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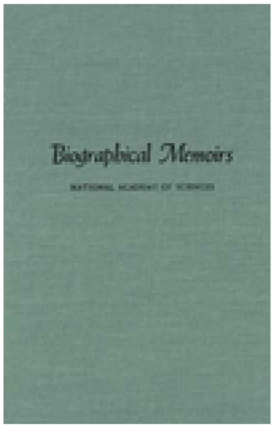

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Biographical Memoirs

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NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA

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Volume 53

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The National Academy of Sciences was established in 1863 by Act of Congress as a private, nonprofit, self-governing membership corporation for the furtherance of science and technology, required to advise the federal government upon request within its fields of competence. Under its corporate charter the Academy established the National Research Council in 1916, the National Academy of Engineering in 1964, and the Institute of Medicine in 1970.

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Preface

The *Biographical Memoirs* is a series of volumes, beginning 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 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.

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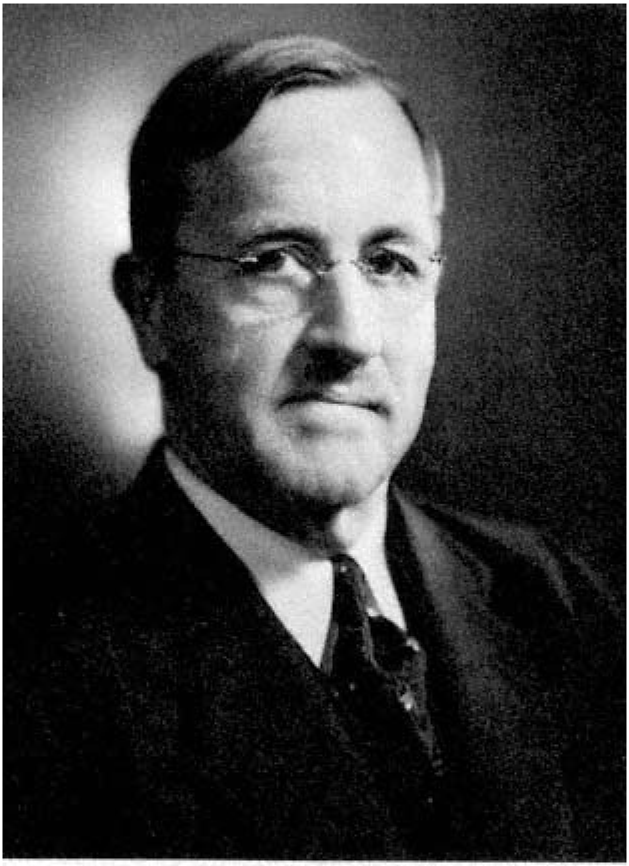
Biographical Memoirs

Volume 53



Roger Adams

Photograph courtesy of Fabian Bachrach

Roger Adams

January 2, 1889-July 6, 1971

by D. Stanley Tarbell
And
Ann Tracy Tarbell

Roger Adams was for a generation the leading organic chemist in the United States. In addition to publishing outstanding research in structural chemistry and stereo-chemistry and to training about 250 Ph.D.'s and postdoctorates at Illinois, he played a key role in the development of graduate education in science. His influence on the growth of industrial chemical research was great, both personally and through the students he trained. His services to the country in two world wars and to the scientific community brought him the role of scientific statesman on the world scene. He combined personal qualities of charm, strength, high intelligence, and extraordinary capacity for hard work.

Roger Adams was a direct descendant of the uncle of President John Adams; his ancestors had moved to southeastern New Hampshire, where Roger's father, Austin W. Adams (1845-1916), was born. Austin taught in a country school, then moved to Boston in 1872, where he was associated with the Old Colony and New Haven Railroads for the rest of his life. In 1880 he married Lydia Curtis from Jamaica Plain, likewise a school teacher, who was related to numerous business and literary figures in Boston and was descended from early colonial settlers.

The Adamses lived for twenty years on Worcester Street in the then attractive residential section of South Boston. Austin Adams, a kind father and a man with scholarly avocations, supported his family on a comfortable but not luxurious scale. Roger was the last child in a family with three daughters, and he apparently had a happy boyhood. Two of Roger's sisters —tall, athletic, and gifted—graduated from Radcliffe and the third from Smith. The family moved to Cambridge in 1900, probably to be nearer the colleges.

After preparation at Boston Latin and Cambridge Latin, Roger entered Harvard in 1905. His first years were undistinguished academically, but he completed the requirements for his A.B. in chemistry in three years, earning high grades in his major courses in chemistry and in his minor of mining. His interest in chemistry may have been aroused by C. L. Jackson's course on "the chemistry of common life," which he took in his first year. He worked very hard at a series of demanding courses and had the satisfaction of receiving a John Harvard honorary scholarship for making four A's. In his last undergraduate year he took advanced courses and started research in organic chemistry with H. A. Torrey. After graduation in 1909, he worked for his Ph.D., aided by a teaching assistantship at Radcliffe. Torrey died prematurely in 1910, and Adams completed his thesis with the help of Jackson, Latham Clarke, and G. S. Forbes.

Among his fellow graduate students in 1911 and 1912 were E. K. Bolton (" Keis," a close friend), Farrington Daniels, Frank C. Whitmore, and James B. Sumner. James B. Conant was then an undergraduate.

As an outstanding Ph.D. of 1912, Adams was awarded a Parker Traveling Fellowship for 1912 and 1913, which he spent partly in the laboratory of Emil Fischer with Otto Diels at Berlin and partly with the brilliant Richard Willstätter in the latter's new laboratory in Dahlem, near Berlin. A ticket stub among Adams' papers shows that he took a Zeppelin flight from Potsdam to Berlin; he also traveled in Finland, Russia, and Sweden.

Although it resulted in no scientific papers, Adams' European year was the beginning of his lifelong interest in European science and scientists. He clearly found the European university system distasteful; one professor in each department or institute controlled absolutely the activities of all research students and junior staff members. This attitude is reflected in Adams' leadership of the Illinois department along more democratic lines, which provided a model for other graduate science departments in American universities. Adams' policy of helping his junior colleagues develop independent research careers was emphasized further in 1954, when he designed the program for the Sloan Foundation that gave unrestricted grants to promising younger workers. As a result of his foresight, generations of young American scientists are in his debt.

Adams returned to Harvard in 1913 as research assistant to C. L. Jackson at $800 a year and shortly undertook the duties of instructor in chemistry. Along with other courses he taught elementary organic chemistry and initiated the first elementary laboratory in that subject at Harvard. He was a very successful teacher, so much so that James B. Conant, who succeeded him, did so with some trepidation about his own ability to interest an elementary class in organic chemistry. During his three years on the Harvard faculty, Adams not only carried a very heavy formal teaching load at Harvard and Radcliffe, but made a strong start on his own research program.

In 1916 Adams accepted an offer from William A. Noyes, head of the Illinois Chemistry Department, to become assistant professor at a salary of $2,800 per year. He was ambitious to accomplish something notable in science; he undoubtedly saw that the Illinois department, already well known for its research and teaching under Noyes' leadership, offered greater opportunities than Harvard, as well as better laboratory facilities and the challenge of a position outside New England. He took the position at Illinois with no reservations and with the determination to develop his own research program and that of the department. Urbana remained his home for the rest of his life, in spite of many attractive offers to move to industry or university positions, including MIT and Harvard. It is indicative of his personal dedication to Illinois as his residence that he later joined the Rotary Club and served as its president in 1932.

At Illinois Adams took charge of the important "prep labs" started by his predecessor, C. G. Derick, for the synthesis of essential organic chemicals cut off by the blockade of Germany. This was expanded and, with the help of graduate students, particularly E. H. Volwiler and C. S. (Speed) Marvel, over 100 key compounds were made available for sale and for use in Illinois. Adams reorganized the operation, introduced strict cost accounting procedures, and made it a financial as well as scientific success. The tested procedures developed in "preps" (officially called Organic Chemical Manufactures) led to the indispensable annual publication, *Organic Syntheses,* of which fifty annual volumes were published under Adams' watchful eye. Conant later said that the publication should have been named "Adams Annual," because he was the moving spirit.

At Urbana Adams pursued research vigorously on the preparation of local anaesthetics with Oliver Kamm of the Illinois faculty and became a consultant to Abbott in 1917, a relationship that lasted on a formal basis until the 1960's. E. H. Volwiler, his first Ph.D., joined Abbott as a research chemist in 1918.

Adams was drawn into research for the army in 1917 and, with other chemists, worked on problems connected with war gases at American University in Washington, D.C. Adams and Conant headed research groups, and E. P. Kohler, Adams' old faculty friend from Harvard, was in charge of the Offense Section. Adams spent the last few months of 1918 in uniform as a major. He was recognized by Conant as the leading figure in the group.

Adams was married to Lucile Wheeler on August 29, 1918 at White River Junction, Vermont. Mrs. Adams came from a well-known Vermont family; she was a Mount Holyoke graduate, had studied dietetics at Columbia, and had taught home economics at Illinois. They had one child, Lucile, and there are four grandchildren, in whom Adams delighted, particularly in his retirement years. The Adamses soon moved to a large house at 603 West Michigan, Urbana, where many guests, students and distinguished visitors alike, were graciously entertained by Mrs. Adams. This house was Adams' headquarters for the rest of his life. He wrote frequently to Mrs. Adams when he was away; he had a gift for light verse and sent a rhymed valentine to her and their daughter each year.

Adams' return to Urbana was followed by intensive research with a large number of Ph.D. students and by many outstanding scientific publications. From 1918 through 1926 he published seventy-three scientific papers and trained forty-five Ph.D.'s, including E. H. Volwiler, J. R. Johnson, A. W. Ingersoll, S. M. McElvain, W. H. Carothers, W. R. Brode, C. R. Noller, R. L. Shriner, C. F. Rassweiler, and many others who became well known in academic or industrial chemistry. This was in spite of a very serious illness in 1924, recovery from which required nearly a year. During this time he took up stamp collecting, a hobby he pursued for many years with characteristic thoroughness and enthusiasm.

The important problems he worked on during his research career included the development of platinum oxide for catalytic hydrogenation (Adams catalyst-discovered by an excellent example of serendipity), local anaesthetics, synthesis of naturally occurring anthraquinones, structure and synthesis of chaulmoogric acid (used at that time in treating leprosy), stereochemistry of compounds with restricted rotation and of deuterium compounds, the structure of gossypol (the yellow material in cottonseed meal), compounds isolated from marihuana, and the structure of the Crotalaria and Senecio alkaloids and quinoneimines. The gossypol structure was perhaps the most difficult, but the voluminous correspondence related to his work on marihuana illustrates well the administrative and scientific ability and the boundless energy and enthusiasm with which he pursued his researches.

As a body, Adams' research represents the high point of structural organic chemistry, particularly on natural products, before the Instrumental Revolution and before the emergence of physical organic chemistry as a major field. He was elected to the National Academy of Sciences in 1929.

In 1926 Adams was chosen unanimously to succeed W. A. Noyes as head of the Chemistry Department at Illinois. By this date the department was recognized as one of the leaders in organic chemistry, and the characteristic features of Adams' conception of a graduate department were clear. The faculty should be of outstanding ability: Adams, Marvel, R. L. Shriner, R. C. Fuson, and, in the 1930's and later, H. R. Snyder, C. C. Price, N. J. Leonard, and their junior colleagues formed the organic group, with few changes. Equipment and facilities were to be kept up to date; graduate students and undergraduates were carefully selected and were caught up in the infectious enthusiasm and hard work of the faculty so that they, too, worked long hours. Research was carefully done, carefully and promptly prepared for publication, and great attention was paid to placing graduates in suitable positions. Adams was actively interested in the progress of all Illinois graduates in chemistry after they left Urbana and recommended them for new positions as the occasion arose; his remarkable memory enabled him to call most of them by name, whoever had directed their research.

Adams developed a highly successful scheme for directing graduate student research. By starting a student on a problem that promised reasonable success and by publishing the work promptly, he built up the student's confidence to undertake more difficult experiments. Such problems as hindered rotation in biphenyls offered indispensable and excellent training in synthesis and physical measurements, and difficult investigations in natural products were usually given to experienced graduate students or to postdoctorates. Adams' personal magnetism made interesting even the drudgery encountered in research projects, and made good results exciting. His buoyant and forward-looking nature, his outgoing personality, his interest in people as individuals, and his breadth of knowledge and experience made conversation with him amusing and delightful, broadly educational in the best sense.

Adams' work on *Organic Syntheses,* his circulation of bound volumes of reprints to leading universities here and abroad, and his active participation in scientific societies increased the reputation of his department. What he had really accomplished by 1930 was the development of a graduate department of national stature that had strength in all significant fields of chemistry; it represented a novel addition to that characteristic American educational institution, the landgrant college. Although A. A. Noyes at MIT (later at Caltech) and G. N. Lewis at Berkeley had built up departments of physical chemistry with several outstanding colleagues, the Illinois department was larger and offered a broader range of research opportunities to students.

The growth and research output of the Illinois department are shown by a survey of papers in organic chemistry in *the Journal of the American Chemical Society*. In the twenty-five years from 1914 through 1939, Illinois was surpassed only four times in number of publications of organic research, advancing from four papers in 1914 to sixty-six papers in 1939, 11 percent of all organic papers published in this journal from all the nation's laboratories in 1939. The Illinois papers were scientifically of high quality. Although figures available do not give doctorates for organic chemistry as such, during the years 1920 through 1939 Illinois produced 346 Ph.D.'s in chemistry, 6 percent of all American Ph.D.'s in this field. Adams directed almost one-third of the Illinois total and accounted for about 2 percent of those in the whole country for the years 1920 through 1939, as shown in the following table; during the 1920's Adams trained 3 percent of the American Ph.D.'s in all fields of chemistry.

Doctorates Granted in All Fields of Chemistry, 1920-1939

Years	*Illinois*[a]	*Total US*
1920-1924	64 (30)	746
1925-1929	73 (26)	1,178
1930-1934	103 (25)	1,751
1935-1939	106 (22)	2,212

[a] Figures in parentheses represent Adams' own Ph.D.'s for these periods.

Of the 105 Ph.D.'s trained by Adams during the years 1918 through 1939 inclusive, seven were women; of the total, fifty-nine spent most or all of their careers in industrial research, twenty-six were in teaching, and nine worked in government laboratories. The remainder included a number of overseas students.

Where did all of these chemists find jobs? Consideration of this question leads to Roger Adams' interaction with the industrial chemical research community, which grew strikingly between the two world wars. It is estimated that in 1920 there were 300 industrial research laboratories in the United States, and that in 1940 the number had grown to 2,200. In 1927 the chemical industry is believed to have had 3,300 research people, and in 1938, in spite of the depression, the number had increased to 9,542.

Adams had a natural inclination toward the business world; although he always regarded himself as a member of the academic profession, he understood finance and business and was liked and trusted by businessmen. He served as consultant for A. E. Staley Co., M. W. Kellogg Co., and Coca-Cola, as well as Abbott, and he and Marvel became consultants for DuPont in 1928. Industrial research was given a large boost by the spectacular success of Adams' brilliant student, Wallace H. Carothers, whose fundamental research on polymers at DuPont from 1928 to 1937 resulted in the discovery of nylon and neoprene rubber. Adams, always close to him, was deeply affected by his death in 1937.

