about plants.

Action spectra for short-day plants were obtained in 1945. These indicated the presence of a blue pigment related to phycocyamin of the blue-green algae as the receptor for light. Long-day plants were found to have the same action spectra but with the opposite flowering response. With this finding of universality in control of flowering, attention was turned to other responses to light. One of these was etiolation of plants growing in darkness. When examined, with cooperation of F. W. Went, who was visiting Beltsville, it, too, had the action spectrum for flowering control. Many students of plant growth at the time were little inclined to believe such findings, but some came to Beltsville on their own volition to observe. Harry's awareness, open mindedness, and coúrtesy encouraged an informality of approach and devotion to finding the meaning of things.

The most basic finding, however, came from close at hand in an unexpected way. For more than a century, many seeds were known to require light for germination. Eben and Vivian Toole, who worked with seeds in a laboratory adjacent to the room where the action spectra were measured, proposed examining the promotion of lettuce seed germination. The flowering-action spectrum again was found. But, most important, the potentiated action of red light, which required a day for display, was found to be immediately reversible by a short exposure to far-red radiation. This indicated that the photoresponsive pigment was photoreversible and thus had at least two forms, only one of which was biologically effective. When attention was returned to flowering and etiolation, their potentiations were also seen to be photoreversible. The action spectra, moreover, were closely the same for all responses.

Harry had throughout these years paid close attention to the agricultural implications of the findings. These were chiefly applicable to the control of weeds and, through plant breeding, to many crops such as soybeans. With H. M. Cathey, who had succeeded Emsweller in floricultural work at Beltsville, Harry studied the control of flowering of poinsettia and chrysanthemum, which came into wide use in flower production. With A. A. Piringer and R. J. Downs, he studied the effect of light on woody perennials (trees and shrubs). These studies clearly emphasized the high energy action of far-red radiation, which had first been sensed in the development of lighting for growth rooms. The main response was control of the dormancy of buds by moderate periods of radiation. This is one of the many aspects of photoperiodism. The years were fruitful ones, with rewards wherever attention turned.

The reversibility of photoresponsiveness was the keystone to progress about photoperiodism in the molecular sense. Through its use the product of molecular absorbancies of the receptive pigment and the quantum efficiency for conversion could be measured after the method used by Otto Warburg in work on cytochrome oxidase. In this way the pigment was established as deeply colored and present only in minute amounts in both albino and green plants long before it was seen. It, moreover, was probably a protein.

Although the work on photoperiodism was steadfastly supported for fifty years in the U.S. Department of Agriculture, the many pressures of limited funds militated against enlarging the effort when promising paths of investigation arose. Harry, foreseeing the possibility that great rewards might result from biochemical applications, used his limited funds to induce H. W. Siegelman to join the group. In a few years it became obvious that the receptive pigment could only be detected in vivo by its photoreversibility. But, would reversibility work in vitro to serve for assay in an attempted isolation?

Again, nearby cooperation was at hand. Karl Norris, an engineer working in the field of agricultural marketing, had developed spectroscopic methods for measuring the quality of apples. The method involved measuring minute absorbancies at two frequencies in optically dense media. With Norris and his associate, Warren Butler, the method, when applied reversibly, immediately indicated the presence of the pigment in etiolated maize tissue, both before and after grinding. With the assay perfected, isolation of the pigment was finally achieved.

With the pigment, by then known as phytochrome, identified, Harry again looked toward finding some clue to its actions. He knowingly left the inviting molecular approaches to others of suitable interest and training. One unexpected clue to the method of action was quick display of response, rather than the long delayed ones that had previously been studied. This was demonstrated by J. C. Fondeville, who had come from France to work temporarily in Beltsville. He had been studying the diurnal movement of mimosa leaflets as an example of photoperiodism or biological rhythm, a favorite topic of P. Bert in Bordeaux more than a century earlier and of Charles Darwin and his son, Francis, in their *Power of Movement in Plants.* It was soon established that the leaflet closure upon placing mimosa plants in darkness could be prevented by changing the phytochrome from the far-red to the red-absorbing form. The response was displayed within ten minutes and was photoreversible. This established the role of phytochrome in the control of turgor with most likely action on a controlling membrane. Others soon turned their attention to this inviting approach and are now elaborating it in many promising directions. Harry by then (1969) had retired, fully aware that in this long and often lonely journey others now stood on the threshold of opportunity to look even deeper and perhaps in the end to find out more about differentiation in flowering, which is still elusive.

In 1972, in his last publication, Borthwick looked back over thirty-five years of research on photoperiodism in plants. Although fully aware of the measurement of time by plants, he had never accepted what others have called "the biological clock," or endogenous rhythm. These were meaningless words to him-words that did more to obscure than to enlighten, and little to advance, experimentation. Also, he had rejected "florigen," the postulate flowering hormone, as more of the imagination than of fact. Instead, the course he had charted into the core of photoperiodism consisted of a sound mixing of biological understanding and physical experimentation.

Honors came to Harry Borthwick in his later years. He was elected to the National Academy of Sciences in 1961. He was President of the American Society of Plant Physiologists for a term and the recipient of its highest honor, the biennial Stephen Hales Award, and of a life membership. He received the Hoblitzelle Award for distinguished service to agriculture, the Joachem-Hafiz award from Switzerland, and the Distinguished Service Award from the U.S. Department of Agriculture. His greatest pleasure and deepest recognition, however, came from his many associates in research. They knew and honored him for his unselfish dedication to a central effort.

# Bibliography

*Key to Abbreviations*

Am. J. Bot. = American Journal of Botany
Am. Nat. = American Naturalist
Ann. Bot. = Annali di Botanica
Annu. Rev. Plant Physiol. = Annual Review of Plant Physiology
Bot. Gaz. = Botanical Gazette
Florists' Rev. = Florists' Review
Plant Physiol. = Plant Physiology
Proc. Am. Soc. Hortic. Sci. = Proceedings of the American Society for Horticultural Science
Proc. Int. Seed Test. Assoc. = Proceedings of the International Seed Testing Association
Proc. Natl. Acad. Sci. USA = Proceedings of the National Academy of Sciences of the United States of America
Proc. Plant Propag. Soc. = Proceedings of the Plant Propagators Society
U.S. Dep. Agric. Misc. Publ. = U.S. Department of Agriculture Miscellaneous Publication

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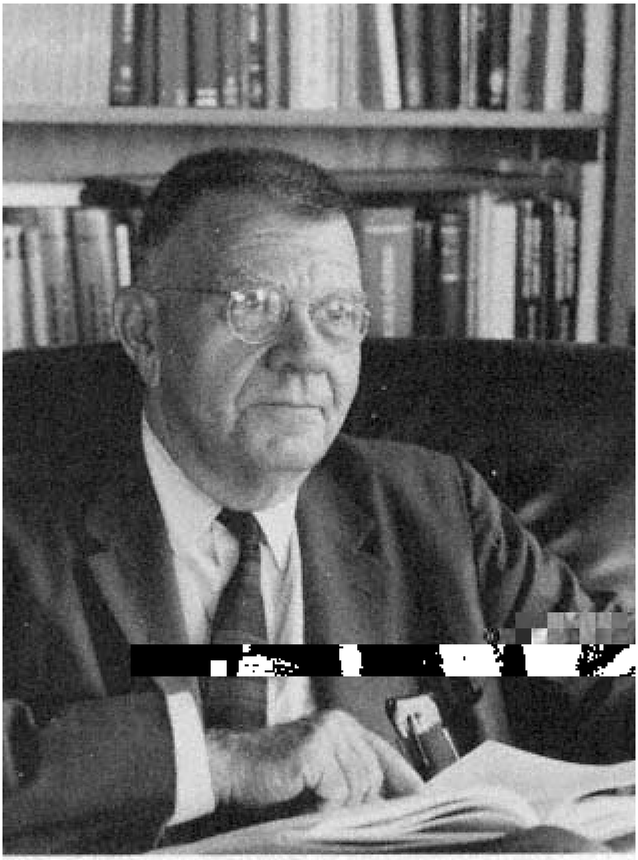

E.U. Condon

# Edward Uhler Condon

March 2, 1902-March 26, 1974

by Philip M. Morse

The middle third of the twentieth century was the era of hegemony of physics in American science. During that period Edward Uhler Condon was a leader in physics, in research of his own, in stimulating research in others, in applying physics, and in calling attention to the effects on all of us of its indiscriminate and irrational application. When he made his first contribution to theoretical physics in 1926, the word *physics* was not in the vocabularies of most Americans and the revolutionary concepts of quantum mechanics and relativity were just being worked out in Europe; by 1960 the applications of electronics and solid-state physics had begun to change our lives irreversibly, and the implications of nuclear physics were manifest to everyone. Ed Condon contributed to each part of this explosive evolution.

Condon's father, William Edward Condon, was a builder of railroads in the West. He and his wife, Carolyn Uhler Condon, moved from place to place as the construction jobs required. When Ed was born, on March 2, 1902, they happened to be in Alamogordo, New Mexico, an ironic coincidence not apparent until July 16, 1945. By the time he was ready for high school, the family had settled down in Oakland, California. Ed's rival interests, science and journalism, pulled him in different directions. In the turbulent year of 1918, when he graduated, rather than going on to college he became a reporter for the Oakland *Enquirer.* His experience in the ensuing three years had a lasting effect on his attitude toward government and society.

In his own words,

"On the *Enquirer* I specialized in the news of organized labor. . . . The dock workers, the timber workers, and the migratory farm laborers . . . were drawn to communism. The California state legislature had passed a strong bill defining criminal syndicalism and making it a felony. The politicians were looking for a place to use it. On November 9, 1919, I was the only reporter from a conservative newspaper to cover the organization meeting of the Communist Labor Party of California, as it was called then. I wrote lurid and sensational stories about this small group of one or two hundred persons, which resulted in indictments against them, and which required that I had to testify against them, in trial after trial, over the next several years. In this connection I became aware of open boasting by a police detective of his having framed some of the defendants in a matter where I knew the facts to be otherwise. The effect of this involvement on me was to wipe out any desire to be even an educated newspaperman; so I entered the university and went into physical science largely as a means of escape from the corruption of the world, in addition to the fact that I was genuinely interested in physical science."[*]

He entered as a freshman in the College of Chemistry of the University of California at Berkeley in 1921, but when he learned that his high school physics teacher, W. H. Williams, had joined the physics faculty at Berkeley he switched from chemistry to take Williams's courses in theoretical physics; thus his choice of career was determined. In that same year, 1922, he married Emilie Honzig, a tiny bundle of energy who en

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[*] Edward Uhler Condon, "Reminiscences of a Life In and Out of Quantum Mechanics," *Proceedings of the 7th International Symposium on Atomic, Molecular, Solid State Theory and Quantum Biology,* ed. Per-Olov Löwdin (New York: John Wiley & Sons, 1973), p. 9 (hereafter cited as "Reminiscences").

couraged Ed in his scientific work and actively supported his extracurricular activities.

At that time, as Condon has commented, "The physics department was comparatively weak in a research way, except for the recent addition to the faculty of R. T. Birge, who concentrated on the early development of the quantum theory interpretation of diatomic molecular band spectra, and of Leonard B. Loeb, who has spent a lifetime making important contributions to processes of ionization in gases.*

Ed did well, for he received his A.B. degree in three years with highest honors, went on directly to graduate work in physics, and received his Ph.D. in 1926. Birge was then making great progress in measuring and analyzing band spectral intensities. Condon put these observations together with a suggestion of James Franck concerning the photodisintegration of diatomic molecules to come up with an explanation of the regularities in the intensities. He wrote it up over a couple of weekends and presented it to Birge as his Ph.D. thesis. The combined suggestion-explanation became known as the Franck-Condon principle, after Condon reworked it later, in the language of the newer quantum mechanics.

In those years an education in physics was not complete without a year or two spent in Germany. Condon received a National Research Council fellowship and he and Emilie, with infant Marie (now Mrs. Wayne Thornton, Jr.), spent the fall of 1926 in Göttingen and the spring of 1927 in Munich. He imbibed the probabilistic interpretation of quantum mechanics from Max Born and, under Arnold Sommerfeld, began the wave mechanical formulation of the Franck-Condon principle.

Again the rivalry of interest between the gaining and the imparting of new knowledge intervened. Quoting Condon again, "By spring and summer of 1927, papers in quantum

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* Condon, "Reminiscences," p. 10.

mechanics were appearing at a great rate. In those days a young theoretical physicist was supposed to keep abreast of progress in every area of theoretical physics. I became discouraged and decided that if this were the normal pace of work in my chosen field (which it was not!) then I was not equal to the task. About this time there appeared a help-wanted advertisement in the *Physical Review* for a man to write popular science for an industrial laboratory, the requirement being stated that the candidate must have newspaper writing experience as well as a Ph.D. in physics. I may well have been the only person in America with that combination at the time. At any rate I applied for the position, was interviewed for it in London, and accepted it. It turned out that the position was in the public relations department of the Bell Telephone Laboratories, then in its old quarters at 463 West Street along the Hudson River in lower Manhattan.

"We returned to America and found an apartment near Columbia in October 1927 . . . At Bell Laboratories, C. J. Davisson and L. H. Germer had just done the experimental work on scattering of low-energy electrons by single crystals of nickel which led to one mode of discovery of electron diffraction. . . . The importance of this work was not at first appreciated in the business management side of the Bell Labs, and I devoted a good deal of attention in the fall of 1927 to explaining to such people that the work was destined to win for the Bell Labs the first Nobel prize to be awarded to an industrial organization. . . .

"In that fall I soon found that the American physicists on the Atlantic Coast were by and large having as much trouble understanding and assimilating quantum mechanics as I had had in Germany. . . . The profession of theoretical physics was much smaller then than now. As I remember it, Gregory Breit, John Slater, John Van Vleck, and Edwin Kemble were about the only ones in America who were really active in research in quantum mechanics. . . . I soon found myself in demand as a colloquium speaker at various universities . . . and King encouraged me to accept such invitations, even though they bore little if any relation to the work I was supposed to be doing for the telephone company.

"I was asked by George Pegram to be a lecturer in physics at Columbia University in the spring of 1928. . . . I accepted and started on my first regular university appointment by giving two graduate courses, one in quantum mechanics and the other on electromagnetic theory of light. . . . Besides giving these courses I traveled around giving colloquium talks on quantum mechanics and also on the Franck-Condon principle. So great was the demand for young faculty who could deal with these subjects that in the spring I was offered six assistant professorships for the fall of 1929. . . . I ended up by taking the offer from Karl Compton to go to Princeton. . . ."*

The chronicler of this biography first met Condon at Princeton in the fall of 1928. He was a new kind of professor. A close-cropped brush of black hair accentuated the roundness of his head, his broad face was usually adorned with a grin, and his brown eyes looked steadily but somewhat skeptically at one through rimless glasses. The western vocabulary, the proletarian outlook, the rough-edged kindliness—all contrasted with the eastern establishment manners that were then the Princeton norm. He was only a year older than the chronicler, but while his greater experience and maturity made a great deal of difference to the student, it made no difference to the professor.

Condon has remarked that this first year at Princeton, 1928-1929, was the most productive in his life. He has said,

"For teaching I gave a course in quantum mechanics again, improving the notes of the previous Columbia course, and a junior course in classical mechanics of which the most outstanding

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* Condon, "Reminiscences," pp. 12-13.

student was E. Bright Wilson. . . now Mallincrodt professor of chemistry at Harvard. Philip M. Morse, who had received a doctorate under K. T. Compton, . . . took my course and we worked up the lecture notes into the book *Quantum Mechanics* (Condon and Morse), which was published by McGraw-Hill in the fall of 1929. . . .

"I personally wrote the paper which gave a fuller statement of the quantum mechanics of the Franck-Condon principle. . . . By far the most important piece of work done that year was the development of the barrier leakage picture of alpha-particle radioactivity, done with R. W. Gurney. The same idea was developed almost simultaneously by George Gamow, then a postdoctoral fellow in Göttingen. This was the first application of quantum mechanics to details of inner structure of atomic nuclei, and at the same time its success gave a big boost to the probability interpretation of the intensity of the Schrödinger wave which was only being reluctantly accepted in some quarters."*

Condon still was footloose. He accepted an offer of full professorship at the University of Minnesota for the fall of 1929. But within a year he decided he preferred the stimulation of congenial colleagues to the kudos of the full professorship, so after giving summer courses at Stanford, he returned to Princeton in 1930, where he remained until 1937. During that decade he began to show his ability to spot, energize, and guide emerging leaders in the next generation of theoretical physicists. Two of them have reported how he did it.

George Shortley, who became professor of physics at Ohio State University and went into the field of operations research during and after World War II, writes:

"I was a senior at the University of Minnesota, taking a physics minor in my electrical

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* Condon, "Reminiscences," pp. 14-15.

engineering program. I signed up for both of his courses. His appearance was quite different from that of any professor I had ever seen. He was jovial, chubby, black-haired, crew-cut and boyish in appearance, wearing cream-colored plus-fours, after the fashion of the day for students, but decidedly not for faculty. One of his courses was the theory of atomic spectra, taught in the quantum-mechanical technique of Dirac before any useable text was available. The other was a course in classical methods of mathematical physics. The two courses meshed perfectly because the same mathematical functions were used in both. Condon was a beautiful lecturer; he had the facility of 'making a difficult subject sound easy' whereas other professors often had the opposite tendency to 'make a simple subject sound difficult.'

"These courses aroused my interest in the theory of atomic spectra and led eventually to my collaboration with Condon on the well-known book on this subject. In fact, later in this same senior year, Condon and I wrote and published our first joint research paper in this field.

"Early in 1930 Condon decided to leave Minnesota and return to Princeton. With considerable difficulty he arranged for me to go with him to Princeton as a graduate student. He also arranged for me to be his research assistant, at a salary that would enable me to support myself. After teaching at Stanford in the summer of 1930, he picked me up in Iowa for the drive back to Princeton with his wife Emilie and their little child, Marie, called Mädi; in fact Mädi sat on my lap for most of the trip.

"When I reported for my duties as research assistant he proposed the collaboration on the monograph on atomic spectra, and we proceeded to outline the chapters then and there. As indicative of the energy he expected of himself and of his students, he asked me the next morning how much I had writ ten. Fortunately I had applied myself the previous afternoon and evening and had the draft of half the introductory chapter to show him."*

And Frederick Seitz, president of the National Academy of Sciences from 1962 to 1969 and since then president of Rockefeller University, writes: "I was a sophomore at Stanford University and decided to do my bit to reverse negative trends in society by becoming a professional physicist. While still enjoying the feeling of euphoria brought on by this decision, I read in the university newspaper that the visiting professor in theoretical physics for the summer quarter would be a brilliant young man, twenty-eight years old, who had discovered the Franck–Condon principle while a graduate student at Berkeley, had spent two years at the great centers of theoretical physics in Europe as a National Research Council fellow, and had held prominent posts at the Bell Telephone Laboratories, Princeton University, and the University of Minnesota. Just a year earlier, he and Ronald Gurney had given an interpretation of spontaneous alpha disintegration of nuclei in terms of quantum mechanical tunneling. To top it all, the campus paper related that he had earned his way through Berkeley as one of the more worldly reporters for the Oakland *Tribune.* In this pursuit he had, among other things, stirred up a lively public discussion of whether a birdcage would weigh more or less when the bird was flying around inside instead of resting on its perch.

"The visitor, Edward Condon, was slated to give a course in modern physics which would be open to duly qualified undergraduates. I succeeded in pursuading an indulgent father to provide the means to attend the summer session and, early in July, found myself perched on a chair in the front row of the lecture room waiting for the show to start. It was not a disappointment.

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* Quotations not footnoted in this memoir are taken from personal letters to Philip M. Morse.

"Precocious and crew-cut, Ed Condon exhibited even then all of the characteristics that have carried him through a lifetime near the center of the stage. He was creative, energetic, perceptive, humorous, restless, eloquent, worldly and friendly. Moreover, he knew, on a first-name basis, most of the top-billed physicists on the planet and loved to spin endless anecdotes about them. This was very rich fare for an undergraduate. Condon's lectures . . . were then as now a wonderful combination of logic, anecdotes and humor. . . . In those days, long before physicists were taken very seriously by the public at large and when they still were all but unknown to congressmen and security officers, Condon was flamboyantly cheerful practically all of the time, his occasional bursts of wrath being directed at the petty annoyances of everyday life which plague us all. His bouts with various prominent individuals—particularly with General Leslie Groves—lay far in the future. . . .

"Condon was so deeply interested in other people that he quickly came to know personally everyone in the class who managed to act reasonably alive. The small band of embryonic physicists who dominated the front row in the lecture hall became his close friends. . . . With Condon's ardent help, continued family indulgence, and some permissiveness on the part of the Princeton admissions committee, I followed him back to Princeton a year and a half later as a graduate student. . . . His lectures that spring were centered on Frenkel's book about the classical electromagnetic theory of light, which he embellished in countless ways. I still cherish a carbon copy of his notes. . . ."*

With the completion of *The Theory of Atomic Spectra,* Condon's interest returned to atomic nuclei. He collaborated with Gregory Breit on a paper on the photodisintegration of

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* Wesley E. Brittin and Halis Odabasi, eds., *Topics in Modern Physics; a Tribute to Edward U. Condon* (Boulder: Colo. Associated Univ. Press, 1971), Foreword by Frederick Seitz, pp. xxi-xxiii (hereafter cited as *Topics in Modern Physics*).

the deuteron. But, as he has written, "Much more important was the work done jointly with Breit and R. D. Present on the theoretical interpretation of the experimental results obtained by Tuve, Hafstad, and Heydenberg at the Carnegie Institution of Washington on the scattering of protons by protons at energies up to about one million volts. These results showed quite clearly the charge independence of the strong nuclear force between nucleons on which all modern nuclear theory is based."*

Between 1928 and 1938 Condon published two books—*Quantum Mechanics* and *The Theory of Atomic Spectra,* both with co-authors; nine papers on general quantum mechanics; six papers on atomic spectra, all but one with co-authors; eight papers on the quantum mechanics of molecules, all but two with co-authors; two papers on solid-state theory, one with a co-author; and two papers on the biological effects of radiation. In addition, there were three articles in the *American Physics Teacher* on simple ways to understand physical concepts, two on semi-philosophical topics, and one, published in the *Proceedings of the U.S. Naval Institute,* that can be considered either as an early example of operations research or as an example of Ed's sense of humor. He had come across, in his omnivorous reading, a set of heuristic rules for the amount of food a shipboard cook should prepare, as a function of the number of men to be served. Assuming that the rule represents a balance between satisfying the men's shipboard appetites and reducing the amount of food left over, he determined the parameters of the normal distribution of the men's appetites that the rule inferred and then embellished it with comments on the implications of the distribution and on the validity of the conclusion that there was a nonzero fraction of the men with negative appetites. The conclusions seemed to puzzle some commentators in later issues of the *Proceedings.* This is by way of illustrating that, in spite

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* Condon, "Reminiscences," p. 18.

of his earlier noted complaint, at keeping abreast of progress in physics, Condon did in fact read, and understand, an unusually large sample of scientific literature.

Princeton could not hold him long. In 1937 he accepted the post of associate director of research at the Westinghouse Electric Corporation. He moved his family, increased by two sons, Paul Edward (now on the physics faculty at the University of California at Irvine) and Joseph Henry (now with the Bell Telephone Laboratories) to Pittsburgh. Westinghouse wanted to strengthen its work in fundamental physics and assured Condon of liberal support and a free hand in developing such work at the laboratories in East Pittsburgh. Construction had already been started on a large pressurized van der Graaff machine for nuclear work. This project was put under Condon's direction and other lines of work were initiated.

Once again Condon became the center of a lively community of stimulating individuals. He purchased a roomy house close to Wilkinsburg that seemed to be undergoing continual growth and was usually bursting with interesting, if occasionally unconventional, visitors. Condon not only brought into closer communication the promising young scientists and engineers already employed at Westinghouse, but soon added new faces, through a system of postdoctoral research fellowships. Under his leadership the laboratory quickly grew to the state where it could become a significant factor in the research and development that was to be necessary in World War II.

The approaching war broke in on those developments. In the fall of 1940 the National Defense Research Committee (NDRC) was authorized by President Roosevelt. It soon established the MIT Radiation Laboratory to develop microwave radar. It was agreed that Condon should devote as much of his group's energy as possible to radar work at East Pittsburgh, in cooperation with the MIT Radiation Laboratory. So, during the winter of 1940-1941 he commuted weekly between Cam bridge and Pittsburgh. Westinghouse made him chairman of the company committee to coordinate the expanding microwave research effort at its electronic laboratory in Bloomfield, New Jersey, and its radio systems factory in Baltimore, adding to his crowded travel schedule. He also served briefly with R. C. Tolman and C. C. Lauritsen on the NDRC committee responsible for the rocket program that led to the establishment of the Jet Propulsion Laboratory of Caltech.

Parallel to these developments was the work on nuclear fission, which grew slowly at first but by 1942 expanded into the huge complex of the Manhattan District. Condon worked for a while with the S-1 Committee, coordinating the start of this work. Later he spent a little time with J. R. Oppenheimer, planning the establishment that was to become Los Alamos. But his duties to the microwave work at Westinghouse prevented his participating further, beyond preparing a text on nuclear physics that became known as the Los Alamos Primer. In 1943 he spent some time at E. O. Lawrence's laboratory at Berkeley, arranging for Westinghouse to build the huge magnets to be used for the electromagnetic separation of uranium isotopes. These multiple contacts with the Manhattan District strengthened Condon's aversion to the military control of scientific research and development. He had many anecdotes, some grim and some humorous, about the military attitude and the consequences of the paranoia for secrecy. For example, he would recall the time he and Oppenheimer and General Groves were discussing the site of what was to be the Los Alamos Laboratory. Ed inserted the question, "As a western boy, I am wondering how we are going to supply this place with water?" General Groves brusquely said that that was his own problem and that Condon should concern himself with physics. "Yes, General," replied Condon, "but just how are we to get the water?" The fact that Condon's worry was justified and that the problem later had to be solved at enormous expense by trucking water up the mesa probably did not endear either of these strong characters to the other.

The end of the war pushed Condon onto the national stage. He had been elected to the National Academy of Sciences in 1944, and in 1945 he became vice-president of the American Physical Society, to become its president the following year. The many physicists who were concerned with the military control of nuclear weapons looked to him for leadership, and his penchant for action set him to writing articles and giving talks about the dangers, as well as the potentialities, of nuclear power. These came to the attention of Secretary of Commerce Henry Wallace, who persuaded President Truman to appoint Condon Director of the National Bureau of Standards. Condon accepted and, before he was confirmed by the Senate, he came to Washington to work with Leo Szilard and others to lobby for the civilian control of atomic energy. The fight was violent and bitter, and affected the rest of Ed's life.

Condon's appointment to the bureau was confirmed by the Senate in November 1945, but before that Senator Brien McMahon asked him to serve as scientific advisor for a special committee on atomic energy that McMahon chaired. For several months Condon gave a course for legislators on the atomic nucleus and its implications in war and peace. Until the summer of 1946, when the McMahon-Douglas bill established the Atomic Energy Commission, under civilian control, Condon held two jobs. With the establishment of the AEC, he felt able to turn his undivided attention to the Bureau of Standards.

Condon was the first director of the NBS to be appointed from outside the bureau ranks, the first director to be recruited from industry, the first theoretical physicist to head the bureau, and the first and only director to live in a house on the bureau grounds. As his colleague at the bureau, Hugh Odishaw, has written:

"The NBS had had a long and honorable history of scientific and technical contributions, but the depression years had seen its budgets slashed. Instruments and facilities were wearing out; there was little if any new gear; no significant opportunities to enter into new areas of research and negligible funds to attract young scientists. Condon was determined to change this.

"The struggle for greatly increased appropriations was limited in success, but Condon drew much larger funds from other agencies. With these he strengthened sound on-going activities and initiated new ones, in mass spectroscopy and betatron studies, for example, and through the creation of new divisions, as in applied mathematics and electronics. These latter two collaborated in a pioneering computer program—SEAC in the East and SWAC in the West. These were the first automatically-sequenced, high-speed digital computers, and much of subsequent computer technology stems from this endeavor."

In addition, he and Odishaw assembled the highly useful *Handbook of Physics,* finally published in 1958.

Condon was also interested in administrative problems. He simplified the bureau's organization, initiated the first complete restatement (Public Law 81-619) of the bureau's functions since its founding, and presided over the establishment of major new facilities at Boulder, Colorado. The results of his initiation of new programs and recruiting of young blood are still quite apparent at the bureau.

Condon believed in removing obstacles, not going around them. This chronicler remembers being castigated for commending the formation of not-for-profit corporations as a means of providing technical assistance to government agencies without becoming enmeshed in civil service red tape. Condon felt this was a cowardly evasion of the Augean task of revising civil service.

Such direct action, of course, makes enemies. He had already roused the ire of the House Un-American Activities Committee (HUAC) by his opposition to the military control of atomic energy. In 1948 the committee's chairman, J. Parnell Thomas, proclaimed that "Dr. Condon is one of the weakest links in our atomic security." Privately Condon described the impossibility of refuting such a charge as follows: "If you say I've got a wart on my nose, I can deny it. But if you just say I'm one of the ugliest men in town, all I can do is to argue that I'm really quite pretty." The verbal duels at the hearing reached heights of invective and illogic. Condon once alleged that someone actually asked how it had come about that Dr. Condon had been born so near the site of the first atomic bomb test. Time and again his security clearance status was reviewed and reestablished, only to be challenged again, long after Congressman Thomas had been jailed for taking kickbacks from his staff.