Adams frequently wrote and spoke about what industry was entitled to expect from its research chemists, and the majority of Illinois Ph.D.'s who did go into industrial research were well informed as graduate students in this regard. Conversely, he was able to make clear to research management in industry how research chemists should be treated to maintain good morale and productivity. As the number of Illinois Ph.D.'s grew, it was a small educational or industrial laboratory that did not have one or more. Hence, the influence of the Illinois department and its graduates became very great, particularly when many of these graduates reached responsible administrative positions in research and teaching. Adams was constantly being asked for advice about academic and industrial positions everywhere, and he spent much effort in finding suitable positions for his students. In 1954 he wrote that his "biggest contribution to chemical industry has been very indirect through many of the students who have been eminently successful in industry." It is clear, however, that this underestimates his personal contribution.

Although it is almost invidious to single out Adams' Ph.D.'s between 1927 and 1959, the following had outstanding careers: M. M. Brubaker, R. M. Joyce, T. L. Cairns, J. F. Hyde, W. M. Stanley (Nobel Laureate), W. E. Hanford, Byron Riegel, R. Clarke Morris, Allene R. Jeanes, B. R. Baker, Nathan Kornblum, R. S. Long, M. W. Miller, Jack Hine, W. H. Lycan, M. T. Leffler, E. E. Gruber, D.J. Butterbaugh, R. O. Sauer, and R. B. Wearn. An equally impressive list could be made of his postdoctorates.

Adams played a key role in the enlistment of unrestricted industrial support for university research, which became particularly significant after World War II; this was sometimes in the form of graduate fellowships, of which there were many at Illinois even before World War II, and sometimes as unrestricted grants to be used for equipment or stipends as needed. In addition, his service for the National Science Foundation, the Sloan Foundation, the Welch Foundation, and the Sloan-Kettering Institute for Cancer Research, among others, increased the influence of his ideas. His attitude in distributing research funds was the same as in his selection of faculty members or graduate students: pick the best people possible, give them what they need to work, and don't bother them unduly.

Several points previously mentioned bear on Adams as an international scientific figure. His acquaintance with European chemistry from his postdoctoral trip in 1912 and 1913 was maintained and increased by extensive correspondence. His papers contain a series of letters written in 1927 acknowledging copies of Illinois reprints from numerous distinguished European chemists and many Americans, and his home guest book contains the names of eminent scientists from all around the world. He maintained a cordial correspondence with nearly all prominent foreign chemists, including numbers from South America, South Africa, and the Far East, and many of them visited Urbana. In 1936 he visited many countries in Europe and greatly increased his professional acquaintances, especially in Switzerland.

During his war years of work for the National Defense Research Committee, 1940 to 1945, Adams was in charge of many projects in chemistry and chemical engineering, requiring constant travel in this country to laboratories and proving grounds. In 1943 he visited Britain, where he renewed several old acquaintances and met many distinguished British scientists whom he had not seen before. He returned to Urbana when possible to talk with his students. One of his associates in Washington during those years wrote that if he was shipwrecked on a distant island, he would like to have Roger Adams with him. He was sure that Adams would figure out some way of getting off, and they "would have a whale of a good time doing it."*

After the war he was in Berlin, from November 1945 until February 1946, as scientific advisor to the U.S. Military Government under General Lucius D. Clay, where he exercised significant influence in reviving the German compendia, Beilstein and Gmelin. He also visited Japan twice with committees of scientists, and the ensuing plan for a complete reorganization of Japanese science resulted in its rebuilding along more democratic lines. His group had a long conference with General MacArthur who strongly commended their report and used it as a basis for scientific policy in Japan.

Although he retired as department head at Illinois in 1954, and as research professor in 1957, he continued his

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* W. M. Latimer to N. A. Parkinson, 20 October 1949.

work with *Organic Syntheses* and *Organic Reactions,* which he had started about 1940, and as a consultant; he was on advisory boards for many groups, some mentioned above, and for the American Chemical Society, of which he had been president in 1935. He was president of the American Association for the Advancement of Science, served on the Natural Resources Board of the State of Illinois, and on the Board of Directors of Battelle Memorial Institute.

His retirement as head of chemistry in 1954 was celebrated by a symposium in Urbana, attended by over 300 former students, colleagues, and friends. The group showed its great affection for Adams, and some of his students wrote a suitably irreverent skit, poking fun at some of his foibles.

Adams made many overseas trips, partly for pleasure and partly to attend international scientific meetings; he visited most of the major countries of the world at least once, and enlarged his circle of acquaintances in every country. As a senior statesman of American science, his visits were major events everywhere. After Mrs. Adams' death in 1964, he continued an active schedule of attending meetings, serving on boards of directors, and traveling, even in his late seventies. One of his colleagues, asked if Adams was working less after his retirement, said he probably was; he was doing only three men's work instead of four.* He died on July 6, 1971 after a brief hospitalization, following a trip to Columbus for a directors' meeting at Battelle Memorial Institute.

Adams' accomplishments had been recognized by the respect and personal affection of his collaborators and associates. He was a member of the American Philosophical Society, the American Academy of Arts and Sciences, and an honorary member of more than a dozen foreign academies and chemical societies. He received honorary doctorates

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* C. S. Marvel to E. H. Volwiler, 12 November 1958.

from ten institutions, including Illinois, Harvard, Yale, Rochester, Pennsylvania, and Michigan. He was the recipient of most of the medals and awards open to scientists, such as the Davy Medal of the Royal Society, the Gibbs, Nichols, Parsons, and Priestly medals, the Medal of the Franklin Institute, the National Medal of Science, Honorary Commander of the British Empire (C.B.E.), and the Medal for Merit of the United States, the last two recognizing his services in World War II. The list of his awards, offices, and honors covers three pages.

More than any other organic chemist of his time, Roger Adams epitomized the coming of age of organic research and advanced training in this country and the phenomenal growth of chemical research and production in industry. His influence on the development of American science rested partly on the period in which he lived, but to a greater extent on his own extraordinary personality and ability. Roger Adams is a good example of Dr. Samuel Johnson's definition of genius: "a mind of large general powers, accidentally determined to some particular direction."

THE BASIS OF THIS PAPER is the research for a book-length biography of Adams by the present authors, now in press. Some of the travel and other costs of this research were paid by grants from the Centennial Fund of Vanderbilt University, the Petroleum Research Fund, and the National Science Foundation. We are indebted to Lucile Adams Brink for gracious assistance and for the loan of family documents. Maynard G. Brichford of the University of Illinois Archives and Jean R. St. Clair of the National Academy of Sciences have been most helpful with the Adams documents under their care. Drs. E. H. Volwiler and R. M. Joyce have given us valuable encouragement and suggestions. Many other librarians, archivists, friends, and associates of Adams have furnished important documents and information. Some of the material in the present paper appeared in a publication by us entitled "The Role of Roger Adams in American *Science, "Journal of Chemical Education,* 56 (1979):163.

Bibliography

1910 With H. A. Torrey. Über in alkalien unlösliche Phenole. Ber. Dtsch. Chem. Ges., 43:3227-28.

1915 With C. L. Jackson. Hexabromodiacetyl. J. Am. Chem. Soc., 37: 2522-36.

With L. Clarke. Normal nonane. J. Am. Chem. Soc., 37:2536-38.

With H. Gilman. Phenyl esters of oxalic acid. J. Am. Chem. Soc., 37:2716-20.

1916 With L. F. Weeks. Action of oxalyl chloride on primary, secondary and tertiary alcohols. J. Am. Chem. Soc., 38:2514-19.

With C. H. Beebe. Action of the Grignard reagent on CN compounds. Synthesis of amidines from cyanamides. J. Am. Chem. Soc., 38:2768-72.

1918 With W. V. Wirth and H. E. French. Oxalyl chloride as a reagent in organic chemistry. II. Preparation of aromatic acid anhydrides; reaction with alcohols. J. Am. Chem. Soc., 40:424-31.

With O. Kamm. Organic chemical reagents. I. Dimethylglyoxime. J. Am. Chem. Soc., 40:1281-89.

With E. H. Vollweiler. The reaction between acid halides and aldehydes. I. J. Am. Chem. Soc., 40:1732-46.

1919 Alkali-insoluble phenols. J. Am. Chem. Soc., 41:247-70.

With R. E. Rindfusz. Cyclic ethers from o-allyl phenols; methylene coumaranes. J. Am. Chem. Soc., 41:648-65.

With V. Voorhees. Organic chemical reagents. IV. The preparation of alkyl iodides. J. Am. Chem. Soc., 41:789-98.

1920 With C. S. Marvel. Organic chemical reagents. VI. Reagents from n-butyl alcohol. J. Am. Chem. Soc., 42:310-20.

With L. H. Ulich. The use of oxalyl chloride and bromide for producing acid chlorides, acid bromides or acid anhydrides. III. J. Am. Chem. Soc. , 42:599-611.

With S. G. Powell. A comparison of the activity of certain unsaturated groups with the activity of the allyl group in certain ethers. J. Am. Chem. Soc., 42:646-58.

With H. B. Bramlet and F. H. Tendick. The action of the Grignard reagent on thiocyanates. J. Am. Chem. Soc., 42:2369-74.

With C. S. Palmer. The reactions of the arsines. Preliminary paper. Condensation of primary arsines with aldehydes. J. Am. Chem. Soc., 42:2375-78.

1921 With H. E. French. The reaction between acid halides and aldehydes. II. J. Am. Chem. Soc., 43:651-59.

With L. H. Ulich. The reaction between acid halides and aldehydes. III. J. Am. Chem. Soc., 43:660-67.

With J. R. Johnson. 2-Phenylquinoline-4-carboxylic acid-6-arsonic acid. J. Am. Chem. Soc., 43:2255-58.

1922 With A. J. Quick. Aliphatic arsonic and arsinic acids, and aliphaticaromatic arsinic acids. J. Am. Chem. Soc., 44:805-16.

With M. F. Fogler and C. W. Kreger. The structure of disalicyl aldehyde. J. Am. Chem. Soc., 44:1126-33.

With C. S. Palmer. The reactions of the arsines. III. Condensation of aromatic primary arsines with aldehydes. J. Am. Chem. Soc., 44:1356-82.

With V. Voorhees. The use of the oxides of platinum for the catalytic reduction of organic compounds. I. J. Am. Chem. Soc., 44:1397-405.

With F. L. Roman and W. N. Sperry. The structure of the compounds produced from olefins and mercury salts: mercurated dihydrobenzofurans. J. Am. Chem. Soc., 44:1781-92.

With W. D. Langley. The condensation of certain nitriles and various polyhydroxyphenols to form phenolic acids. J. Am. Chem. Soc., 44:2320-30.

With A. W. Ingersoll. Optically active dyes. I. J. Am. Chem. Soc., 44:2930-37.

1923 With J. E. Bullock and W. C. Wilson. Contribution to the structure of benzidine. J. Am. Chem. Soc., 45:521-27.

With W. C. Wilson. Rings through the *meta* and *para* positions of benzene. A study of certain ethers of resorcinol and *meta*-aminophenol. J. Am. Chem. Soc., 45:528-40.

With J. B. Segur. ß-Arylamino ethanols. J. Am. Chem. Soc., 45:785-90.

With J. S. Pierce. Tetrahydro-1,3,2-oxazones and substituted -amino propanols. J. Am. Chem. Soc., 45:790-95.

With W. H. Carothers. Platinum oxide as a catalyst in the reduction of organic compounds. II. Reduction of aldehydes. Activation of the catalyst by the salts of certain metals. J. Am. Chem. Soc., 45:1071-86.

With J. R. Johnson. Arsenated derivatives of phenyldiketopyrrolidine. J. Am. Chem. Soc., 45:1307-15.

With R. J. Kaufman. Production of imido thiol esters by the condensation of thiocyanates with resorcinol or phloroglucinol. J. Am. Chem. Soc., 45:1744-52.

With L. E. Mills. Mercurated 1-methyl-1,2-dihydrobenzofurans. J. Am. Chem. Soc., 45:1842-54.

With R. L. Shriner. Platinum oxide as a catalyst in the reduction of organic compounds. III. Preparation and properties of the oxide of platinum obtained by the fusion of chloroplatinic acid with sodium nitrate. J. Am. Chem. Soc., 45:2171-79.

With I. Levine. Simplification of the Gatterman synthesis of hydroxy aldehydes. J. Am. Chem. Soc., 45:2373-77.

With A. W. Sloan and B. S. Taylor. Aryl 1,3-benzodioxanes (aryl methylene-saligenins). J. Am. Chem. Soc., 45:2417-20.

With G. D. Graves. Trihydroxy-methyl-anthraquinones. I. J. Am. Chem. Soc., 45:2439-55.

With J. H. Gardner. Trihydroxy-methyl-anthraquinones. II. J. Am. Chem. Soc., 45:2455-62.

With C. G. Tomecko. The allyl ethers of various carbohydrates. J. Am. Chem. Soc., 45:2698-701.

With S. M. McElvain. Synthesis of a new bicyclic nitrogen ring. Isogranatanine derivatives. Preparation of an isomer of homococaine. J. Am. Chem. Soc., 45:2738-50.

With W. E. Kaufmann. The use of platinum oxide as a catalyst in the reduction of organic compounds. IV. Reduction of furfural and its derivatives. J. Am. Chem. Soc., 45:3029-44.

With C. W. Rodewald. Arsono-arylamino alcohols. J. Am. Chem. Soc., 45:3102-5.

1924 With R. A. Jacobson. Trihydroxy-methyl-anthraquinones. III. Synthesis of emodin. J. Am. Chem. Soc., 46:1312-16.

With Edna Montgomery. Simplification of the Gatterman synthesis of aromatic aldehydes. II. J. Am. Chem. Soc., 46:1518-21.

With W. H. Carothers. Platinum oxide as a catalyst in the reduction of organic compounds. V. The preparation of primary alcohols by the catalytic hydrogenation of aldehydes. J. Am. Chem. Soc., 46:1675-83.

With R. L. Shriner. The preparation of palladous oxide and its use as a catalyst in the reduction of organic compounds. VI. J. Am. Chem. Soc., 46:1683-93.

With C. R. Noller. The use of aliphatic acid anhydrides in the preparation of ketones by the Friedel and Crafts reaction. J. Am. Chem. Soc., 46:1889-96.

With W. W. Bauer. Diarsono-diphenyl and derivatives. J. Am. Chem. Soc., 46:1925-31.

With W. R. Brode. Optically active dyes. II. Adsorption, absorption spectra and rotation. J. Am. Chem. Soc., 46:2032-43.

With C. F. Rassweiler. The structure of dehydro-acetic acid. J. Am. Chem. Soc., 46:2758-64.

With I. A. Koten. Certain reactions of the alkyl and aryl mercuric hydroxides. J. Am. Chem. Soc., 46:2764-69.

With R. A. Jacobson. Polyhydroxy-methylanthraquinones. IV. Condensation of opianic acid with substituted phenols. Orientation in the preparation of anthraquinones. J. Am. Chem. Soc., 46:2788-93.