In 1951, the year that a star-chamber hearing had removed the clearance of J. R. Oppenheimer, Condon regretfully decided that the bureau would fare better if he left. He had expected to stay at the bureau for much of the rest of his life, devoting his energy and skill to making it one of the greatest scientific laboratories in the world, but it was clear that the continuing attacks on him were hindering further support of the bureau by Congress. So he accepted an offer to become director of research and development for the Corning Glass Works.

Here again Condon recruited new scientists and initiated original research on the structure of glass and applications of its properties. He published a highly useful sequence of four papers on the physics of the glassy state in 1954. At the Corning laboratories, he initiated a number of new projects. Unfortunately one of them, on missile nose cones, was supported by the Navy, and thus clearance was required.

In September 1952 Condon was again called before the HUAC to answer further charges, one of which was that there was reason to believe he might be disloyal "in that your wife was critical of the foreign policy of the United States and you did not reprove her."[*] After a long delay, during which the strong support of his scientific colleagues was shown by his election to the presidency of the American Association for the Advancement of Science, Condon had his fourth hearing before the clearance review board and again was given a completely favorable verdict. Within four months, however, the Secretary of the Navy demonstrated the irrelevance of the semijudicial clearance hearings by arbitrarily suspending Condon's clearance. A few days later Vice President Nixon implied, in a campaign speech, that he had requested the suspension.[†]

As Condon has written:

"The Republicans were still bent on smearing the Truman record by pretending to a concern over the loyalty of his appointees. Eisenhower and Nixon campaigned in 1952 that they would strengthen the personnel security investigation procedures and clean the 'reds' out of Washington and elsewhere. They kept their campaign promise to the extent that the procedures were revised and a number of persons were subjected to long and tiresome hearings. . . . One of these was J. Robert Oppenheimer, who was finally deprived of his security clearance. . . . Another was myself where the outcome was favorable to me as it had been in three previous loyalty hearings. However, in October 1954 this favorable verdict was arbitrarily suspended by a Secretary of the Navy. . . .

"I had been under intermittent harassment in this way since 1947 and decided that I would subject myself to it no longer. So I arranged to become a consultant to Corning Glass Works [a position he held till his death]. In the spring of 1955 I was offered professorships by the faculties of two major universities, but in both cases the trustees refused to confirm the appointments under pressure from Washington. Finally I was allowed to become chairman of the physics department at Washington

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[*] Brittin and Odabasi, *Topics in Modern Physics,* Preface by Wesley E. Brittin and Halis Odabasi, p. xviii.

[†] Lewis M. Branscomb, "Edward Uhler Condon," *Physics Today* 27(1974):69.

University in St. Louis, and later to come to Boulder as a professor and fellow of the Joint Institute for Laboratory Astrophysics [joint with the Bureau of Standards, thus formally reestablishing Ed's relationship with the Bureau that had never really been broken]. As the cold war slowly died down the Department of Defense finally granted me the security clearance which had been improperly suspended in 1954 but this, I am proud to say, I have never used."*

The appointment to Washington University was the result of the efforts of Chancellor Arthur Compton, who not only wanted to add an outstanding physicist to the staff, but also realized that the nation as well as Condon would be the loser if the irrational chain of events were allowed to continue. At the University of Colorado he could finally settle down again to research in atomic theory with Halis Odabasi (now at the University at Istanbul) with the intent of rewriting *The Theory of Atomic Spectra,* and in further work on the properties of glass. He continued to write and lecture on the need for peaceful, worldwide cooperation; he took on the job of editor of the *Reviews of Modern Physics;* he actively participated in the research of the Joint Institute of Laboratory Astrophysics; and he found time to be president of the American Association of Physics Teachers in 1964.

And, in an incautious moment, he agreed to head a project, supported by the Office of Scientific Research of the Air Force, to investigate the many reports of unidentified flying objects (UFOs), with which the Air Force had been plagued for nearly twenty years. This occupied much of his time during 1967 and 1968. The report of this project was published in 1969. Condon gave a light-hearted account† of some of his experiences at a talk before the American Philosophical Society. The report has

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* Condon, "Reminiscences," p. 21.

† Edward Uhler Condon, "UFO's I Have Loved and Lost," *Proceedings of the American Philosophical Society* 113(1969):425.

been the subject of vituperative comment from persons anxious to continue to believe that flying saucers are visitors from outer space and who wish to see the government spend vast sums on further studies. Despite the views of many of his colleagues that the investigation was a waste of Condon's time, Seitz has said, "The introductory chapter of the report on UFO's, in which Condon describes with characteristic clarity his own view as a scientist on what constitutes worthwhile research, is a classic. It deserves to be a landmark in the journey science has taken since the days of Stevin, Galileo and Kepler."*

Edward Uhler Condon died on March 26, 1974. Two comments may serve to close this survey of his life. One is by one of his colleagues at the National Bureau of Standards, Churchill Eisenhart:

"Condon was a brilliant scientist, with highly original ideas, a wide range of interests, a restless probing mind with voluminous information indexed for instant retrieval. He could meet with scientists of diverse specialities and stimulate each with fresh enthusiasm and new insights. He could elucidate scientific intricacies to non-scientists with clarity in layman's language. Whatever he knew he saw with crystal clarity; he could summarize it in a nutshell on a moment's notice or discuss it in detail with experts, with equal ease.

"He had an ever ready and exuberant sense of humor, a gift of repartee and could be wittily caustic when provoked. He was a cordial, genial, straightforward individual; fond of people, mathematics, science, chamber music and conversation; allergic to formality, fuzzy mindedness, pomposity and all forms of physical exercise. He was an active Quaker, a firm believer in human dignity, an outspoken liberal and anti-isolationist, who fervently hoped that international understanding and world peace could be furthered by continuance of World War II alliances. He

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* Brittin and Odabasi, *Topics in Modern Physics,* Foreword by Frederick Seitz, p. xxvii.

gave freely of his counsel and his time; generously of his finances and his home."[*]

The other comes from Lewis M. Branscomb, a colleague at Boulder, now with IBM:

"Watergate came as no surprise to Edward Condon, nor did its aftermath. I imagine he would like to have lived to see the outcome of the impeachment inquiry. But Condon understood and paid his share of the price of liberty. Somehow his idealism, his sense of humor and his inexhaustible energy made his relentless quest for a better world look like optimism. He was elected president of the AAAS during the height of his troubles with HUAC. He was president of the Society for Social Responsibility in Science (1968-1969) and co-chairman of the National Committee for a Sane Nuclear Policy (1970). He was appropriately honored on his retirement from JILA and the University of Colorado in the summer of 1970 by the volume[†] edited by Brittin and Odabasi mentioned earlier. Brittin relates a comment about Condon by E. Bright Wilson: 'Sometimes I think he looks for trouble,' Wilson said. Condon's comment: 'It's not hard to find.' "[‡]

Unfortunately it is not easy to find a brilliant scientist who is willing to speak out on questions of public policy, often with humor but always with determination, even in the face of official persecution.

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[*] Churchill Eisenhart, "Edward Uhler Condon," The Technical News Bulletin of the National Bureau of Standards, *Dimensions* 58(1974):151.

[†] Brittin and Odabasi, *Topics in Modern Physics.*

[‡] Branscomb, "Edward Uhler Condon," p. 70.

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*Key to Abbreviations*

Am. J. Phys. = American Journal of Physics
Am. Phys. Teach. = American Physics Teacher
J. Appl. Phys. = Journal of Applied Physics
J. Chem. Phys. = Journal of Chemical Physics
J. Franklin Inst. = Journal of the Franklin Institute
J. Opt. Soc. Am. = Journal of the Optical Society of America
Philos. Mag. = Philosophical Magazine
Phys. Rev. = Physical Review
Proc. Natl. Acad. Sci. USA = Proceedings of the National Academy of Sciences of the United States of America
Rev. Mod. Phys. = Reviews of Modern Physics
Westinghouse Eng. = Westinghouse Engineer

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* This bibliography of Edward U. Condon's works is taken from *Topics in Modern Physics,* ed. by Wesley E. Brittin and Halis Odabasi, pp. xxix-xxxiii, 1971, by Colorado Associated University Press.

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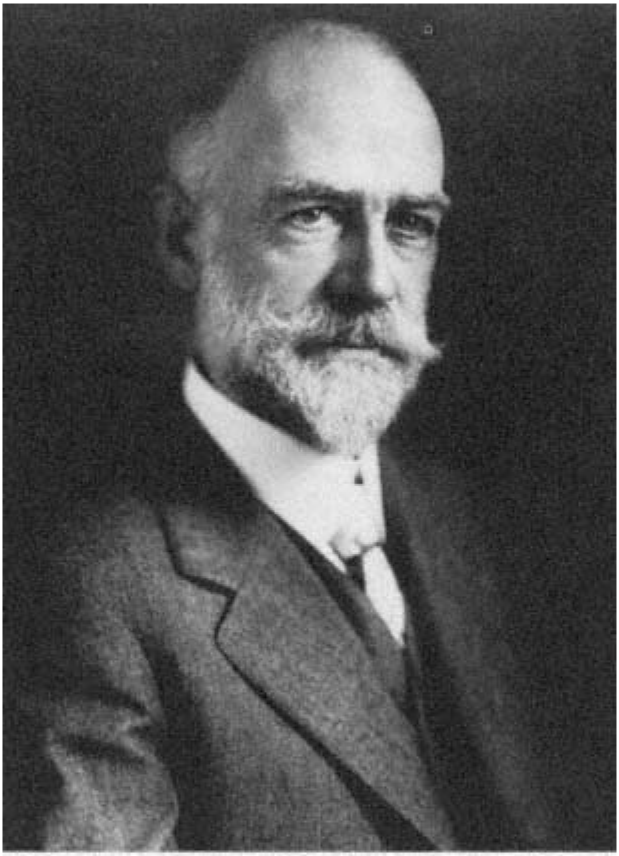

W. F. Durand.

# William Frederick Durand

March 5, 1859-August 9, 1958

by Frederick Emmons Terman

William Frederick Durand began his professional career in 1880, when upon graduation from the U.S. Naval Academy he was assigned to the U.S.S. *Tennessee* to look after its steam engine and associated boiler. This was the largest vessel in the U.S. Navy and was the flagship of the North Atlantic fleet. It had a wooden hull and was full-rigged with mast, sails, and spars; the steam engine was for use when the wind was not favorable. Durand's last important assignment was assumed in 1941 at age eighty-two, when Vannevar Bush, Chairman of the National Advisory Committee on Aeronautics, appointed Durand chairman of a committee that was assigned the responsibility of getting a jet engine for aircraft propulsion designed and manufactured in the United States. Durand carried this responsibility, as well as concurrently serving as Chairman of the Engineering Division of the National Research Council, until mid-1945, a few months beyond his eighty-sixth birthday.

Durand came from early New England stock and was of mixed English and French-Huguenot blood. He was born March 5, 1859, at a village now known as Beacon Falls, Connecticut, and grew up on a farm near Derby, Connecticut, which is approximately eight miles west of New Haven. His boyhood environment was that typical of New England farm and country town life in the period immediately after the Civil War.

In school Durand showed unusual aptitude and interest for mathematics. He also had a special fondness for working with tools in the way of devising implements and apparatus—usually related to some phase of farm work. As an example, he spent much time and effort on the design and construction of a horse-drawn hay rig with operating features that he regarded as superior to the rig then being used on the family farm.

On the basis of these qualities, his older brother urged him to compete for entrance to the U.S. Naval Academy because of its engineering course. The permit necessary to attend the entrance examination was obtained through a congressman friend. To further his chances for selection to Annapolis, he dropped attendance at the high school for the spring of 1876 and spent the time in the tool room of a factory in a nearby village, riding to work daily on horseback. Here he gained familiarity with machine tools, a factor in the examination at Annapolis. Also that summer he supplemented his rather skimpy high school education by an intensive coaching review of the subjects covered by the Annapolis examination offered at the Maryland Agricultural College (now the University of Maryland). In the entrance examination Durand ranked tenth among the eighty applicants. He entered the Naval Academy in the fall of 1876.

The years at the Naval Academy were a turning point in Durand's life. His school days in New England had provided little in the way of competition, and he had developed no special ambition, love of study, or definite purpose. Life at the Naval Academy, with its keenly competitive features, and with its appeal to the ambitions and visions of young adults, awakened him. Although the lessons were long and the examinations searching, he responded to the challenge. Seventy-three years later he wrote, "I give emphatic praise to the course of instruction and to the thorough training at Annapolis. Whatever I may have been able to accomplish in later years, I credit unreservedly to this institution and to the training received there." Scholas tically, his class rank was four, three, two, and one in his successive years, with an overall rank for the four years of number two. In his last year he was also given the highest cadet rank for which he was eligible—the so-called three-stripe rank, which was based upon the general record, including not only class scholastic standing, but also aptitude for the service. This placed him in command of the four classes of engineering students at all general functions, such as dress parades, and also of his own class at all formalities. Thus did the farm boy from rural New England discover that he could achieve leadership in a broader world.

Upon graduation from the Naval Academy in 1880, Durand was assigned to the U.S.S. *Tennessee,* as previously indicated. The next three years were spent on cruises up and down the Atlantic Coast and among the West Indies. In June of 1883 he was detached from the *Tennessee* and ordered to duty in the design room of the Bureau of Steam Engineering of the Navy Department, in Washington, D.C. Here he worked on the design of the engines for the cruiser *Chicago,* one of the first four ships of the new steel navy that Congress had authorized a few years before.

During the three-year cruise on the *Tennessee,* Durand had begun to question whether a career in the Navy was best suited to his tastes and capacities. He also desired to be in a position to marry and to enjoy family life. A few years earlier a bill had been passed by Congress providing for the detail of officers in the Engineering Corps of the Navy to scientific and technical institutions of learning for the purpose of giving instruction in steam engineering and iron shipbuilding. Teaching had always appealed to Durand. He recognized that such an assignment would provide an opportunity to try out academic work in a provisional way without a final commitment. Accordingly, learning that Lafayette College at Easton, Pennsylvania, had applied for such a detail, Durand took the necessary steps and in due time was ordered there on temporary detached duty, where he spent two pleasant years. In the fall, on October 23, 1883, he married Charlotte Kneen, five months his junior and a classmate from Derby High School. On June 14, 1885, a son, William Leavenworth, was born, their only child.

After leaving Lafayette, Durand spent the next eighteen months on engineering assignments connected with the construction and testing of the new steel ships being built for the Navy. His most notable experience in this connection was as a member of the crew of the U.S.S. *Dolphin,* a so-called dispatch boat, when this vessel demonstrated its structural soundness by deliberately steaming full speed into the teeth of a storm off Cape Hatteras and coming through this ordeal unscathed. This ship had become the center of a political controversy, having been contracted for and designed under a Republican administration, but offered for acceptance to a succeeding Democratic administration that questioned its seaworthiness.

During this period, Durand continued to think about his future and looked with increasing favor upon a career in the academic world. He sought and obtained an assignment by the Navy to Worcester Polytechnic Institute in March 1887, with the thought that this could be a stepping-stone to something more permanent. The following summer he was offered a position at Michigan State College (now Michigan State University) to organize and direct a new Department of Mechanical Engineering. This he accepted and concurrently resigned from the Navy as of September 1, 1887. The following year Lafayette awarded him the Ph.D. degree as a result of studies that he had initiated during his tour of duty at that institution and had completed in absentia in 1887-1888.

Durand undertook the organization and development of the Michigan State Mechanical Engineering Department with great enthusiasm and within a few years had laid a good foundation for the future. However, fate had other plans in mind for him.

In the late spring of 1891 he was offered a professorship in Mechanical Engineering at Purdue University. Feeling this position would be a step upward in his academic career, he accepted, only to discover later in the summer that an opening existed at Cornell University as head of a new postgraduate program in Naval Architecture and Marine Engineering in Cornell's Sibley College. Naturally this opening was attractive to him, so he contacted Dr. R. H. Thurston, Director of Sibley College. After due consideration, Thurston informed Durand that they would like to have him as head of this new program if he could honorably clear himself of all obligation to Purdue. This he accomplished through finding a suitable substitute for his position.

At Cornell Durand had an opportunity for the first time to engage systematically in research and other creative work. He took full advantage of this situation, as is evidenced by the prodigious stream of publications that came from his pen during his thirteen years at Cornell.

Durand's most important work during this period was his study of the screw propeller. From his early days on the cruise of the *Tennessee,* Durand had been interested in the screw propeller and its theory of operation. It appeared to him that through tests on models it would be possible to relate the physical proportions and operating conditions of the propeller to its performance. Cornell had a Hydraulic Laboratory with a concrete-lined canal that was suitable for such an investigation if it could be equipped for carrying out the necessary experimental observations. The apparatus needed included: (1) a car with wheels running on rails laid on either side of the canal and fitted for carrying the model propeller and the necessary electrical equipment for its operation at any speed along the canal, at any desired number of revolutions per minute of the model, together with the measuring equipment for determining the thrust and input power to the propeller, as well as revolutions per minute; and (2) electrical equipment for propelling the car at any desired speed along the track with electrical contacts and registering equipment for determining such speed. Since Cornell had no funds for the necessary equipment, Durand applied to the Carnegie Institution of Washington for financial help and was awarded the necessary grant.

Altogether some forty-nine models, each one foot in diameter, were tested for varying form and proportion, area, and pitch ratio. With the results of such model tests and by the use of laws of comparison in stepping from model to full scale, it was then a simple matter to determine the characteristics of a full-scale propeller to meet any proposed conditions of operation. The result was to make it possible to design and calculate the performance of a marine screw propeller on a systematic basis. Some years later Durand applied this same approach to pioneering studies of the airplane propeller, as will be subsequently recounted.

Durand's work at Cornell was not, however, limited exclusively to work on the screw propeller, as evidenced by the titles of various papers that he wrote during this period. Two additional contributions are worthy of particular attention. The first of these was the introduction around 1892 or 1893 of logarithmic cross-section paper. Durand was apparently the first ever to have had the idea of ruling cross-section paper with intervals corresponding to the logarithms of the numbers set down on the axis. As late as 1936 the general catalog of Keuffel and Esser Co. listed logarithmic paper under the title, "Durand's Logarithmic Paper."

Another important contribution during the Cornell period was the invention of a planimeter for averaging the ordinates of a diagram plotted in radial (polar) coordinates. As clock recording instruments were becoming common in power plants and elsewhere, Durand obtained a patent on the device and for years received royalties from a manufacturer of scientific instruments licensed under this patent.

The pleasant rhythm of the Cornell years was suddenly upset by the unexpected death of Thurston from a heart attack in October 1903. Durand, who had been made Secretary of Sibley College by Thurston some years before, was appointed Acting Director for the remainder of the year while a search was launched for a successor to Thurston. After the appointment in the spring of Professor A. W. Smith of Stanford University as the new Director of Sibley College, Durand decided to apply for a year of sabbatical leave during which plans could be made for the future.

However, late in the spring of 1904, fate again intervened in Durand's life, when President David Starr Jordan of Stanford persuaded him to fill the vacancy created at Stanford by the departure of A. W. Smith. Durand served as professor and head of Mechanical Engineering at Stanford until his retirement in 1924, and as Professor Emeritus until his death on August 9, 1958.

During his first year at Stanford, Durand was also in charge of the Electrical Engineering Department, along with an assignment from Dr. Jordan to find a head for that department. To this end he consulted extensively with his close friend Harris J. Ryan of Cornell, who had already achieved distinction for studies of corona generated by high-voltage electrical power lines. During the course of these consultations, he discovered that Ryan might himself be interested in this opening. The result was that Ryan came to Stanford in 1905 as head of the Department of Electrical Engineering, a position that he filled with great distinction, as indicated by his election to the National Academy of Sciences in 1920.

At Stanford the Durands quickly adjusted to their new circumstances. In 1905 they built a home on a hill that overlooked the university buildings and that also provided a breathtaking view of San Francisco Bay and the northern end of the Santa Clara Valley. They were delighted when the Ryans built a home on an adjacent lot in 1906.

The San Francisco earthquake of April 18, 1906, caused major damage to Stanford's buildings. During the following two years, Durand gave much of his spare time to service on a three-man faculty Board of Engineering responsible for making the temporary repairs required to enable the University to reopen in the fall, and for planning the permanent restoration of the damaged structures.

In planning for the Panama Pacific International Exposition, held in San Francisco in 1915, the major engineering societies of the United States decided to hold a worldwide Engineering Congress. Durand was appointed chairman of the local Committee of Management and spent much of his time for several years getting together an adequate staff, making plans for the Congress, and inviting delegates from the leading nations to write papers to be read before it. Finally came the Congress itself, with a great opening session, an address by General Goethals, who had accepted the post of Honorary President, receptions, banquets, technical sessions, etc. Thereafter came the work of gathering together all the papers read before the Congress and the related discussions, and preparing this material for publication. This latter phase was interrupted by World War I, and was not finally completed until around 1920.

With his fluid mechanics background, it was natural for Durand to become interested in the water and power problems of the western United States. He retained this interest until the end of his life. For some thirty years he served as a consultant to the Bureau of Power and Light of the City of Los Angeles in the construction and design of its water and electrical supply systems. For the City of San Francisco there was a period of three or four years' service in connection with the design and construction of the Hetch Hetchy Dam and the associated water supply and hydroelectric power installations. He also helped the Metropolitan Water District of California in the design and construction of pumping equipment for the Colorado River canal to bring river water to Los Angeles and other cities. As a by-product of these assignments, he did significant research on hydraulic machinery, the hydraulics of pipelines, and the theory of the surge chamber.

In spite of these new activities, Durand never lost his interest in the problems of the screw propeller. With the development of the airplane, he began to give attention to the airplane propeller, and in the 1914 volume of the *Journal of the Franklin Institute* he has a twenty-seven-page paper, "The Screw Propeller; with Special Reference to Airplane Propulsion." In that same year he attended a conference in Washington, D.C., called by Charles D. Walcott, Secretary of the Smithsonian Institution, the purpose of which was to consider ways and means for awakening and stimulating interest in aeronautical science, with particular reference to activity on the part of the government. This conference developed a background that led to a bill enacted by Congress the following year providing for the organization of the National Advisory Committee on Aeronautics (NACA, now NASA), which was "to supervise and direct scientific study of the problems of flight with a view to their practical solution." President Wilson appointed Durand as one of the five civilian members of this Committee, which held its first meeting in the spring of 1915.

At this first meeting Durand set forth the need for experimental studies of the air propeller analogous to those of ship propellers he had carried on at Cornell. This proposal met with favor by the Committee, and led later to a contract with Stanford University for carrying out such an investigation under NACA sponsorship. This contract called for an expenditure of $4000 for the initial year, including the cost of building and instrumenting the wind tunnel that would be used in carrying out the tests. Even allowing for the change in purchasing power that has occured since 1916, this is indeed a modest sum for the first definitive investigation on a basic topic, especially when compared with present-day grants.

This was the beginning of a long series of researches on air propellers performed at Stanford with the help of Professor E. P. Lesley, which extended over the following dozen or so years.[*] Over one hundred model propellers were tested in a wind tunnel under widely varying conditions of operation, and principles of design were established. The resulting reports were the authoritative sources for design data for many years. This investigation included the first experimental study made in the United States of the variable-pitch propeller, which in time came into almost universal use in all propeller-driven airplanes. It also included a thorough comparison of the measured data with theoretically computed characteristics for eight of the model propellers, as well as a comparison of model results with full-scale flight-test data, which were obtained under Lesley's direction at the Langley Laboratory of NACA. The latter comparison, which necessarily involved the testing of the propeller in combination with a fuselage, gave rise to the important concept of *propulsive efficiency* for such combinations. This became a standard analytical tool in the design of propeller-driven aircraft.

At the second meeting of NACA, held in the fall of 1916, Durand was chosen chairman. Upon the entry of the United States into World War I in the spring of 1917, Durand took a leave of absence from Stanford and moved to Washington, D.C.

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[*] It is significant that these studies of propellers were conducted with little or no involvement of graduate students. Neither were they used as a vehicle to recruit and develop young faculty members of outstanding promise. In spite of his diverse accomplishments and leadership qualities of high order, Durand was not notable as an organizer of academic programs or as a developer of faculty talent.

He was soon in the midst of feverish activity preparing for U.S. participation in the war. Under the general supervision of NACA, the Liberty airplane engine was designed and built by the American automotive industry; a cross-license agreement was negotiated whereby all important aircraft patents were pooled for common use during the period of the war; ground training schools for aviators were established at a number of universities; etc. In the autumn of 1917 Durand played an important personal role in initiating the development of the first successful airplane supercharger for increasing the performance of airplane engines at high altitudes. He was also a primary force in the establishment of the NACA Laboratory at Langley Field.

During this period Durand took his first flight in an airplane, a ride in an Italian triplane from Langley Field to Washington, D.C., some 120 miles.

In the spring of 1917 Durand was elected to membership in the National Academy of Sciences. With the onset of war, the Academy had come to the conclusion that it could more adequately meet its obligations as advisor to the government in scientific and technical matters by opening its membership to the engineering profession and, accordingly, in 1917 elected three engineers to membership. This nucleus of engineers was initially attached to the section of Physics, but later, with enlarged numbers, provided the basis of a section of Engineering. Durand soon became involved with the Engineering Division of the National Research Council (NRC), an operating arm of the National Academy of Sciences, and was made vice chairman of the Engineering Division, in charge of its Washington activities. The offices of NRC and NACA were in the same building, so his working day was spent shuttling between floors.

Among its other activities, NRC organized "a Research Information Service," which was to have liaison offices in London and Paris that would maintain continuous and close contact with developments in the application of science to warfare. Durand was offered and accepted the post at Paris. He picked Karl Compton, later President of MIT, as his chief assistant and left for France in January 1918 on a convoy bound for England. In England he looked up Admiral Sims, who was in charge of the U.S. Naval forces in Europe. Sims and Durand had been classmates at the Naval Academy and afterwards shipmates on the three-year cruise on the U.S.S. *Tennessee,* so they knew each other well. Sims arranged for Durand to be housed in the Paris headquarters of the Navy, and he saw to it that appropriate contacts were established with the U.S. Embassy and the French Army and Navy.

While in Paris, a young naval aviator, Harry F. Guggenheim, became closely associated with Durand. Some eight years later Harry Guggenheim became president of the Daniel Guggenheim Fund for the Promotion of Aeronautics, which played a critically important role in the development of the science of aeronautics and civil aviation in this country. Durand served as a Trustee of this Foundation for a number of years.

The Paris assignment turned out to be an interesting and pleasant episode in Durand's life. His effectiveness was facilitated by an excellent working knowledge of the French language gained while at the Naval Academy. This mastery had come about in an interesting way. The Academy curriculum included two years of French, but Durand found this subject uncongenial and give it little attention and effort, with the result that at the end of the first year his grade in French was barely above passing. Upon reflection this gave him a distinct feeling of shame. He thereupon gave himself the equivalent of a good scolding, and decided that since French was part of the curriculum, it was incumbent upon him to change his attitude toward the subject. Accordingly, before embarking on the summer cruise, he bought a French book and occupied his spare moments during the cruise reading this book with the aid of a dictionary, thereby acquiring a vocabulary. In this self-study he developed the technique of passing directly from the printed French word to its meaning without the intermediary of an English word. He thus acquired an appreciation of the quality and character of the French language. His grade in French showed a gratifying improvement in the next year. He also utilized every opportunity to practice conversational French. By these means his facility with both the spoken and printed language became so anchored in his memory that it remained with him throughout his life. Subsequently, Durand applied the same technique to Spanish during the one-year course required at the Academy. Having become interested in languages, Durand acquired a German grammar and dictionary while on the U.S.S. *Tennessee,* and, with the aid of a fellow officer of German extraction, taught himself to read and speak German with fair facility, without ever having a formal course in the subject. He then followed the same strategy with Italian. This learning of French, Spanish, German, and Italian illustrates how Durand, once having set a goal for himself, had the self-discipline to achieve his objective.

In the summer of 1918 the Royal Aeronautical Society of England invited Durand to deliver the Wilbur Wright Memorial Lecture, the first American to be so honored. This was the occasion for a memorable cross-channel trip from Paris to London. In the lecture he dealt with the problems of aircraft design, construction, and operation as factors in a war effort, carefully working out his ideas to avoid disclosing information of value to the enemy. It was attended by an appreciative audience of some 2,000. This occasion led to Durand's election as a Fellow in the Royal Aeronautical Society.

Shortly after the armistice Durand returned from Paris to Stanford. Here he resumed a normal program of university activities, including, in particular, active, direct participation in the airplane propeller studies that had continued at reduced pace during his absence. He also introduced a new course, "Theory of Flight," one of the first courses taught in the United States on this subject. He likewise resumed work in the field of hydraulics, with special attention to problems of hydraulic shock and surge chambers, as evidenced by several publications in the early 1920s, including the book *Hydraulics of Pipe Lines.*

In 1924 Durand reached Stanford's retiring age. This changed the character of his activities but not their tempo. In the fall of 1924 he was elected President of the American Society of Mechanical Engineers (ASME) and spent most of the year 1925 resident in New York City. During this year he traveled a great deal, carrying the Society's message to the local branches; his diary lists ninety addresses at fifty-seven different locations.