1925 With R. A. Jacobson. Trihydroxy-methyl-anthraquinones. V. Synthesis of morindone. J. Am. Chem. Soc., 47:283-90.

With Katharine Ogden. Arsonophenyl-cinchoninic acid and derivatives. J. Am. Chem. Soc., 47:826-30.

With W. H. Carothers. Platinum oxide as a catalyst in the reduction of organic compounds. VII. A study of the effects of numerous substances on the platinum catalysis of the reduction of benzaldehyde. J. Am. Chem. Soc., 47:1047-63.

With J. S. Pierce. Platinum oxide as a catalyst in the reduction of organic compounds. VIII. The reduction of alkyl furyl carbinols. J. Am. Chem. Soc., 47: 1098-104.

With J. W. Kern and R. L. Shriner. Platinum and palladium oxides as catalysts in the reduction of organic compounds. IX. The reduction of olefins. J. Am. Chem. Soc., 47:1147-58.

With H. Heckel. Platinum oxide as a catalyst in the reduction of organic compounds. X. Reduction of aminophenols to cyclic amino-alcohols. J. Am. Chem. Soc., 47:1712-18.

With R. A. Jacobson. Polyhydroxy and polyhydroxymethyl anthraquinones. VI. Syntheses from opianic acid and phenols or cresols. J. Am. Chem. Soc., 47:2011-18.

With C. L. Butler, Jr. Search in the diphenylmethane series for the isomerism characteristic of certain diphenyl derivatives. J. Am. Chem. Soc., 47:2610-20.

With R. L. Shriner. Structure of chaulmoogric and hydnocarpic acids. I. J. Am. Chem. Soc., 47:2727-39.

With H. O. Calvery and C. R. Noller. Arsonophenyl-cinchoninic acid (arsono-cinchopen) and derivatives. II. J. Am. Chem. Soc., 47:3058-60.

With W. F. Tuley. Reduction of cinnamic aldehyde to cinnamyl alcohol in the presence of platinum-oxide platinum black and promoters. XI. J. Am. Chem. Soc., 47:3061-68.

1926 With G. S. Hiers. Über die Reduktion der *o*-Phenylen-essigpropionsäure und verschiedener aromatischer Amine mit Wasserstoff unter Verwendung von Platinoxyd-Platinschwarz als Katalysator. XII. Ber. Dtsch. Chem. Ges., 59:162-70.

With B. S. Garvey. Selective reduction of citral by means of platinum-oxide platinum black and a promoter. XIII. J. Am. Chem. Soc., 48:477-82.

With J. V. Supniewski. Organic bismuth compounds. I. Preparation of tri-carboxy-triphenylbismuth dichlorides and certain nitrotriaryl bismuth compounds. J. Am. Chem. Soc., 48:507-17.

With C. R. Noller. The preparation and use of aldehyde esters formed by ozonation of the methyl esters of various unsaturated acids. J. Am. Chem. Soc., 48:1074-80.

With C. R. Noller. Synthesis of dihydrochaulmoogric and dihydrohydnocarpic acids. II. J. Am. Chem. Soc., 48:1080-89.

With G. S. Hiers. The synthesis of homologs of dihydrochaulmoogric and dihydrohydnocarpic acids containing a cyclohexyl in place of a cyclopentyl group. III. J. Am. Chem. Soc., 48:1089-93.

With E. K. Rideal, W. B. Burnett, R. L. Jenkins, and E. E. Dreger. Chemical constitution, physiological action and physical properties in a series of alkyl *para*-aminobenzoates. J. Am. Chem. Soc., 48:1758-70.

With E. B. Vliet. The relationship between hydrogen-ion concentration and chemical constitution in certain local anesthetics. J. Am. Chem. Soc., 48:2158-62.

With W. R. Brode. Optically active dyes. III. Physical properties, dyeing reactions and mechanism of dyeing. J. Am. Chem. Soc., 48:2193-201.

With W. R. Brode. Optically active dyes. IV. Asymmetric dyes from meta-aminomandelic acid. J. Am. Chem. Soc., 48:2202-6.

With G. S. Hiers. w-Cyclohexyl derivatives of various normal aliphatic acids. IV. J. Am. Chem. Soc., 48:2385-93.

With R. H. VanDyke. Synthesis of chaulmoogrylacetic acid. V. J. Am. Chem. Soc., 48:2393-95.

With J. Sacks. The synthesis of homochaulmoogric acid, homohydnocarpic acid and chaulmoogrylamines. VI. J. Am. Chem. Soc., 48:2395-99.

With C. R. Noller. Synthesis of a homolog of chaulmoogric acid. A2-Cyclopentenylacetic acid. VII. J. Am. Chem. Soc., 48: 2444-48.

1927 With S. V. Puntambeker. Polyhydroxy-anthraquinones. VII. Structure and synthesis of hydroxyanthrarufin and of rufiopin. J. Am. Chem. Soc., 49:486-91.

With C. G. Tomecko. The synthesis of 9-, 10-, 11-, 12-, and 13-hydroxystearic acids. J. Am. Chem. Soc., 49:522-30.

With F. L. Cohen and O. W. Rees. Reduction of aromatic nitro compounds to amines with hydrogen and platinum-oxide platinum black as a catalyst. XIV. J. Am. Chem. Soc., 49:1093-99.

With G. S. Hiers. Catalytic reduction of di- and tri-phenylamines with hydrogen and platinum-oxide platinum black. XV. J. Am. Chem. Soc., 49:1099-103.

With H. Heckel. Cyclic alkamine esters of *para*-aminobenzoic acid. II. J. Am. Chem. Soc., 49, 1303-7.

With O. A. Barnes. Piperidyl and substituted piperidyl alkyl *para*-aminobenzoates. III. J. Am. Chem. Soc., 49:1307-15.

With R. H. Talbot. Alicyclic derivatives of resorcinol. J. Am. Chem. Soc., 49:2040-42.

With Florence D. Stouder. Polyhydroxy-methylanthraquinones. IX. Contribution to the structure of rubiadin. J. Am. Chem. Soc., 49:2043-45.

With R. H. Bray. Selective reduction of furfuracrolein by means of platinum-oxide platinum black and hydrogen. XVI. J. Am. Chem. Soc., 49:2101-6.

With M. M. Brubaker. The structure of the condensation products of ortho-phthalaldehydic acids with phenols and phenol esters. VIII. J. Am. Chem. Soc., 49:2279-96.

With W. M. Stanley, S. G. Ford, and W. R. Peterson. Various -cyclohexylalkyl alkyl acetic acids and their action toward *B. Leprae.* VIII. J. Am. Chem. Soc., 49:2934-40.

With J. A. Arvin. Certain 2-cyclopentenyl alkyl acetic acids and their action toward *B. Leprae.* IX. J. Am. Chem. Soc., 49: 2940-42.

1928 With W. M. Stanley and H. A. Stearns. Cyclohexyl and cyclohexylmethyl alkyl acetic acids and their action toward *B. Leprae.* X. J. Am. Chem. Soc., 50:1475-78.

With G. R. Yohe. Cyclopentyl alkyl acetic acids and o-cyclopentylethyl alkyl acetic acids and their bactericidal action toward *B. Leprae.* XI. J. Am. Chem. Soc., 50:1503-8.

With Letha A. Davies. The structures of convolvulinolic and jalapinolic acids. Synthesis of 1 -hydroxypentadecanoic and 11-hydroxyhexadecanoic acids. J. Am. Chem. Soc., 50:1749-55.

With J. A. Arvin. ^{2}-Cyclopentenylethyl alkyl acetic acids and their bactericidal action toward *B. Leprae.* XII. J. Am. Chem. Soc., 50:1790-94.

With J. R. Marshall. The use of platinum-oxide platinum black in

the catalytic reduction of aromatic hydrocarbons. XVII. J. Am. Chem. Soc., 50:1970-73.

With J. A. Arvin. Cyclopropylmethyl alkyl acetic acids and their bactericidal action toward *B. Leprae.* XIII. J. Am. Chem. Soc., 50:1983-85.

With T. S. Hamilton. Reduction of pyridine hydrochloride and pyridonium salts by means of hydrogen and platinum-oxide platinum black. XVIII. J. Am. Chem. Soc., 50:2260-63.

With J. F. Hyde and E. Browning. Synthetic homologs of *d,* -ephedrine. J. Am. Chem. Soc., 50:2287-92.

With Letha A. Davies. Di-(cyclohexylalkyl) acetic acids. XIV. J. Am. Chem. Soc., 50:2297-98.

With J. F. Hyde. Study of the possible isomerism of certain analogs of resolvable diphenyl compounds. J. Am. Chem. Soc., 50: 2499-506.

1929 With W. H. Lycan. w-Hydroxy aliphatic acids. Synthesis of sabinic acid. J. Am. Chem. Soc., 51:625-29.

With W. W. Moyer. Stereoisomerism of diphenyl compounds. Resolution of 3,3'-diaminodimesityl. II. J. Am. Chem. Soc., 51:630-38.

With W. M. Stanley. Stereochemistry of diphenyl compounds. The resolution of 2,2'-dihydroxy-3,3'-dicarboxy-l,l'-dinaphthyl. III. Rec. Trav. Chim. Pays-Bas, 48:1035-40.

With W. M. Stanley and Marian S. Jay. The preparation of certain octadecanoic acids and their bactericidal action toward *B. Leprae.* XV. J. Am. Chem. Soc., 51:1261-66.

With W. M. Stanley. The synthesis of chaulmoogric acid from hydnocarpic acid. J. Am. Chem. Soc., 51:1515-18.

With W. H. Lycan. o-Hydroxy aliphatic acids. II. Conversion of w-hydroxydecanoic acid to chain poly-intermolecular esters and to a dimeric cyclic ester. J. Am. Chem. Soc., 51:3450-64.

1930 With E. W. Bousquet. Substituted phenylethylbarbituric acids. J. Am. Chem. Soc., 52:224-29.

With W. M. Stanley. The stereoisomerism of diphenyl compounds. IV. J. Am. Chem. Soc., 52:1200-1205.

With S. G. Ford. Cyclobutylalkyl alkyl acetic acids and their bactericidal action toward *B. Leprae.* XVI. J. Am. Chem. Soc., 52: 1259-61.

With E. Browning and H. W. Woodrow. Preparation and bacteriological action toward *B. Leprae* of certain olefinic acids. XVII. J. Am. Chem. Soc. , 52:1281-83.

With B. F. Armendt. Certain dialkyl acetic acids containing 12, 13, and 14 carbon atoms and their bactericidal action toward *B. Leprae.* XVIII. J. Am. Chem. Soc., 52:1289-91.

With L. H. Bock and W. W. Moyer. Stereochemistry of diphenyl compounds. V. Preparation and resolution of 2,4,6,2',4',6'hexanitro-3,3'-dicarboxydiphenyl. J. Am. Chem. Soc., 52: 2054-60.

With H. A. Stearns. Stereochemistry of diphenyl compounds. VI. Preparation and resolution of 2,4,6,2',4'-pentanitro-3-carboxydiphenyl. J. Am. Chem. Soc., 52:2070-75.

With C. M. Greer. Preparation and bactericidal properties of certain pentadecanoic, heptadecanoic and nonadecanoic acids. XIX. J. Am. Chem. Soc., 52:2540-43.

With R. W. Maxwell. Study of possible isomerism of certain analogs of resolvable diphenyl compounds. VII. J. Am. Chem. Soc., 52:2959-72.

With E. Browning. Stereochemistry of diphenylbenzenes. Preparation of stereoisomeric 3,6-di-(2,4-dimethylphenyl)-2,5-dibromohydroquinones and their derivatives. IX. J. Am. Chem. Soc., 52:4098-107.

With W. M. Stanley. The stereochemical study of diphenyl-2,2'disulfonic acid. VIII. J. Am. Chem. Soc., 52:4471-76.

With Catherine C. Steele. Stereochemistry of phenylpyridine compounds: the preparation and investigation of 2-(2-carboxy-6-chlorophenyl)-pyridine-3-carboxylic acid and 3-(2-carboxyphenyl)-6-phenylpyridine-2,4-dicarboxylic acid. X. J. Am. Chem. Soc., 52:4528-35.

With J. B. Hale and W. H. Lycan. Synthesis of nervonic acid. J. Am. Chem. Soc., 52:4536-39.

With R. W. Stoughton. Stereochemistry of diphenyl compounds. The preparation and resolution of 2-methyl-6-nitro-2'-carboxydiphenyl. XI. J. Am. Chem. Soc., 52:5263-67.

1931 With P. R. Shildneck. Stereochemistry of diphenylbenzenes. Meso and racemic 2,5-di-(3-bromo-2,4,6-trimethylphenyl)-3,6-dibromohydroquinones and the corresponding quinones. XII. J. Am. Chem. Soc., 53:343-52.

With L. H. Bock. The stereochemistry of N-phenylpyrroles. The preparation and resolution of N-2-carboxyphenyl-2,5-dimethyl-3-carboxypyrrole. XIII. J. Am. Chem. Soc., 53:374-76.

With E. C. Kleiderer. Stereochemistry of diphenyl compounds. The preparation and resolution of 3,5,3',5',-tetra-methyl2,2'-difluoro-6,6'-diaminodiphenyl. XIV. J. Am. Chem. Soc., 53:1575-80.

With P. R. Shildneck. Stereochemistry of diphenylbenzenes. The *cis* and *trans* forms of 2,5-di-(3-bromo-2,4,6-trimethyl-phenyl)- 1,3, 4,6-tetrahydroxybenzenes and the corresponding acylates. XV. J. Am. Chem. Soc. , 53:2203-14.

With Chin Chang. Stereochemistry of N,N'-dipyrryls. Resolution of N,N',2,5,2',5'-tetramethyl-3,3'-dicarboxy-dipyrryl. XVI. J. Am. Chem. Soc., 53:2353-57.

With W. M. Stanley. Stereochemistry of diphenyls. Preparation and properties of 4,4'-dicarboxy- 1, l'-dianthraquinoyl. XVII. J. Am. Chem. Soc., 53:2364-68.

With P. R. Shildneck. The synthesis of polyporic acid and atromentin dimethyl ether. J. Am. Chem. Soc., 53:2373-79.

With D. W. Hill. Stereochemistry of phenylquinones. The preparation and resolution of 2-(3-bromo-2,4,6-trimethylphenyl)-5methylbenzoquinone-3,6-di-(acetic acid). XVIII. J. Am. Chem. Soc., 53:3453-61.

With L. J. Roll. The structure of enol-acetates and the corresponding vinyl-amines. J. Am. Chem. Soc., 53:3469-76.

With L. H. Bock. Stereochemistry of phenyl pyrroles. XIX. J. Am. Chem. Soc., 53:3519-22.

1932 With W. M. Stanley. The surface tension of various aliphatic acids previously studied for bactericidal action to *Mycobacterium leprae.* XX. J. Am. Chem. Soc., 54:1548-57.

With E. H. Woodruff. Stereochemistry of dipyridyls. Preparation and resolution of 2,4,2',4',-tetracarboxy-6,6'-diphenyl-3,3'dipyridyl. XX. J. Am. Chem. Soc., 54:1977-82.