Toward the end of 1925 his work for the ASME was largely displaced by his appointment by President Coolidge as a member of a Board of Aeronautic Inquiry. This board was appointed as a response to the public úproar created by the episode in which General Billy Mitchell charged that the military services were neglecting and suppressing aviation in national defense. For these allegations, Mitchell was court-martialed. The Board of Aeronautic Inquiry was charged with studying the nation's situation in aeronautics and advising President Coolidge as to the best national policy to follow, with particular reference to national defense.

Working with Durand were financier Dwight Morrow, a U.S. judge, a senator, two congressmen, a retired general, a retired admiral, and an industrialist. When the Board organized, Morrow became chairman and Durand secretary. After extensive hearings and the assembly of a great mass of information, advice, and opinion, the Board undertook the task of analyzing, weighing, and judging the great variations in viewpoints that had been expressed. After many executive conferences, the task of drafting a report with a content acceptable to the full Board was done largely by Durand and Morrow. The final report, as submitted to President Coolidge and signed by all members of the Board, proposed a general plan of development for Army and Navy aircraft for the immediate future. It also recommended the creation of the offices of Assistant Secretary of War for Air and Assistant Secretary of Navy for Air in order that the War and Navy departments would each have an official whose whole time and effort could be directed to developing the role of the airplane in warfare. This recommendation was promptly implemented and provided the administrative basis for handling military aviation until after World War II, when the Department of Defense was established.

In early 1927 Secretary Work of the Interior Department set up a five-man Board of Advisors to make a study of the many problems associated with the development of the Colorado River. This subject was very much in the public eye at the time and, because of varying sectional and ideological interests, had become politically highly controversial. Besides Durand, the Board included a former Secretary of the Interior, the Governor of Wyoming, a former Governor of Nevada, and a U.S. Senator.

The Board consulted with many people, public and private, gathered together numerous collections of photographs, reviewed many documents covering earlier studies of the river, and traveled on and along the river by boat, automobile, and horseback from Lee's Ferry, in Arizona, to where the river disappeared in a reed morass just above its junction with the Gulf of California. Each member of the Board wrote individual reports. These reports were favorably received and facilitated the passage of legislation that led to the construction of a dam and the installation of associated hydroelectric power-generating equipment. Durand's report covered the engineering and economic problems involved and demonstrated the desirability and feasibility of the construction of what is now known as the Hoover Dam.

Durand's report found particular favor with Dr. Mead, the Commissioner of Reclamation. He told Durand that if the dam and its associated installations were to be built, he planned to establish a Board of Consulting Engineers to advise the Bureau of Reclamation on the details of its design and construction. Accordingly, when construction of the Hoover Dam was assured, Durand was appointed a member of such a board, and at its first meeting became its secretary. It thus became his duty to record the discussions, findings, conclusions and recommendations, and to develop this material into suitable reports for the signature of the members. Durand's membership on this Board of Consulting Engineers continued until he became absorbed in World War II activities, and it led to his involvement with a number of other large dams, including the Grand Coulee, Shasta, and Friant, as well as numerous smaller ones in various parts of the West and Southwest.

Around 1926 Durand was asked by Mr. Daniel Guggenheim to become a member of the Board of Trustees of the Daniel Guggenheim Fund for the Promotion of Aeronautics. Early in its life this Board received a number of suggestions proposing some form of encyclopedic treatment of the entire field of aeronautics. Durand was asked by the Board to review the situation in this respect and to make recommendations as to a suitable Board policy. He concluded that airplane design and methods of construction were changing so rapidly that an encyclopedic treatment of these topics would be out of date before the work could be completed. On the other hand, he stated, there was beginning to appear a core of fundamental knowledge springing from the theory of fluid mechanics and the application of such theory to aeronautical problems that was sufficiently basic to be definitely secure from marked change with time. The resulting report was well received by the Board of Trustees, who decided that the fund would finance such an undertaking, provided Durand would serve as editor.

The resulting project occupied his time almost completely during the period 1929-1935. He decided that the work should be fully international, and, to achieve this, enlisted the help of

Theodore von Kármán. In a meeting at the University of Aachen, where von Kármán was then teaching, the two developed a complete plan for the work, together with a list of desirable contributors to be contacted and won over. As laid out, the project was divided into twenty divisions, of which Durand wrote three himself. The grant from the Guggenheim Fund provided only for the preparation of the various manuscripts. The work was expected to stand by itself as far as publication was concerned. However, as the first set of manuscripts became ready for the printer, the American firm that had originally agreed to undertake the publication work hesitated due to a feeling of uncertainty as to prospective sales. Accordingly, the agreement lapsed. At this critical juncture the publishing firm of Springer and Co. of Berlin, Germany, contacted Durand with an offer to assume all risks and to publish the complete work in English. In 1936 the last of the six volumes, totaling 2200 pages, was published.

The work was so well received that early in 1939 the publishers proposed the preparation of a supplementary volume to cover new developments. Again working with von Kármán, a plan was blocked out and potential authors contacted. However, the outbreak of World War II put an end to the project, although not before one manuscript had actually been received.

The great importance of the airplane in World War II resulted in a renewed demand for the volumes of the original edition. However, by this time the original edition was entirely sold out. To meet this situation, a limited private company was formed at the California Institute of Technology, which reproduced the entire work by the photo offset process.

The above paragraphs by no means catalog all of Durand's activities in the years between his retirement in 1924 and the onset of World War II. Thus in 1936 he served on a five-man committee to look into the question of tidal power at Passamaquoddy Bay, Eastport, Maine. The construction of dams and a tidal basin for the generation of hydroelectric power based on the abnormally high tide at this location had become a very live political issue, and President Roosevelt had asked Secretary of the Interior Ickes to appoint a special committee to look carefully into the matter. As in previous work of this type, the project involved on-site visits, the gathering and analysis of data, and consultation with many individuals. The resulting report was that the project was possible as an engineering undertaking, but was uneconomic as a source of power in comparison with other sources. The report furnished the means of quietly burying the proposal for the time being.

In the middle 1930s Durand was chosen as the President of the World Power Conference scheduled to be held in Washington, D.C., in September 1936, and this consumed much of his time in early 1936. Durand's welcoming address to the visiting delegates was first presented in English, and then repeated by him in French, German, and Spanish, the official languages of the conference. This multilingual performance was the occasion of much complimentary comment. Other speakers at the conference included President Roosevelt and Secretary of State Hull. The conference was well attended, with the result that no hotel had a dining room large enough to accommodate the entire group at the grand banquet. This problem was finally solved by using the large waiting room at the Union Railroad station as the banquet hall.

After the loss of the airships *Akron* and *Macon,* the Secretary of the Navy in 1935 appointed Durand as chairman of a Special Committee on Airships to study technical and policy questions relating to airships. Three comprehensive reports were made to the Secretary of the Navy over a period of three years.

In 1891, while a faculty member at Cornell, Durand had been elected to membership in the Society of Sigma Xi. That organization had been founded five years earlier at Cornell to provide a place in the domains of science and engineering com parable with that held by Phi Beta Kappa for the humanities. In 1936 and 1937 Durand served as national president for the Society. Since 1936 was the fiftieth anniversary year of Sigma Xi, it involved more than the usual activities.

For several years around 1937 Durand was occupied from time to time with the problem of how to care for the salmon that were prevented by the Grand Coulee Dam from reaching their normal spawning grounds on the Columbia River, above the dam. This problem was of sufficient importance that the Reclamation Bureau appointed a special three-man committee—Durand, an ichthyologist, and an economist—to investigate and report on the situation. After the usual site visits, assembling of data, and conferences with various people, a report was written that made up a substantial little book. It recommended that the salmon collecting in shoals at the foot of Rock Island Dam (a low dam a few miles below Grand Coulee) be trapped in elevator cages and transported to fish culture stations on tributaries of the Columbia *below* Grand Coulee, where they would reach final maturity, and that the eggs then be removed from the females, fertilized, and hatched. The young fry were then to be fed and tended until large enough to be put back into the tributaries, from where they would go to the Columbia, and thence to the ocean, there to live until old enough to return to the upper reaches of the particular tributary in which they spent their youth, and so to repeat the cycle. This plan was adopted with apparently satisfactory results. Later, a study was made of the analogous problem associated with the Shasta Dam, on the Sacramento River.

Not all of Durand's extracurricular activities were associated with engineering and science. For some twenty years, beginning in the early 1920s, he was Vice Chairman of the Board of the Stanford Convalescent Home and Chairman of its Committee on Buildings and Grounds. This was a charitable organization that provided special care for children from underprivileged homes during the final stages of convalescence and was located on the grounds of the Leland Stanford family home at the edge of the Stanford campus.

In March 1941 Durand was called to Washington by Vannevar Bush, who was developing a national research program for defense under the auspices of the White House and who was also Chairman of NACA. Durand had been Chairman of NACA in 1917-1918, as previously noted, and had served as a member for eighteen years (1915-1933). His new assignment was to become a member of NACA again and to chair a new NACA committee charged with the study and development of jet propulsion for application to aircraft.[*] After considering the problem, this Special Committee on Jet Propulsion proposed that a turbojet approach be tried out in addition to the more conventional turbine-driven propeller. Rapid progress was made, with the result that the first test flight of a jet-propelled aircraft using a U.S.-built jet engine[†] took place in the latter part of 1942. A foundation was thus laid that enabled U.S.-made jet engines to assume a strong position in international aviation. With reference to these historic events of 1941 and 1942, Robert Schlaifer has written:

"It was apparently owing in large part to Durand, who was an especially energetic chairman, that jet propulsion was very seriously considered by the Committee. . . . Until this time almost no one in the U.S. had believed that jet propulsion was practical. Engineers generally had previously tended to think of the gas turbine purely as a substitute for the reciprocating engine driving a propeller."[‡]

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[*] It was reported by the late Hugh Dryden that, when Bush requested President Roosevelt to make this appointment, the President suggested that a man of Durand's age should be allowed to enjoy his retirement and that a younger man be sought. Bush was persuasive, however, and got his way.

[†] This engine was a modified version of a British design.

[‡] See Robert Schlaifer and S. D. Heron, *Development of Aircraft Engines and Fuels* (Cambridge: Harvard Univ. Press, 1950), p. 460.

Durand was barely settled in Washington in 1941 when he was asked to take on the chairmanship of the Engineering Division of the National Research Council as an added duty. After studying the situation, he decided that he could handle the two jobs with the aid of a highly qualified executive secretary for the NRC activity. Once again, as in World War I, Durand found himself dividing his time between NACA and the Engineering Division of NRC. He had two offices and typically spent mornings at NACA and afternoons at NRC. The NRC duties involved the supervision of various committees, the drafting of reports, receiving callers, conferring with government officials and civilians, attending meetings, and so on.

One of the by-products of these World War II activities was service on a committee set up by the National Defense Research Committee (NDRC). This committee was charged with supervisory responsibility over research on the phenomena associated with the movement of a ship in a circular path, as when making a sharp turn, and over research on cavitation phenomena associated with the movement of bodies in water at high speeds. These activities led to his appointment as Chief of Section 12.1 of NDRC.

Durand also served as chairman of an NRC committee set up to advise as to the order of need under war conditions of six additional wind tunnels that had been proposed by certain airframe manufacturers. The resulting report served as a guide to the War Production Board in granting priorities for materials and equipment.

In the summer of 1945 when it was clear that the war was coming to an end, Durand resigned from all of his Washington activities and returned to Stanford. He was then more than eighty-six years old and had begun to realize that four years of war activity in Washington had taken something out of him physically. He accordingly decided that in the future he would avoid outside work of all description and settle down to a con dition of full retirement. The next five years passed quietly but pleasantly, punctuated by special events such as receipt in 1946 from the hand of General Spaatz of the Presidential Medal for Merit for his services in World War II and of the first award of the Wright Memorial Trophy for notable work in aeronautical science.

During this period Mrs. Durand's health gradually failed, and she passed on in December 1950 at the age of ninety-one years. A few months later Durand closed his house at Stanford and established himself in an apartment hotel in Brooklyn, New York, a three-minute walk from the home of his son and not too far from his various grandchildren.

Here Durand spent the final seven years of a long and productive life. During this period he was frequently visited by friends, who found him very much alert mentally and also surprisingly active physically, considering his age. In these last years he would attend the parties associated with aeronautics seminars and conferences at nearby Polytechnic Institute of Brooklyn whenever the speaker was a well-known aerodynamicist. At these affairs he would speak fluently in French, when there were French guests. At one such party, held in Durand's ninety-sixth year, he confided to von Kármán that in the last year or two he had begun to notice that he was getting a little old!

Many honors came to Durand during his life. In addition to those that have already been mentioned in this narrative, items of special significance include: Gold Medal, American Society of Naval Engineers (1889); election to membership in the American Philosophical Society (1917); Daniel Guggenheim Medal (1935); John Fritz Medal (1935); Franklin Institute Medal (1938); J. J. Carty Medal of the National Academy of Sciences (1944); Medal of the American Society of Mechanical Engineers (1945); also, honorary degrees from the University of California (1923), University of Utah (1927), and Worcester Polytechnic Institute (1938).

The period of years during which Durand was highly productive was most unusual. Thus, if his life had ended upon retirement in 1924 at the age of sixty-five, he would have been considered as having had a distinguished career. On the other hand, if his life work had consisted only of those things that he carried out after sixty-five, he would have been equally, if not more, distinguished.

Durand had broad interests and an extraordinary ability to grasp and retain ideas dealing with a great diversity of subjects. To quote a statement of one of his close friends, Hugh Dryden, he was "the engineering statesman at work, with sound technical knowledge, creative and imaginative, tactful, intellectually honest, and trustworthy," with an unusual ability to find common ground in the midst of divergent viewpoints. He further had an exceptional gift for both verbal and written expression, such that in informal discussions words and ideas flowed with a polished eloquence that made complex situations clear and understandable even to nonspecialists. This lucidity also characterized his writing, with the result that in a committee he was commonly the man who was asked to draft the report.

These same qualities of scientific expertise, breadth, articulateness, intellectual integrity, and the ability to see all sides of a question, made Durand particularly effective in dealing with complex situations where political, economic, and social problems were intertwined with engineering. Examples include his influential role in such groups as the Morrow Board, the original Colorado River Board, and the Passamaquoddy Committee, where politically controversial questions of a socio-economic-engineering character were at issue.

Durand was not only respected but was also beloved by all who came in contact with him. In spite of his great achievements and many honors, Durand was a modest man, gentle, kind, generous, and without a trace of conceit or selfish ambition. While dignified and reserved to an extent that he had very few close intimates, he was fundamentally friendly and genuinely enjoyed interesting himself in the problems and activities of others. He had innumerable friends whose attachment to him was based upon mutual respect. Although at the time of his death on August 9, 1958, at the age of ninety-nine years and five months, Durand had outlived all of his contemporaries, he left behind a great number of devoted friends made up of former students and of men who had worked with him at one time or another. While these admirers were saddened by Durand's passing, their feelings were tempered by the fact that he led a life that was rich in significant contributions to mankind, made continuously over an unusual span of years.

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*Key to Abbreviations*

Am. Electr. = American Electrician
Am. Mach. = American Machinist
Am. Microsc. J. = American Microscopical Journal
Automot. Ind. = Automotive Industries
Cassier's Mag. = Cassier's Magazine
Electr. World = Electrical World
Eng. News = Engineering News
Eng. Rec. = Engineering Record
J. Am. Soc. Mech. Eng. = Journal of the American Society of Mechanical Engineers
J. Am. Soc. Nav. Eng. = Journal of the American Society of Naval Engineers
J. Electr. Power Gas = Journal of Electricity, Power, and Gas
J. Franklin Inst. = Journal of the Franklin Institute
Mar. Eng. = Marine Engineering
Mar. Rev. = Marine Review
Mech. Eng. = Mechanical Engineering
Pac. Mar. Rev. = Pacific Marine Review
Proc. Am. Assoc. Adv. Sci. = Proceedings of the American Association for the Advancement of Science
Proc. Electr. Soc. and Soc. Mech. Eng. Cornell Univ. = Proceedings of the Electrical Society and Society of Mechanical Engineers of Cornell University
Sci. Am. Suppl. = Scientific American Supplement
Sibley J. Eng. = Sibley Journal of Engineering
Trans. Am. Soc. Civ. Eng. = Transactions of the American Society of Civil Engineers
Trans. Am. Soc. Mech. Eng. = Transactions of the American Society of Mechanical Engineers
Trans. Soc. Nav. Archit. Mar. Eng. = Transactions of the Society of Naval Architects and Marine Engineers
Univ. Mich. Eng. Annu. = University of Michigan Engineer's Annual

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Carl Eckart

# Carl Henry Eckart

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by Walter H. Munk and Rudolph W. Preisendorfer

Carl Eckart was a major participant in the development of quantum mechanics and atomic physics. At the age of forty, with the advent of World War II, he turned his attention to underwater acoustics and related problems in geophysical hydrodynamics; this was to remain Eckart's primary interest for the rest of his life. His contributions to physics and oceanography are about equally divided. For ten years he directed the University of California Division of War Research and its successor, the Marine Physical Laboratory. For two years he was Director of the Scripps Institution of Oceanography. A shy man, he discharged these responsibilities with precision, elegance, and gentle care.

Carl Eckart, an only child and the son of conservative people of German heritage, was born in St. Louis, Missouri. During his high school years in St. Louis, Eckart's interests were in science and mathematics. These interests, along with an innate ability in fine draftsmanship, left little time for social pursuits. Upon graduation he was awarded a full scholarship to Washington University, in St. Louis, where he received B.S. and M.S. degrees with a major in engineering. Eckart's intention was to turn his interest to mathematics, but this changed to physics, evidently under the influence of Arthur Holly Compton, a physics faculty member (later Chancellor). Compton influenced Eckart to con tinue graduate work at Princeton University, where he went on an Edison Lamp Works Research Fellowship and received a Ph.D. in 1925. It was during this period that Eckart produced his first recorded research paper (with G. E. M. Jauncey) suggesting an extension of Compton's classic photon-scattering experiment to X rays in crystal lattices. Other papers in this period followed (jointly with Arthur's older brother Karl): a study of low-voltage arcs, particularly the oscillatory phenomena arising in the diffusion of electrons against low-voltage fields. He continued this work as National Research Council Fellow at the California Institute of Technology from 1925 to 1927.

During the winter of 1925, Max Born came to Pasadena and gave a lecture on quantum mechanics. This lecture aroused Eckart's interest in a possible general operator formalism for quantum mechanics. Working through the winter of 1925-1926, Eckart developed the formalism and completely familiarized himself with what is now known as the Schrödinger energy operator. In January 1926, when Schrödinger's first paper (of the famous set of four) on wave mechanics appeared in the *Annalen der Physik,* Eckart immediately recognized its revolutionary content. There was, in particular, the puzzling presence of another formulation alternative to Schrödinger's wave mechanics: the matrix mechanics of Heisenberg, which used not the partial differential equation for matter waves, but rather infinite-ordered matrices. Despite their outwardly different structure, the theories yielded identical predictions of atomic spectra and identical relations between atomic constants. Evidently, they were equivalent ways of viewing the same physical phenomena, and Eckart felt that there should be a general mathematical framework that would encompass both formalisms as alternative representations. Working in relative isolation in California, far from the exciting German scientific centers, Eckart soon found the connecting link between the Hilbert space of eigenfunctions of Schrödinger's equation and the matrices of the Jordan-Born matrix algebra (which lay at the base of Heisenberg's mechanics). Eckart's solution was submitted to the *Proceedings of the National Academy of Sciences* on May 31, 1926. But the credit generally went to Schrödinger, whose note to the *Annalen der Physik* containing essentially the same solution was dated March 18. Later that year, in June 1926, Eckart completed his general study of the operator calculus (see the "note in proof" appended to his paper in *Physical Review*). This near miss was a source of disappointment for Carl; on the few occasions when a friend could approach him on the subject, he would comment on his isolation in 1926 from the mainstream of quantum physical activity.*

But this was soon to change. In 1927, Eckart received a Guggenheim Fellowship to study with Arnold Sommerfeld in Munich. Here he worked on the quantum mechanical behavior of simple oscillators using Schrödinger's equation, developing further the operator calculus that would allow rapid and almost mechanical manipulations of the newly discovered matrix mechanics and gaining new insights into the correspondence principle. Applications were made to the electron theory of metallic conduction using Fermi statistics, with particular attention to the Volta effect.

The German fellowship coincided with the culmination of the twenty-year search by European physicists for the key insights that would consolidate the long series of experimental and theoretical advances in the "old" quantum mechanics begun in 1905 by Planck. The search came to an end in the period 1925-1928 with the advent of Heisenberg's matrix mechanics and Schrödinger's wave mechanics. As we saw, Eckart was an integral part of these exciting developments. During his Ger

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* For further discussion of this period of time, see M. Jammer, *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill Book Co., 1966), p. 275. (We have used the above-cited communication dates as they appear in the original papers.

man fellowship, Eckart became ever more deeply absorbed in the mathematics of the period, which was miraculously made available and compiled for quantum physicists in almost fully developed form* by the applied and pure mathematics schools at Göttingen headed by Felix Klein and David Hilbert. It was this mathematics that would guide Carl Eckart's approaches to all his subsequent theoretical investigations.

On his return to the United States in 1928, Eckart was appointed to an Assistant Professorship in the Physics Department at the University of Chicago. Although once again removed from the physics centers of Munich and Göttingen, Eckart continued his quantum mechanical studies over the subsequent fourteen-year period. Particularly noteworthy is the paper (with H. Hönl) on the foundations of wave mechanics, an exposition of the role of group theory in the quantum dynamics of monatomic systems, and the comparisons of the nuclear theories of Heisenberg and Wigner. It was in this period that Eckart built on his formulations of the so-called Wigner-Eckart theorem, a link between the symmetry transformation groups of space (applied to the Schrödinger equations) and the laws of conservation of energy, momentum, and angular momentum.† It is of practical use in atomic spectroscopy. These researches went hand in hand with teaching activities and with a translation (together with F. C. Hoyt) of Heisenberg's tract on the *Physical Principles of Quantum Theory.* In all, the decade of the 1930s saw twenty important papers by Eckart in quantum physics.

Eckart's paper on the electrodynamics of material media (in 1938) suggests a transition in his interests. By that time he had begun to lose interest in the submicroscopic world of matter

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* H. Weyl, "David Hilbert and His Mathematical Work," *Bulletin of the American Mathematical Society* 50(1944):612 (the section on integral equations).

† The basic idea occurs in E. P. Wigncr, "Some Consequences for Term Structure from Schrödinger's Theory," *Zeitschrift für Physik* 43(1927):624. The idea was elaborated in Eckart's 1930 group theory paper. Our description covers only the simpler cases. For a fuller description, see P. Roman, *Advanced Quantum Theory* (Reading, Mass.: Addison-Wesley Publishing Co., Inc., 1965), p. 583.

waves. Perhaps he felt that the trend of quantum mechanical research into atomic systems was toward less-rigorous and only partial analyses of solutions of the associated Schrödinger equations. Physicists, facing the complicated multiple interactions of electron systems in the heavier atoms, were adopting simplified models (such as the shell and liquid drop model) that could only partially describe the physical facts. On the other hand, such venerable subjects as electrodynamics and thermodynamics, worked over as they were by several generations of physicists, still contained obscurities and curious gaps between the pure and applied levels. For example, the thermodynamic basis of heat transport and the mechanism of mixing of fluids needed attention.

The title of the 1938 paper is somewhat misleading, for it implies a reworking of the Minkowski or Lorentz formulations of the subject. In fact, Eckart achieved a unified theory of Maxwellian and quantum electromagnetics, leading to a gaugeinvariant formulation of electrodynamics (previously attempted notably by Mie and Weyl, but without success). He was not successful in extending the formulation to contain as special cases Schrödinger's, Heisenberg's, and Dirac's equations of electrodynamics, but his approach did yield equations closely resembling these famous equations and also portions of gas theory for irrotational motion. This latter feature may seem somewhat incongruous, but it falls out quite naturally from a general approach that postulates a set of moving particles of matter characterized by "states" that can be electric, magnetic, or of other forms. (The resultant variational formulation of the kinetic theory of such particles is subject to the constraint of Ampère's law.) By shutting off the electric states, a portion of gas theory is recovered. This paper is one of Eckart's major contributions towards a physical synthesis.

The 1938 paper prepared the way for "The Thermodynamics of Irreversible Processes," parts I, II, and III of which were published in 1940. The first paper showed how the entropy increase in a simple viscous fluid can be calculated and the Kelvin—Fourier hypothesis of heat conduction can be rigorously deduced from the laws of thermodynamics. Thus, this ostensibly empirical law was placed into the fold of classical thermodynamics. In the second paper, in a similar vein, Ohm's law and Fick's law of diffusion were shown to be special cases of more general thermodynamic laws. This paper also discussed the general theory of entropy increase in fluid mixtures. (Eckart's subsequent work in hydrodynamics of the oceans and the atmosphere had its beginnings in this work.) In a third paper, the concepts of relativity and of fluid dynamics were related. The influence of these three papers in the field of irreversible processes was immediate and long-lasting, as a study of the subsequent work by Tolman and Prigogine shows. Twice more, Eckart would reach for a thermodynamic formulation of physical laws, starting from the particle level. Paper IV (1948) deals with elasticity and anelasticity; paper V, with shock waves and phase boundaries (written in 1965 but never submitted for publication).

In December 1941, the United States entered World War II. It was already clear to responsible scientists in mid-1941 that preparations for the national defense must be made realistically and without further delay. Axis submarines were taking their toll of shipping. University scientists were being approached by the U.S. Navy concerning the problems of optical and acoustical detection of enemy submarines. V. O. Knudsen, director of the embryonic University of California Division of War Research, and his close associate L. P. Delsasso asked Eckart for advice and help. He spent a week in June of that year in San Diego reviewing reports by the British Naval Laboratories and the Naval Research Laboratory in the United States. Impressed with the need for understanding the fundamentals underlying submarine detection, he took leave of absence from the University of Chicago (still an Associate Professor), a momentous decision not only in his own life, but also in the lives of many others. When

Eckart began his thirty-one-year stay in California, during those hectic days of preparation for the national defense, he was forty years old. He had at his beck all of classical mathematics and physics of that time. Now, before him, there were new problems to solve and new concepts to clarify.

Eckart found himself in the unaccustomed role of working with technicians and engineers on a day-to-day basis. He was greatly stimulated by this contact, and many of his later contributions had their roots in this period. In a series of classified reports (some of which are now available), we can trace his growing interest in the problems of sound attenuation in the sea, in the effect of randomly moving sea surfaces on the reflection of sound waves and electromagnetic waves, and in the analysis of time series of acoustical signals. Following the war, Eckart collected his work and the work of others into *Principles and Applications of Underwater Sound,* first issued in 1946, declassified in 1954, and reprinted in 1968. *Principles* serves as standard reference even today. By 1946 many of Eckart's wartime colleagues had returned home, but Eckart decided to remain in California. He terminated his appointment at the University of Chicago and became Professor at the University of California and the first Director of its Marine Physical Laboratory (NIPL), established by Eckart, Roger Revelle and Admiral Rawson Bennett to continue geophysical research of common interest to the academic and navy communities. MPL became an integral part of the Scripps Institution of Oceanography in 1948, and Eckart served as Director until 1952. Under the present leadership of Fred Spiess, MPL continues its vital function.

One of the major puzzles unsolved in the war research was the anomalously high attenuation of sound in seawater. Eckart encouraged experimentalists to work on this problem, particularly Leonard Liebermann, whom Eckart had brought from Woods Hole Oceanographic Institution to join the staff of MPL. As a result of the work by Liebermann and the late

Robert Leonard at UCLA, the attenuation could be ascribed to molecular resonances of certain trace constituents. Eckart's wartime acoustical studies had called attention to the fact that the usual linear formulation was incomplete and that additional effects could be predicted if nonlinear terms were included in the equations. He showed that irradiation of a fluid by sound led to streaming and that the streaming could be used to measure the "second viscosity" of fluids, as distinct from the classical notion of dilational viscosity. This theoretical work was directly responsible for a series of experiments by Liebermann.

During this period there was time to consolidate some prewar studies into two major review papers. Eckart's exposition of the one-dimensional Schrödinger equation for the *Reviews of Modern Physics* was enriched by his wartime experiences with sound and light waves and internal waves in the sea.

Eckart's close attention to the logical development of ideas with regard to fundamentals is perhaps brought out in its most explicit form in his *Encyclopaedia Britannica* article on the ether in physics. Eckart clearly put an immense amount of work into this, and it should remain a classic. It is unfortunate that with the passing of the 1948 edition this article will no longer be generally available. The article traces the evolution of ether from its initial concept as a passive backdrop of space-time events for matter to that of an active participant in these events. In its passive role, Eckart likened the ether to a movie screen that is unaffected by the events cast on it by the movie projector. The Einstein equations changed the passive role of space-time to an active one by equating the Einstein curvature tensor of space-time to the energy momentum tensor of matter. In this way, seemingly empty space between atoms, planets, and stars responded to their presence by accommodating its curvature; and the matter in turn evolved and moved in response to space's curvature. Eckart concluded his 1948 article on a tentative note, anticipating future changes in the ether concept when quantum effects would direct attention to the structure of the ether (space-time) in "the small." Such concepts were in fact developed in the following decade, mainly by the Princeton research group headed by Eckart's friend and colleague John Archibald Wheeler. In 1957, Wheeler and one of his students, Charles Misner, showed that Einstein's field equations could be so interpreted that matter itself (their example used electromagnetic fields) was a property of *empty* space.[*] In conversations with Eckart about these advances, he expressed a neutral attitude, preferring to defer judgment until more empirical evidence was available.

In 1948, Harald Sverdrup, Director of the Scripps Institution, decided that he would return to his native Norway and that Roger Revelle (a Scripps Ph.D. then on duty with the Office of Naval Research in Washington, D.C.) should succeed him. There was some determined opposition, which was resolved by Eckart's appointment to the Scripps directorship, with Revelle as Associate Director. After two years, Eckart resigned, and Revelle succeeded him. Concerning this period, Revelle has written (personal communication):

"After I assumed the job, I rapidly gained a reputation as a poor administrator. But in some ways, compared to Carl, I was an administrative genius. The difficulty was that he took the job too seriously. The rigor in definition and precision of thought, and the inability to leave any loose strings untied, which were his great strengths as a scientist, were just what was *not* needed as an administrator. I remember he spent a good deal of time trying to tidy the Scripps Institution up; it was quite a messy place in those days and this was a completely frustrating job for him.

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[*] C. W. Misner and J. A. Wheeler, "Classical Physics as Geometry. Gravitation, Electromagnetism, Unquantized Change, and Mass as Properties of Curved Empty Space," *Annals of Physics* 2(1957):525. C. W. Misner, "Feynman Quantization of General Relativity," *Reviews of Modern Physics* 29(1957):497.

"In other ways, however, he was a great leader. He had good taste in people, in choosing staff, and even more, he had the ability to see what was good about them, what was original about them, and to help them fulfill their promise."

Following the two-year directorship, Eckart returned with vigor to research and teaching. Several generations of students were to benefit from his outstanding teaching abilities. Many of Eckart's studies are in the form of lecture notes and Scripps reports that have never been published. This is particularly the case for his work on stochastic processes and geophysical time series. These studies originated in wartime and were conducted in parallel with, but quite independently of, a large effort at the Massachusetts Institute of Technology's radiation laboratory. The MIT work was published promptly after the war, but Eckart's contributions have not been generally recognized.

Eckart's blackboard work was a reflection of the working of his mind: he would start in the upper left-hand part of the leftmost board and then work slowly, with his elegant handwriting, down the board, and then onto the next, developing as he went a set of ideas woven through with a logical thread. Eckart's "rough" notes, written in ink as they occurred, usually without corrections, are so well-worded, annotated, and spaced that they could not be improved. (They lose some of their clarity when subsequently set in print.) In the solitude of his study, there would be held lightly in the fingers of his left hand the ever-present smoldering cigarette; its fumes randomly swirling about his head as he strove, oblivious to the thickening haze, toward the end of some syllogistic trail.

Eckart depended on his colleagues for ideas and stimulation; yet he was impatient and driven to distraction by the lack of rigor with which some of his scientific associates presented their problems. Turbulence, described with an appropriate wave of the hands, was the all-encompassing sink of oceanographic ignorance. This drove Eckart to his 1948 paper on stirring and mix ing, emphasizing the fundamental orthogonality between these two processes, which many had thought to be equivalent. This is one of the basic papers in the modern theory of turbulence. At about the same time Walter Munk gave a seminar on the wandering of the Earth's poles, based on the concept of a "Maxwell body" that exhibits the property of a viscous fluid under low-frequency disturbances and that of a solid at high frequencies. Eckart questioned Munk strenuously concerning this use of the "Maxwell solid," and when Munk gave as his defense that the model had been used by everyone, including Lord Kelvin himself, Eckart said that this was no excuse whatsoever. Eckart went home and overnight wrote a set of notes, drawing on his prior studies of irreversible processes and his recent review paper on ether (a Maxwell solid according to Stokes's theory); these notes grew into the fourth paper in the thermodynamics series. He emphasized the fundamental difference between the concepts of *strain* and of *deformation* in elastic and anelastic materials. In making this distinction, as in the previous work on *stirring* and *mixing,* Eckart followed his usual working procedure: to start by carefully defining some fundamental hitherto not well-formulated concepts and to develop their implications through rigorous mathematical reasoning.

During this period Eckart lectured to Scripps students on the analogy between the ray theory of (ocean) waves and the trajectories of particles, providing a convenient formalism for the propagation of ocean waves over irregular bottoms. This led to one of the earliest applications of computer technology to geophysical problems. The difficulty inherent in the theory of scattering of ocean waves by irregular bathymetry is great: the traditional methods of transform techniques, separation of variables, and simple boundary value problems are no longer available. Accordingly, recourse to numerical methods must eventually be made. Henry Stommel, of Woods Hole, and Walter Munk were invited to Princeton to discuss with John von

Neumann problems in oceanography that could be suitably attacked with the newly developed computers. Before going on to this visit, Munk had asked Eckart's advice, who had suggested the problem of wave scattering by an irregular seafloor. Von Neumann's response was, "Well, anything that Carl Eckart thinks worth doing is worth doing." In subsequent years, the study of ocean tides, ocean circulation, and near-shore wave processes would become major numerical research problems.

In 1953, Eckart took a year's sabbatical at the Institute of Advanced Study in Princeton. In rapid succession he produced a remarkable quartet of papers on wave propagation in stochastic media. "The Theory of Noise in Continuous Media" presented an original view of the propagation of the covariance field as governed by a wave equation. "Relation Between Time Averages and Ensemble Averages in the Statistical Dynamics of Continuous Media" expanded the idea of using the equations of the dynamics of a continuum to aid in studying the connections between time and ensemble averages. "Generation of Wind Waves on a Water Surface" developed the theory of ocean wave generation by random wind gusts. (This work predates the important contribution by O. M. Phillips.) The fourth paper, "Scattering of Sound from the Sea Surface," has been widely applied; here Eckart inferred the two-dimensional spatial spectrum of the sea surface from the scattering function of relatively long sound waves. Analogies were developed among sound scattering from the sea surface, light (Rayleigh) scattering in the atmosphere, and scattering of light by molecular (crystal) media. The work also made connections with the ray theory of scattered light from the sea surface. The principal finding was that short acoustic waves were less effective than long waves in describing the spatial wave spectra of the sea (counter to the well-known crystal molecular case). The mathematical methods are extendable to the scattering by the sea of electromagnetic (particularly over-the-horizon radar) waves. This paper represents a contribu tion to geophysics of an important type of inverse problem: wherein the local structure of a medium (the atmosphere, the sea, or earth) is inferred from its response to appropriately directed remote field probes.

Shortly before leaving on this productive sabbatical, Eckart was seen by Ellen Revelle walking along the street in deep concentration. Her cheery hellos were ignored. Some time later he apologized for having been so grumpy and distracted: "The trouble was that I was pregnant—pregnant with ideas."

By the mid-1950s Eckart's experience with linearized perturbation theory applied to geophysical hydrodynamics began to take definitive form. He reexamined the classical methods of linearizing the Navier-Stokes and thermodynamics equations and developed (with Horace Ferris) a set of unified hydrodynamic/thermodynamic equations of motion of oceans and atmospheres. This formed the basis of his monograph, *Hydrodynamics of Oceans and Atmospheres,* which stands as a bridge between the early formulations of the *Physikalische Hydrodynamik* and the perturbation expansions of later investigators. Only the simplest plane-parallel and spherical-parallel geometries are considered, but with a fully stratified atmosphere and ocean on a rotating Earth. The manifold types of oscillatory motion of the air and sea are unraveled, from the grand, slow, free oscillations of the global atmosphere and oceans to the rapid oscillations of sound in these same media. Out of the many possible mathematical formulations of this subject, Eckart characteristically chose that which was most elegant mathematically and, to him, physically meaningful. In particular, he departed from the usual way of representing the sea motion by its surface elevation function and instead used a normalized entropy function. Consequently, his equations take on great simplicity in which the self-adjointness of the hydrodynamic system is manifest. But this elegance had been gained at the expense of ready visualization (and some of the audience).

In the late 1950s and early 1960s, Eckart became concerned with the equation of state of seawater. His primary objective was a critical examination of the $p$-$v$-$T$ data on seawater, and the construction of a simple but adequate empirical formula for the equation of state. Such a formula is useful, for example, in determining accurately the eigenmodes of internal waves in the sea, a subject that then occupied Eckart. In this period he wrote a closely connected pair of papers on the stability of unidirectional laminar flow of a stratified compressible fluid. The key to these studies was the transformation of the hydrodynamic equations to general coordinates so as to exhibit the general form of a pseudopotential energy function. This was applied to the stability problem concerned with the extension to compressible flow of Howard's circle theorem for incompressible laminar flow. The result has applications to atmospheric jet streams. He also attempted to deduce the equations of the macroscopic theory of matter from those of the N-particle problem without using the concept of probability. A difficulty arose: The method could not yield the derivation of entropy. In this failure, Eckart nevertheless deepened our perspective of the inherently probabilistic nature of entropy.

In the 1960s Eckart participated in the development of the new campus of the University of California that had grown from the Scripps Institution. In 1963-1965 he served as Chairman of the Academic Senate of the University of California, San Diego, and in 1965-1969 as Vice Chancellor. He was responsible for Academic Planning of the fledgling campus, and his projections have turned out to be remarkably accurate. Yet he did not derive much satisfaction from these responsibilities, and returned to his research during every possible moment. Paper V in the series on irreversible thermodynamics falls into this period. The manuscript (unpublished) is an important contribution to the thermodynamic characterization of evaporation and condensation. Following the Rankine-Hugoniot boundary condition developed earlier for shock waves, Eckart specifically considered phase changes and added to the classical boundary conditions the generation of entropy across the phase discontinuity boundary. This led to a theory of evaporation and condensation, predicting that energy is added or removed via radiation rather than convection. In evaporation, a thermal boundary layer is formed in the liquid, and a high-pressure gradient opposes the flow in a thin layer in the vapor. In condensation, the thermal layer occurs in the vapor, the pressure gradient in the liquid. The mathematical and physical similarities between shock waves and phase boundaries in chemically homogeneous substances are thus established.

This study, as his earlier ones, was marked by Eckart's abiding preoccupation with the powerful methods of classical mathematical physics. The mathematical themes were those of the Sturm-Liouville differential systems, of integral operators, of classical groups of motions in Euclidean spaces, of useful analytical transformations, of variational principles; in short, of all the notions arising in prerelativistic, prequantum physics. Revelle remarked to von Neumann that he never really understood Eckart's mathematics, and that they were difficult to follow. Von Neumann replied that Eckart's mathematics were quite simple and easy to follow, and that Eckart had great ability as a mathematician; but that he was first, last, and always a physicist.

The single-minded and intense devotion to his work took their toll, first of Carl Eckart's private life, and later of his health. He was a lonely man, who had little supportive home life in his first marriage. This finally ended in divorce after some eighteen years of his constant, but unsuccessful, attempts to help his wife through severe psychological problems. Carl Eckart, always shy, remained somewhat aloof from the social activities of La Jolla, then a maturing seaside village. After the death of Eckart's great and good friend John von Neumann, Klara von Neumann turned to Carl Eckart for solace and companionship.

Their marriage led to a brief period of happiness and active participation in the sprawling life of the postwar Scripps Institution and beginnings of a general university campus, until Klara's tragic drowning in 1963.

During the two remaining years of his life after retirement in 1971, Eckart's thoughts turned to a task which he had been considering for years: the summing up in book form of his beliefs concerning the role of mathematical science in furthering human society. This task became urgent as his eyesight began to fail. With the help of friends, notebooks and reference materials were supplied as needed. The evolving manuscript was called, "Our Modern Idol: Mathematical Science." Eckart found that the promise of mathematical science in furthering man's *social* progress was hollow. This realization, though unpleasant for him at the time, was in the end salutary. For he realized that great scientists such as Ernst Abbe and Bertrand Russell could use their clear insights into social problems without recourse to mathematical analysis. These men were for Eckart fine examples of concerned scientists who could use their knowledge to educate fellow humans so that the latter could in turn further the social progress of mankind. In Essay 9, Part IV, of the manuscript, Eckart writes:

"When men of proven mathematical creativity become seriously concerned with the problems of people and society, they abandon the mathematical methods of which they are masters. Their actions show that they do not consider that problems of society are amenable to mathematical theories and calculations. No matter how pessimistic they may be about the future, or how ungratefully their efforts to improve it are received, they do not become fatalistic. Their hope for improving the future of Man rests not on inexorable mathematical calculations but on the ability of people to make decisions, to make plans, and to implement them."

Another principal concern of Eckart in this, his final study, was the often fallacious use of language by the ancient western philosophers in cataloging their perceptions and conceptions of the real world, and the malevolent persistence of their confusions down to the present. In particular, these errors arose in the improper separation of the kind of thinking that scientists do about the real world from that kind of thinking all of us do about the thinking of humans. These two kinds of thinking are designated by A. N. Whitehead as *homogeneous* and *heterogeneous* thinking, respectively.* Eckart made three applications of this classification: to the long-standing problems of the faulty development of knowledge and its faulty communication from one generation to another; to the needless mental confusion of ethical and scientific matters; and last and most painful for him, to the seeming impotence of mathematical reasoning to show society the way clear of its political, economic, and social problems. These applications were developed by Eckart in a series of several dozen essays over a span of 2,000 handwritten pages, showing his concern for social progress and the responsibility of scientists to assure the proper use of their discoveries.

Death overtook Eckart before this work was finished. Plans are being made by friends to prepare the incomplete manuscript for publication as a book.

When Eckart first accepted the challenge of the oceans and the Earth as a test of his mathematical and physical insight, he had available the most powerful tools of his generation. He felt that if only he could spend ten concentrated years on the problems of oceanography and geophysics, he would "solve" them on some level of satisfaction. As he made progress, the complexity and difficulty of the problems grew at approximately the same rate as the evolving solutions, perhaps at a slightly greater rate. After having worked in this oceanographic setting during his

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* A. N. Whitehead, *The Concept of Nature* (Cambridge: Cambridge University Press, 1964), chap. 1.

mature lifetime, Eckart probably overreacted and was left with the impression that the problems of the oceans (like the social problems) are unsolvable. Nevertheless, in the thirty or so years between the time when he thought he could solve the problems and the times when he thought that they were unsolvable, he provided inspiration to a generation of oceanographers.

Eckart was elected to the National Academy of Sciences in 1953 and received the Academy's Alexander Agassiz Medal in 1966.

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*Key to Abbreviations*

Am. J. Sci. = American Journal of Science

C. U. W. of C. E. = Collected Unpublished Works of Carl Eckart*

J. Acoust. Soc. Am. = The Journal of the Acoustical Society of America

J. Mar. Res. = Journal of Marine Research

Phys. Fluids = The Physics of Fluids

Phys. Rev. = Physical Review

Proc. Natl. Acad. Sci. USA = Proceedings of the National Academy of Sciences of the United States of America

Rev. Mod. Phys. = Reviews of Modern Physics

Scripps Inst. Oceanogr., Ref. No. = Scripps Institution of Oceanography, Reference Number

Univ. Calif. Div. War Res. Rep. = University of California Division of War Research Report

Z. Phys. = Zeitschrift fir Physik

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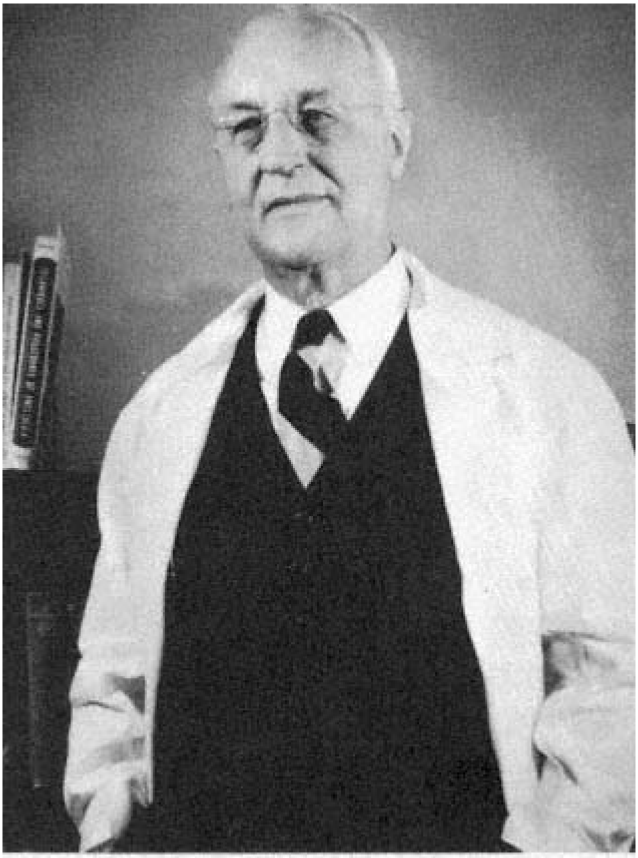

Evarts A. Graham

# Evarts Ambrose Graham

March 19, 1883-March 4, 1957

by Lester R. Dragstedt

Evartz Ambrose Graham was born in Chicago, on March 19, 1883, and died in St. Louis of cancer of the lung on March 4, 1957. At the time of his death Dr. Graham was widely recognized as the leading surgeon of his day. He was, in every sense, a surgical statesman and was for many years the most influential voice in surgical meetings all over the world. He had devoted many years to the study of cancer of the lung and, together with Dr. Alton Ochsner of New Orleans, had pointed out the important role of cigarette smoking in the cause of this disease. In 1933, he first successfully removed the lung from a patient with lung cancer. This patient survived and was cured of his disease. Ochsner and Graham noted that practically all of the patients with lung cancer upon whom they operated were habitual cigarette smokers. Not long before his final illness, Dr. Graham and his wife, together with my wife and I, attended a surgical convention in Glasgow, Scotland, and were houseguests of Professor and Mrs. Arthur Mackey. Mrs. Mackey, a charming young lady, was smoking a cigarette when the Grahams and Dragstedts arrived at their home. To our consternation, shortly after the introductions, Dr. Graham took the cigarette away from Mrs. Mackey and told her that that was the last cigarette that she was to smoke. He said that he had been a confirmed cigarette smoker all of his life and that it was too late for him, but not too late for her, to quit. Possibly he knew at that time that he had lung cancer and that the involvement of both lungs made removal by a surgical operation impossible. Before Dr. Graham and Dr. Ochsner reported their clinical studies, cigarette smoking was so common among surgeons that their convention rooms were often so clouded that it was difficult to see the speakers. By the time Dr. Graham died, it was almost impossible to find a surgeon smoking a cigarette.

Evarts Graham attended public schools and subsequently the Lewis Institute in Chicago. In the fall of 1900 he entered Princeton University and in 1904 graduated. His father, Dr. David W. Graham, was a leading surgeon on the west side of Chicago. He was a charter member of the staff of the Presbyterian Hospital and was president of the medical staff from 1898 to 1901. Although David Graham had contact with Christian Fenger, the Danish physician who first brought to Chicago and the Midwest knowledge of cellular pathology, bacteria, and infectious disease, he remained skeptical and paid scant attention to aseptic techniques in his surgical work. As a beginning medical student in 1911, I recall seeing "Daddy" Graham, as we students called him, perform an operation for the removal of tuberculous lymph glands in the neck of a child. Evarts Graham was his assistant and did all that he could to persuade his father to observe the principles of aseptic surgery. However, when Daddy Graham had finished scrubbing his hands and rinsing them in an antiseptic solution, as a final measure he washed his beard in the solution to the dismay of his son Evarts. We students were delighted, because, at this time, we had been taught something of bacteriology and were persuaded of course about the aseptic method of surgery.

Evarts's mother, Ida Barnett Graham, was a woman of extraordinary intelligence and energy, who devoted much of her life to public service, especially in connection with the Presbyterian church and hospital. For many years she was chairman of the woman's board of the hospital, a voluntary organization repre senting the Presbyterian churches of the Chicago area and including a general membership of public-spirited women. This remarkable woman was not only an inspiration to her husband and son, but also to other surgeons as well. With this background it is not surprising that Evarts embarked on a career in surgery.

After completing his studies at Princeton, Evarts Graham pursued medicine at Rush Medical College. At that time, the first two years of the medical course were given at The University of Chicago and the last two years at Rush Medical College, on the west side of Chicago, near the Cook County Hospital. At The University of Chicago, he was exposed to the inspiring teaching of Dr. A. J. Carlson, H. Gideon Wells, R. R. Bensley, and many others. After the completion of these two years, Evarts entered Rush Medical College and began his training in the clinical subjects. He made an outstanding record as an undergraduate student and was given an appointment in pathology with Ludvig Hektoen. During this period, he collaborated with Dr. Ernest E. Irons in a report on generalized blastomycosis. He received an M.D. degree in 1907 and spent the following year as an intern in the Presbyterian Hospital, where he became a close personal friend of Dr. Rollin T. Woodyatt, an internist some ten years his senior.

Woodyatt had just returned from a year of postgraduate study in the clinic of Professor Friedrich Muller, in Munich, and was charged with enthusiasm for the scientific spirit and investigative insight of this man. He sought to develop in Chicago a scientific clinic patterned on that of Muller, who was an able chemist in addition to being a leading internist; and this no doubt was responsible for Woodyatt's advice to Evarts to secure more training in chemistry. Dr. Arthur Dean Bevan, Chairman of the Department of Surgery in Rush Medical College, thought that Evarts was making a mistake in withdrawing from clinical work to spend two or three years in chemistry. He, as well as Evarts's father, failed to see how a knowledge of chem istry could be useful to a general surgeon. Evarts's persistence in studying chemistry in spite of this opposition is testimony to his independence and determination. However, Evarts has said this about his father: "I shall always be grateful to him. He supported me very eagerly both financially and by sympathetic understanding during the time that I stretched out my period of graduate training, even including the two years that I spent in the study of chemistry."

It was at The University of Chicago that Dr. Graham met Helen Tredway, who was also a graduate student in pharmacology. They were married in 1916. Throughout his life, Dr. Graham enjoyed the enthusiasm and intellectual support of this remarkable woman. In addition to her household duties and the care of two young children, Helen Tredway Graham became an associate professor of pharmacology at Washington University in St. Louis and continued an active career in teaching and research until she retired, in 1959. She was also active in a wide range of educational and civic matters, including civil liberties and air pollution control. She served as vice-president of the St. Louis League of Women Voters and a board member of the St. Louis Civil Liberties Committee. Mrs. Graham helped draft the civil service provisions of the St. Louis County Charter and was a member of the Board of Freeholders that drafted the metropolitan district plan for the coordination of services in St. Louis County. Like her husband, Mrs. Graham became concerned over the health dangers caused by air pollution and was instrumental in helping to secure air-sampling stations in St. Louis. She died of a heart attack in 1971, when she was eighty years old.

In 1915 Dr. Graham entered upon the private practice of surgery in a clinic in Mason City, Iowa. This was, on the whole, a disappointing experience. It was here that be became impressed with the evils of fee splitting and ghost surgery. It was often the practice of medical men to refer patients to surgeons for operations and receive in return a portion of the surgeon's fee. The surgeon often felt that the medical man was insufficiently rewarded for his diagnostic work and so agreed to the split. Unfortunately, some surgeons returned a larger portion of the fee in order to secure more referred patients; and some medical men chose often inferior surgeons who gave a larger return. When Dr. Graham became President of the American College of Surgeons, he used his great influence to persuade the surgical societies to stop this practice among their members.

In 1918 Dr. Graham enlisted in the U.S. Army, was commissioned a captain, and sent to Fort Lee. He had been assigned to take a course in neurosurgery when he was visited by Dr. Allen B. Kanavel, a leading Chicago surgeon, who was on duty as a consultant in the office of the Surgeon General of the Army. Dr. Kanavel told Graham that there was growing apprehension about the treatment of empyema (collections of pus in the chest cavities) in the various army camps. The country was then in the first year of an influenza epidemic that would undoubtedly increase in severity. Pneumonia, accompanied by empyema, often followed the influenza and was the chief cause of death. Dr. Kanavel suggested that Graham work on this problem because of his unusual chemical training. Dr. Graham agreed and was shortly sent to Camp Lee to join with bacteriologist Edward K. Dunham and chemist Richard D. Bell to become what came to be known as the Empyema Commission.

With the help of Dr. Kanavel, a questionnaire was sent to the army camp hospitals; and it was found that the mortality from influenza pneumonia was about 30 percent. Many patients whose pulmonary reserve had been crippled by massive, often bilateral bronchopneumonia, were being hurried to an operating room as soon as fluid containing bacteria was found in the chest. The operation was rib resection with open tube drainage. Death often occurred within a half hour after the operation.

At the time when Dr. Graham and his colleagues on the

Empyema Commission were doing their work, the writer of this memoir was also in the army serving as pathologist at the general hospital in Camp Merritt, New Jersey. The influenza was then at its height, and in the camp of 35,000 men, as well as in the surrounding cities, there was a general feeling of foreboding—almost of fear like that described in London and Paris during the plague. As many as twenty to thirty young soldiers died daily and were brought to the morgue for autopsy. My examination usually revealed both pleural cavities filled with pus and causing such compression on the lungs as to seriously interfere with breathing. At times I requested the physicians who were caring for these patients to drain the pleural and pericardial cavities at an earlier date. They responded by saying that such attempts had proved invariably fatal. The contribution of Dr. Graham and his colleagues consisted of devising methods for the closed drainage of these cavities without permitting air to enter and collapse the lungs. It was a great contribution to the treatment of empyema and opened the way for Dr. Graham's subsequent career as one of the leaders of the new thoracic surgery. *Streptococcus hemolyticus* usually accompanied the influenza in this epidemic and was responsible for most of the deaths. Penicillin, which controls this deadly infection, was not then available. Fortunately, influenza accompanied by *Streptococcus hemolyticus* seems now to have disappeared.

At his urgent request, Dr. Graham was given overseas duty as commanding officer of U.S. Evacuation Hospital #34 in France. On returning to the United States after the war, in the spring of 1919, he was assigned to Fort Sheridan, in Illinois. The following account of Dr. Graham's appointment as Professor of Surgery at Washington University Medical School was given to me by Dr. Philip Anderson Shaffer:

"At that time, members of the staff of base hospital #21 from Washington University were also returning from France. During their absence many circumstances had changed. Dr. Fred

Murphy, chief surgeon of base hospital #21 was also Professor of Surgery in Washington University and head of that department. During his absence the full time system in medicine had been adopted for heads of the clinical departments. This plan displeased Murphy and led to his retirement thus making it necessary to seek his successor.

"In 1916, I had been drafted as Dean of the Washington University Medical School and, in that capacity, went to Chicago in search of a candidate for our department of medicine. My friend Dr. Rollin T. Woodyatt, whom I consulted, told me that if I had wanted a surgeon he could have named an excellent candidate. He cited the talents and accomplishments of Evarts Graham who, however, had just accepted appointment to a clinic in Mason City, Iowa.

"In 1917, I had been sent to France as an officer in the section of food and nutrition in the sanitary corps attached to the surgeon general's office. At that time I received the resignation of Dr. Murphy as professor of surgery at Washington University Medical School. I recalled the praise of Woodyatt and others of a young surgeon whose name I had forgotten. My files however disclosed it. Dr. Graham was located at Fort Sheridan and a committee of the faculty was sent to confer with him as to his qualifications and interest in the position in St. Louis. He was invited to visit the school, which he did on June 6 and 7, 1919. The corporation approved his appointment as Professor of Surgery effective July 1, 1919.

"Evarts's prompt acceptance of this appointment after such a short visit was surprising to me but was explained many years later when by chance I recognized his face in a group photograph of a large attendance at the first convention of the Federation for Experimental Biology and Medicine to meet in St. Louis. Examination of the program of that meeting showed that Evarts had read a paper there and had taken part in the discussion. He had already explored the plan for a modern medical school and appreciated the opportunity for the development of his ambitions. With the acceptance of that appointment he entered into associations that continued for the rest of his life: devoted and loyal to his friends and to his responsibilities, of unshakable mental integrity, outspoken and with wide vision. He was invaluable not only to his department and field, but as a member of the executive faculty of the whole medical school and from this post his influence in the field of medical education and practice spread worldwide."

This eloquent tribute by Dean Shaffer was re-echoed by the many faculty members who attended Dr. Graham's retirement dinner.