With Julius White. Stereochemistry of diphenyls. XXI. Resolution of 2,4,6,2',4',6'-hexachloro-3,3'-dicarboxydiphenyl. J. Am. Chem. Soc., 54:2104-8.

With W. M. Stanley, G. H. Coleman, C. M. Greer, and J. Sacks. Bacteriological action of certain synthetic organic acids toward *Mycobacterium leprae* and other acid-fast bacteria. XXI. J. Pharmacol. Exp. Ther., 45:121-62.

With L. J. Roll. The stereochemistry of carbodiimides. XXII. J. Am. Chem. Soc., 54:2494-98.

With H. C. Yuan. Stereochemistry of diphenyl. XXIII. Optically active 2,5-dimethoxy-2'-nitro-6'-carboxydiphenyl and the mutarotation of its salts. J. Am. Chem. Soc., 54:2966-73.

With B. C. Becker. Stereochemistry of diphenyls. XXIV. Preparation and properties of 2,2'-difluoro-3,3'-dicarboxy-6,6'-dimethoxydiphenyl. J. Am. Chem. Soc., 54:2973-82.

With E. H. Johnson and V. Weinmayr. Substitution products of *alpha* -naphthoyl-*ortho*-benzoic acid. J. Am. Chem. Soc., 54: 3289-95.

With R. W. Stoughton. Stereochemistry of diphenyls. XXV. The relative interfering effects of the groups F, OCH_3, Cl, Br as determined by the relative rates of racemization of the 2'-substituted 2-nitro-6-carboxydiphenyls. J. Am. Chem. Soc., 54:4426-34.

With H. C. Yuan. Stereochemistry of diphenyls. XXVI. The effect of substitution on the rate of racemization of certain optically active diphenyls. J. Am. Chem. Soc., 54:4434-43.

1933 With W. M. Stanley and Edward McMahon. Stereochemistry of diphenyls. XXVII. Comparison of the racemization of 2,2'-difluoro-6,6'-dicarboxydiphenyl and 2,2'-dimethoxy-6,6'dicarboxydiphenyl. J. Am. Chem. Soc., 55:706-16.

With E. C. Kleiderer. Stereochemistry of diphenyl. XXVIII. Preparation and properties of 2,2'-difluoro-3,3'-dicarboxy-5,5'dimethyl-6,6'-dinitrodiphenyl. J. Am. Chem. Soc., 55:716-19.

With W. I. Patterson. Stereochemistry of N-phenylpyrroles. XXIX. Preparation and properties of o-N-carbazyl- and o-N-(3nitrocarbazyl)-benzoic acid. J. Am. Chem. Soc., 55:1069-72.

With N. E. Searle. Stereochemistry of diphenyls. XXX. Preparation and resolution of 2,2'-diiodo-4,4'-dicarboxydiphenyl. J. Am. Chem. Soc., 55:1649-54.

With H. C. Yuan. The stereochemistry of diphenyls and analogous compounds. Chem. Rev., 12:261-338.

With E. C. Kleiderer. Stereochemistry of diphenyls. XXXI. Preparation and properties of 2,2'-,6,6'-tetrafluoro-3,3'-dicarboxy5,5'-dichlorodiphenyl. J. Am. Chem. Soc., 55:4219-25.

With A. M. VanArendonk and M. E. Cupery. Stereochemistry of diphenyls. XXXII. Preparation and properties of certain 2,2'-, 6,6'-tetramethoxydiphenyls. J. Am. Chem. Soc., 55:4225-30.

With A. M. VanArendonk and B. C. Becker. Stereochemistry of diphenyls. XXXIII. Preparation and properties of 2,3'-dinitro-6-carboxy-2',6'-dimethoxydiphenyl and 2-nitro-6-carboxy-2'fluoro-6'-methoxydiphenyl. J. Am. Chem. Soc., 55:4230-34.

With Marion E. Maclean. Diastereoisomers of 2,5-diphenylsulfinylhydroquinone diacetate. J. Am. Chem. Soc., 55:4681-83.

With Marion E. Maclean. Study of certain analogs of resolvable diphenyls. J. Am. Chem. Soc., 55:4683-88.

With A. E. Knauf. Stereochemistry ofdiphenyls. XXXIV. Preparation and properties of 2,2',4,4',5,5',6,6'-octamethyldiphenyl3,3'-disulfonic acid and diphenyl-3,3'-disulfonic acid. J. Am. Chem. Soc., 55:4704-9.

1934 With S. L. Chien. Stereochemistry of diphenyls. XXXV. The effect of 3'-substituents on the rate of racemization of 2-nitro-6-carboxy-2'-methoxydiphenyl. J. Am. Chem. Soc., 56:1787-92.

With Chin Chang. Stereochemistry of diphenyls. XXXVI. Preparation and resolution of certain substituted dipyrrylbenzenes. J. Am. Chem. Soc., 56:2089-92.

With D. F. Holmes. The use of l-menthoxyacetyl chloride for the resolution of amino acids. J. Am. Chem. Soc., 56:2093-94.

With A. E. Knauf and P. R. Shildneck. Stereochemistry of diphenyls. XXXVII. The resolution of certain substituted diphenylbenzenes. J. Am. Chem. Soc., 56:2109-11.

With N. E. Searle. Stereochemistry of diphenyls. XXXVIII. Resolution of 2,2'-dibromo-4,4'-dicarboxydiphenyl. J. Am. Chem. Soc., 56:2112-14.

With W. E. Hanford and Poe Liang. The constitution of vasicine. J. Am. Chem. Soc., 56:2780-83.

1935 With Q. R. Bartz and R. F. Miller. The introduction of isobutyl groups into phenols, cresols and homologous compounds. J. Am. Chem. Soc., 57:371-76.

With W. I. Patterson. Stereochemistry of diphenyls. XXXIX. Synthesis of active 2,6-dibromo-3,3'-diamino-4,4'-ditolyl. J. Am. Chem. Soc., 57:762-64.

With W. E. Hanford. The structure of vasicine. II. Synthesis of desoxyvasicine. J. Am. Chem. Soc., 57:921-24.

With R. C. Morris and W. E. Hanford. The structure of vasicine. III. Position of the hydroxyl group. J. Am. Chem. Soc., 57:951-54.

With C. C. Li. Stereochemistry of diphenyls. XL. The effect of temperature and solvent on the rate of racemization of 2-nitro-6-carboxy-2'-alkoxydiphenyls. J. Am. Chem. Soc., 57:1565-69.

With W. E. Hanford. Stereochemistry of diphenyls. XLI. The effect of 4'-substitution on the rate of racemization of 2-nitro-6-carboxy-2'-methoxydiphenyl. J. Am. Chem. Soc., 57:1592-95.

With E. E. Gruber. Synthesis of certain hydrogenated phenanthrenes. J. Am. Chem. Soc., 57:2555-56.

1936 With C. Y. Hsing. Relative rates of racemization of substituted diamides of 2,2'-dimethoxy-6,6'-dicarboxydiphenyl. XLII. J. Am. Chem. Soc., 58:587-89.

With R. F. Miller. Contribution to the multiplanar isomerism of cyclohexanes. J. Am. Chem. Soc., 58:787-91.

With A. McLean. Succinic -d_2, x'-d_2 acid and its derivatives. J. Am. Chem. Soc., 58:804-10.

With M. T. Leffler. Succinic- -*d*, -'-*d* acid and its derivatives. II. Stereochemistry of the type RR'CHD. J. Am. Chem. Soc., 58:1551-55.

With M. T. Leffler. Stereochemistry of deuterium compounds of the type RR'CHD: camphane-2,3-d_2. J. Am. Chem. Soc., 58:1555-58.

1937 With C. R. Kinney. Dideuteriovaline and dideuterioleucine. J. Am. Chem. Soc., 59:897-98.

With F. C. McGrew. Stereochemistry of deuterium compounds of the type RR'CXHXD: Ethyl-d_4-ethylcarbinol. J. Am. Chem. Soc., 59:1497-1500.

With K. N. Campbell and R. C. Morris. The structure of gossypol. I. J. Am. Chem. Soc., 59:1723-28.

With R. F. Miller and D. J. Butterbaugh. Structure of gossypol. II.. Acylation. J. Am. Chem. Soc., 59:1729-31.

With R. C. Morris. The structure of gossypol. III. Gossypol ethers. J. Am. Chem. Soc., 59:1731-35.

With R. F. Miller. Structure of gossypol. IV. Anhydrogossypol and its derivatives. J. Am. Chem. Soc., 59:1736-38.

With W. B. Burnett, R. L. Jenkins, C. H. Peet, and E. E. Dreger. Dialkylamino-alkanol esters of *p*-aminobenzoic acid. J. Am. Chem. Soc., 59:2248-52.

With M. T. Leffler. Aminophenyl-2-oxazolines as local anesthetics. J. Am. Chem. Soc., 59:2252-58.

With A. Novelli. Aminophenyl-2-pentoxazolines. J. Am. Chem. Soc., 59:2259-60.

With S. H. Babcock. Aminophenyl thiazolines and thiazines. J. Am. Chem. Soc., 59:2260-61.

With B. S. Friedman and Meredith Sparks. Aminophenyl oxazoles and thiazoles. J. Am. Chem. Soc., 59:2262-64.

With Allene Jeanes. The addition of alkali metals to phenanthrene. J. Am. Chem. Soc., 59:2608-22.

1938 With D. S. Tarbell. The attempted resolution of phenyl-$d_5$phenylaminomethane. J. Am. Chem. Soc., 60:1260-62.

With H. R. Snyder. Stereochemistry of biphenyls. XLIII. The effect of substitutents in the 4-position of 2-nitro-6-carboxy-2'methoxybiphenyl. J. Am. Chem. Soc., 60:1411-15.

With R. M. Joyce, Jr. Stereochemistry of biphenyls. XLIV. *Meso* and racemic isophthalamides of 3-nitro-3'-aminobimesityl. J. Am. Chem. Soc., 60:1489-90.

With R. M. Joyce, Jr. Stereochemistry of biphenyls. XLV. Stereoisomeric dipyrryl biphenyls. J. Am. Chem. Soc., 60:1491-92.

With C. C. Price and W. R. Dial. Structure of gossypol. V. Anilino derivatives. J. Am. Chem. Soc., 60:2158-60.

With B. S. Friedman, C. C. Price, R. C. Morris, and E. C. Kirkpatrick. Structure of gossypol. VI. Addition products with butadienes. J. Am. Chem. Soc., 60:2160-62.

With T. A. Geissman. Structure of gossypol. VII. Gossypol dimethyl ether. J. Am. Chem. Soc., 60:2163-66.

With T. A. Geissman. Structure of gossypol. VIII. Derivatives of the ethers of gossypol. J. Am. Chem. Soc., 60:2166-70.

With R. C. Morris and E. C. Kirkpatrick. Structure of gossypol. IX. Oxidation and degradation of gossypol hexamethyl ether; gossic acid. J. Am. Chem. Soc., 60:2170-74.

With D. J. Butterbaugh. Structure of gossypol. X. Apogossypol and its degradation products. J. Am. Chem. Soc., 60:2174-80.

With E. C. Kirkpatrick. Structure of gossypol. XI. Absorption spectra of gossypol, its derivatives and of certain dinaphthalene compounds. J. Am. Chem. Soc., 60:2180-84.

With T. A. Geissman. The structure of gossypol. XII. Gossylic acid lactone tetramethyl ether. J. Am. Chem. Soc., 60:2184-88.

With R. C. Morris. Structure of gossypol. XIII. Conversion of gossic acid to apogossypolic acid. J. Am. Chem. Soc., 60:2188-90.

With R. C. Morris, D. J. Butterbaugh, and E. C. Kirkpatrick. Structure of gossypol. XIV. Apogossypolic acid. J. Am. Chem. Soc., 60:2191-93.

With R. C. Morris, T. A. Geissman, D. J. Butterbaugh, and E. C. Kirkpatrick. Structure of gossypol. XV. An interpretation of its reactions. J. Am. Chem. Soc., 60:2193-204.

With E. E. Gruber. Lactone formation in the addition product of maleic anhydride and bicyclohexenyl. J. Am. Chem. Soc., 60: 2792-94.

With T. A. Geissman and R. C. Morris. Structure of gossypol. XVI. Reduction products of gossypolone tetramethyl ether and gossypolonic acid tetramethyl ether. J. Am. Chem. Soc., 60: 2967-70.

With T. A. Geissman and R. C. Morris. Structure of gossypol. XVII.

Nitration products of gossypol hexamethyl ether, gossypolone tetramethyl ether and gossypolonic acid tetramethyl ether. J. Am. Chem. Soc., 60:2970-72.

With Madison Hunt and R. C. Morris. Structure of gossypol. XVIII. Synthesis of 1,2-dimethoxy-3-isopropyl-5-aminobenzene, a degradation product of gossypol. J. Am. Chem. Soc., 60:2972-74.

1939 With Madison Hunt. Structure of gossypol. XIX. Synthesis of 1,2-dihydroxy-3-isopropyl-6-benzoic acid. J. Am. Chem. Soc., 61:1132-33.

With Madison Hunt and B. R. Baker. Structure of gossypol. XX. Synthesis of 1,2-dihydroxy-3-isopropyl-5-benzoic acid. J. Am. Chem. Soc., 61:1134-37.

With B. R. Baker. Structure of gossypol. XXI. Synthesis of 1,2-dimethoxy-3-isopropyl-4-benzoic acid and of apogossypolic acid. J. Am. Chem. Soc., 61:1138-42.

With W. R. Dial. Structure of gossypol. XXII. Gossypol ethers and their reduction products. J. Am. Chem. Soc., 61:2077-82.

With T. A. Geissman. Structure of gossypol. XXIII. Attempts to prepare desapogossypolone tetramethyl ether. Condensation of hexadiene-2,4 with dibenzoylethylene. J. Am. Chem. Soc., 61:2083-89.

With T. L. Cairns. Stereochemistry of biphenyls. XLVI. 2-Substituted biphenyls. J. Am. Chem. Soc., 61:2179-81.

With G. C. Finger. Stereochemistry of biphenyls. XLVII. Certain 3,6-di-(2,4-dimethylphenyl)-hydroquinones and their derivatives. J. Am. Chem. Soc., 61:2182-83.

With P. H. Long and A. J. Johanson. Sulfanilamide derivatives. I. J. Am. Chem. Soc., 61:2342-46.

With P. H. Long and Allene Jeanes. Sulfanilamide derivatives. II. J. Am. Chem. Soc., 61:2346-49.

With T. L. Cairns. Attempts to prepare optically active ethyleneimine derivatives containing an asymmetric nitrogen atom. J. Am. Chem. Soc., 61:2464-67.

With E. F. Rogers. The structure of monocrotaline, the alkaloid in *Crotalaria spectabilis* and *Crotalaria retusa. I.* J. Am. Chem. Soc., 61:2815-19.

With E. F. Rogers and F. J. Sprules. Structure of monocrotaline. II. Monocrotic acid obtained by alkaline hydrolysis of the alkaloid. J. Am. Chem. Soc., 61:2819-21.

With E. F. Rogers and R. S. Long. The structure of monocrotaline. III. Monocrotalic acid. J. Am. Chem. Soc., 61:2822-24.