Dr. Graham entered upon his work as professor of surgery at Washington University with enthusiasm and high hopes. He had long been interested in the work of Peyton Rous and P. D. McMaster on the function of the gallbladder. These men had demonstrated that the thin bile from the liver was stored in the gallbladder between meals and concentrated there by the absorption of water by the gallbladder mucosa. John J. Abel and Leonard Rowntree had discovered that the chemical phenoltetrachlorphthalein, when injected into the blood stream, was selectively removed from the blood by the liver and excreted in the bile. A similar compound, phenoltetrabromthalein, was being used as a test of liver function. Dr. Graham speculated that if iodine could be substituted for chlorine in the molecule of this drug then perhaps the phenol tetraiodothalein would also be selectively excreted in the bile. Iodine being opaque to X rays would make the bile cast an X-ray shadow, and so the gallbladder could be visualized. He was able to secure sodium tetraiodophenolphthalein from the Eastman Kodak Company and began his work in the laboratory on experimental animals. Drs. Warren Cole and Glover Copher assisted in these experiments. Dr. Cole relates that, although they were able to visualize the gallbladder in dogs, when they first administered the drug to patients with gallstones or suspected gallstones, no visualization of the gallbladder occurred. This was a great disappointment, as it was hoped that by this method of visualizing the gallbladder by X ray a diagnosis of gallstones could be made or confirmed. Fortunately, they later gave the drug to patients without symptoms of gallbladder disease and found that the gallbladder visualized perfectly. Subsequent experiments revealed that the method provided a good test for the function of the gallbladder. If the gallbladder mucosa were normal, it would concentrate the bile; and the concentrated bile containing the drug was visualized by X ray. When the gallbladder wall was diseased and did not concentrate the bile, there was no visualization of the gallbladder. Nonvisualization of the gallbladder indicated that the mucous membrane of the gallbladder was not normal and so did not concentrate the bile. Often the visualized gallbladder displayed gallstones that, because of the absence of the drug in the stones, cast a negative shadow. These discoveries by Dr. Graham and his associates made the diagnosis of diseases of the gallbladder much more accurate and, in addition, proved very useful in further investigations of the function of the gallbladder in other conditions. Undoubtedly Dr. Graham's work on the gallbladder was influential in his election as a member of the National Academy of Sciences in 1941.

Although teaching and administrative burdens consumed much of Dr. Graham's time, he devoted himself with great success to the study of chest diseases along with his work on the gallbladder. His department became one of the leading centers for thoracic surgery in the United States. At that time removal of a lobe of the lung was occasionally done in patients with cancer of the lung where the tumor was thought to be limited to one lobe. Dr. Graham was operating upon a fellow physician when exploration of the lung revealed that the cancer involved more than one lobe. To the awe of the surgeon spectators, he then proceeded to remove the entire lung. It is probable that he had considered this eventuality before and that his decision to remove the entire lung was not so casual as it seemed. The patient recovered and was cured of his disease. This surgical triumph in 1933 electrified the surgical world, and, in addition to adding to Dr. Graham's fame, stimulated other surgeons to try to cure these unfortunate patients. In succeeding years, Dr. Graham and his associates operated upon many more patients sent to them from all parts of the world. I recall one of his forceful statements, namely, that every patient upon whom he had operated for cancer of the lung had been an inveterate cigarette smoker.

Four days after Dr. Graham's death, the Board of Directors of Washington University on March 8, 1957, passed the following resolution:

"Whereas, Dr. Evarts Ambrose Graham lighted man's way to longer life and better health by his diligent pursuit of truth and by his brilliant and courageous achievements in surgery and medical research; and

"Whereas, Dr. Graham devoted thirty-eight years of his life to a distinguished career with the Washington University Medical School, receiving international acclaim for his valuable leadership in medical education; and

"Whereas, Dr. Graham served the University faithfully and with excellent results in many special assignments, including chairmanship of faculty committees to select new chancellors;

"Therefore, be it resolved that the Board of Directors of Washington University express its gratitude for the life of this great man and pay tribute to a memory that will forever deserve a place of honor in the annals of man."

# HONORS AND DISTINCTIONS

## Academic Positions

1910-1914 Assistant in Surgery, Rush Medical College

1919-1951 Professor of Surgery, Washington University School of Medicine, and Surgeon-in-Chief, Barnes Hospital and St. Louis Children's Hospital

## Military Service

1918 Major, Medical Corps, U.S. Army, with Empyema Commission. Later Commanding Officer, Evacuation Hospital #34 in France

## Honorary Degrees

1926 Ll.D. Central College

1927 Sc.D. University of Cincinnati

1928 M.S. Yale University

1929 Sc.D. Princeton University

1931 Sc.D. Western Reserve University

1940 Sc.D. University of Pennsylvania

1941 Sc.D. University of Chicago

## Lectureships

1924 and 1934 Harvey Lecturer

1924 Mutter Lecturer

1926 McArthur Lecturer

1928 Shattuck Lecturer

1930 Melbourne (Australia) Permanent Postgraduate Committee

1930 Alvarez Lecturer

1931 Joyce Lecturer

1932 Arthur Dean Bevan Lecturer

1933 Caldwell Lecturer

1935 Balfour Lecturer

1937 Judd Lecturer

## Awards and Distinctions

1920 Co-editor, *Archives of Surgery*

1920 Gross Prize

1921 Société Internationale de Chirurgie

1922 Sent by Rockefeller Foundation to Great Britain to investigate teaching of surgery in British Medical Schools

1924-1933 Member, National Board of Examiners

1925 Temporary Surgeon-in-Chief, Peter Bent Brigham Hospital

1925 Editor, *Yearbook of Surgery*

1925 Gold Medal, Radiological Society of North America

1925 Leonard Research Prize, American Roentgen Society

1925-1939 Member, Medical Fellowship Board, National Research Council

1927 Gold Medal, St. Louis Medical Society

1928 President, American Association for Thoracic Surgery

Society of Clinical Surgery

1931 Editor, *Journal of Thoracic Surgery*

1932 Kaiserlich Deutsche Akademie der Naturforscher

1933 Gold Medal, Southern Medical Association

1934 Co-editor, *Annals of Surgery*

1937 John Scott Medal of the City of Philadelphia

1937 President, American Surgical Association

1937-1941 Chairman, American Board of Surgery

1938 Honorary Fellowship, Association of Surgeons of Great Britain and Ireland

1938 Honorary Membership, Society of Thoracic Surgeons of Great Britain and Ireland

1939 Temporary Professor of Surgery, St. Bartholomew's Hospital, London

1940 Chairman, Committee on Surgery, National Research Council

1940-1941 President, American College of Surgeons; elected to the Board of Regents, 1941

1941 Member, Royal Society of Sciences, Uppsala, Sweden

1941 Member, American Philosophical Society

1941 Member, National Academy of Sciences

1941 Honorary Member, Argentine Society of Surgeons

1942 Member of committee appointed by Secretary of War to study the Medical Department of the Army

1942 Lister Medal for 1942

1942 St. Louis Award

1943 Honorary Fellowship, Royal College of Surgeons of England

# Bibliography

*Key to Abbreviations*

Am. J. Med. Sci. = American Journal of Medical Sciences
Am. J. Roentgenol. = American Journal of Roentgenology
Am. J. Surg. = American Journal of Surgery
Am. Rev. Tuberc. = American Review of Tuberculosis
Ann. Clin. Med. = Annals of Clinical Medicine
Ann. Surg. = Annals of Surgery
Annu. Rep. Barnes Hosp. = Annual Report of Barnes Hospital
Arch. Pathol. = Archives of Pathology
Arch. Surg. = Archives of Surgery
Cancer Res. = Cancer Research
Dis. Chest = Diseases of the Chest
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Exp. Med. = Journal of Experimental Medicine
J. Infect. Dis. = Journal of Infectious Diseases
J. Mo. State Med. Assoc. = Journal of the Missouri State Medical Association
J. Thorac. Surg. = Journal of Thoracic Surgery
N. Engl. J. Med. = New England Journal of Medicine
Postgrad. Med. = Postgraduate Medicine
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
South. Med. J. = Southern Medical Journal
Surg. Clin. North Am. = Surgical Clinics of North America
Surg. Gynecol. Obstet. = Surgery, Gynecology, and Obstetrics
Trans. Chic. Pathol. Soc. = Transactions of the Chicago Pathological Society
Wash. Univ. Med. Alumni Q. = Washington University Medical Alumni Quarterly

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Latency of carcinoma. Surg. Gynecol. Obstet., 4(6):701-4.

1908 On the phagocytability of pneumococci in the sputum in pneumonia. J. Infect. Dis., 5:273-78.
1909 Olive oil for post-anesthetic vomiting. J. Am. Med. Assoc., 53(25): 2094-95.
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1911 The effect of ether and ether anesthesia on bacteriolysis, agglutination and phagocytosis. J. Infect. Dis., 8:147-75.
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1914 The origin and nature of foetal movements. Surg. Gynecol. Obstet., 19:360-64.
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Samuel Kirkland Lothrop

# Samuel Kirkland Lothrop

July 6, 1892-January 10, 1965

by Gordon R. Willey

Samuel Kirkland Lothrop was born in Milton, Massachusetts, on July 6, 1892, the elder son of William Sturgis Hooper Lothrop and Alice Putnam (Bacon) Lothrop. His was a distinguished family in the New England Brahmin intellectual tradition. His great-grandfather, for whom he was named, was a leading Unitarian minister of his time and is represented in library card files by almost as many author cards as his great-grandson.

Young Samuel spent his childhood in Massachusetts and Puerto Rico, his father having sugar interests on that island at the turn of the century. He attended Groton school, where he was distinguished by being chosen as Senior Prefect and where he played end on the football team and stroked the crew. He entered Harvard College in 1911, graduating with the class of 1915. Subsequently, he was to pursue graduate work in anthropology and archaeology at that institution.

The beginnings of Sam (as he was to be known to his colleagues) Lothrop's interest in archaeology are obscure; however, his brother Francis, six years his junior, remembers that he had a great friend and Groton classmate, William Crocker, whose father was a collector of antiquities of all kinds, and suggests that this may have provided a stimulus. In any event, he was an archaeology and anthropology undergraduate con centrator at Harvard, and very early he came under the influence of that remarkable teacher of Mexican and Central American archaeology and ethnology, Alfred Marston Tozzer. He had his first field experience in archaeology in the summer of 1915, at Pecos, New Mexico. This was a field excavation program under the auspices of the R. S. Peabody Foundation of Andover, Massachusetts, and it was under the direction of A. V. Kidder, later to become one of America's leading archaeologists. Next to Tozzer, Kidder was an important influence in Lothrop's archaeological education and general training. Following this summer's work, Lothrop traveled extensively in Central America and in Puerto Rico as an associate of the Peabody Museum of Harvard, visiting sites, making small excavations, and studying collections.

His archaeological career was interrupted by the World War I years 1917-1918, when he served as a Second Lieutenant in the U.S. Army Military Intelligence. But he returned to formal graduate work at Harvard in 1919. His first archaeological publication, an article on Chiriquian goldwork from Panama, appeared in that year; and from then on his course was set in Central and South American studies. His Ph.D. thesis, submitted in 1921, was on the ceramics of Costa Rica and Nicaragua. This represented more than two years of research on museum and private collections in Central America, the United States, and Europe. Among the important by-products of this study, as Lothrop once told this biographer, was making the acquaintance of the British Museum's very distinguished Americanist, Thomas Joyce, who with his broad knowledge of Central Americana aided and encouraged the young Harvard scholar in his task. Later, in 1926, the thesis was published in the classic two-volume work, *Pottery of Costa Rica and Nicaragua,* which, fifty years later, is still the basic reference on the subject.

After taking his doctorate, Lothrop was employed by the

Carnegie Institution's Historical Division to carry out field investigations in Yucatan and in Guatemala. As the result of this period of research, carried out in 1923, he published the first major monograph on the Yucatecan Maya ruin of Tulum in 1924. This was to remain a lifetime pattern. All of Lothrop's field researches resulted in some substantial addition to the printed record; he never allowed field investigations to run far ahead of getting some of the results down on paper and available to his colleagues.

Between the years 1924 and 1930, he was on the staff of the Museum of the American Indian, Heye Foundation, in New York City. This was to be one of the most productive periods of his career and, in many ways, one of the happiest, although it ended somewhat abruptly with the sudden dissolution of the Foundation's research staff and interests as a result of the stock market crash in late 1929. During this period Sam explored widely in Latin America and established himself as the outstanding overall Latin American authority in archaeology. Very much of an "internationalist" by nature, he became a good friend of the Argentine archaeologists of the time, particularly the late Fernando Marquez Miranda; and through these relationships, he was one of the very few North Americans who was ever invited to conduct excavations in Argentine territory. He explored a series of sites near the mouth of the Plate River, and his monograph on these, "Indians of the Parana Delta, Argentina," was eventually brought out by the New York Academy of Sciences in 1932. He also did ethnological fieldwork in the mid-1920s in Argentina and Chile, producing another distinguished work, *The Indians of Tierra del Fuego* (1928). Central America then claimed his attention, with explorations in Guatemala, in the vicinity of Lake Atitlán, and in El Salvador. All of this work was "consolidated" by prompt publication. After the drastic curtailment of the Heye's publication program, Lothrop turned to other institutions and sources in his determination to see his work made a part of the permanent scientific record.

At the termination of his appointment with the Heye Foundation, Lothrop, again, returned to Harvard's Peabody Museum, where he continued as a Research Associate and as the Curator of Andean Archaeology until his retirement. Actually, he continued on beyond that, in a very active emeritus status, until his death in 1965. A man of independent means, he was not dependent on the very small stipend that the museum could afford to pay him during those years. Indeed, his out-of-pocket monetary contributions to archaeology were much greater than his formal income from that subject; but, fully a professional in his dedication to archaeology and anthropology, Sam Lothrop always prized his curatorial status at the museum, to which he was very loyal.

His first important archaeological job of the 1930s was to take over as Field Director of the Peabody Museum's exciting archaeological dig at the Sitio Conte, in the Coclé Department of Central Panama. The Sitio Conte had been found, by amateurs, as the result of seasonal river flooding. Amazing gold specimens, along with pottery and handsomely carved objects of colored stone and bone, had been washed out along the banks of the Rio Coclé. Professor Tozzer and other Peabody Museum archaeologists had visited the site and arranged for its excavation with the Conte family, the owners of the property. The excavations revealed unusually rich tombs, fully consistent with the early sixteenth-century Spanish descriptions of the burials of warrior chiefs of the region. These petty but all-powerful dignitaries had been interred, along with retainers sacrificed at their deaths, with profusions of grave goods, including cast and hammered gold jewelry. The style of these metal objects, while related to that of the better-known Chiriqui goldwork of northern Panama and Costa Rica, was, if anything, even more hand some. Along with the pendant frogs, bats, and human figures were embossed breastplates and diadems or crowns, as well as polished stone items set or bound in gold. Among these last were occasional emeralds. Lothrop proved equal to the task of directing the careful explorations that laid bare the dispositions of grouped human skeletons and the numerous accompanying artifacts in these chiefly graves. His two volumes on the Coclé culture, *Coclé; An Archaeological Study of Central Panamá,* published in 1937 and 1942, are masterpieces of archaeological description and presentation. He was always extraordinarily careful with his illustrative material—both photographic and line and stipple drawings; and in the Coclé volumes he did himself, and American archaeology, proud with superlative work of this sort by topflight professional photographers and artists.

Early in 1941, before America's entry into World War II, Sam was in Peru, directing a unit of the Institute of Andean Research's program in Latin American archaeological studies. This was the first time this author came to know him well. He gave generously of his time in guiding some of us younger colleagues to archaeological sites up and down the Peruvian coast. His knowledge of the ceramics, textiles, and other arts of the area was enormous, so that he was an excellent consultant for those who were tyros to that particular field. Although he carried out no excavations in this 1941-1944 period, being largely occupied through much of it with U.S. governmental matters, he was able to travel widely and to make numerous surface collections as well as compile field notes on sites. Later, he published articles on the little-known Chira-Pariñas region of the far north coast (1948) and, in collaboration with Joy Mahler, papers on Zapallan and Chaviña grave finds (both in 1957).

In the 1950s Lothrop was again in the field as an excavator, working in southern Costa Rica and in the Canal Zone. The

Costa Rican explorations, made in the Diquis delta country of the Pacific drainage, were the subject of his last major field report, brought out in 1963. He was at work on the Canal Zone Venado Beach site collections, among other projects, at the time of his death in 1965.

This very brief rundown of Lothrop's field career fails to include the numerous articles of synthesis or of special topical interest that he also authored. Among the outstanding of these are his detailed analysis of the goldwork from the sacred Maya cenote at Chichen Itza (1952), "Metals from the Cenote of Sacrifice"; "A Re-appraisal of Isthmian Archaeology" (1959); and "Early Migrations to Central and South America" (1961).

Sam Lothrop was highly regarded by his colleagues and contemporaries. He received the A. V. Kidder Medal for Achievement in Archaeology in 1957, the Huxley Memorial Medal of the Royal Anthropological Institute in 1960, and the Wenner-Gren Medal for Archaeology in 1961. In 1951 he was elected to membership in the National Academy of Sciences of the United States of America, and in Great Britain he was honored by being made an Honorary Fellow of the Royal Anthropological Institute. He was also a longtime fellow or member of the American Anthropological Association, the Society for American Archaeology, the Société des Americanistes de Paris, and many other European or Latin American scientific bodies. It has been noted that he was very internationalist in outlook, and this is underlined by the fact that he was a moving spirit in the International Congress of Americanists and, certainly, in foreign circles its best-known U.S. member. A founder and longtime member of the Institute of Andean Research, he helped direct its policies, establish its foreign ties, and carry out its investigations for many years. The esteem of his colleagues was given special emphasis by the publication of the volume, *Essays in Pre-Columbian Art and Archaeology* (1961), a unique *Festschrift* presentation in that it contained an article by the dedicatee, a reminiscent survey of archaeology in the Latin American field, entitled "Archaeology, Then and Now."

As a man, Sam Lothrop had considerable charm. He was a *bon vivant* and a gentleman of breeding. There was very little "side" or stuffiness to him. He presented, instead, a rather shy diffidence. At the same time, he was readily approachable on matters archaeological, whether scientific or practical. For the most part, he tended to withdraw from open controversy about his work or that of others; yet he was an archaeologist of very definite opinions and in group meetings or face-to-face conversation was never hesitant to express disagreement.

His mode of work is of interest, and it reflected much in his life-style. This biographer has observed Lothrop in his study of a large collection, that of the Diquis region or of Venado Beach, and others confirm his procedures from his work on the Coclé materials. Pottery, goldwork, and artifacts of all kinds would be spread out over laboratory table space and the available room on the floor. Sam would then spend days looking at the objects, checking excavation notes, and directing the efforts of his photographer and artists with the utmost patience. Weeks, even months, would pass in this manner, with little or no descriptive observations being made by the archaeologist.

Finally, at the end of this laboratory session, carried out at the Peabody Museum at Harvard, would come a relatively brief period of writing and note-taking. With these notes, and with the voluminous photographs and pen-and-ink drawings, Sam would then retire to his library-office in New York City. Here, surrounded by all the pertinent literature, and deeply immersed in it, he would prepare the final report, a document that would be very carefully related to the extant body of scientific writings that could in any way bear upon the subject. His comparative work was done largely from the very rich illustrative record that he brought to his library with him and that would, eventually, end up in his monograph.

He had no peer in the Americanist literature. He was an avid bibliophile, and it is no exaggeration to say that he had the finest library on the archaeology of the Latin American areas of any scholar of his time. This, as mentioned, was for many years housed in his office-library, quarters he maintained separately from his New York apartment residence. To many of the younger members of the profession, Sam's library, with its adjacent well-stocked bar, became "archaeological headquarters" for the whole northeastern United States; and it was here that he hosted the annual meetings of the Institute of Andean Research for many years. A few years prior to his death, when Sam left New York City, he transferred the library to his spacious home in Belmont, Massachusetts. The entire library was left to the Peabody Museum of Harvard in his will.

Lothrop was married to Rachel Warren, of Boston, in 1914, and they had three children, Samuel K., Jr., Joan, and John Warren. His second wife was Eleanor Bachman, of Philadelphia, whom he married in 1929. His third wife was Joy Mahler, of New York City, also a professional archaeologist, who collaborated with him on various archaeological publications, and whom he married in 1958.

Lothrop was a sports fan, especially of boating and ice hockey. This author, raised in southern California and Arizona, had, in those days, seen little of the latter sport. He remembers being taken by an enthusiastic Sam to watch that swift and furious game at the Boston arena. Sam's own participation, in later years at least, was yachting. He was a member of the Union Boat Club of Boston, and, in his role of enthusiast, he provided Edward Wood, Jr., with many of the photographs that were used as illustrations in the latter's history of the Mattapoisett Yacht Club, entitled *Sailing Days at Mattapoisett, 1870-1890.* For many years Sam maintained a summer residence at Mattapoisett. His sailing interests overlapped, to a degree, with his archaeology in his preparation of what was to be an important article, "Aboriginal Navigation off the West Coast of South America" (1932).

Sam Lothrop's great contribution to American archaeology was heavily substantive. The variety and nature of this substance can be appraised in the appended extensive bibliography covering the years 1919 through a posthumously published article of 1966. He was a pioneer and an explorer. This refers not so much to the connotations of cutting one's way along jungle trails (although Sam did some of this) as to appraising, describing, and laying the groundwork for the archaeology of many South and Central American regions. At the same time Sam's work was in no way superficial. He believed in the objects and materials that were recovered archaeologically. He believed in the value of their most complete description and intrinsic analyses. This is evident in his great respect for technical and artistic craftsmanship and in his attempts to find out all that he could about these. The late Dudley Easby, Jr., in his obituary statement on Lothrop (*American Antiquity,* 31:256-61, 1965), stated:

"He wrote with brilliance and clarity on pottery, lapidary work, fine metalwork, navigation, and, together with Rivet and Nordenskiold, was one of the first to consult technical specialists instead of dreaming up technological phantasies." Easby, himself a leading authority on Pre-Columbian metallurgy, went on to praise Lothrop's pioneering efforts in this field and to credit him with encouragement and stimulation to others. In this regard, it should be noted that Lothrop was the one who brought the metallurgist, W. C. Root, into Pre-Columbian studies. Root later wrote the definitive articles on the subject of his time, and he later collaborated with Lothrop on the study of the Chichén Itzá cenote metals referred to above.

Sam's esthetic appreciation and judgment was as finely developed as his sense of craft technology. He had an all-encompassing visual memory for specimens and for the details of these and a rare good taste that transcended the barriers among cultures. This visual acuity was interesting, in one sense, in that, owing to a childhood accident, he had only 25 percent vision in one eye; yet, despite this handicap, he was one of the great connoisseurs of Pre-Columbian art. He demonstrated this in numerous articles and in two great "art books," one on the Robert Woods Bliss Collection of Dumbarton Oaks (1957) and another brought out by the Skira publishers of Geneva (1964).

But the understructure of Sam Lothrop's substantive contribution was in sheer systematic exploration and recording in little-known regions of Latin America, especially in what has come to be referred to as "Lower Central America," or that part of Central America south and east of the Maya frontier and down to and including the Isthmus of Panama. His research and publication on this part of the New World still stands as greater than that of any other scholar for that region. In recent years younger workers have entered this field, and we are coming to know much more about the archaeolgy of these Central American republics than formerly; but Sam laid much of the groundwork, and he was instrumental in encouraging Doris Stone and others who have followed him in Isthmian studies.

On the theoretical side, Lothrop's outstanding contribution was in the linking of archaeology and ethnohistory, again especially with the data of lower Central America. In this he was an exacting documentary scholar, and his studies of this kind have not yet been duplicated for the Nicaraguan, Costa Rican, and Panamanian regions. Sam probably would not have looked upon this as something that was in the "theoretical" realm. To him it was straightforward history or history-and-archaeology. Younger workers may question some of his assumptions about archaeological-to-ethnographical continuities in his attempts to explain some prehistoric phenomena; but Lothrop had the very great advantage of knowing his particular areas of work in their depths of both archaeological and ethnohistorical detail so that the major guidelines of his reconstructions, such as those concerning the Panamanian chiefdoms, are probably very sound even though they appear, to a degree, to be intuitive. He showed little interest in *in situ* processes of cultural development; and, as an investigation of these is now enjoying current favor, some of his writings may seem "old-fashioned." He was, perhaps, something of a diffusionist. At least many of his shorter papers dealt with themes of probable relationships in styles and technologies as these were found across great distances of South and Central America; but he "rode no particular horse" in insisting on special diffusionistic interpretations of American cultural history. He was particularistic and immersed in the data, and he knew these data very well. When H. J. Spinden put forth his imaginative idea of the "Archaic hypothesis," Lothrop, along with G. C. Vaillant, pointed to the exceptions and irregularities in the data that the hypothesis could not smooth over or reconcile. Still, he, too, could take the broad view of the American field.

In retrospect, one sees Samuel Kirkland Lothrop as a very "catholic" archaeologist of his time. Fittingly, for his generation, he bridged the earlier great scholars, such as Eduard Seler and W. H. Holmes, and the somewhat more anthropologically, or "social-science"-minded, group that was to follow. He was less "developmentally oriented," or "chronology-minded," than his contemporaries, A. V. Kidder and G. C. Vaillant; but he was more adept as an ethnohistoric scholar and a technological-esthetic appraiser than they. But, as they were, he was also both an anthropologist and a humanist. And, as is evident in all of his work, he really enjoyed archaeology.

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Compiled by Mary L. Mallory

*Key to Abbreviations*

Am. Anthropol. = American Anthropologist

Am. Antiq. = American Antiquity

Art Archaeol. = Art and Archaeology

Bull. Bur. Am. Ethnol. = Bulletin of the Bureau of American Ethnology

Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington Publication

Contrib. Mus. Am. Indian, Heye Found. = Contributions from the Museum of the American Indian, Heye Foundation

Indian Notes Monogr. Mus. Am. Indian, Heye Found. = Indian Notes and Monographs, Museum of the American Indian, Heye Foundation

J. R. Anthropol. Inst. G. B. Irel. = Journal of the Royal Anthropological Institute of Great Britain and Ireland

Mem. Peabody Mus. Archaeol. Ethnol. Harvard Univ. = Memoirs of the Peabody Museum of Archaeology and Ethnology, Harvard University

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* The Peabody Museum houses a significant number of Samuel Kirkland Lothrop's unpublished notes, photographs, and site plans related to his work in British Honduras, highland Guatemala, Panama, Puerto Rico, and other areas of Central and South America. In addition, other miscellany, such as his personal correspondence, is kept in the Archives of the Museum.

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Peyton Rous

# Francis Peyton Rous

October 5, 1879-February 16, 1970

by Renato Dulbecco

Peyton Rous was awarded the Nobel Prize in 1966, when he was eighty-six years old, for discoveries he had made fifty years before. He was born on the 5th of October 1879 in Baltimore, Maryland, to a family that valued humanistic education. Thus, after the death of his father, when Peyton Rous was a child, his mother rejected the idea of joining her family in Texas and stayed in Baltimore, where excellent education for the children was available. Of the two sisters of Peyton Rous, one became a musicologist, the other a painter; and Peyton himself had a flair for writing.

Peyton Rous enrolled in the Medical School of the recently created Johns Hopkins University, which he attended without special distinction. As an undergraduate he showed a naturalist's tendency and published articles about Baltimore's flowers. After graduating in medicine, he went to the University of Michigan, where he began his research career in pathology, which he perfected during a year in Dresden. In 1909 he joined the Rockefeller Institute under Simon Flexner, to engage in cancer research, against the opinion of influential friends who thought it was a hopeless field. There he remained until his death. In 1921 he became co-editor (and later editor) of the *Journal for Experimental Medicine.* In 1927 he was elected to the National Academy of Sciences. He died in 1970, at the age of ninety, and is survived by his wife, Marion, and three daughters, Marion, Ellen, and Phoebe.

In addition to the Nobel Prize, Peyton Rous received many honors and honorary degrees, which are listed at the end of this memoir.

The name of Peyton Rous became widely known to biologists in the fifties and sixties for his earlier discovery of a virus causing sarcoma in chickens, which became aptly known as the Rous Sarcoma Virus. At the time he became famous, Peyton Rous appeared as an elderly, highly educated, gentleman with silvery hair. But in his youth he was a very hardworking scientist with a determined, fiery, and highly critical personality. He was a medical man who wished to learn about cancer as a disease and a biologist who did not want to follow the beaten track, and he was willing to hunt for new clues in well-designed but slow experiments.

I, like most of my contemporaries, became acquainted with Peyton Rous's fundamental discovery in the early fifties, when Harry Rubin came to my lab to work with the Rous Sarcoma Virus. He started using a focus technique on the chorioallantoic membrane of the chicken embryo, which Rous had invented many years before. Later I had occasion to meet Peyton Rous several times on the platform as a speaker, or across the discussion table, or in his laboratory at the (then) Rockefeller Institute. I remember the man-rather small in stature with silvery hair and penetrating eyes. I also remember that before our first meeting I was inclined to think of him as a figure of the past, but soon changed my mind at that meeting and even more so at subsequent ones. Clearly, he was very much alive until his very last days, with a keen interest in new developments in virology and cancer research. He was able to discuss his past work with equanimity and to accept new interpretations of his data. I remember I suggested to him an explanation of the clonal characteristic of the neoplastic transformation of papillomas in terms of somatic cell genetics, a concept that was not part of cancer research in the period of his active work. His interest was immediately aroused; he asked me for a thorough clarification of what I meant and then argued, with passion but no animosity. We parted like old friends who have found something new to talk about. At the time when phage lysogeny was the domain of a very small group of virologists, I suggested to him that it might represent a good model for some features of viral cancer. Again his interest was acute, and I had to embark on a detailed discussion of phage integration, immunity, and lysogenic conversion.

Peyton Rous discovered the viral etiology of a chicken sarcoma in 1911 through his interest in tumor transplantability to new hosts by a filtrate. He commented: "The behaviour of the new growth has been throughout that of a true neoplasm, for which reason the fact of its transmission by means of a cell-free filtrate assumes exceptional importance" (1911).

He fully realized from the outset that this was "a unique and significant finding" (1911). He also realized that the significance of the discovery depended on the true nature of the induced growth. As an experienced pathologist he could see that it was a true cancer: "The (pathological) picture (of the growth) does not in the least suggest a granuloma . . . it exhibits to a special degree, not merely a few, but all those features by which the malignant neoplasms are characterized" (1911).

For about forty years this momentous discovery had little impact, because the minds of scientists were not prepared to think of viruses as agents of cancer. It was expedient to say that the chicken tumor was not a cancer, but some kind of reaction to the virus more akin to inflammation than neoplasia, and perhaps a peculiarity of chicken biology. Peyton Rous soon recognized himself that the tumor would not be accepted as a cancer *because* it was transmitted by a cell-free extract: "A passing reference should perhaps be made to the ill-defined group of pathological products called granulomata, with which this neoplasm of the fowl may by some be classed, owing to its transmission by an agent separable from the tissue cells" (1911). Many years later he wrote, "This disclosure (that certain chicken tumors were proved due to viruses), which conflicted with the negative findings in mammalian growths, was determined forthwith as erroneous" (1952).

One wonders how firmly in the early years Peyton Rous himself was convinced that he had demonstrated the induction of a cancer by viruses. The statements he made at the time are very cautious and full of qualifications. At first he used to refer to the "agent" that induced the sarcoma; but a year later, after he discovered a new, different tumor transmissible by filtrate, he proposed that the "agent is probably a living virus" (1912).

During the years 1911-1914, Peyton Rous worked hard at disproving the objections on the nature of the induced tumors by isolating other viruses that induced tumors in chickens and by carefully studying their pathology. He could show that the tumors induced by the different viruses were capable of invading neighboring tissues and of metastasizing to distant organs; thus they were true cancers. Moreover, each independently isolated virus caused a tumor of a different kind. These facts should have been convincing evidence that the growths were specific responses of the host, yet this conclusion was not generally accepted. However, these discoveries seem to have been convincing for Rous, who wrote, "The findings with the chicken tumors largely demolish the theoretical basis in which objections to an extrinsic cause for cancer have been built up" (1912).

In order to find more generally acceptable evidence, Peyton Rous attempted to extend his observations "especially through carefully devised experiments with the tumors of other species of animals" (1911). Evidently for the viral etiology to be accepted, similar findings were needed in mammals. The strategy of Rous's future work was determined at that time. However, the extension to other species came only many years later with Richard Shope's discovery of the rabbit papilloma virus.

In the meantime Peyton Rous studied many features of the cell-free transmission of the tumor. Examining the effect of the age of the host, he showed that the virus induces characteristic foci on the chorioallantoic membrane of the chicken embryo. This result supplied an assay for the virus that was universally employed until the fifties, when it was superseded by the focus formation in tissue culture.

In this extensive and careful work, Peyton Rous observed the host resistance to the transmission of the tumor, in the form of either absence of growth, slow growth, or normal growth followed by regression. Other experiments showed how essential the conditions of the host are for the development of a tumor after inoculation of the virus. From this observation Peyton Rous began to recognize the existence of limitations to the expression of the oncogenic potential of the virus: "How does it happen that the sarcoma, though ultimately dependent on an extrinsic agent, is dominated in its behaviour by the cells composing it?" (1912). Perhaps the agent depends "on a special set of conditions in order that it might produce a neoplastic change" (1912). He returned later to this point on several occasions.

After discovering the second chicken tumor agent, Peyton Rous started wondering about the etiology of cancer in general: "The demonstration that extrinsic agents are the cause of two connective-tissue growths of the fowl which are characteristic malignant tumors renders it necessary to suppose either that such tumors of the fowl have an entirely different etiology from mammalian tumors, or else that the latter are of similar origin" (1912). This point was also developed to a much greater extent later on.

As further evidence for a viral nature of the chicken tumors, the resistance of the host to the tumor cells could be separated from its resistance to the tumor-inducing agent. Moreover, Rous discovered a third chicken tumor, transmissible by filtrate, markedly different in properties from the two previously described: "The findings with the three tumor-producing agents have a striking similarity and it is difficult to avoid the conclusion that the three are of one class, whatever that class may be ... It is perhaps not too much to say that their recognition points to the existence of a new group of entities which cause in chickens neoplasms of diverse characters" (1914).

At the beginning of World War I, Peyton Rous, under the pressure of wartime medical needs, gave up his work with chicken tumor viruses. For the following twenty years until 1934, his interest was in the fields of blood transfusion and attending immune reactions, liver and biliary functions, cellular functions, and vascular permeability. I will return to these activities later on.

A turning point in Peyton Rous's work on cancer was the discovery of the Shope papilloma. In 1933 Richard Shope reported his discovery that a mammalian tumor, the papilloma of cottontail rabbits, was transmitted by a virus-like agent. As in the case of the chicken tumor, Peyton Rous's first concern was whether the papilloma was a true neoplasm. He decided that it was, because, when the papillomas were transplanted deep inside the body, they developed into carcinomas that grew invasively and killed the host. Furthermore, in domestic rabbits the virus-induced papillomas often grew progressively, invading the neighboring tissues and producing metastases, and this malignant evolution could be enhanced by exposing the papillomas to various substances, such as Scarlet Red.

These findings seem to have been for Rous the decisive argument for the validity of his conclusions concerning the chicken tumors, since in a mammal cancer could also be transmitted by a virus. He, therefore, returned to the study of carcinogenesis using the papilloma virus as a new tool. He focused at first on the malignant evolution of the papillomas. By careful observations, following small hints, such as the shape of their growths, color, or the degree of pigmentation, he showed that a few cells in a papilloma became cancerous and generated clones, each with different characteristics.

In trying to understand how such evolution to cancer occurs, Peyton Rous studied the effects of tar, as both a carcinogen and tumor promoter. He found that tar not only strongly enhanced the induction of papillomas or carcinomas by the Shope virus in domestic rabbits but by itself elicited similar papillomas. Could tar papillomas also be virus-induced?

This new phase of Peyton Rous's work, although a natural development of his earlier work, had more ambitious goals, for it aimed at testing the hypothesis that "this disease (cancer) is an infection. . . . A main attraction of this hypothesis is its accessibility to test." However, he clearly saw that this hypothesis could only be true under certain conditions, one of which is that "a living entity responsible for such growths must require for effectiveness a very special basis of predisposition" (1932). He sought to possibly disprove the infectious nature of cancer by comparing the frequency of cancer induction by tar in the skin of two groups of mice with different exposure to the environment: "The animals of one group have been placed under conditions which would facilitate the entrance into the body of extraneous living agents, whereas those of the others have been sedulously protected" (1932). The results proved "that the mouse cancer cannot be caused by living entities reaching the body from the surrounding world during adult life" but "fail to exclude the possible activity of entities residing habitually in or upon the body" (1932). This experiment showed another requirement of the hypothesis on the infectious nature of cancer: "The supposition (that tumors in general are due to viruses or other extraneous entities) is tenable only if such entities are widely distributed throughout the animal population, being constantly present in or upon the body, like the colon bacillus or the staphylococcus; and if their opportunity to cause tumors is restricted by the need for very special conditions . . . . The more considerable an agent is conditioned in its activity, the more often must it be present if it is to cause disease at all" (1934). These words were prophetic, as shown by the recent developments in the field; yet they were simply the result of cool, logical assessment of the facts then in hand. However, for Peyton Rous this hypothesis was only a guide for the experiment: "The demonstration of the cause for the generality of tumors, whatever this is, waits upon the provision by the investigator of the conditions necessary to its effectiveness" (1934).

He tried several new approaches. One of the major tools was still the technique of inducing skin tumors by application of tar. He used it to create favorable cellular conditions for revealing the neoplastic potential of viral agents. Another tool was the immunity of the infected rabbits against the Shope virus. Peyton Rous found no demonstrable antibodies in rabbits without papillomas or in those with tar papillomas or Brown-Pierce tumors: these findings "speak decisively against the possibility that these growths are caused by viruses antigenically related to the one causing papillomas. Yet this does not exclude a virus causation for them, since the sera of fowls with Chicken Tumor I and Fujinami Sarcoma respectively, though possessed of neutralizing power for the virus causing the growth carried by the host, have no cross-neutralizing effect whatsoever" (1936).

Shortly afterwards, in taking a bird's eye view of his past work and of the cancer problem, he concluded: "How far should one be led by the assumption that certain tumors may be due to viruses? Only so far as to make tests with these growths. The tumor problem has withstood the most corrosive reasoning. Yet since what one thinks determines what one does in cancer research, as in all else, it is as well to think something. And it may prove worthwhile to think that one or more tumors of unknown causes are due to viruses" (1936). He thus recognized that the problem that he so clearly formulated and actively pursued eluded experimental attack and remained unsolved. In fact he later restated the basic question: "What is the papilloma doing in the cancer, if anything?" (1940).

In a renewed effort to answer this question, Peyton Rous used as a new tool the famous line of transplantable rabbit cancers, derived from a viral papilloma called at first "carcinoma V2" (1940), and then, after World War II, V × 2 because during the war V2 "came to have another significance" (1952). This line did not contain infectious papilloma virus, but for many serial transfers in rabbit it continued to elicit the production of virus-specific antibody.

The result suggested that the virus may play a determining role although in "masked or altered form" (1940). This was a new idea in virology, which had enormous developments many years later. For the next three years, during serial transplantation from one rabbit to another, the V × 2 carcinoma continued to elicit this immune response. However, when it was retested after an interval of one and a half years, four and a half years after its origin, the tumor was found unable to immunize against the papilloma virus; the loss of this property "was not attended by any perceptible change in the V × 2 carcinoma" (1952). This "wholly unexpected" result must have been quite shattering; and Peyton Rous was led to rethink the role of the virus in the production of the cancer. In this agonizing reappraisal he proposed that the virus might have undergone "wider variation" (1952); but he recognized that "at this uncertain point the problem of the cause for the V × 2 carcinoma must perforce be left" (1952). In this way the work of Peyton Rous went full circle: from complete ignorance on the role of viruses in cancer to definitely establishing such a role through brilliant discoveries, to postulating a wider and possibly general role of viruses in spontaneous cancers, and ending up again in a condition of uncertainty. I should not say full circle, but rather one turn of the helix, because the uncertainty was now of a different kind.

The emphasis of Peyton Rous's work in the forties and fifties shifted from the viruses to chemical carcinogens. Many articles were dedicated to the potentiating effect of tar and other carcinogens on virus-induced papillomas. During this work it also became clear that tar alone induces papillomas very similar to those induced by the virus on normal skin or on skin pretreated by tar. In all cases the growth showed progression, i.e., remained benign for some time and then developed into carcinomas, which arose in a few isolated cells. However, many observations also showed that the role of the virus and of the chemicals was different: "The generality of the carcinogens bring about tissue conditions out of which tumors may or may not arise for reasons still undetermined. They may be fitly called provocative carcinogens. The viruses, on the other hand, both initiate tumors and determine their character and behaviour. They are actuating carcinogens" (1943).

In a new series of experiments, Peyton Rous convincingly demonstrated that the viral and the chemical agents have a cooperative action, producing in combination cancers at much higher frequency and after shorter time than either agent alone. On the basis of this cooperation, Peyton Rous made three important suggestions. One bears on the mechanism of carcinogenesis. He proposed that *in utero* or at a young age the human or animal body becomes invaded by viruses that "would give no sign of their presence in most instances. . . . But if a provocative carcinogen happened to work on the cells with which such a virus was associated . . . it might undergo variation and . . . give rise to a tumor. The new pathogenic variant would not be transmitted to other animals . . . but would be a dead-end virus, though the harmless source virus liable to the same or other variation would be passed on" (1943). This hypothesis is very similar to some prevalent at the time of this writing, if the dead-end variant is interpreted as a defective integrated provirus that has incorporated a silent oncogene, causing its expression.

Another suggestion was that chemical carcinogens might cause the so-called spontaneous tumors. He added, "A list of human tumors which have been traced to the action of provocative carcinogens is in no small degree a sociological document, reflecting as it does the ways of life, vocation, avocations, habits and environmental stresses of people and individuals" (1943).

The third suggestion was that viruses and chemicals in combination might have a continued role in spontaneous cancer: "The recent discovery that viruses of some sorts lie latent for long periods, causing disease only on special occasion, coupled with the realization that some tumors have viruses as their cause, has led to a supposition already mentioned that agents of this sort may reside in animal tissues, perhaps throughout the lifetime of the organism, doing no harm unless the cells with which they are associated undergo special pathological changes, when they undergo variation as a result of the new, abnormal milieu and render the cells neoplastic. According to this supposition, tar and methylcholanthrene are carcinogens because they alter the environment of viruses . . ." (1944). Such a possibility is very much in the minds of virologists and oncologists today.

During his work on carcinogenic hydrocarbons, Peyton Rous identified important features of the neoplastic process they initiate. One is that their cancer-inducing activity is greatly enhanced by promoters that stimulate cell proliferation, for instance, wounds. Another feature is that "cancers arise by a step-like progression" (1941). Peyton Rous recognized the importance of this observation, because "the cells of not a few tumors attain to their worst by further neoplastic changes which are scarcely less significant than the one primarily responsible for their state. Indeed the practical significance of these changes is often greater as meaning death to the patient" (1955).

In his later years Peyton Rous continued his experimental work, but his contributions declined in number and relevance. During that time tremendous changes were occurring in biology, especially the great development of genetics and the birth of molecular biology. Although Peyton Rous showed great interest in these developments, he failed to assimilate them. Obviously, even a brilliant mind is subject to the limitations of age. He became attached to old concepts. The main consequence was his rejection of somatic mutations as a possible cause of cancer and his failure to recognize viruses as new genetic material in the cells they infect. It may be said about him what he said about Leo Loeb: "He outlived his era of discoveries about cancer but what he did for science endures" (1960).

The work on cancer is the big basis on which Peyton Rous's fame rests. The other work, which I already mentioned, is permeated by a similar perceptiveness, imagination, and experimental ability. I should mention especially the work on blood preservation and substitutes, which Peyton Rous carried out during World War I, because it shows another facet of his personality, i.e., the ability to respond to urgent medical needs of society. For instance, in 1918 he wrote: "There exists at present a great and urgent need for an injection fluid that can be satisfactorily employed instead of blood for transfusion in cases of hemorrhage. It is common knowledge that casualty clearing stations, after a 'push', are crowded with men who have lost too much blood to be operated on, who cannot be revived by means of salt solutions and supportive measures, but who would undoubtedly respond to transfusion. For the latter neither time nor donors are available." Quite rapidly, at the beginning of the war, Peyton Rous and associates perfected a method for storing human red blood cells, using a weak gelatin solution to protect them during washing and sugars to preserve them. The procedure was used to establish the first blood bank during the war, which was operated by one of Rous's collaborators. And the solution for suspending the red cells, known as the Rous-Turner solution, is still in use. Such an accomplishment would be a sufficient reason for fame, because it has both scientific and humane values. We know that Peyton Rous was very proud of it.

In addition to his experimental work, Peyton Rous had another absorbing interest: the editorship of the *Journal of Experimental Medicine.* It is known that he dedicated to it an immense amount of time and energy. He was reputed for the accuracy of his editing, both in regard to scientific content and style. I well remember when I, as a prospective author, first encountered him as an editor. He returned my manuscript with many remarks, mostly of style. I remember I was at first baffled, but then, after studying his comments further, came to appreciate their reasons, which went beyond the mere words. I realized that for him a word, every word, was a concept, which should be examined not only in its present, but also future, context. He reminded me in his letter that I should think that a certain word might become widely adopted and that I should therefore choose it with deliberate care. He was anticipating the flooding of scientific literature with laboratory slang, which has happened in recent times and which was a trend of which he strongly disapproved. But during his editorship, he succeeded in maintaining the *Journal of Experimental Medicine* at a high level, both in purity of language and strength of content.

Peyton Rous remains in the minds of those who knew him, especially the younger generation, as the image of a man fully dedicated to his work, a scientist with vision, a strong although kind person, with a good sense of humor. His experiments were always designed to test hypotheses and developed in a logical sequence on the basis of results already secured; they were very methodical and thorough and were reported in detail with ex treme clarity. He was independent in thought and was against conventional beliefs unsubstantiated by evidence. For instance, he commented:

"Not so long ago in the dark ages of medicine, one could think nearly anything about disease because one knew almost nothing. Theoretical system succeeded system, from humours to homeopathy. Opinions strongly held appeared like realities and were acted upon as such. Now for most diseases all this is at an end: fact has killed fancy. . . . The tumor problem is the last stronghold of metaphysics in medicine" (1936).

Although during his career he formulated some penetrating hypotheses not amenable to direct test, he was fundamentally interested in facts: "No explanation of the cause of cancer is worthy of attention that cannot be tested" (1932).

The language of his reports was vivid, full of images from everyday life, as shown by a few examples:

"The fowl limps and its wings seem stiff" (1913).

"The growth gives it (the fowl) a factitious plumpness" (1913).

"During the outward extension of the membrane (of the Kupffer cells), lava-like flows can be seen on its surface, when the light is cut down, and at its edges fimbriated or 'petaloid' extrusions, at times appearing whip-like, which are in constant slow motion" (1934).

Comparing his work with the chicken viruses, which started with a cancer and led to a virus, and with the Shope virus, which started with a virus and led to a cancer: "The trails have met at the same look-out. What does one see from this?" (1936).

His humor was sometimes biting: Speaking of new reactions elicited in the human body by surgery, he saw their positive aspects: "All that surgery has done in such instances is to make plain the relation of effect to cause, as for example in showing that tetanus is due to insufficiency of the parathyroids, and myxedema to a thyroid lack" (1929).

The most visible side of Peyton Rous was his interest in scientific truth, in the younger people, the equanimity of judgment and the warmth of human relations he was able to establish both inside his family and outside. I had a hint of this when recently his daughter, Marion, referred to him as "daddy" in an affectionate way; and when several years ago he wrote to me sending his congratulations for the Ehrlich Darmstaedter award (which he nominated me for) and suggesting the nicest Ratskeller in Frankfurt, with the best food, wine, and atmosphere.

Peyton Rous received many official honors in his life, including the highest. But the paramount recognition was the admiration and respect of his younger colleagues, which continues after his death.

## HONORS AND DISTINCTIONS

Honorary degrees from the universities of Cambridge, Michigan, Yale, Birmingham, McGill, Chicago, and Zurich

Member of the National Academy of Sciences

Member of the American Philosophical Society

Foreign Member of the Royal Society

Member of the Royal Danish Academy of Sciences

National Medal of Science

Cleveland Medal of the American Cancer Society

Gold-headed cane of the Association of Pathologists and Bacteriologists

United Nations Prize

Gold Medal of the Royal Society of Medicine

Albert Lasker Award

Landsteiner Award of the American Society of Blood Banks

Distinguished Service Award of the American Cancer Society

Kovalenko Award of the National Academy of Sciences

Ehrlich Darmstaedter Prize

Kober Medal of the Association of American Physicians

Benter Medal and Award of the University of Texas

Walker Prize of the Royal College of Surgeons

John Scott Medal and Award of the City of Philadelphia

Nobel Prize for Medicine (shared with Charles Huggins)

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*Key to Abbreviations*

Am. J. Med. Sci. = American Journal of Medical Sciences
Cancer Res. = Cancer Research
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Exp. Med. = Journal of Experimental Medicine
J. Mt. Sinai Hosp. = Journal of Mount Sinai Hospital
Nature, Lond. = Nature, London
Obit. Not. Fell. R. Soc. Lond. = Obituary Notices of Fellows of the Royal Society of London
Perspect. Biol. Med. = Perspectives in Biology and Medicine
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine

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Donald D. van Slyke

# Donald Dexter Van Slyke

March 29, 1883-May 4, 1971

by A. Baird Hastings

Donald Dexter Van Slyke died on May 4, 1971, after a long and productive career that spanned three generations of biochemists and physicians. He left behind not only a bibliography of 317 journal publications and 5 books, but also more than 100 persons who had worked with him and distinguished themselves in biochemistry and academic medicine. To all who knew him, he was affectionately known as Van, and as Van I shall refer to him in this synoptic account of his life.

Van was born in Pike, New York, a small rural community, and he received his early education in the elementary schools and high school of Geneva, New York. His father was the distinguished chemist Lucius L. Van Slyke, who received his Ph.D. at the University of Michigan in 1882 and was on its staff at the time Van was born, on March 29, 1883. His mother, Lucy Dexter Van Slyke, died two years later. In 1890, L. L. Van Slyke became Chief Agricultural Chemist of the New York Agricultural Experiment Station, a post that he held until his retirement, in 1929. Van and his father and his father's profession were closely intertwined as Van was growing up, which doubtless made chemistry a natural choice of study for him, though for a time he leaned toward architecture. Van had no chemistry courses in high school, but he used to credit his English teacher, Miss Florence Parker, with the lucidity that later characterized all his scientific publications.

Van spent his first college year at Hobart College in Geneva, where he took his first course in chemistry. Since the college had but one chemistry course, he transferred to the University of Michigan, from which he received a B.A. degree in 1905 and a Ph.D. in chemistry in 1907. He once stated: "The chief reason I went there was that Moses Gomberg was there. If there was any outstanding American organic chemist, it was he." In addition to courses in organic, physical, and analytical chemistry, Van also took bacteriology and plant physiology as minor subjects. His doctoral thesis, published with Gomberg in the *Journal of the American Chemical Society* in 1907, was entitled: "The Action of Molecular Silver, of Silver Sulfate and Chloride, and of Sulfuric Acid upon Halogenated Derivatives of Triphenyl-Carbinol Chloride." This occurred shortly after Gomberg's exciting discovery in 1900 of the free radical triphenylmethyl. Van has delightfully reminisced about his days when he was working in Gomberg's laboratory. One day he needed a two-liter bottle that had once contained metallic sodium under anhydrous ether. Thinking the pieces of sodium in the bottom had long since reacted, he dumped them in the sink with running water. "Flashes went off like cannon firecrackers, and when it stopped, Gomberg looked in through his door and said: 'Now, Van Slyke, you know what metallic sodium and water makes'. . . . Those were days when your professor was not at a distance."

After receiving his Ph.D. and marrying Rena Mosher in the same year, Van became an assistant to Phoebus A. Levene at the newly established Rockefeller Institute for Medical Research in New York City. Since this came about somewhat by accident, the circumstances are worth recording. Van had expected to follow in his father's footsteps and become an agricultural chemist. To this end he had taken and passed a civil service examination for a position in the Bureau of Chemistry. He was scheduled to report right after getting his doctor ate. But fate took a hand, and at the spring meeting in 1907 of the American Chemical Society, Van's father chanced to sit next to Levene, who was recruiting for his department at the Institute. Luckily for Van and for the Institute, L. L. Van Slyke mentioned the approaching graduation of his son Donald. The upshot was that Van received an invitation from Dr. Simon Flexner, Director of the Rockefeller Institute, to come to New York for an interview. (In those days—and for many years after—Simon Flexner personally interviewed all staff members, no matter how low their rank, before offering them an appointment.) After consultation with his father, Van accepted the offer and thereby began his Rockefeller Institute career as biochemist and clinical chemist that was to last forty-one years—from 1907 to 1948.

## THE ROCKEFELLER INSTITUTE, LEVENE PERIOD

The first seven years were spent with Levene, which Van has described as a "wonderful time" working on proteins and amino acids. In 1911, Levene arranged for Van to spend a year in Berlin with Emil Fischer, who was then the leading chemist of the scientific world. He even had the privilege of working with Fischer in his private laboratory. Van was particularly impressed by Fischer's performing all laboratory operations quantitatively—a procedure Van followed throughout his life.

Prior to going to Berlin, Van had published eight papers with Levene and two by himself—one of which concerned his classic nitrous acid method for the quantitative determination of primary aliphatic amino groups. This method, which was in widespread use by chemists and biochemists for many years, depended upon the measurement of the gaseous nitrogen ($N_2$) evolved by the reaction between alpha amino groups and nitrous acid. It was the first of the many gasometric procedures devised by Van, and made possible the determination of amino acids in small amounts of blood and other biological materials.

Until the development of microbiological and chromatographic procedures, it was the primary method used to study amino acid composition of proteins.

Following his return from Berlin, Van continued his study of amino acid composition of proteins with Levene, and began his studies of protein digestion and metabolism. With his colleague G. M. Meyer, he first demonstrated that amino acids, liberated during digestion in the intestine, are absorbed into the bloodstream, that they are removed by the tissues, and that the liver alone possesses the ability to convert the amino acid nitrogen into urea.

This work led to a study with his assistant, G. E. Cullen, of the enzyme, urease, which decomposes urea to ammonia and carbon dioxide. The quantitative determination of both end products was subsequently the basis of gasometric procedures for measuring urea concentration in blood and urine.

From the study of the kinetics of urease action, Van Slyke and Cullen developed equations that depended upon two reactions: (1) the combination of enzyme and substrate in stoichiometric proportions and (2) the reaction of the combination into the end products. Published in 1914, this formulation, involving two velocity constants, was similar to that arrived at contemporaneously by Michaelis and Menten in Germany in 1913.

Thus were Van Slyke's activities during his first seven postdoctoral years. They centered around the development of better methodology for protein composition and amino acid metabolism. Van was remarkably productive and happy in his work with Levene. As he has stated, work on proteins and amino acids was "his first and enduring love."

## HOSPITAL OF THE ROCKEFELLER INSTITUTE PERIOD

Then in 1914 came an opportunity to become the chief chemist of the newly opened Hospital of the Rockefeller Institute, at the invitation of Dr. Rufus Cole, Director of the

Hospital. Van did not decide to make this change lightly. Years later, he recalled, "I was so distrustful of my ability to develop a department of chemistry in the hospital and so reluctant at leaving Levene, that I made Flexner write me a letter saying that if I didn't like it in the hospital I could go back to Levene." But, he continued, "I began to pick up medicine pretty fast and found it fascinating. So I stayed in the Hospital the rest of the time I was at the Rockefeller." That amounted to thirty-four years.

Van once told me that he studied textbooks of physiology and medicine diligently in preparation for his new responsibility. He was fortunate in being able to take Dr. Glenn E. Cullen, his assistant in Levene's laboratory, with him. Cullen, a chemical engineering graduate from the University of Michigan, was mechanically minded, resourceful, and had an outgoing personality. He and Van were a harmonious and effective team that developed the chemical laboratory of the hospital into a facility notable for its contributions to the budding science of biochemistry and to the yet-to-be-born science of clinical chemistry.

Van Slyke also had the good fortune at this time to obtain the services of John Plazin, a young emigré from Latvia, as his personal laboratory assistant. John's ambition was to be the best assistant conceivable for Donald Dexter Van Slyke. This he achieved and maintained until he died forty-seven years later. They worked as one through all those years and their loyalty to and admiration of each other is a tribute to the faithful character of each man. To John, Van was always "Dr. Van Slyke."

Though Van at age thirty entered upon his new responsibilities at the Hospital with some trepidation, he found the clinical staff so helpful and friendly that he experienced little difficulty in making the transition from Levene's laboratory to the clinically oriented environment. After all, under the directorship of Dr. Rufus Cole, the entire Hospital staff was embarking upon a new undertaking in medical research—the intensive study of disease as a scholarly pursuit—in patients, in animals, and in the laboratory. "Men who were studying disease clinically had the right to go as deeply into its fundamental nature as their training allowed, and in the Rockefeller Institute's Hospital every man who was caring for patients should also be engaged in more fundamental study," wrote Dr. Cole in 1911. Though commonplace today, this was a revolutionary idea at the time.

Van Slyke and Cullen lost no time in applying their sound organic and physical chemical knowledge and technology to the clinical problems under study at the Hospital. The study of diabetes was already under way by Dr. F. M. Allen, the advocate of the "starvation treatment" of diabetics. Though this worked temporarily in some cases, eventual death from acidosis continued to occur. Since acidosis manifested itself in several different chemical ways, and no easy, reliable method for its early detection existed, Van Slyke turned his attention to this problem. Characteristically, he went to the heart of the matter directly. He reasoned that if incomplete oxidation of fatty acids in the body led to the accumulation of acetoacetic and $\beta$-hydroxybutyric acids in the blood, then a reaction would result between these acids and the bicarbonate ions that would lead to a lowerthan-normal bicarbonate concentration in blood plasma. The problem thus became one of devising an analytical method that would permit the quantitative determination of bicarbonate concentration in small amounts of blood plasma. Again Van turned to a gasometric procedure. He ingeniously devised a volumetric glass apparatus that was easy to use and required less than ten minutes for the determination of the total carbon dioxide in one cubic centimeter of plasma and other aqueous solutions. His original method had an accuracy of about 1 percent.

After the demonstration of the value of using this procedure in the diagnosis and therapy of patients with diabetes and some other disease states, the method was widely adopted in hospital and research laboratories. It also was soon found to be an excellent apparatus by which to determine blood oxygen concentrations, thus leading to measurements of the percentage saturation of blood hemoglobin with oxygen. This found extensive application in the study of respiratory diseases, such as pneumonia and tuberculosis. It also led to the quantitative study of cyanosis and a monograph on the subject by C. Lundsgaard and Van Slyke.