With J. B. Hale. Stereochemistry of biphenyls. XLVIII. A comparison of the racemization rates of three isomeric 2,2',6-nitro-, carboxy-, methyl-biphenyls. J. Am. Chem. Soc., 61:2825-28.

With G. C. Finger. Stereochemistry of biphenyls. XLIX. Comparison of the racemization rates of the 2,2',6-nitro-, carboxy-, and methoxy-biphenyls. J. Am. Chem. Soc., 61:2828-30.

1940 With M. W. Miller. Restricted rotation in aryl olefins. I. Preparation and resolution of *ß*-chloro-*ß*-(2,4,6-trimethyl-3-bromophenyl)a-methylacrylic acid. J. Am. Chem. Soc., 62:53-56.

With Marvin H. Gold. The synthesis of 1,3-diphenyldihydroisobenzofurans, 1,3-diphenylisobenzofurans and o-dibenzoylbenzenes from the diene addition products to dibenzoylethylene. J. Am. Chem. Soc., 62:56-61.

With Madison Hunt and J. H. Clark. Structure of cannabidiol, a product isolated from the marihuana extract of Minnesota wild hemp. I. J. Am. Chem. Soc., 62:196-200.

With C. K. Cain and Hans Wolff. Structure of cannabidiol. II. Absorption spectra compared with those of various dihydric phenols. J. Am. Chem. Soc., 62:732-34.

With Madison Hunt and J. H. Clark. Structure of cannabidiol. III. Reduction and cleavage. J. Am. Chem. Soc., 62:735-37.

With R. B. Wearn. Diene addition products to diaroylethylenes and their transformation products. II. J. Am. Chem. Soc., 62: 1233-37.

With Hans Wolff, C. K. Cain, and J. H. Clark. Structure of cannabidiol. IV. The position of the linkage between the two rings. J. Am. Chem. Soc., 62:1770-75.

With M. H. Gold. Absorption and fluorescence spectra of certain dihydroisobenzofurans and isobenzofurans. J. Am. Chem. Soc., 62:2038-42.

With H. M. Teeter. Stereochemistry of biphenyls. L. Comparison of the interference of a methoxyl and an hydroxyl group. J. Am. Chem. Soc., 62:2188-90.

With L. J. Dankert. Restricted rotation in arylamines. I. Preparation and resolution of N-succinyl-N-methylbromomesidine. J. Am. Chem. Soc., 62:2191-93.

With D. C. Pease and J. H. Clark. Isolation of cannabinol, cannabidiol and quebrachitol from red oil of Minnesota wild hemp. J. Am. Chem. Soc., 62:2194-96.

With D. C. Pease, J. H. Clark, and B. R. Baker. Structure of cannabinol. I. Preparation of an isomer, 3-hydroxy-l-n-amyl-6,6,9trimethyl-6-dibenzopyran. J. Am. Chem. Soc., 62:2197-200.

With C. K. Cain and B. R. Baker. Structure of cannabinol. II. Synthesis of two new isomers, 3-hydroxy-4-n-amyl and 3-hydroxy-2-n-amyl-6,6,9-trimethyl-6-dibenzopyrans. J. Am. Chem. Soc., 62:2201-4.

With B. R. Baker and R. B. Wearn. Structure of cannabinol. III. Synthesis of cannabinol, I-hydroxy-3-n-amyl-6,6,9-trimethyl-6dibenzopyran. J. Am. Chem. Soc., 62:2204-7.

With B. R. Baker. Structure of cannabinol. IV. Synthesis of two additional isomers containing a resorcinol residue. J. Am. Chem. Soc., 62:2208-15.

With Hans Wolff, C. K. Cain, and J. H. Clark. Structure of cannabidiol. V. Position of the alicyclic double bonds. J. Am. Chem. Soc., 62:2215-19.

With D. C. Pease, C. K. Cain, B. R. Baker, J. H. Clark, Hans Wolff, and R. B. Wearn. Conversion of cannabidiol to a product with marihuana activity. A type reaction for synthesis of analogous substances. Conversion of cannabidiol to cannabinol. J. Am. Chem. Soc., 62:2245-46.

With R. S. Long. Structure of monocrotaline. IV. Monocrotalic acid. J. Am. Chem. Soc., 62:2289-94.

With B. R. Baker. Structure of cannabinol. V. A second method of synthesis of cannabinol. J. Am. Chem. Soc., 62:2401.

With D. C. Pease, C. K. Cain, and J. H. Clark. Structure of cannabidiol. VI. Isomerization of cannabidiol to tetrahydrocannabinol, a physiologically active product. Conversion of cannabidiol to cannabinol. J. Am. Chem. Soc., 62:2402-5.

With B. R. Baker. Structure of cannabidiol. VII. A method of synthesis of a tetrahydrocannabinol which possesses marihuana activity. J. Am. Chem. Soc., 62:2405-8.

With S. Loewe, D. C. Pease, C. K. Cain, R. B. Wearn, B. R. Baker, and Hans Wolff. Structure of cannabidiol. VIII. Position of the double bonds in cannabidiol. Marihuana activity of tetrahydrocannabinols. J. Am. Chem. Soc., 62:2566-67.

1941 With N. Kornblum. Stereochemistry of biphenyls. LI. Resolution of diphenic acids having many-membered bridges across the 5,5'-positions: a novel type of restricted rotation. J. Am. Chem. Soc., 63:188-200.

With E. F. Rogers. Structure of monocrotaline. V. Retronecine, a derivative of 1-methylpyrrolizidine. J. Am. Chem. Soc., 63: 228-36.

With T. A. Geissman, B. R. Baker, and H. M. Teeter. Structure of gossypol. XXIV. Attempts to prepare desapogossypolone tetramethyl ether. J. Am. Chem. Soc., 63:528-34.

With B. R. Baker. Structure of gossypol. XXV. Synthesis of desapogossylpolone tetramethyl ether. J. Am. Chem. Soc., 63:535-37.

With E. F. Rogers. Structure of monocrotaline. VI. The structure of retronecine, platynecine and retronecanol. J. Am. Chem. Soc., 63:537-41.

With D. E. Burney. The reactions of 2,8-dihydroxynaphthaldehyde. J. Am. Chem. Soc., 63:1103-7.

With A. W. Anderson and M. W. Miller. Restricted rotation in aryl olefins. II. Preparation and resolution of certain ß-chloro-ß-(2,4,6-trimethyl- and 2,4,6-triethyl-3-bromophenyl)acrylic acids. J. Am. Chem. Soc., 63:1589-93.

With S. Loewe, Charles Jelinek, and Hans Wolff. Tetrahydrocannabinol homologs with marihuana activity. IX. J. Am. Chem. Soc., 63:1971-73.

With C. M. Smith and S. Loewe. Tetrahydrocannabinol homologs and analogs with marihuana activity. X. J. Am. Chem. Soc., 63:1973-76.

With C. K. Cain and S. Loewe. Tetrahydrocannabinol analogs with marihuana activity. XI. J. Am. Chem. Soc., 63:1977-78.

With L. N. Whitehill. Many-membered ring compounds by direct synthesis from two w,w'-bifunctional molecules. J. Am. Chem. Soc., 63:2073-78.

With C. K. Cain, W. D. McPhee, and R. B. Wearn. Structure of cannabidiol. XII. Isomerization to tetrahydrocannabinols. J. Am. Chem. Soc., 63:2209-13.

With T. A. Geissman, W. R. Dial, and J. T. Fitzpatrick. Structure of gossypol. XXVI. Gossypolic acid. J. Am. Chem. Soc., 63:2439-41.

With L. O. Binder. Restricted rotation in aryl olefins. III. Preparation and resolution of *ß*-chloro-*ß*-(2-methyl-l-naphthyl)-acrylic acids. J. Am. Chem. Soc., 63:2773-76.

With H. W. Stewart. Restricted rotation in arylamines. II. Preparation and resolution of N-succinyl-N-ethyl-3-bromomesidine and 5-alkoxy-4-N-succinyl-4-alkylamino- 1,3-dimethylbenzenes . J. Am. Chem. Soc., 63:2859-64.

1942 Marihuana. In: *Harvey Lectures,* 37:168-97.

With M. Carmack and E. F. Rogers. The alkaloid of *Crotalaria grantiana.* I. Grantianine. J. Am. Chem. Soc., 64:571-73.

With S. Loewe, C. M. Smith, and W. D. McPhee. Tetrahydrocannabinol homologs and analogs with marihuana activity. XIII. J. Am. Chem. Soc., 64:694-97.

With A. A. Albert. Restricted rotation in aryl amines. III. Preparation and resolution of N-succinyl-1-methylamino-2-methylnaphthalene and N-succinyl-l -methylamino-4-chloro-2-methylnaphthalene. J. Am. Chem. Soc., 64:1475-78.

With W.J. Gross. Restricted rotation in aryl olefins. IV. Preparation and resolution of *ß*-chloro-*ß*-(2-methoxy-4,6-dimethyl-5-chlorophenyl)- -methylacrylic and the corresponding acrylic acid. J. Am. Chem. Soc., 64:1786-90.

With L. O. Binder and F. C. McGrew. Restricted rotation in aryl olefins. V. ß-Bromo-ß-(2-alkoxynaphthyl)-a-alkylacrylic acids. J. Am. Chem. Soc., 64:1791-94.

With M. W. Miller, F. C. McGrew, and A. W. Anderson. Restricted rotation in aryl olefins. VI. Substituted ß-(2,7-dimethoxy-1naphthyl)-a-methylacrylic acids. J. Am. Chem. Soc., 64: 1795-801.

With C. M. Smith and S. Loewe. Optically active synthetic tetrahydrocannabinols; *d*- and *1*-l-hydroxy-3-*n*-amyl-6,9,9-trimethyl7,8,9,10-tetrahydro-6-dibenzopyrans. XIV. J. Am. Chem. Soc., 64:2087-89.

With J. E. Mahan. Basicity studies of tertiary vinyl amines. J. Am. Chem. Soc., 64:2588-93.

With Marvin Carmack and J. E. Mahan. Structure of monocrotaline. VII. Structure of retronecine and related bases. J. Am. Chem. Soc., 64:2593-97.

With K. E. Hamlin, Jr. Structure of monocrotaline. VIII. The proof of primary and secondary hydroxyl groups in retronecine. J. Am. Chem. Soc., 64:2597-99.

With S. Loewe, C. W. Theobald, and C. M. Smith. Tetrahydrocannabinol analogs with marihuana activity. XV. J. Am. Chem. Soc., 64:2653-55.

With K. E. Hamlin, Jr., C. F. Jelinek, and R. F. Phillips. The structure of riddelliine, the alkaloid in *Senecio riddellii.* I. J. Am. Chem. Soc., 64:2760-63.

1943 With W. D. McPhee, R. B. Carlin, and Z. W. Wicks. The addition of dienes to coumarin and to certain substituted cinnamic acids. I. J. Am. Chem. Soc., 65:356-60.

With R. B. Carlin. The addition of dienes to certain di-o-methoxysubstituted cinnamic acids. II. J. Am. Chem. Soc., 65:360-63.

With J. E. Mahan. Structure of monocrotaline. IX. Proof of the position of the double bond in retronecine. J. Am. Chem. Soc., 65:2009-12.

With J. M. Wilkinson, Jr. Structure of monocrotaline. X. Monocrotalic acid. J. Am. Chem. Soc., 65:2203-8.

With C. W. Theobald. Restriction rotation in aryl olefins. VII. A new synthesis of hindered *ß*-substituted-*ß*-arylacrylic acids. J. Am. Chem. Soc., 65:2208-12.

With C. W. Theobald. Restricted rotation in aryl olefins. VIII. The synthesis and resolution of certain ß-substituted-ß-arylacrylic acids. J. Am. Chem. Soc., 65:2383-87.

1944 With J. H. Clark, Nathan Kornblum, and Hans Wolff. Substituted aminobenzofuroquinolines. J. Am. Chem. Soc., 66:22-26.

With N. J. Leonard. Structure of monocrotaline. XI. Proof of the structure of retronecine. J. Am. Chem. Soc., 66:257-63.

With J. W. Mecorney. The formation of a chromone by the von Pechmann condensation of ethyl acetoacetate with 4-chloro3,5-dimethylphenol. J. Am. Chem. Soc., 66:802-5.

With Z. W. Wicks. 2,3,5,8-Tetramethoxy-6,7-dimethyl-l-naphthaldehyde. J. Am. Chem. Soc., 66:1315-16.

1945 With S. J. Cristol, A. W. Anderson, and A. A. Albert. The structure of leucenol. I. J. Am. Chem. Soc., 67:89-92.

With R. S. Ludington. Restricted rotation in aryl olefins. IX. Effect of various substituents on the olefin grouping. J. Am. Chem. Soc., 67:794-97.

With J. W. Mecorney. Restricted rotation in aryl olefins. X. *β*-methyl*β*-arylacrylic acids. J. Am. Chem. Soc., 67:798-802.

With K. H. Chen and S. Loewe. Tetrahydrocannabinol homologs with a s-alkyl group in the 3-position. XVI. J. Am. Chem. Soc., 67:1534-37.

1947 With V. V. Jones. The structure of leucenol. II. J. Am. Chem. Soc., 69:1803-5.

With T. R. Govindachari. The structure of leucenol. III. Synthesis of 2,5-dihydroxypyridine (5-hydroxy-2-pyridone). J. Am. Chem. Soc., 69:1806-8.

With V. V. Jones and J. L. Johnson. The structure of leucenol. IV. J. Am. Chem. Soc., 69:1810-12.

1948 With B. F. Aycock, Jr. and S. Loewe. Tetrahydrocannabinol homologs. XVII. J. Am. Chem. Soc., 70:662-64.

With S. MacKenzie, Jr. and S. Loewe. Tetrahydrocannabinol homologs with doubly branched alkyl groups in the 3-position. XVIII. J. Am. Chem. Soc., 70:664-68.

With Jean Mathieu. A new synthesis of atranol (2,6-dihydroxy-4methylbenzaldehyde) and the corresponding cinnamic acid. J. Am. Chem. Soc., 70:2120-22.

With N. K. Sundholm. Restricted rotation in aryl amines. IV. Preparation and resolution of N-succinyl-1-methylamino-2,4-dimethyl-6-substituted benzenes. J. Am. Chem. Soc., 70:2667-73.

With R. G. Chase. Restricted rotation in substituted aromatic amines. V. Derivatives of N,N'-dimethyldiaminomesitylene. J. Am. Chem. Soc., 70:4202-4.

With J. J. Tjepkema. Restricted rotation in substituted aromatic amines. VI. Stereoisomers of N,N'-dialkyl-N,N'-dibenzenesulfonyl-diaminomesitylene. J. Am. Chem. Soc., 70:4204-13.

1949 Restricted rotation. Rec. Chem. Prog., 10:91-99.

With J. Hine and J. Campbell. Triarylpyridylmethanes. J. Am. Chem. Soc., 71:387-90.

With R. D. Lipscomb. Amine bisulfite addition products of aldehydes and ketones. I. J. Am. Chem. Soc., 71:519-22.

With J. D. Garber. Amine bisulfites. II. Their use as resolving agents for aldehydes and ketones. J. Am. Chem. Soc., 71:522-26.