In all, Van Slyke and his colleagues published twenty-one papers under the general title "Studies of Acidosis," beginning in 1917 and ending in 1934. They included not only chemical manifestations of acidosis, but Van Slyke, in No. 17 of the series (1921), elaborated and expanded the subject to describe in chemical terms the normal and abnormal variations in the acid-base balance of the blood. This was a landmark in understanding acid-base balance pathology and has not been materially improved for fifty years.

Van Slyke and his colleagues, both clinical and chemical, did not confine their interests solely to diabetes and acid-base abnormalities. Van kept work going on proteins and their products of hydrolysis and on better methods for blood chlorides, urea, and ketone bodies in blood and urine. Within seven years after Van moved to the Hospital, he had published a total of fifty-three papers, thirty-three of them coauthored with clinical colleagues. Quantitative clinical chemistry was well on its way at the Hospital, and Van Slyke's contribution to it was well established.

In 1920, Van Slyke and his colleagues undertook a comprehensive investigation of gas and electrolyte equilibria in blood. This was not only a logical outgrowth of the ongoing study of the acid-base balance of the blood, but was also encouraged by Franklin C. McLean at the behest of Prof. L. J. Henderson. McLean and Henderson at Harvard had made preliminary studies of blood as a physico-chemical system, but realized that

Van Slyke and his colleagues at the Rockefeller Hospital had superior techniques and the facilities necessary for such an undertaking. A collaboration thereupon began between the two laboratories, which resulted in rapid progress toward an exact physico-chemical description of the role of hemoglobin in the transport of oxygen and carbon dioxide, of the distribution of diffusible ions and water between erythrocytes and plasma, and of factors such as degree of oxygenation of hemoglobin and hydrogen ion concentration that modified these distributions. Publications from the two laboratories were independent and complementary. It was a happy intellectual collaboration.

A key development in the progress made was Van Slyke's revision of his volumetric gas analysis apparatus into a manometric apparatus. Briefly, this amounted to liberating and isolating the desired gas contained in a known volume of solution, and recording in millimeters of mercury the pressure of that gas at a known fixed volume. The manometric apparatus proved to give results that were from five to ten times more accurate than the volumetric apparatus, and, in addition, made possible the determination of very small concentrations of gas in solution. A series of papers on the $CO_2$ titration curves of oxy- and deoxyhemoglobin, of oxygenated and reduced whole blood, and of blood subjected to different degrees of oxygenation and on the distribution of diffusible ions in blood resulted.

One of these papers was especially notable. In it were developed equations that predicted the change in distribution of water and diffusible ions between blood plasma and blood cells when there was a change in pH of the oxygenated blood. (This work was done in 1923 at the Peking Union Medical College with F. C. McLean and Hsien Wu.) In a later paper, this was extended to reduced blood as well. A significant contribution of Van Slyke and his colleagues was the application of the Gibbs-Donnan Law to the blood—regarded as a two-phase system, in which one phase (the erythrocytes) contained a high concentration of nondiffusible negative ions, i.e., those associated with hemoglobin, and cations, which were not freely exchangeable between cells and plasma. By changing the pH through varying the $CO_2$ tension, the concentration of negative hemoglobin charges changed in a predictable amount. This, in turn, changed the distribution of diffusible anions such as $Cl^-$ and $HCO_3^-$ in order to restore the Gibbs-Donnan equilibrium. Redistribution of water occurred to restore osmotic equilibrium. The experimental results confirmed the predictions of the equations. A total of fifteen papers, under the general title "Studies of Gas and Electrolyte Equilibria in Blood," were published between 1922 and 1928. Van regarded this work as among the best of his scientific output.

As a spin-off from the physico-chemical study of the blood, Van undertook, in 1922, to put the concept of buffer value of weak electrolytes on a mathematically exact basis. By differentiating the mass law equation for weak acids with respect to pH, he arrived at the generalization

$$\beta = \frac{\mathrm{dB}}{\mathrm{dpH}} = 2.3\ \frac{\mathrm{K}^1\,[\mathrm{H}^+]\,\mathrm{C}}{(\mathrm{K}^1 + [\mathrm{H}^+])^2} + [\mathrm{H}^+] + [\mathrm{OH}^-],$$

where $\beta$ = buffer value. This proved to be useful in determining buffer values of mixed, polyvalent, and amphoteric electrolytes, and put the understanding of buffering on a quantitative basis. It was applied in Van's laboratory to the determination of dissociation constants of polyvalent weak acids such as citric acid, whose three acid groups have overlapping dissociation constants.

While this work on blood was going on, Van was preparing to make a detailed and comprehensive study of nephritis and its varied manifestations. In this he had a number of clinical associates, including Dr. Alma Hiller, who was in charge of his clinical chemical laboratory (1918-1948). (After Cullen left in

1921, the basic chemical work of Van's laboratory was successively supervised by A. B. Hastings, 1921-1926; J. Sendroy, Jr., 1926-1937; D. A. MacFadyen, 1937-1940; and R. M. Archibald, 1940-1946.)

Van always had a number of problems under investigation at the same time at the Institute, but he never hurried to publish the results. It was customary for him to put each paper through several drafts and revisions.

Van had a great capacity to concentrate intensely and effectively on the problem at hand, and at the same time keep track of several research problems going on in the laboratory. This is why the publications in any one year often covered a wide range of subjects. For example, during 1928 Van and eight of his associates published twelve papers: one on a gasometric method for sugar determination in blood and urine, another on a new method for hemoglobin determination, three on factors affecting urea excretion in health and disease, and seven additional entries in the series "Studies of Gas and Electrolyte Equilibria in Blood." One of these, "The Solubility of Carbon Dioxide at 38° in Water, Salt Solution, Serum, and Blood Cells," was notable in that the first measurements on the subject were made in 1922, six years prior to publication. Each year the results would be written up for publication and each year Van would say: "We'll take another look at this in the fall, to make sure we can't improve on the accuracy." This was repeated annually until it met Van's standards. It was typical of his publications that one could count on their data and results without question.

The period of preoccupation with the study of blood as a physico-chemical system was followed by intensive study of nephritis, undertaken with a number of clinical colleagues. They followed and minutely documented the life history of the disease through its various stages in patients. This resulted in 1930 in a detailed publication by Van Slyke and nine colleagues of a monograph in *Medicine* entitled "Observation on the Courses of Different Types of Bright's Disease, and on the Resultant Changes in Renal Anatomy." It was a landmark in that it related the changes occurring at different stages of renal deterioration to the quantitative changes taking place in kidney function.

With his laboratory associates, he continued for many years to study the kidney in health and disease, with particular attention to its metabolism and its ability to excrete waste products, particularly urea. Though in subsequent years, improved techniques for evaluating kidney function have appeared, the work of the Van Slyke laboratory stands as a pioneering model for the clinical study of this excretory organ.

During this period, Van Slyke and R. M. Archibald identified glutamine as the source of urinary ammonia. During World War II, Van and his colleagues documented the effect of shock on renal function and, with R. A. Phillips, developed a simple method, based on specific gravity, suitable for use in the field, for determining red blood cell concentration in whole blood and protein concentration in blood plasma. In postwar years, this method, in Phillips's hands, proved of incalculable value in detecting the severity of, and in following the results of therapy in, cholera.

Also, it was during this period of the 1940s that Jordi Folch joined Van Slyke's laboratory and the manometric apparatus was adapted for the determination of carbon in organic compounds. This led to the detailed study of plasma lipids and in 1948 to the identification by Folch of the important phospholipid, phosphatidyl serine.

Over 100 of Van's 300 publications were devoted to methodology. Most were new methods; some were devoted to improvements he had made in earlier descriptions—either to increase accuracy or to reduce the size of the sample of blood or other material required for analysis. The importance of Van

Slyke's contribution to clinical chemical methodology cannot be overestimated. His gasometric procedures alone accounted for about two dozen of the methods that were applicable to compounds of biological and clinical significance, but they were always devised to answer a physiological or clinical question. These included the blood organic constituents (carbohydrates, fats, proteins, amino acids, urea, nonprotein nitrogen, and phospholipids) and the inorganic constituents (total cations, calcium, chlorides, phosphate, and the gases carbon dioxide, carbon monoxide, and nitrogen). It was said that a Van Slyke manometric apparatus was almost all the special equipment needed to perform most of the clinical chemical analyses customarily performed prior to the introduction of photocolorimeters and spectrophotometers for such determinations.

Though colorimetric procedures were available, they required the development of a color that was often not specific for the substances being measured. Hence, the chemical reaction quantitatively yielding a gas from the specific substance that could be isolated and measured had certain advantages.

## VAN SLYKE AND QUANTITATIVE CLINICAL CHEMISTRY

The progress made in the medical sciences in genetics, immunology, endocrinology, and antibiotics during the second half of the twentieth century obscures at times the progress that was made in basic and necessary biochemical knowledge during the first half. Methods capable of giving accurate quantitative chemical information on biological material had to be painstakingly devised; basic questions on chemical behavior and metabolism had to be answered; and, finally, those factors that adversely modified the normal chemical reactions in the body so that abnormal conditions arise that we characterize as disease states had to be identified.

At the beginning of the century, biochemistry was in its infancy, and quantitative clinical chemistry did not exist as such. It was given to a few individuals professionally trained as chemists, who found themselves engaged in the study of disease in association with clinicians to change the course of the practice of medicine. Donald Van Slyke was one of these chemists.

In 1901, the year before Van Slyke received his Ph.D. from the University of Michigan, the new medical buildings of Harvard University were dedicated. President Charles Eliot, originally a chemist, stated at the dedication, "There is an increasing need of men who have a working knowledge of several sciences which were formerly treated as distinct and whose best representatives in medical schools labored apart, each in his own field. The most promising medical research of our day makes use of biological, chemical and physical science combined. Physiology advances by making applications of the principles, the methods and the implements of all three sciences. Bacteriology and biological chemistry go hand in hand in serving pathology and the public health."

Beginning in 1906 and for the next sixty-five years, it almost seemed as if Donald Van Slyke planned and conducted his activities with these goals in mind. Viewed in retrospect, he combined in one scientific lifetime (1) basic contributions to the chemistry of body constituents and their chemical behavior in the body, (2) a chemical understanding of physiological functions of certain organ systems (notably the respiratory and renal), and (3) how such information could be exploited in the understanding and treatment of disease. That outstanding additions to knowledge in all three categories were possible was in large measure due to his sound and broadly based chemical preparation, his ingenuity in devising means of accurate measurements of chemical constituents, and the opportunity given him at the Hospital of the Rockefeller Institute to study disease in company with physicians.

Since Van Slyke's scientific life was spent at the Rockefeller Institute for Medical Research from 1907 through 1948 (thirty five of the years at the Hospital of the Rockefeller Institute), followed by twenty-two years in the Medical Department of the Brookhaven National Laboratory, it covers rather uniquely the evolution of biochemistry and notably quantitative clinical chemistry. During the period 1921-1926, while I was his assistant, the problems under investigation in his laboratory included the development of methods; the study of blood as a physicochemical system and its relation to respiratory diseases; the study of proteins and amino acids and their metabolism; his early work with Dr. Alma Hiller on what ultimately proved to be the new amino acid hydroxylysine; and, finally, in collaboration with clinical colleagues, a definitive study of various types of nephritis. Meantime, while all these different problems were under way, he found time to work collaboratively with Dr. John P. Peters of Yale on the classic, two-volume *Quantitative Clinical Chemistry.* At the time it was published in 1931, it contained practically all that could be stated with confidence about those aspects of disease that could be and had been studied by chemical means. It was widely accepted throughout the medical world as the "Bible" of quantitative clinical chemistry, and to this day some of the chapters have not become outdated.

It is of interest to recall how this collaboration came about. In 1922, John P. Peters, who had just gone to Yale from Van Slyke's laboratory as an Associate Professor of Medicine, was asked by a publisher to write a modest handbook for clinicians describing useful chemical methods and discussing their application to clinical problems. It was originally to be called "Quantitative Chemistry in Clinical Medicine." He soon found that it was going to be a bigger job than he could handle alone and asked Van Slyke to join him in writing it. Van agreed, and the two men proceeded to draw up an outline and divide up the writing of the first drafts of the chapters between them. They also agreed to exchange each chapter until it met the satisfaction of both. This may have improved the accuracy and completeness of the chapters, but it delayed publication of the book until 1931. Each chapter turned out to be a monograph, and the book had grown to two volumes. One volume, *Interpretations,* dealing only with the physiological and clinical significance of those substances for which quantitative methods were available, contained 1,200 pages and twenty-one chapters. The second, entitled *Methods,* consisted of about 1,000 pages describing in detail those methods that had been proven accurate and useful. Somewhere along the line, the title was shortened to *Quantitative Clinical Chemistry.* It must have met a need, because the first edition sold out rather quickly here and abroad and the publishers soon asked the authors to prepare a second edition. Though they tried, medical research was progressing faster than they could keep up-at least to both their satisfactions. The upshot was that a second edition was never completed.

Though their collaboration was a fortunate one, it was difficult because Van was accustomed, as a chemist, to be content with nothing less than proven accuracy, whereas Peters was used to being confronted constantly with disease manifestations in patients—no two of which were the same. This made it difficult to satisfy both of them at the same time on any one subject. However, the first edition was well worth doing and remains a classic in the subject.

## BROOKHAVEN PERIOD (1948-1971)

The year 1948 proved to be a fateful one for Donald Van Slyke. Rena, his wife for forty years and mother of his daughter, Elsa, and son, Karl Keller, had died the year before, and he had reached the Rockefeller retirement age of sixty-five. Though vigorous physically, he was lonely and depressed mentally.

At this juncture, he accepted the position of Deputy Director of Biology and Medicine at the newly formed Brookhaven National Laboratory and met Else von Bardenfleth Brock, whom he married. The challenge of his new responsibilities cured his depression, and the understanding companionship of Else banished his loneliness. Van thereby entered upon the second position he was to hold in his lifetime with the vigor and enthusiasm that had characterized his forty-one years at the Rockefeller Institute. Van retained the title of Deputy Director only long enough to ensure the appointment of able chairmen in the departments of biology and of medicine and then renewed his life in the laboratory with John Plazin, his lifetime assistant, who had accompanied him from the Institute. Though previously inexperienced with the use of isotopes, he and John were soon at home with them and in 1951, with Robert Steele, published a much-improved method for the determination of $^{14}C$.

During the course of the next few years, Van devised a micro version of the manometric apparatus and adapted his various gasometric procedures to it. As a result, determinations that had previously required one milliliter samples now required samples only one-tenth as large, with no loss in accuracy. These micro methods were published as a monograph by Van Slyke and Plazin in 1961, with typical Van Slyke attention to accuracy, clarity, and essential detail.

With his Brookhaven colleagues, Van continued his study of nephritis and nephrosis, of metabolism, and of improved methodology in evaluating acid-base balance clinically.

Among his last papers published from the Hospital of the Rockefeller Institute in 1949 were two on pH determination, with J. R. Weisiger and his son, K. K. Van Slyke, as coauthors. Since his first paper, in 1906, had been with his father, Lucius L. Van Slyke, this must have given him special satisfaction.

From 1951 to 1956, Van served part-time as counselor to Eli Lilly Research Grants. In this capacity, he had the responsibility of identifying promising investigators and making recommendations for their support in the basic medical sciences. He carried out this responsibility with his customary conscientious and deliberate care. He would visit the laboratories of the investigators here and abroad and keep meticulous notes of his observations, following which he reported his recommendation to the Lilly Research Grants office. In all, his recommendations resulted in the distribution of about $400,000 over the five-year period, which aided many young investigators to get on with their research at a time when seed money was in short supply.

At the end of this venture, he again took up his full-time laboratory life as a Research Biochemist in the Department of Medicine of the Brookhaven National Laboratory, a position he held for the rest of his life.

## HYDROXYLYSINE

In the course of his analysis of proteins at the Rockefeller Institute, Van encountered a discrepancy between the amount of colorimetrically determined histidine in a gelatin hydrolysate and that calculated from arginine and nonamino-nitrogen determinations. This observation led in 1921, after Van had attempted to isolate the substance, to publication in the *Proceedings of the National Academy of Sciences* of a paper entitled "An Unidentified Base among the Hydrolytic Products of Gelatin." Finally, in 1938, Van Slyke, Hiller, Dillon, and MacFadyen announced that the "unidentified base" was the new amino acid, hydroxylysine. Its synthesis had to wait another twelve years, being simultaneously achieved by Weisiger in Van Slyke's laboratory and by Sheehan and Bolholfer at MIT. Thus, the requirements for acceptance of a new amino acid were met at last. Van continued to study its biosynthesis and its role in collagen throughout his Brookhaven period. It would have given him great satisfaction had he lived to see the importance it plays today in providing linkage with mucopolysaccharides in plasma membranes.

Thus, Van takes his place with his early teacher, Emil Fischer, as one who discovered a highly important amino acid.

## VAN SLYKE AND CHINA

Shortly following the dedication in 1921 of the Peking Union Medical College (P.U.M.C.), Van Slyke spent several months in 1922-1923 in Peking as a Visiting Professor of Biochemistry. Although he became deeply engaged in the laboratory with professors F. C. McLean and Hsien Wu in studies of blood equilibria, he found time to learn much about Chinese history, culture, and people and returned a profound admirer of China and the Chinese. From these early impressions he never deviated.

As early as 1937, he joined with other former P.U.M.C. faculty to provide medical aid to the Chinese people. In 1938, when the American Bureau for Medical Aid to China was formed, Van Slyke was elected a Director; and, in 1941, he became its President, a post he held throughout World War II. He became Honorary President in 1947, and continued to serve actively on the Board of Directors until a few months before his death.

In 1961, he spent two months at Taipei, Taiwan, as a visiting investigator at the Navy's Cholera Research Laboratory, known as NAMRU-2. He was thus able to renew his friendship with former P.U.M.C. faculty who had migrated with Chiang Kai-Shek to Taiwan. They had formed the National Defense Medical College (N.D.M.C.), which Van assisted in various ways while he was there.

In appreciation of his services to the Chinese people, Van received two decorations from the Republic of China: in 1939, the Order of the Jade, and in 1947, the Order of the Brilliant Star. These were among the most treasured of his possessions, and he never relinquished his faith that the Chinese people will eventually triumph with a national life worthy of their culture, ability, and aspirations.

## VAN SLYKE, THE PERSON

Van Slyke served as Managing Editor of the *Journal of Biological Chemistry* from 1914 to 1925, an activity to which he devoted many hours of close personal attention. During his editorship, the *Journal* flourished, and the high standards for clarity of presentation, convincing data, and justifiable conclusions were set that continue to characterize this publication.

He worked and reworked each publication from his laboratory until he could think of no way to improve it, either through experiment or through rewriting. His papers describing new methods were models of clarity and exactness. Nothing was left to the imagination, so that it was said, "If you follow Van Slyke's directions to the letter, your results will have the accuracy he predicts."

As far as I am aware, he never had to correct the data or retract the conclusions contained in his publications. Subsequent advances in technique and knowledge have in some instances led to his work being superseded, but it was nevertheless correct for the time it was published.

Van's usual unadorned use of the English language could be felicitous when he thought it appropriate. As an example, I quote from his Harvey Lecture of 1916, "The Present Significance of the Amino Acids in Physiology and Pathology":

"It is a pleasure, as well as a duty, to acknowledge my indebtedness to Dr. Levene, for six years my chief at the Rockefeller Institute. The work detailed this evening is a direct outgrowth of Levene's own researches on the proteins, was carried out with the constant inspiration of his enthusiasm, and help of his counsel, and of his generosity in making available every facility which the laboratory afforded, even at times to the delay of his own immediate work, the ultimate sacrifice that can be taken from a spirit such as his."

Van presented at all times a serious mien to the world of science. Only his family and close friends were aware that he was an irresistible punster. As far as I know this frivolous indulgence never found its way into print.

Once, however, he allowed expression to his subsurface humor in a footnote to be found on page 276 of *Quantitative Clinical Chemistry,* Volume 2, *Methods.* It is in the chapter describing the manometric gas apparatus. It reads:

"The closed manometer really owes its origin to our laboratory comrade of many years, Glenn E. Cullen. The numerous genuflexions required during a day's work in reading the low lying zero points of the open tube told heavily on Cullen's jovial proportions, and the laboratory felt so much the loss of his usual contagious spirits, that the more humane closed tube had to be devised."

Van loved to play tennis, which he did up to within a few months of his final illness. Not that he was a master of sparkling strokes, but rather that he accurately and persistently returned almost any ball with which his opponent challenged him. He won points, games, and sets by untiring consistency and precision. He played tennis in much the same way as he attacked and conquered laboratory problems.

As was said on the occasion of his 80th birthday celebration, "Van plays science the way he plays tennis: he senses where the ball (or the problem) is going to be before it gets there, he gets to the ball directly with no lost motion, he never takes both feet off the ground, and when he hits the ball, it is with a firm, straight, and accurately aimed blow. Most exasperating of all, he uses no fancy strokes—but just keeps putting it back until he wins the point. He wins lots of sets, and he solves lots of problems."

In the course of preparing to write this memoir, I encoun tered in my files a handwritten memorandum written during the summer of 1926. My five years with Van at the Rockefeller Institute were over, and my new life at the University of Chicago had not yet started. I include it here to help recapture the appearance and personality of the then forty-three-year-old Donald Van Slyke at the height of his scientific productivity.

A Retrospective Log

August 13, 1926

From October first nineteen hundred and twenty-one until June twentieth nineteen hundred and twenty-six I was engaged as one of the staff of the Hospital of the Rockefeller Institute for Medical Research.

The man responsible for my initial appointment, for my mental and scientific growth while there, and my advancement to an opportunity of greater responsibility, was Donald Dexter Van Slyke. His is such an extraordinary scientific personality that I feel impelled to chronicle my impressions of him.

Physically, he immediately attracts your attention and admiration. Short, stocky but well proportioned, his well-formed head, now sparsely covered with graying hair, sits solidly upon square shoulders. In his profile you see expressed his decisiveness. A strong mouth and chin with straight high forehead denote the man. He speaks with crispness and decision yet unkind words never pass his lips.

I've never interviewed him—I could not. But suppose I had—what would the result be like?

Were I to be announced to him on the phone, he would be waiting for me in front of the elevator on the seventh floor of the hospital. Of what other man of equal prominence could that be said? Yet I have seen him do it many, many times during these precious five years with him. Nor does he limit this courtesy to his peers. It is always so, even for the youngest tyro.

Through a little vestibule, I should enter his office, a small room, about 12' × 12', and be seated in a straight chair beside his desk. The office appearance merits a few words. A flat top desk sets in the center at an angle so that light falls over his left shoulder as he writes. Along one wall is an open bookcase with volumes he is using now for reference, his own collected reprints and many folders of work in various stages of completion for publication. On top of the bookcase are framed photographs, perhaps twenty. They are some of the men who have worked with him. To catalogue them would take me far afield but a cursory glance shows strong, clean-visaged, intelligent men and today they fill chairs of medicine, of chemistry, of physiology, or are in successful practice. They are all successes. Nor are they confined to America. Danes and Englishmen are among them. The wall back of me—the one Dr. Van Slyke faces—has a group of older faces. There is Emil Fischer with whom Van worked in Berlin, Moses Gomberg of the University of Michigan under whom he took his doctorate degree, P. A. Levene with whom he first worked when he came to the Rockefeller Institute in 1908. Below these are Lawrence J. Henderson of Harvard, who together with Van Slyke has advanced physiology most in this decade, and William Mansfield Clark of the Hygienic Laboratory in Washington (later to head physiological chemistry at Hopkins from 1927 to 1952) whose career had paralleled Van's in success if not in clinical relevance and international recognition. These then are the faces which Van sees when he lifts those steady and penetrating eyes of his from the work on his desk.

In the forty-five years that followed the writing of the above, Van did not change in any important way. It might be said that he did not age significantly until after his terminal disease was discovered.

Even then, he retained a scientific interest in the course of his condition as late as March 28, 1971, the day before his 88th birthday. As I sat chatting with him, he quoted to me the course that his plasma proteins were taking, as if he were discussing one of his Institute patients under investigation. Though fully aware of the ultimate outcome, there was no evidence that he had any changes to make in the present or past.

Van is survived by a daughter, Elsa Van Slyke, born in 1912, a son, Karl Keller Van Slyke, M.D., born in 1915, and by his second wife, Else Bardenfleth Van Slyke. In spite of Van's lifelong preoccupation with his laboratory research, he was an attentive and companionable father and a thoughtful and affectionate husband. His home was a happy and hospitable haven to his friends and colleagues throughout his life.

Van's contributions to science and to medicine were nationally and internationally recognized and honored by medals, awards, honorary degrees, and memberships in professional societies here and abroad. He accepted them all humbly and gratefully as tributes to his colleagues quite as much as to himself. These honors are itemized at the end of this memoir.

## VAN SLYKE'S LEGACY

Donald Van Slyke will long be remembered for his legacy to the following:

*To Biochemistry and Physiology:*

Exact and accurate methods for the determination of constituents of biological material.

Sound physico-chemical interpretations of the role of hemoglobin in the transport of $O_2$ and $CO_2$ by the blood and of the distribution of water and anions between plasma and erythrocytes.

The role of the liver in amino acid metabolism.

The role of the kidney in urea excretion and ammonia formation.

The mathematical definition of buffer value in terms of hydrogen ion concentration, dissociation constants, and buffer concentrations.

A new and important amino acid, hydroxylysine.

*To Medicine:*

His development to useful maturity of quantitative clinical chemistry through identifying clinical questions capable of chemical attack and devising the methods necessary to answer the questions.

Publication in 1931 with the late John P. Peters of the classic two-volume collection of existing chemical knowledge relevant to disease, entitled *Quantitative Clinical Chemistry.*

The clarification of the subject "acidosis" and the meaning of other acid-base balance abnormalities.

A thoroughly documented description of nephritis as it progresses through its various stages.

A large number of Doctors of Medicine, trained in the chemical approach to clinical investigation, who became leaders in academic medicine.

To his friends and colleagues Van has left the memories of his kindness and evenness of temper, his directness of approach to problems, his ability to avoid distractions that were irrelevant to his objective, his economy of words in speech and publications, his penchant for exactness, clarity, and completeness, and his thoughtfulness in his relations with others.

Altogether, Donald Van Slyke was a prolific scientist, a pioneer in bringing quantitative clinical chemistry to the service of medicine, humble, unselfish, considerate, magnanimous, and rigid only in his adherence to the truth.

Those of us who worked with him loved him, and those who knew him only through his works admired and respected him.

Though the light that was Donald Dexter Van Slyke in life has gone out, the glow that has illumined so much of chemistry and medicine and so many of those who worked with him shines on.

In preparing this memoir, the author has consulted most of Van Slyke's publications, the transcript of an Oral History prepared by Dr. Peter D. Olch in 1969 and on file at the National Library of Medicine, and correspondence between Van Slyke and the author extending over a fifty-year period.

# HONORS AND DISTINCTIONS

## Honorary Doctor of Science Degrees

Yale University, 1925
University of Michigan, 1935
Northwestern University, 1940
University of Chicago, 1941
University of London, 1951
Rockefeller University, 1966

## Honorary Doctor of Medicine Degrees

University of Oslo, 1938
University of Amsterdam, 1962
University of Ulm, 1970

## Medals and Awards

Charles Mickle Fellowship, University of Toronto, "to the member of the medical profession who has done most during the preceding ten years to advance sound knowledge of a practical kind in medical art or science," 1936

Phillip A. Conne Medal, Chemists' Club of New York, for contributions to clinical chemistry, 1936

Willard Gibbs Medal, Chicago Section of the American Chemical Society, for contributions to chemistry, 1939

Order of the Jade, Republic of China, 1939

Kober Medal, Association of American Physicians, for "distinguished research in preventive medicine," 1942

Order of the Brilliant Star, Republic of China, for "meritorious service to the Chinese People," 1947

Fisher Award in Analytical Chemistry, American Chemical Society, 1953

John Phillips Memorial Award, American College of Physicians, for "achievement in internal medicine," 1954

First Van Slyke Award in Clinical Chemistry, American Association of Clinical Chemists, 1957

First Scientific Achievement Award, American Medical Association, 1962

Ames Award, American Association of Clinical Chemistry, 1964

National Medal of Science, USA, 1965

Elliott Cresson Award, Franklin Society of Philadelphia, 1965

Medal of the New York Academy of Medicine, 1966

## American Memberships

National Academy of Sciences, 1921
American Philosophical Society, 1938
American Society of Biological Chemists (President, 1920-1922)
Harvey Society (President, 1927-1928)
American Bureau for Medical Aid to China (President, 1940-1947)
American Academy of Arts and Sciences
Rudolf Virchow Medical Society in the City of New York
American College of Cardiology (Honorary Member)
American Chemical Society
New York Academy of Medicine
Association of American Physicians
American Association of Clinical Chemistry
Society of Experimental Biology and Medicine

## Foreign Memberships (Honorary)

Società di Biologia Chimica, 1928
Deutsche Akademie der Naturforscher, 1932
Società Lombarda di Medicina, 1935
Academy of Science of India, 1935
Society of Biological Chemists of India, 1936
Royal Society of Sciences of Upsala, 1942
Danish Society for Internal Medicine, 1952
Société de Pathologie Rénale, 1952
Società Italiana di Biologia Sperimentale, 1953
Association of Clinical Biochemists, Britain, 1953
Royal Society of Medicine, Britain, 1958
Academia Nazionale dei Lincei, Italy, 1962
Danish Academy, 1956

# Bibliography

*Key to Abbreviations*

Abderhalden Handb. biol. Arbeitsmethod. Abt.——Teil = Abderhalden Handbuch der biologischen Arbeitsmethoden Abteilung——Teil

Am. J. Dis. Child. = American Journal of Diseases of Children

Am. J. Physiol. = American Journal of Physiology

Ann. N.Y. Acad. Sci. = Annals of the New York Academy of Sciences

Arch. Intern. Med. = Archives of Internal Medicine

Ber. Dtsch. Chem. Ges. = Berichte der Deutschen Chemischen Gesellschaft

Biochem. Z. = Biochemische Zeitschrift

Biomed. Newsl. = Biomedical Newsletter

Clin. Chem. = Clinical Chemistry

Clin. Chim. Acta. = Clinica Chimica Acta

Fed. Proc. = Federation Proceedings

J. Am. Chem. Soc. = Journal of the American Chemical Society

J. Am. Med. Assoc. = Journal of the American Medical Association

J. Biol. Chem. = Journal of Biological Chemistry

J. Clin. Invest. = Journal of Clinical Investigation

J. Exp. Med. = Journal of Experimental Medicine

Mod. Med. = Modern Medicine Proc. Natl. Acad. Sci. USA =

Proceedings of the National Academy of Sciences of the United States of America

Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine

Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians

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Carl J Wiggers

# Carl John Wiggers

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by Eugene M. Landis

Whenever historians of science deal with the development of physiology and medicine in the United States, they give special significance to the period 1900-1950. At the turn of the century, physiology was beginning its growth in this country as a science in its own right and as a discipline useful to medicine and surgery. The life and contributions of Carl John Wiggers coincide almost precisely with this period. In 1901 he entered medical school with the customary aim of becoming a physician. Almost at once he attracted the attention of his professors in the preclinical sciences. Stimulated and encouraged by them, he became interested in physiological research and enhanced that interest by advanced study abroad. Upon his return to the United States, he became a member of a small but important group of leaders who developed highly individual and productive research laboratories in several medical schools and in several specialties of physiology.

Carl Wiggers's chief location was the medical school of Western Reserve University for thirty-five years (1918-1953), and his lifelong interest was cardiovascular physiology. Two generations of physiologists and clinicians were stimulated to follow research careers because of his influence as teacher, preceptor in research, lecturer, author, and editor. He was prominent among those who formulated the physiological principles that underlie many of the recent, often spectacular, advances in the diagnosis and treatment of cardiovascular diseases. He was elected a member of the National Academy of Sciences in 1951.

Carl John Wiggers was born in Davenport, Iowa, on May 28, 1883. He was the first of two children; a younger sister died in childhood. His father, Jürgen Wiggers, accompanied by a brother, John, had left the limited prospects of a small family farm in the duchy of Holstein to seek opportunity in this country. To pay their passage across the Atlantic, they served as sailors and in 1876 arrived in New York as immigrants. Jürgen, during a period of orientation, worked his way west as far as Colorado through temporary farm jobs and even some unsuccessful prospecting for gold. He subsequently returned eastward to Scott County, Iowa, and settled in Davenport. There, in 1882, he met and married Anna Margaretha Kundel, whose family had also emigrated from Holstein. Eventually, Jürgen Wiggers became manager of a social club called Lahrmann's Halle, which resembled the celebrated *Ratskellern of* Germany and was a favorite rendezvous of professional and business men in the downtown area of Davenport. In his autobiography Carl Wiggers describes how his father, while still in Holstein, had aspired to an education in law or even medicine. For the father these ambitions could not be fulfilled; but the son received parental encouragement for education toward a profession, in addition to the usual German domestic traditions and thrift.

Carl Wiggers attended elementary and high schools in Davenport. Before he went to high school, his parents required that he have in mind aims for a specific vocation or profession. To keep within the family's limited resources, he chose pharmacy. In high school his teachers of chemistry, physics, and zoology provided special stimulation to young Wiggers; and he, an eager student, proved especially interesting to them. They took "every opportunity to discuss his future aspirations and tactfully awakened the idea" that he should aim for medicine rather than pharmacy. In a carefully reasoned comparison between the newer Johns Hopkins medical school (college degree required) and the slightly older medical school of the University of Michigan (high school diploma then still sufficient), Wiggers chose the latter. In his choice he noted that he really couldn't afford four years of college, that the organization and curricula of the two schools seemed similar, and, more significantly, that Johns Hopkins, in developing its own medical faculty, had, in fact, chosen three of its professors from Michigan's faculty.

Early in medical school at Ann Arbor, Wiggers must have shown again some qualities that attracted the immediate interest of his first professors. His initial intentions to prepare for public health work, or for practice in obstetrics and pediatrics, gave way to curiosity concerning physiology and research. He ascribed this change, first, to an "experiment in education" by Professor Warren P. Lombard, which involved a "research problem" assigned to students in the final weeks of their course in physiology. Second, this was followed by Professor Arthur A. Cushny's emphasis on the physiological actions of drugs. And, third, Lombard offered Wiggers a paid student assistantship in physiology that continued until he received his M.D. in 1906. He was then promoted to an instructorship in physiology, which he held from 1907 to 1911.

His publications began while he was a medical student. In 1905 he described the action of adrenalin on the cerebral blood vessels and demonstrated his findings at the American Physiological Society meeting in Ann Arbor that same year. At this meeting he also heard reports and observed demonstrations by well-known investigators such as Macleod, Brodie, Erlanger, and Y. Henderson. By his own account the stimulation from informal conversations with these and other physiologists firmly established his interest in physiology as a career. In the thirteen papers that Wiggers published from 1905 to 1911, he described the responses of cerebral, coronary, and pulmonary blood vessels to electrical or chemical stimulation; presented an improved apparatus for measuring blood flow; and reported studies on hemorrhage with particular emphasis on the ineffectiveness, or even harmfulness, of adrenalin injections. As a group, these papers present a general preview of areas and topics that Wiggers continued to study in greater depth for forty years. His chief lifelong interest, cardiac physiology, became obvious several years later.

In 1907 Carl Wiggers married Minerva E. Berry, a junior medical student and thus, as he worded it, "two careers—one scientific, the other domestic—were launched almost simultaneously," and their "coexistence proved facilitatory and salutary." This was verified many times over by his wife's assistance as part-time secretary and by her understanding companionship and help during long hours of research and writing. They had two sons, both of whom became eminent. One, Harold, became himself a Professor of Physiology and later, in 1953, Dean of Medicine at Albany Medical College. The other, Raymond, entered the field of industrial advertising and became an award winner in that area. In later years a lifelong habit of hospitality and personal interest enlarged the social boundaries of the Wiggers family and home to include research fellows, junior staff members, colleagues, and visitors from other laboratories and countries. It is significant that in 1952 the affection of this larger family was expressed by their establishing at Western Reserve University a joint honor entitled "The Minerva and Carl Wiggers Annual Prize in Physiology."

In 1907, during his first year as an Instructor, Wiggers was given the responsibility for a didactic and demonstration course in physiology for dental students, who had become seriously discontented with Professor Lombard's offerings. In this first course, he revealed his conviction that, if subject matter was chosen wisely, basic courses could be made not only valuable to students but also interesting. In describing the results of this first responsibility for teaching, he made the modest and gently humorous appraisal that "the attitude of dental students toward physiology was changed; it no longer was regarded as an ordeal but as a satisfaction, if not exactly a pleasure." The course led to the publication in 1914 of his first book, *A Brief Text of Physiology*.

An important career decision was made by Wiggers early in his instructorship. To supplement their low salaries, members of basic science departments were allowed, and even encouraged, to engage in limited practice of medicine. A brief test of such part-time clinical work led him to conclude that it was hindering, not helping, his academic career, and reinforced his decision to make physiology his sole activity. In 1910 Professor Lombard took a sabbatical year to study abroad, and Wiggers became acting head of the department. He mentioned later that it was helpful to learn at an early age the nature of administrative work. At this time, with the advice of A. W. Hewlett, the new Professor of Medicine, he used some hours in the basic physiology course to demonstrate to first-year students how physiologic information could be applied to clinical problems and help interpret the signs and symptoms of disease in patients.

During this period Wiggers found that his studies were being limited by the unreliability of the pressure recorders then available to physiologists. Professor Hewlett told him of the optical recording methods devised by Professor Otto Frank in Munich, and Wiggers promptly arranged, through Lombard, then in Europe, for a year of study abroad. But this had to be postponed to 1912 and abbreviated to a spring and summer because, on the recommendation of W. H. Howell of Johns Hopkins, Graham Lusk invited him to come to Cornell Medical School in New York as Instructor of Physiology. It was agreed, however, that after a few months at Cornell, he would have a leave of absence for study with Professor Frank at the Physiological Institute of Ludwig-Maximilian University. Arriving there in May 1912, Wiggers found that, as a representative of Lusk's group, he was granted exceptional privileges in observing the optical equipment and methods that the usually reserved, even secretive, Otto Frank was developing in his laboratory for measuring rapidly and accurately not only blood pressures, but also pulse contours and rates of blood flow. By some additional visits elsewhere, Wiggers also gained a "traveller's acquaintance with other laboratories in Europe."

When Wiggers returned to New York in the summer of 1912, his research interests were not limited to physiology, but continued to include cardiovascular disease also. To learn more about the latter, he devoted one morning each week to attending ward rounds in Bellevue Hospital. He kept improving the reflecting mirror manometer of Otto Frank and devised a mobile unit that brought his equipment into use at the bedside. He was promoted to Assistant Professor of Physiology in 1913. His research from 1912 to 1918 dealt with many areas of cardiovascular function but began to deal increasingly with an analysis of cardiac function, normal and abnormal. Early in this period he published the first optically recorded pressure pulses in the pulmonary arteries of dogs through cannulas inserted directly. These records, together with Otto Frank's studies of the central arterial pulse in the aorta, permitted comparisons of pressure levels in the lesser and greater arterial circulations. In 1917 he compared the timing of pressure changes in the heart chambers with the contractions of heart muscle and with the electrocardiogram. He was thus in a position to amplify the findings of Thomas Lewis (later Sir) in London, who was challenging Einthoven's view that the electrical and mechanical events of cardiac contraction were simultaneous. These observations indicated that electrical events preceded the contraction of heart muscle and were a measure of the progressive conduction of the excitation process throughout the heart. With clinical co-workers in Bellevue Hospital, he studied atrial fibrillation, analyzed the supraclavicular venous pulse in man, and began registering heart sounds. Even at this early stage he was convinced that physiologists should at intervals describe in review form the status of ongoing research in order to keep clinicians well informed. His second book, *Modern Aspects of the Circulation in Health and Disease,* published in 1915, was such a progress report. It was a monograph that described, with examples, the usefulness of newer methods in the clinical diagnosis and treatment of cardiovascular diseases as seen in hospital practice. He was always a vigorous supporter of the view that advances in medicine and surgery must be based upon an investigative and basically physiologic approach to disease.

In 1917-1918 the entry of the United States into World War I presented a new series of cardiovascular problems, ranging from cardiac-fitness examinations in draftees, through neurocirculatory asthenia or "soldier's heart" in stressed individuals, to the physiologic principles underlying the diagnosis and treatment of hemorrhage and shock in the severely wounded. In 1918 Wiggers served on a medical appeal board and used his experience with electrocardiography to help adjudicate in cases of draftees when physical fitness was disputed. At this time, too, he became a member of a National Research Council Committee on Shock. Sir Thomas Lewis invited him to come abroad to share in a study of neurocirculatory asthenia, but he had to decline, chiefly because Graham Lusk was on leave for war research abroad, and again he was acting chairman of a department. However, as a "Contract Surgeon" in U.S. Army General Hospital #9, he spent a short time during the summer of 1918 in a research laboratory, headed by Professor Francis W. Peabody from Harvard, for the purpose of studying cardiac disabilities in draftees and soldiers.

With all this in progress, and during a meeting of the Com mittee on Shock in Baltimore, Torald Sollman, Professor of Pharmacology at Western Reserve Medical School, told Wiggers that J. J. R. Macleod, then Professor of Physiology at Western Reserve, had decided to accept a position with the University of Toronto, and then urged him to consider accepting the vacant professorship beginning with the autumn term of 1918. Wiggers accepted, but only after making sure that he could arrange matters at Cornell so that Graham Lusk would be inconvenienced as little as possible by his leaving.

Thus in 1918, and at age thirty-five, Wiggers began to establish in Cleveland his own center of cardiovascular research, first in a small loft of the older medical building, and then from 1924 onward, in a separate floor of a new medical building in which the physiology department had more space and facilities as planned by Wiggers himself. The problems of new equipment, staff, courses for students, and budgetary matters were burdensome at times but did not perceptibly interrupt his research and publications. During the thirty-five years from 1918 to 1953, almost every part, and almost every physiological function, of the heart and blood vessels were the direct or indirect topics of some study by Carl Wiggers or by his many graduate students and co-workers. Approximately 400 papers were published from the laboratories that Wiggers established and supervised. In matters of authorship and credit, he was again characteristically generous. In over half of these papers, his colleagues and students were granted sole authorship. Wiggers believed firmly that beginning investigators deserved sole authorship of papers dealing with their work, even though the head of a laboratory may have given essential assistance in ideas, advice, and editing. Wiggers also published a total of seven books in editions ranging from one to five.

To do justice to the content of all these papers and books is impossible in any brief memoir. It is fortunate that Wiggers wrote in 1958 an autobiography entitled *Reminiscences and*

*Adventures in Circulation Research.* That book provides a detailed, chronological account of the questions that prompted his research, his development of suitable equipment, and his experiments and results. Descriptions of successes are balanced by frank discussion of failures, oversights, and those second thoughts that increasing experience and new facts produced. In this brief memoir it is more appropriate to describe a few of the main lineages of ideas that determined the major pathways and methods used by Wiggers in his researches.

One of the earliest of these lineages dealt with analyzing the interrelations of electrocardiographic, excitatory, contractile, and hemodynamic events during the cardiac cycle. This was not easy, because in the early 1900s it was necessary to import almost all research equipment of any intricacy from instrument makers abroad, and at great expense. When Wiggers began working in Cleveland, conditions were improving, but it was still necessary for him to establish a departmental workshop to produce his own improved optical manometers for recording pressures or sound, as well as cardiometers and flowmeters for volume measurements. Moreover, each of these devices had in the past been used separately by individual investigators and usually for limited and special problems. Most difficult of all was the task of obtaining the best possible string galvanometers for electrocardiography, but this, too, was accomplished. In viewing the future of cardiac research, Wiggers saw that:

". . . by aligning such a galvanometer with optical recorders for pressure, muscular contraction and heart sounds, the interrelations of electric and contractile events in the heart could be determined more accurately than before. This problem was—and remains—one of cardiologic as well as scientific interest, for it is basic to the usefulness of electrocardiographic interpretation of impulse conduction."

Each of the new instruments presented technical problems, and combining several devices produced a really formidable challenge. Nevertheless, Wiggers collected the data necessary to produce a graphic summary that correlated the dynamic, mechanical, acoustic, and electrocardiographic events during the normal cardiac cycle, lasting approximately 0.9 seconds. As a chart this summary became a standard textbook figure and was reprinted in reviews and books dealing with physiology, electrocardiography, and cardiology in general. It is still used as a diagram upon which new data, such as single-cell membrane action potentials, can easily be added.

In 1921 Wiggers wished to measure more accurately the effects on the cardiac cycle of changing venous return, as in muscular exercise, and of aortic pressure, as in hypertension. It was necessary to subdivide ventricular systole and diastole into smaller and more precise units. Eventually eight phases were identified: isometric contraction, maximal ejection, reduced ejection, protodiastole, isometric relaxation, rapid inflow, diastasis, and atrial systole. The beginning and end of each phase was defined precisely by relevant simultaneous changes of blood pressure levels in the left ventricle, aorta, or atrium; by small, more detailed changes in the configuration of the pressure pulses; and by heart sounds. Also, for each phase, the normal range of duration was measured. This subdivision of the cardiac cycle was also widely used in physiology and cardiology. Moreover, from these studies developed a second lineage of problems, ideas, and research.

During experiments on the exposed heart, Wiggers had been impressed with its great resistance to drastic manipulations and even injury. "However, it happened far too often that the thrust of a cannula or stylus through the ventricular walls was followed by irreversible fibrillation whose occurrence could not be related to the region involved, the nutrition or dynamic state of the ventricles, or to the age of the animal. The hazard of terminating an experiment by fibrillation was materially increased when strong shocks were applied during systole. . . ."

He tried many expedients to diminish the incidence of ventricular fibrillation but results were discouraging.

In the 1920s, as electrical appliances in home and industry became more numerous, so had also the deaths of users by accidental electrocution from faulty wiring. After a "Conference on Electric Shock" in 1929, Professor W. H. Howell of Johns Hopkins asked Carl Wiggers to join efforts with Donald R. Hooker in exploring possible ways of restoring normal rhythm to hearts in which fibrillation had been produced by electric currents. Wiggers accepted, but with the cautious feeling that the most he "could hope for was some discovery that might reduce the mortality rate of experimental animals."

His first approach was a screening study to explore chemical methods of restoring normal cardiac rhythm. Although intraventricular injections of potassium chloride often made the fibrillating heart quiescent and calcium salts sometimes restored a normal heartbeat, these effects were not consistent enough to be satisfactory, even in experimental animals. After deciding that not enough was known about the genesis of ventricular fibrillation, Wiggers and his group in 1930 used coordinated cinematographic and electrocardiographic methods to study the earliest stages of the arrhythmia, which then emerged as an evolving process that progressed irregularly through four fairly characteristic, but often overlapping, stages. These might take a few to many minutes to develop fully from the first undulating contractions to the fourth, and longest, atonic stage, lasting from twenty to sixty minutes. Because this final atony did not develop when the ventricles were perfused artificially with oxygenated blood, Wiggers suggested that the final lethal flaccidity of unperfused, fibrillating hearts could "probably be referred to failure to reconstitute energy yielding material during the progressing anoxia."

In 1932 Kouwenhoven, Hooker, and Langworthy, while studying the distribution of electric current in the bodies of dogs, tested and confirmed an almost forgotten statement published in 1899 by Prevost and Battelli to the effect that if ventricular fibrillation occurred it could be terminated by applying a strong electric current to the heart. Wiggers had accumulated methods and experience that placed him in an ideal position to provide speedy, additional confirmation of the beneficial effects of countershock, though he found it effective only when applied within two or three minutes after induction of fibrillation. In the meantime, H. B. Williams and associates had also confirmed observations by Wiggers that electric shocks during the "vulnerable period of the heart cycle" induced fibrillation and that immediate application of strong currents to the chest wall could stop that fibrillation. As Wiggers expressed it, the problem became one of "extending the period during which countershock could be effective after development of fibrillation."

Success in doing this was "not a matter of chance; it resulted from a logical application of observations from contemporaneous experiments . . ." These pointed to the conclusion "that the fibrillating ventricles lose their power of effective contraction after 2 or 3 minutes, because they are deprived of oxygenated blood. The remedy apparently needed was a supply of oxygen for the myocardium *before* application of countershock." In 1936 Wiggers suggested that gentle manual rhythmic compression of the ventricles be started as quickly as possible to raise arterial blood pressure and to restore blood flow through the coronary vessels in the heart muscle. When this maneuver was continued until vigorous fibrillary movements were reestablished, then countershock usually restored coordinated ventricular contractions. In addition, however, dogs with larger hearts often required "serial defibrillation," i.e., administration of three to five brief shocks at intervals of two seconds to reach the deeper lying parts of the ventricular wall. By 1940 he could demonstrate by means of these innovations that a dog could survive if its heart were fibrillated and defibrillated reversibly at will many times in succession. Over a decade elapsed, however, before these principles were utilized clinically in the emergency treatment of ventricular fibrillation in patients. During this time he and his co-workers continued to study in the laboratory many related topics, including quantitative fibrillation thresholds, fibrillation following induced coronary occlusion, effects of type of current and voltage, and actions of drugs. Collectively, these research reports form an essential part of the physiologic foundation that supports modern cardiac resuscitation and defibrillation, cardiac monitoring, and pacemakers, as well as the feasibility and relative safety of "elective cardiac arrest" in open-heart surgery.

In still another lineage of interlocking experiments, Wiggers and his students dealt with the hemodynamics and control of blood flow through the heart and past its valves, through a distensible aorta, and through a branching system of peripheral vessels, including especially the coronary arteries in the heart muscle itself. The number and ramifications of these experiments defy any brief summary. They included studies on the energetics and biophysics of the heart as a pump, wave forms in the aorta and its branches, and peripheral resistance to blood flow through arteries and arterioles. His interest in abnormalities such as valvular lesions, arteriosclerosis, and hemorrhage can be traced back to his instructor days at Michigan, and especially to his hours on the wards of Bellevue Hospital, in New York. His experiments were sometimes analytic—e.g., central and peripheral pulse wave forms, factors determining coronary blood flow, accuracy of blood pressure measurement in animals and man—and sometimes integrative—e.g., his classification of stages in severe hemorrhage and ensuing shock. The wider implications of these integrations are found in his seven books: *Modern Aspects of the Circulation in Health and Disease* (two editions, 1915 and 1923), *Pressure Pulses in the Cardiovascular System* (1928), *Principles and Practice of Electrocardiography* (1929), *Physiology in Health and Disease* (five editions, 1934 to 1949), *Physiology of Shock* (1950), *Circulatory Dynam* *ics: Physiologic Studies* (1952), and *Reminiscences and Adventures in Circulation Research* (1958).

In whatever he did Carl Wiggers was always ready to describe clearly and unequivocally his basic principles and convictions. For instance, he emphasized repeatedly that an ideal department of physiology in a medical school had three main functions: first, to offer medical students the broadest possible education in physiology and its importance for medicine; second, to contribute actively and regularly to physiological knowledge by research; and, third, to educate and train experimenters and teachers for both physiology and medicine.

In the course of accomplishing the first two objectives, as already summarized above, Wiggers provided an exceptionally good environment for the third. In his thirty-five years at Western Reserve, his department provided direct research experience for almost 200 individuals. By 1953 his alumni included a score of professors of physiology or heads of closely affiliated research laboratories. Among these were research centers, almost as eminent as the parent one, in special topics such as coronary blood flow, membrane action potentials in cardiac muscle, peripheral resistance to regional blood flow, clinical electrocardiography, hemorrhage, and shock. Visiting students came from foreign countries and returned to important positions in physiology or medicine abroad. One of them, Corneille Heymans, from Belgium, received a Nobel Prize in 1930. By conservative estimate almost a hundred young physicians and surgeons held fellowships that gave them direct experience with the principles of physiological research. In most instances this advanced work was related informally to research in progress, but in 1949 Wiggers felt that new conditions made a new training procedure appropriate.

In 1945, the end of World War II began a period of expanded support for research. In addition, by 1947, clinical interest in catheterization technics for early diagnosis and treatment of congenital cardiac defects and coronary artery disease was growing faster than the supply of clinical investigators. To meet this need Wiggers proposed a more formal "apprenticeship" program in cardiovascular research and writing. Under the joint sponsorship of the American Heart Association and the U.S. Public Health Service, and with the facilities in his laboratories, he and his staff graduated about twenty "U.S.P.H.S. Training Course Fellows" during the three years from 1949 to 1952. These graduates brought to a number of new clinical cardiology centers the skills and judgment of experienced investigators.

In developing this apprenticeship program, Wiggers emphasized still another of his basic principles, namely that any research effort is not complete until the results are carefully written, thoroughly edited, and promptly published in a form that is both clear and useful to others. With characteristic humor he describes in his reminiscences how the critiques of his own first papers in 1905 by Professor W. P. Lombard seemed devastating temporarily and then helpful eventually. In 1925 Wiggers shared in founding the *American Heart Journal,* was a member of the original Advisory Editorial Board, and remained a member until 1937. In 1952 he was only a year away from retirement and accepted still another editorial challenge. The American Heart Association had adopted a policy of increasing support for basic research and found it timely to publish a new journal that could emphasize a multidisciplinary approach to fundamental studies of the cardiovascular system. Wiggers was asked to organize and edit this new journal, *Circulation Research,* with the first issue scheduled for 1953. He turned over the administration of the Department of Physiology in 1952 to a newly appointed successor, Dr. George Sayers. He then accepted an appointment as Honorary Professor of Physiology with the Frank E. Bunts Educational Institute of Cleveland and organized an editorial office in space provided by the Cleveland

Clinic. As the first editor of *Circulation Research* from 1953 to 1958, he brought the new journal rapidly into a leading position worldwide for reporting the results of basic research on heart and blood vessels. His editorials were widely quoted, and authors accepted with gratitude the conscientious reviews and criticisms that their papers received from him and his carefully chosen editorial board members.

Among his duties as a physiologist, Wiggers also included travel to learn firsthand of outstanding work elsewhere, combined with lecturing to review for others recent advances in ongoing research. During his thirty-five years in Cleveland, he made several trips abroad to attend and address congresses and symposia, with visits to laboratories added. In 1939 he was invited to lecture in Argentina and elsewhere in South America. For this he prepared by learning Spanish. Other invitations to give honorary lectures took him to Germany, Holland, England, and from coast to coast in the United States. Because Wiggers had many interests in both physiology and medicine, his memberships in scientific societies included a number in each category, with service as officer in many. His earliest and longest loyalty went to the American Physiological Society, where, in annual meetings for fifty years, he was a dependable source of lively discussion and admonitions to young and old investigators, delivered over his ever-present cigar and with a friendly humor to temper any incisive criticism. He served the Society as Secretary, Treasurer, and in 1949 as President. His other memberships ranged from the American Society of Pharmacology and Experimental Therapeutics to the American College of Physicians, American Medical Association, American Heart Association (Vice-President, 1947), and numerous cardiology groups, ranging from a local Cleveland area heart society (President, 1955-1956) to the Inter-American Cardiological Society (President, 1956).

Honors came to Carl Wiggers in abundance both in early life and later, both at home and abroad. The gratitude and homage of his many students and co-workers led to frequent "Anniversary Dinners" in his honor. These included annual gatherings during meetings of the American Physiological Society and special testimonial dinners in Cleveland celebrating his twentieth and twenty-fifth years there and his emeritus year, when his portrait was presented to Western Reserve Medical School. The Circulation Group of the American Physiological Society grants annually to an outstanding physiologist its "Carl J. Wiggers Award and Lectureship." In 1951 an issue of the journal *Circulation* was dedicated to him. In 1958 a group of his former pupils volunteered research papers that made up an entire number of *Circulation Research* to commemorate two events: first, the "retirement of Carl J. Wiggers after his five years of devoted service as its first editor," and second, "the seventy-fifth birthday of this man, one of the surviving giants of the legendary age of American physiology."

Wiggers received honorary doctorates from his alma mater, the University of Michigan (1951); the Royal Ludwig-Maximilian University of Munich, Germany (1952); Free University of Brussels, Belgium (1956); and Ohio State University (1958). Honorary memberships in academies, professional societies, and university faculties began at home with the Cleveland Academy of Medicine and included honors from many others in the United States, England, Mexico, and several countries in South America. From the American Heart Association he received the Gold Heart Award (1952) and the Albert Lasker Award (1955). In both he was named "Dean of Cardiovascular Physiologists." *Die Deutsche Gesellschaft für Kreislaufforschung* awarded him their Carl Ludwig Medal (1954) and the Royal Academy of Medicine in Belgium awarded him their gold medal (1956).

Carl John Wiggers died suddenly at his home in Cleveland on April 29, 1963, one month short of his eightieth year. His death meant the loss of still another of that small group of brilliant leaders in research who initiated and shaped the rapid growth of physiology in the United States during the first half of this century. Not lost at all, but persisting unchanged, is the lasting heritage of his example, bestowed on everyone who had the privilege of associating with him. To quote from only one of many paragraphs of eulogy and gratitude: "Author, pioneer research worker in cardiovascular physiology, his monument is the host of great students sent into the world imbued with his own philosophy and the values of precision, promptness, hard work and thoroughness. All over the world his work will continue in many other leading physiologists and physicians."

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# Bibliography

*Key to Abbreviations*

Alumni Bull. Sch. Med. Affil. Hosp. = Alumni Bulletin of the School of Medicine and Affiliated Hospitals, Western Reserve University

Am. Heart J. = American Heart Journal

Am. J. Med. Sci. = American Journal of Medical Sciences

Am. J. Physiol. = American Journal of Physiology

Ann. Intern. Med. = Annals of Internal Medicine

Annu. Rev. Physiol. = Annual Review of Physiology

Arch. Intern. Med. = Archives of Internal Medicine

Circ. Res. = Circulation Research

Exp. Med. Surg. = Experimental Medicine and Surgery

Fed. Proc. Am. Soc. Exp. Biol. = Federation Proceedings of the American Society for Experimental Biology

J. Am. Med. Assoc. = Journal of the American Medical Association

J. Exp. Med. = Journal of Experimental Medicine

J. Lab. Clin. Med. = Journal of Laboratory and Clinical Medicine

J. Pharmacol. Exp. Ther. = Journal of Pharmacology and Experimental Therapeutics

Physiol. Rev. = Physiological Reviews

Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine

West. Reserve Univ. Sch. Med. Clin. Bull. = Western Reserve University School of Medicine Clinical Bulletin

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