With J. L. Johnson. Leucenol. VI. A total synthesis. J. Am. Chem. Soc. , 71:705-8.

With T. R. Govindachari. Senecio alkaloids: a- and ß-longilobine from *Senecio longilobus.* J. Am. Chem. Soc., 71:1180-86.

With A. W. Schrecker. Condensation reactions of N-substituted pyridones. J. Am. Chem. Soc., 71:1186-95.

With M. Rothstein. Restricted rotation in substituted aromatic amines. VII. Stereoisomers of N,N'-dimethyl-N,N'-diarylsulfonyldiamino mesitylenes. J. Am. Chem. Soc., 71:1620-23.

With M. Harfenist and S. Loewe. New analogs of tetrahydrocannabinol. XIX. J. Am. Chem. Soc., 71:1624-28.

With T. R. Govindachari. Senecio alkaloids: the isolation of senecionine from *Senecio cineraria* and some observations on the structure of senecionine. J. Am. Chem. Soc., 71:1953-56.

With T. R. Govindachari. Senecio alkaloids: the alkaloids of *Senecio douglasii, carthamoides, eremophilus, ampullaceus* and *parksii.* J. Am. Chem. Soc., 71:1956-60.

With W. Herz. Helenalin. I. Isolation and properties. J. Am. Chem. Soc., 71:2546-51.

With W. Herz. Helenalin. II. Helenalin oxide. J. Am. Chem. Soc., 71:2551-54.

With W. Herz. Helenalin. III. Reduction and dehydrogenation. J. Am. Chem. Soc., 71:2554-59.

With J. B. Campbell. 2-Triphenylmethylaminopyridine. J. Am. Chem. Soc., 71:3539-42.

With V. V. Jones. Addition and condensation reactions of 2-pyridone. J. Am. Chem. Soc., 71:3826-33.

1950 With J. B. Campbell. Restricted rotation in aryl amines. VIII. Ring nitrogen derivatives of diaminomesitylene. J. Am. Chem. Soc., 72:128-32.

With N. K. Nelson. Restricted rotation in aryl amines. IX. Diaminodurene derivatives. J. Am. Chem. Soc., 72:132-35.

With B. Englund. Restricted rotation in aryl amines. X. Stereoisomers of N,N',N''-trialkyl-N,N',N''-tribenzenesulfonyltriamino-mesitylenes. J. Am. Chem. Soc., 72:135-38.

With J. Campbell. Hindered tetraarylmethanes. J. Am. Chem. Soc., 72:153-55.

With W. Herz. Synthesis of viridifloric acid. J. Am. Chem. Soc., 72:155-57.

With T. R. Govindachari. Structure of monocrotaline. XII. Monocrotalic acid. J. Am. Chem. Soc., 72:158-63.

With W. M. Whaley. The amaroids of quassia. I. Quassin, isoquassin and neoquassin. J. Am. Chem. Soc., 72:375-79.

With J. Campbell. 2,4,5- and 2,4,7-trimethylquinolines. J. Am. Chem. Soc., 72:1021-22.

With F. L. Warren, M. Kropman, T. R. Govindachari, and J. H. Looker. The identity of ß-longilobine with retrorsine. J. Am. Chem. Soc., 72:1421-22.

With A. S. Nagarkatti. 2,4- and 4,6-dinitro- and diamino-*m*-xylenes. J. Am. Chem. Soc., 72:1831-32.

With J. R. Gordon. Restricted rotation in aryl amines. XI. Influence of groups decreasing the basicity of the nitrogen atom. J. Am. Chem. Soc., 72:2454-57.

With J. R. Gordon. Restricted rotation in aryl amines. XII. Resolution of certain N-benzenesulfonyl-N-carboxymethyl derivatives. J. Am. Chem. Soc., 72:2458-60.

With A. S. Nagarkatti. Quinone imides. I. *p*-Quinone disulfonimides. J. Am. Chem. Soc., 72:4601-6.

With A. S. Nagarkatti. Restricted rotation in aromatic amines. XIII. The effect of monosubstitution in the *ortho* position. J. Am. Chem. Soc., 72:4606-8.

With N. K. Nelson. Restricted rotation in aryl amines. XIV. Isopropyl derivatives of dibenzenesulfonamidomesitylene. J. Am. Chem. Soc., 72:5077-79.

With J. L. Johnson and B. Englund. Polymers of -acetamidoacrylic acid and its methyl ester. J. Am. Chem. Soc., 72:5080-82.

With J. L. Anderson. Quinone imides. II. *p*-Quinone diacyl and diaroylimides. J. Am. Chem. Soc., 72:5154-57.

1951 With R. A. Wankel. Quinone imides. III. 1,4-Naphthoquinone disulfonimides. J. Am. Chem. Soc., 73:131-34.

With J. H. Looker. The identity of -longilobine and seneciphylline. J. Am. Chem. Soc., 73:134-36.

With J. L. Anderson. The effect of -methyl substituents on the anhydridization of aliphatic dibasic acids. J. Am. Chem. Soc., 73:136-41.

With J. H. Looker. Quinone imides. IV. *p*-Quinone monosulfonimides. J. Am. Chem. Soc., 73:1145-49.

With K. R. Eilar. Quinone imides. V. Aluminum chloride-catalyzed arylations of *p*-quinone dibenzenesulfonimides. J. Am. Chem. Soc., 73:1149-52.

With C. R. Walter, Jr. Quinone imides. VI. Addition of dienes to *p*-quinone sulfonimides. J. Am. Chem. Soc., 73:1152-55.

With R. A. Wankel. Quinone imides. VII. 1,2- and 2,6-naphthoquinone disulfonimides. J. Am. Chem. Soc., 73:2219-20.

With C. N. Winnick. Quinone imides. VIII. Synthesis and reactions of *o*-quinone diimides. J. Am. Chem. Soc., 73:5687-91.

1952 With F. B. Hauserman. The total structure of monocrotaline. XIII. Synthesis of dihydroanhydromonocrotalic acid. J. Am. Chem. Soc., 74:694-99.

With T. R. Govindachari, J. H. Looker, and J. D. Edwards, Jr. Senecio alkaloids: -longilobine; structure of -longinecic acid. J. Am. Chem. Soc., 74:700-703.

With W. Moje. Quinone imides. IX. Addition of dienes to 1,4-naphthoquinonedibenzenesulfonimide. J. Am. Chem. Soc., 74:2593-96.

With K. A. Schowalter. Quinone imides. X. Addition of amines to *p*-quinonedibenzenesulfonimide. J. Am. Chem. Soc., 74: 2597-602.

With J. D. Edwards, Jr. Quinone imides. XI. Addition of dienes to *p*-quinonedibenzenesulfonimide. J. Am. Chem. Soc., 74: 2603-4.

With J. D. Edwards, Jr. Quinone imides. XII. Addition of dienes to *p*-(quinonemonobenzenesulfonimide. J. Am. Chem. Soc., 74: 2605-7.

With E. F. Elslager and K. F. Heumann. Quinone imides. XIII. Orientation of groups in adducts of hydrogen chloride with quinone diimides. J. Am. Chem. Soc., 74:2608-12.

With D. S. Acker. Quinone imides. XIV. Addition of hydrogen chloride to *p*-quinonedibenzimides and related compounds . J. Am. Chem. Soc., 74:3029-32.

With R. R. Holmes. Quinone diimides. XV. Diphenoquinonediimides. J. Am. Chem. Soc., 74:3033-37.

With R. R. Holmes. Quinone diimides. XVI. Diphenoquinonediimides. J. Am. Chem. Soc., 74:3038-41.

With B. H. Braun. Chlorination of benzenesulfonyl derivatives of aromatic amines. J. Am. Chem. Soc., 74:3171-73.

With D. S. Acker. Quinone imides. XVII. Addition of organic acids top-quinonedibenzimide and related compounds. J. Am. Chem. Soc., 74:3657-59.

With J. M. Stewart. Quinone imides. XVIII. *p*-Quinonedipivalimides and their reactions. J. Am. Chem. Soc., 74:3660-64.

With Irwin J. Pachter. Structure of the 2-pyridone and -bromoacrylic acid adduct and its derivatives. J. Am. Chem. Soc., 74:4906-9.

With B. L. Van Duuren. Trachelanthic and viridifloric acids. J. Am. Chem. Soc., 74:5349-51.

With T. E. Bockstahler. Preparation and reactions of *o*-hydroxycinnamic acids and esters. J. Am. Chem. Soc., 74: 5346-48.

With I. J. Pachter. Ultraviolet spectra and structures of the pyrido[l,2-a]-pyrimidones. J. Am. Chem. Soc., 74:5491-97.

With W. Moje. Quinone imides. XIX. Addition of active methylene compounds to 1,4-naphthoquinonedibenzenesulfonimide. J. Am. Chem. Soc., 74:5557-60.

With W. Moje. Quinone imides. XX. Additions to 1,4-naphthoquinonedibenzenesulfonimide. J. Am. Chem. Soc., 74:5560-62.

With W. Moje. Quinone imides. XXI. Addition of hydrogen cyanide to 1,4-naphthoquinonedibenzenesulfonamide. J. Am. Chem. Soc., 74:5562-63.

With R. S. Voris and L. N. Whitehill. Restricted rotation about the aliphatic carbon-carbon bond. J. Am. Chem. Soc., 74:5588-92.

With B. L. Van Duuren and B. H. Braun. The structure of monocrotaline. XIV. Synthesis of monocrotalic acid. J. Am. Chem. Soc., 74:5608-11.

With P. R. Shafer and B. H. Braun. The structure of monocrotaline. XV . J. Am. Chem. Soc., 74:5612-15.

With B. H. Braun. Quinone imides. XXII. The reaction of primary aromatic amines with 2,3,5,6-tetrachloro-*p*-quinonedibenzenesulfonimide. J. Am. Chem. Soc., 74:5869-71.

With D. S. Acker. Quinone imides. XXIII. Addition reactions of *p*-quinonedibenzimide and related compounds. J. Am. Chem. Soc., 74:5872-76.

With J. M. Stewart. Quinone imides. XXIV. *o*-Quinonemonoimides. J. Am. Chem. Soc., 74:5876-80.

1953 With E. F. Elslager and T. E. Young. Quinone imides. XXV. Addition of mercaptans to *p*-quinonedibenzenesulfonimide. J. Am. Chem. Soc., 75:663-66.

With P. R. Shafer. Quinone imides. XXVI. Adducts of pquinonebis(dimethylaminosulfonimides) and their hydrolysis products. J. Am. Chem. Soc., 75:667-70.

With D. C. Blomstrom. Restricted rotation in aryl amines. XV. Stereoisomeric diglutarimidomesitylenes. J. Am. Chem. Soc., 75:2375-77.

With B. L. Van Duuren. Dicrotaline. The structure and synthesis of dicrotalic acid. J. Am. Chem. Soc., 75:2377-79.

With T. E. Young. Quinone imides. XXVII. Addition reactions of substituted *p*-quinonedibenzenesulfonimides. J. Am. Chem. Soc., 75:3235-39.

With D. C. Blomstrom. Quinone imides. XXVIII. Addition of active methylene compounds to *p*-quinonedibenzenesulfonimide and its derivatives. J. Am. Chem. Soc., 75:3403-5.

With D. C. Blomstrom. Quinone imides. XXIX. Addition of hydrazoic acid and phenols top-quinonedibenzenesulfonimide and its derivatives. J. Am. Chem. Soc., 75:3405-8.

Uber die Chemie der Senecio-Alkaloide und verwandter Verbindungen. Angew. Chem., 65:433-42.

With B. L. Van Duuren. Usaramoensine, the alkaloid in *Crotalaria usaramoensis* E. G. Baker. Integerrimine from *Crotalaria incana*

Linn and senecionine from *Senecio globellus* D. C. Stereochemical relationships. J. Am. Chem. Soc., 75:4631-35.

With B. L. Van Duuren. Structure of heliotrinic acid. J. Am. Chem. Soc., 75:4636-38.

With B. L. Van Duuren. Riddelliine, the alkaloid from *Senecio riddelii.* II. Structure of riddellic acid and total structure of riddelliine. J. Am. Chem. Soc., 75:4638-42.

With B. H. Braun and S. H. Pomerantz. Quinone imides. XXX. Addition of primary and secondary aromatic amines. J. Am. Chem. Soc., 75:4642-44.

With R. R. Holmes and J. W. Way. Quinone imides. XXXI. 3,3-Dimethoxydiphenoquinonedibenzenesulfonimide and its reaction products. J. Am. Chem. Soc., 75:5901-4.

1954 With S. H. Pomerantz. Quinone imides. XXXII. Reactions of 1,4-naphthoquinonedibenzenesulfonimide with pyridine and its derivatives. J. Am. Chem. Soc., 76:702-6.

With T. E. Young and R. W. P. Short. Quinone imides. XXXIII. Orientation of adducts from 2-benzenesulfonyl-*p*-quinonedibenzenesulfonimide. J. Am. Chem. Soc., 76: 1114-18.

With I. J. Pachter. The action of alkali on bromopyridopyrimidones. J. Am. Chem. Soc., 76:1845-47.

With R. W. P. Short. Quinone imides. XXXIV. Addition of dienes to substituted *p*-quinonedibenzenesulfonimides. J. Am. Chem. Soc., 76:2408-10.

With J. W. Way. Quinone imides. XXXV. *o*-Quinonedibenzimides. J. Am. Chem. Soc., 76:2763-69.

With S. Miyano. 2-Aminopyridine 1-oxides. J. Am. Chem. Soc., 76:2785-86.

With S. Miyano. Condensation reactions of picoline 1-oxides. J. Am. Chem. Soc., 76:3168-71.

With R. S. Colgrove. Quinone imides. XXXVI. Orientation of groups in adducts of quinone diimides with different Nsubstituents. J. Am. Chem. Soc., 76:3584-87.

With R. H. Mattson. Restricted rotation in aryl amines. XVI. 4-Substituted 1-amino-2-methylnaphthalenes. J. Am. Chem. Soc., 76:4925-28.

With K. V. Y. Sundstrom. Restricted rotation in aryl amines. XVII. Effect of varying the 4-substituent on the stability of optically active N-benzenesulfonyl-N-carboxymethyl- 1-amino-2-methylnaphthalene. J. Am. Chem. Soc., 76:5474-77.

With D. C. Blomstrom and K. V. Y. Sundstrom. Restricted rotation in aryl amines. XVIII. Resolution of N-carboxymethyl-Nmethyl-3-bromomesidine. J. Am. Chem. Soc., 76:5478-79.

With B. L. Van Duuren. Stereochemistry of the pyrrolizidine bases. J. Am. Chem. Soc., 76:6379-83.

1955 With W. P. Samuels, Jr. Quinone imides. XXXVII. Conversion of *p*-quinone diimides to indoles. J. Am. Chem. Soc., 77:5375-82.

With W. P. Samuels, Jr. Quinone imides. XXXVIII. Adducts to *p*-quinonedimethanesulfonimide and their hydrolysis products. J. Am. Chem. Soc., 77:5383-85.

With E. J. Agnello and R. S. Colgrove. Quinol imides and o-quinone imide diacetates. I. Conversion of 4-benzenesulfonamidotriphenylmethane to 4-benzohydryl-*o*-quinonediacetatebenzenesulfonimide. J. Am. Chem. Soc., 77:5617-25.

1956 With M. Gianturco. Senecio alkaloids. The composition of "hieracifoline" and "jacobine." J. Am. Chem. Soc., 78:398-400.

With L. Whitaker. Quinone imides. XXXIX. Adducts of quinone monoimides and conversion of active methylene adducts to benzofurans. J. Am. Chem. Soc., 78:658-62.

With K. R. Brower. Restricted rotation in aryl amines. XVIII. Effect of remote substituents on the stability of optically active N-benzenesulfonyl-N-carboxymethyl-3-benzylmesidine. J. Am. Chem. Soc., 78:663-66.

With M. Gianturco. The alkaloids *of Crotalariajuncea.* J. Am. Chem. Soc. , 78:1919-21.

With M. Gianturco. Senecio alkaloids. Structure of tri-chodesmine. J. Am. Chem. Soc., 78: 1922-25.

With M. Gianturco. Crotalaria alkaloids. The structure of junceine. J. Am. Chem. Soc., 78:1926-28.

With M. Gianturco and B. L. Van Duuren. The alkaloids from *Senecio tomentosus.* Observations on the alkaloid jacobine and on the structure of jaconecic acid. J. Am. Chem. Soc., 78:3513-19.

With W. Reifschneider. Addition of phenylmagnesium bromide to aliphatic conjugated dibenzenesulfonimides. J. Am. Chem. Soc., 78:3825-28.

With M. Gianturco. The structures of grantianine and sceleratine. A suggested biogenesis of the acids in alkaloids from Senecio and Crotalaria species. J. Am. Chem. Soc., 78:4458-64.

With K. R. Brower. Quinol imide acetates. 2,4,6-Trimethyl-oquinobenzenesulfonimide acetate and 2,4-dimethyl-*o*-quinolbenzenesulfonimide acetate. J. Am. Chem. Soc., 78:4770-73.

With J. E. Dunbar. Quinol imide acetates. III. Addition reactions of 2-methyl-*o*-naphthoquinol-*p*-toluenesulfonimide acetate. J. Am. Chem. Soc. , 78:4774-78.

With M. Gianturco. Senecio alkaloids: the alkaloids of *Senecio brasiliensis, fremonti* and *ambrosioides.* J. Am. Chem. Soc., 78:5315-17.

With M. D. Nair. Quinone imides. XL. Orientation in addition of hydrogen chloride to 2-substituted *p*-quinonedibenzimides. J. Am. Chem. Soc., 78:5927-31.

With M. D. Nair. Quinone imides. XLI. Orientation in addition of thiophenol and benzenesulfinic acid to 2-substituted *p*-quinonedibenzimides. J. Am. Chem. Soc., 78:5932-38.

1957 With M. Gianturco. Senecio alkaloids: mikanoidine, the alkaloid from *Senecio mikanoides.* J. Am. Chem. Soc., 79:166-69.

With H. H. Gibbs. Restricted rotation in aryl amines. XIX. Effect of substituents in the 4-position on the optical stability of 1-amino-2-methylnaphthalene derivatives. J. Am. Chem. Soc., 79:170-73.

With M. Gianturco. Senecio alkaloids: spartioidine, the alkaloid from *Senecio spartioides;* stereochemical relationship to other Senecio alkaloids. J. Am. Chem. Soc., 79:174-77.

With M. D. Nair. Quinone imides. XLII. Orientation of adducts from substituted *p*-quinonedimethanesulfonimides. J. Am. Chem. Soc., 79:177-79.

With E. L. DeYoung. Quinone imides. XLIII. The reactions of *o*-quinonedibenzenesulfonimides. J. Am. Chem. Soc., 79: 417-19.

With E. L. DeYoung. Quinol imide acetates. IV. The reactions of 2,4-dimethyl*o*-naphthoquinol-*p*-toluenesulfonimide acetate. J. Am. Chem. Soc., 79:705-8.

With W. Reifschneider. The reaction of 2-bromopyridine N-oxides with active methylene compounds. J. Am. Chem. Soc., 79: 2236-39.

With H. J. Neumiller, Jr. Quinone imides. XLIV. The orientation of groups in addition reactions to substituted p-quinonedibenzimides. J. Am. Chem. Soc., 79:3808-12.

With M. J. Gortatowski. Restricted rotation in arylamines. XX. Effect of *meta* substitution on the optical stability of some Nbenzenesulfonyl-N-carboxymethylmesidines. J. Am. Chem. Soc., 79:5525-28.

1958 With L. M. Werbel and M. D. Nair. Quinone imides. XLVI. Addition of heterocyclic active methylene compounds to p-benzoquinone diimides. J. Am. Chem. Soc., 80:3291-93.

With J. E. Dunbar. Restricted rotation in arylamines. XXII. Nbenzenesulfonyl-N-carboxymethyl-3-amino-2,4,6-trimethylpyridine and its N-oxide. J. Am. Chem. Soc., 80:3649-51.

With J. S. Dix. Restricted rotation in arylamines. XXI. Effect of 3-substituents on the optical stability of some N-benzenesulfonyl-N-carboxymethylmesidines. J. Am. Chem. Soc., 80: 4579-81.

With J. S. Dix. Reaction of 2-aminopyridine with a-haloketones. J. Am. Chem. Soc., 80:4618-20.

With L. M. Werbel. Preparation of quinol imide acetates. VI. Scope and limitations. J. Am. Chem. Soc., 80:5799-803.

1959 With W. Reifschneider. A synthesis of 6-hydroxy-4-quinolizones. J. Am. Chem. Soc., 81:2537-41.

With A. Ferretti. Thioethers from halogen compounds and cuprous mercaptides. II. J. Am. Chem. Soc., 81:4927-31.

With A. Ferretti. Thioethers. III. Preparation of aromatic di- and tri-mercapto compounds by dealkylation of aryl alkyl thioethers. J. Am. Chem. Soc., 81:4939-40.

With D. Fles. The absolute configuration of the C, atom in retronecanone (1-methyl-7-oxopyrrolizidine). J. Am. Chem. Soc., 81:4946-51.

With D. Fles. The absolute configuration of the C_8 atom in pyrro lizidine moieties of Senecio alkaloids. J. Am. Chem. Soc., 81:5803-5.

With C. C.J. Culvenor, C. N. Robinson, and H. A. Stingl. Synthesis of heliotramide. Aust. J. Chem., 12:706-11.

1960 With S. Miyano and D. Fles. 1-Hydroxypyrrolizidine and related compounds. J. Am. Chem. Soc., 82:1466-68.

With M. D. Nair. Stereochemistry of necic acids. J. Am. Chem. Soc., 82:3786-87.

With T. A. Geissmann and J. D. Edwards. Gossypol, a pigment of cottonseed. Chem. Rev., 60:555-74.

1961 With M. D. Nair. The structure of riddellic acid and the stereochemistry of necic acids. J. Am. Chem. Soc., 83:922-26.

With A. Ferretti. Restricted rotation in aryl amines. XXII. Effect of *meta* substitution on the optical stability of some N-benzenesulfonyl-N-carboxymethylmesidines. J. Am. Chem. Soc., 83: 2559-63.

With S. Miyano and M. D. Nair. Synthesis of substituted pyrrolidines and pyrrolizidines. J. Am. Chem. Soc., 83:3323-27.

With M. D. Nair. Benzimidazole syntheses by oxidative cyclization with peroxytrifluoroacetic acid. J. Am. Chem. Soc., 83:3518-21.

Alfred Blalock

Alfred Blalock

April 5, 1899-September 15, 1964

by A. Mcgehee Harvey

Alfred Blalock was born in Culloden, Georgia on April 5, t 1899, the first son of George Z. Blalock and Martha (Davis) Blalock. Blalock's father was a merchant, his mother a "remote cousin" of Jefferson Davis. George Blalock, who was almost thirteen years older than his wife, died in 1931. He exercised a firm hand as head of the house, demanding perfection of his children and laying great stress on education, so that "it was a sad day when anyone brought in a report card that left something to be desired."* Alfred's sister Elizabeth remembers hearing Alfred say "he had rather mother use the hairbrush on him than father look at him hard."† His mother remembered Alfred as a conscientious young boy who was unwilling to go to bed until his homework had been mastered and even cried if forced to bed before he knew every spelling word perfectly. He was characterized as having an attractive smile, a soft manner of speech, a retiring gentle way, and an effective manner of saying clearly what was on his mind.

In view of his father's ill health, when Alfred was eleven years of age the family moved to Jonesboro, a town about

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* Mark M. Ravitch, *The Papers of Alfred Blalock* (Baltimore: The Johns Hopkins Press, 1966), vol. 1, p. xv.

† *Ibid.*

forty miles north of Culloden where medical assistance would be more readily available. By the age of fourteen Alfred had completed the ninth grade at Jonesboro and was granted admission to the senior class at the Georgia Military College at Milledgeville. He entered the University of' Georgia as a sophomore in the fall of 1915 and graduated from that institution with an A.B. in 1918.

Blalock did not consider himself to have been an excessively diligent student either in college or medical school. His academic record at the University of Georgia, however, revealed a satisfactory performance with most of his grades in the 80's or 90's. The yearbook, of which he was associate editor, listed him as secretary and treasurer of the senior class and a member of' the college debating society, the junior cabinet, and the Gridiron Club. Election to the Gridiron Club was considered to be the second highest honor on the campus, members being chosen for overall excellence rather than specific academic achievement. He was a good tennis player and entered many of the college tournaments. Dr. John P. Campbell, Professor of Zoology at the University of Georgia, wrote to J. Whitridge Williams, the dean at the Johns Hopkins University School of Medicine, regarding Blalock's admission application: "As you will see his record is not unusual. He went in for college activities and had no thought of being a 'grind.' He only decided to take up medicine in his senior year. One day he came to me and said that he was going to do his very best in the hope that I might feel that I could give him a strong recommendation for Johns Hopkins. He certainly made good. I have no hesitation in saying that he is mature enough and his habits of study are sufficiently well formed to be admitted. I hope you will take him."*

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* *Ibid.,* p. xvii.

MEDICAL EDUCATION

Blalock entered the Johns Hopkins University School of Medicine in the fall of 1918. His record in medical school was not outstanding. Tinsley Harrison, one of Blalock's closest friends, wrote about him as follows:

> While Al Blalock was in medical school he ran the student bookstore and from this earned a major fraction of his expenses at Hopkins. In addition to this he was devoted to both tennis and golf, and it was our mutual interest in these sports that first made us decide to room together and started a friendship that meant a great deal to me throughout the years. Also, he was very much the ladies' man and often had social engagements, usually at Goucher, two or three evenings a week.
>
> On the other hand, he never wasted a minute. When he was not actively working in the bookstore or following the pursuits mentioned above, he worked at his medical studies continually. I never saw him stop in the living room of the fraternity house just to sit around and gossip. I never saw him waste time—as I did—playing cards.
>
> Because of these several interests, Al was not an outstanding student when it came to grades. As I recall he ranked somewhere toward the bottom of the upper half of the class scholastically, but I am not certain about this. In any case he was not considered to be one of the ten or twenty top students in the class in terms of grades.
>
> I imagine it was because of the heavy (in my opinion excessive) reliance on grades that he did not get the surgical internship for which he applied.*

Surgical internships at Johns Hopkins in those days were awarded on the basis of class standing. Although Blalock failed to get the appointment in general surgery, he was accepted as "house medical officer-urology" under Hugh Hampton Young. In spite of having a nephrectomy and temporary facial nerve palsy during that internship year, Blalock's performance was sufficiently satisfactory to gain him an assistant residency on the general surgical service for the following year. He was turned down for a reappointment

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* *Ibid.,* p. xix.

to the surgical house staff (an event to which Blalock never reconciled himself), and in July 1924 he began a year as "extern in otolaryngology" under Samuel J. Crowe.

As a medical student, Blalock was interested in research. In later years he often credited Tinsley Harrison with the awakening of his interest in this area. Of his early research experience Blalock commented:

> When I was a medical student, and I think the year was 1920 or 1921, I worked for a short while in the Hunterian Laboratory with Dr. Jay McLean. My problem was on the lymphatics and no publications resulted. It was at this time that Jay McLean found a heparin-like substance in the liver, and subsequently Dr. Howell continued his work with the discovery of heparin.
>
> Two years following my graduation from medical school, that being July 1924, 1 spent a year as extern in otolaryngology on Dr. Crowe's service. One of the subjects on which I worked was that of regeneration of the recurrent laryngeal nerves of dogs. The thing that distressed me most about the Hunterian was that I was told that I could work in it on Saturday mornings from 10 to 12. I had a good deal of free time and could have gotten along better had I been allowed to work there more. Dr. Halsted had died in 1922 else I suspect he would have given me a better opportunity.*

During his early period at Johns Hopkins, Blalock published two papers with Harrison and C. P. Wilson. The first, "The Effects of Changes in Hydrogen Ion Concentration on the Blood Flow of Morphinized Dogs," appeared in the *Journal of Clinical Investigation* in 1925, and the second, "Partial Tracheal Obstruction. An Experimental Study on the Effects on the Circulation and Respiration of Morphinized Dogs," was published in the *Archives of Surgery* in 1926.

In 1925 Blalock accepted the chief residency in surgery at the newly reorganized school of medicine at Vanderbilt. Again Harrison was instrumental in Blalock's career. Harrison had interned at the Peter Bent Brigham Hospital and

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* *Ibid.,* p. xxi.

returned to Johns Hopkins in the fall of 1924 as an assistant resident in medicine. The following year he went to Vanderbilt at the invitation of G. Canby Robinson, dean and chairman of the Department of Medicine. Harrison mentioned to Robinson that Blalock was available for the position of chief resident in surgery. Robinson recommended Blalock to Barney Brooks, who was moving from Washington University in St. Louis to assume the chairmanship of the Department of Surgery at Nashville, and Brooks offered Blalock the position.

When he arrived at Vanderbilt, Blalock had hoped that Brooks would allow him to be in charge of the surgical pathology laboratory. He was initially disappointed that Brooks placed him in charge of the experimental laboratory, but subsequently was pleased that Brooks had made this choice for him.

Harrison and Blalock continued work in research together. Of this work Harrison wrote:

> During the 1924-25 year at Hopkins, and the subsequent 1925-26 year, when we were both chief residents at Vanderbilt, we managed to do a lot of work in the laboratory together. At that time the Van Slyke apparatus was relatively new, and the availability of a simple and accurate method for measurement of blood oxygen made it possible to, for the first time, perform accurate determinations of cardiac output in the intact animal. Almost nothing had been done about cardiac output before that.... Therefore Al and I became interested in studying the influence of various things on cardiac output. After a couple of years my interest was getting more and more toward the heart per se and Al's was moving more and more toward problems of cardiac output that seemed to have some direct application to the clinical problems of surgery. Therefore, we decided to split up as a team and still help each other, but that he would work on shock and I would work on cardiac output. Within a year after this decision he had completed his beautiful work on hemorrhage and trauma and its effect on the circulation of dogs. Then he came down with tuberculosis before he had been able to complete his manuscript. He went up to Saranac for a time, and I made the trip with him because we were afraid of a pulmonary

> hemorrhage or something like that. All the way up on the trip he was very unhappy because he had been forbidden to do any work of any kind and his data for the first paper on shock were all set and ready to be prepared for publication. I promised him faithfully that I would write the paper on shock for him and send it to him for his approval. This I did. I have derived a permanent satisfaction from the thought that I was able in this manner to help a dear friend and to play a minor role in a research problem which, looking back after 40 years, seems to have opened a lot of doors.
>
> Do not get me wrong. I had nothing whatever to do with the conception or planning of the work on shock. This was entirely Al's own. I did take his data and wrote the paper for him without a true realization on my own part of the importance of the work. This was done purely to help out a friend in distress.[*]

After spending a year (1927) at the Trudeau Sanatorium, Blalock went abroad for a few months where he worked in the Department of Physiology in Cambridge under G. V. Anrep and Sir Joseph Barcroft. When he returned to Vanderbilt in the latter part of 1928, he continued to work prodigiously in the laboratory, doing essentially all his own work, making his own animal preparations and doing his own blood gas determinations. He had students working with him from the first and to be chosen by him for a summer's work was considered to be a real plum. His younger collaborators then and later remember the method of writing joint papers. When a project was completed Blalock took the experimental data and rough notes for a clinical paper and, shortly, often by the next morning, would have written the entire paper, longhand, in very close to final form, frequently placing the names of his associates before his own.

In January 1930 Vivien Thomas, a young black who was forced for lack of funds to leave his first year of college, came to work for Blalock in the laboratory. At that point Blalock's increasing obligations were cutting into the time he could spend in the laboratory and he needed someone to free him

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[*] *Ibid.,* p. xxii.

from the more routine chores. A more fortunate choice could not have been made. Vivien Thomas learned to perform the surgical operations and the chemical determinations needed for their experiments, to calculate the results, and to keep precise records; he remained throughout Blalock's career as an invaluable associate. As time went on Blalock and Thomas worked together so closely that it was enough to suggest to Thomas the experimental preparation and the measurements to be made. Thomas often contributed his own ideas in developing the operative and manipulative techniques.

RESEARCH AT VANDERBILT

In 1928 Blalock began studies, with the aid of Hubert Bradburn, in which the oxygen content of blood withdrawn from veins in various parts of the body was determined during shock produced by different methods, including the injection of histamine in some cases and trauma to an extremity in others. The difference in the results led to the following statement by Blalock:

> These observations suggest a local accumulation of blood at the site of trauma to a large area such as the intestinal tract or an extremity, and are evidence against the action of a histamine-like substance that produces a general bodily effect. The (earlier) prevailing theory ... was that traumatic shock was due to a toxin, possibly histamine. The strongest evidence that had been put forward in favor of the toxic theory was derived from the experiments of Cannon and Bayliss, in which they found that shock resulting from trauma to an extremity of the cat could not be accounted for on the basis of the local loss of whole blood and plasma. In brief, they traumatized one posterior extremity and, when shock resulted, they amputated the two posterior extremities and determined the difference in the weight. After completing the studies on blood gases, I repeated the experiments of Cannon and Bayliss using anesthetized dogs. It was noted that the swelling extended to a higher level than the uppermost limit of the trauma and suggested that the two physiologists had not performed their amputations at a sufficiently high level. In my experiments the posterior part of the animal was bisected and the difference in the weights of the traumatized

> and non-traumatized parts was determined. A comparison of this difference with the results of other experiments in which shock was produced by the slow withdrawal of blood showed that the trauma resulted in a sufficient loss of whole blood and plasma to explain the development of shock. In my paper published in 1930, entitled "Experimental Shock, The Cause of the Low Blood Pressure Produced by Muscle Injury to Dogs," it was stated: "The experiments which are presented in this paper offer no evidence that trauma to an extremity produced a toxin that caused a general dilatation of capillaries with an increase in capillary permeability and a general loss of fluid from the bloodstream."*

An important paper that originated in Blalock's laboratory described a method for transplanting the adrenal gland of the dog with reestablishment of its blood supply. This successful transplantation of the adrenal to the neck of the dog, apart from such early trials as Carrell and Guthrie's, was the first successful transplantation of an endocrine organ by direct anastomosis of its vascular supply. The most important aspect, however, was the success of the vascular suture techniques that led Blalock to suggest to his laboratory associates that the central end of the divided subclavian artery be connected to the pulmonary artery to see whether this would result in pulmonary hypertension. The results of this investigation were reported in the *Journal of Thoracic Surgery* in 1939 in a paper entitled "Experimental Observations on the Effects of Connecting by Suture the Left Main Pulmonary Artery to the Systemic Circulation." Blalock was to use this operation in 1944 for the relief of the Tetralogy of Fallot. It is a measure of his breadth as a physiologist that he was interested in pulmonary hypertension, a physiological problem that was not to attract the attention of other surgeons or cardiologists until almost two decades later.

The studies in shock, however, were the principal occupation of Blalock's laboratory in Nashville. He and his group

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* A. Blalock, "Reminiscence: Shock After Thirty-tour Years," *Review of Surgery,* 21 (1964):231.

carefully explored every facet of the problem and compiled the evidence that clearly connected shock with the loss of fluid outside the vascular bed and with the resulting decrease in blood volume. His experiments were simple and direct; his discussions and conclusions concerning the results were straightforward, forceful, and convincing. Other investigators, of course, were reaching the same conclusion, particularly Phemister in Chicago, but the massive amount of data that Blalock accumulated on the characteristics of hemorrhagic and traumatic shock, his carefully planned experiments that eliminated one possible cause after another of the then current explanations of shock, and the clarity with which he put forth his views, based on sound experimental work, led to a new understanding of this important problem. The firm recognition of the need for volume replacement was corroborated in the treatment of the wounded during World War II. Large quantities of blood, blood substitutes, and plasma expanders were used, which resulted in the saving of many lives. Blalock himself considered his best work to have been that on traumatic shock.

On October 25, 1930 Blalock married Mary Chambers O'Bryan of Nashville. They had three children: William Rice, Mary Elizabeth, and Alfred Dandy.

THE BALTIMORE PERIOD

In 1938 Dean DeWitt Lewis, then chairman of the Department of Surgery at Johns Hopkins, resigned because of illness. The committee to select a successor recommended several prominent surgeons in the country who turned the position down for one or another reason. One of those who declined was Evarts A. Graham, the distinguished chairman of the Department of' Surgery at Washington University. Graham strongly recommended Alfred Blalock to President Bowman of Johns Hopkins. When the offer was made to

Blalock, he accepted without hesitation and assumed the position in 1941.

Establishing himself in Baltimore proved somewhat difficult for a variety of reasons, but Blalock successfully disposed of the numerous problems that arose. He began at once to operate daily and to be actively concerned in the training of the resident staff and in the teaching of medical students. His Friday noon clinics rapidly developed into masterpieces of clinical instruction. George Duncan, an assistant resident in surgery at Vanderbilt, was brought to Baltimore to continue the experiments on shock.

At the time of Blalock's arrival in Baltimore, A. McGehee Harvey, the medical resident, and J. L. Lilienthal, Jr., were studying the physiology and pharmacology of myasthenia gravis. It was their conclusion that there might be a circulating substance, similar to curare, responsible for the neuromuscular blockade and the resulting muscular weakness. The well-known changes in the thymus gland, consisting of hypertrophy of that organ as well as the occurrence of tumors, made this structure the most likely source of such a substance. Harvey and Lilienthal had discussed their evidence on numerous occasions with Frank Ford, who was the senior neurologist of the hospital, and it was decided that a trial of total thymectomy in patients with this disease, regardless of whether there was tumor or simple hypertrophy, was indicated. Ford agreed to present this proposal to Blalock. While in Nashville Blalock had successfully removed a thymic tumor from a patient with myasthenia gravis. The decision was made to do a total thymectomy in a series of patients with myasthenia. The patients were operated on by Blalock and their pre- and postoperative care and study was managed by Harvey and Lilienthal. In the early cases the results were highly encouraging, and objective evidence was obtained for the first time of a function of the thymus gland in man from the resulting improvement in neuromuscular function. With further experience the results were not uniformly predictable, and Blalock soon lost interest in the project. This operation is still done, however, and in the ensuing years it has become clear that the thymus probably has a definite role in the pathogenesis of the disease.

In 1943 Blalock was called into consultation by Dr. Edwards A. Park, the professor of pediatrics, to see a child who had a congenital lung cyst. Surgery was advised. As Blalock was leaving the ward, Park asked him whether he thought anything could be done for coarctation of the aorta. Blalock's response was noncommittal, but as he walked out of the Harriet Lane Home (the pediatric building) he turned to his resident and said: "I wonder how that could be approached in the laboratory."*

As indicated, Blalock had a special interest and competence in thoracic surgery that already extended to vascular surgery. He had successfully operated on a stab wound of the aorta while at Vanderbilt and ligated the patent ductus arteriosus in several patients at Vanderbilt and at Johns Hopkins. In addition, his published results with the operative treatment of constrictive pericarditis had attracted attention. A few months after Park made his remark about coarctation he found on his desk the manuscript of a paper by Blalock and Park that described an operation for coarctation of the aorta—anastomosing the proximal end of the divided subclavian artery to the distal aorta. Blalock expressed concern to Park about employing this coarctation operation in humans since so many of' his dogs had become paraplegic because of the necessity for clamping the aorta during the procedure. Their delay in actually trying the operation resulted in prior operations by Crafoord in Sweden and Gross

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* *The Papers of Alfred Blalock,* p. xxxvii.

in Boston, both of whom employed excision of the coarctation and direct anastomosis. There need have been no fear of paraplegia from cross clamping the coarcted aorta since the great collateral circulation that develops in individuals with this anomaly provides insurance against this type of paralysis. Nevertheless, Blalock's first coarctation procedure on a patient, using the turned-down subclavian artery, did result in paraplegia because several pairs of intercostal arteries had been carefully divided to facilitate mobilizing the aorta.

At a pediatric conference at which Blalock reported this coarctation work, Helen Brooke Taussig, then in charge of pediatric cardiology in the Department of Pediatrics at the Johns Hopkins University School of Medicine, inquired whether some operation could be devised to improve the pulmonary circulation in children with pulmonic stenosis. Blalock, of course, had the operative remedy at hand-the subclavian-pulmonary artery anastomosis performed in Nashville some years earlier in the attempt to produce experimental pulmonary hypertension. The first question was whether this procedure would relieve the cyanosis in patients with pulmonic stenosis and the second was whether the patients would tolerate the procedure. He and Vivien Thomas had great difficulty in the laboratory trying to produce a satisfactory cyanotic preparation in dogs. Finally, successful preparations were made and the cyanosis was significantly diminished by the subclavian-pulmonary anastomosis.

On November 29, 1944 the first operation was undertaken. At that time all the modern vascular instruments were lacking and there was little but Blalock's determination to carry his surgical team through the procedure. The child had extensive collateral vessels full of thick dark blood. The pulmonary artery was identified with some difficulty and was isolated back into the mediastinum. In the words of William P. Longmire, the surgical resident assisting Blalock at the operation: "It was quite amazing to see the professor gently but blindly insert a right angle clamp into the mediastinum and after dissecting over his index finger pull out the innominate artery."*

Vivien Thomas stood in back of Blalock at the procedure and offered a number of helpful suggestions in regard to the actual technique employed. The operation was successful at the initial attempt. The acclaim that followed the first report brought a flood of patients and visitors to the hospital.

This monumental accomplishment, resulting from the collaborative effort of a scientifically oriented surgeon and a dedicated pediatric cardiologist, brought fame to them both and ushered in the modern era of cardiac surgery.

If one can be judged by the record of the people he has trained, Blalock stands at the very top; no other surgeon in his era trained thirty-eight residents who hold important academic appointments—most of them full professors and more than ten department chairmen.

A great part of Blalock's success as a teacher, scientist, and medical statesman was due to his philosophy of research. In his presidential address to the American Surgical Association, entitled "The Nature of Discovery," he stated:

> Contrary to popular belief, there is nothing magical about science or scientific investigators. The conception of the scientist as an intellectual superman, achieving important results through sheer mental brilliance, is quite unfounded. Too often in talking to a bright young surgeon I have heard the statement that he does not wish to go into academic work because he has no originality, when as a matter of fact he has not had the opportunity or the inspiration to demonstrate his ability. The only way an interested person can determine whether or not he has aptitude in research is to give it a trial.†

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* *The Papers of Alfred Blalock,* p. xli.

†A. Blalock, "Our Obligations and Opportunities" (Presidential Address, American College of Surgeons, Nov. 19, 1954), *Bulletin of the American College of Surgeons,* 40:1.

Blalock firmly believed in the autonomy of a department chairman. He felt that a conscientious head of a department is better qualified to make many decisions than is a committee composed of individuals from other departments in the school. He always maintained a deep interest in students and generally opposed any changes in the curriculum that might interfere with their elective time and opportunity to develop an interest in creative scholarship.

Blalock lent his best energies to the development of a children's surgical unit at Johns Hopkins. This dream finally culminated in the great Children's Medical and Surgical Center in Baltimore, which was dedicated the year of his retirement. Dr. William P. Longmire, Jr., one of Blalock's residents who realized at an early stage in their acquaintance that this was no ordinary man, began to record some of the comments Blalock made and other events that he witnessed. A notation that Longmire made on April 29, 1948 reads as follows:

> The professor said the other day that in looking back over his life, the things that meant the most to him, that gave him the greatest satisfaction, were the contributions to medicine he has made. These creative endeavors meant so much more to him than the positions he had held, the honors he had received, or the societies to which he had been elected, and in many cases in which he had held high offices. One should never try to picture the professor as a saint or a god; one of his chief virtues is his "earthly human shortcomings," faults that color his character and give it a spark and interest that so many men in comparable positions fail to have. Professor Blalock is an active, struggling person, in whom one can take an interest as a man and not just in his accomplishments alone.*

The respect for Blalock's accomplishments as a scientist and particularly his unique contributions to the Johns Hopkins Medical Institutions led the trustees of the hospital and the university to change the name of the clinical science building to "The Blalock Building."

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* Remarks by W. P. Longmire, Jr., at the Society of Clinical Surgeons, 1964.

In the preparation of this memoir extensive use has been made of the excellent biography of Blalock written by Mark M. Ravitch, *The Papers of Alfred Blalock,* volumes 1 and 2 (Baltimore: The Johns Hopkins Press, 1966), and of material in the Alan M. Chesney Archives of the Johns Hopkins Medical Institutions.

HONORS AND DISTINCTIONS

Degrees

1918	A.B., University of Georgia
1922	M.D., The Johns Hopkins University School of Medicine

Honorary Degrees

1946	Sc.M., Yale University
1951	M.D., Honoris Causa, University of Turin
1951	Sc.D., University of Rochester
1951	Sc.D., University of Chicago
1953	Sc.D., Lehigh University
1954	LL.D., Hampden-Sydney College
1954	Sc.D., Emory University
1959	Sc.D., Georgetown University
1963	LL.D., University of Saskatchewan

Hospital and University Appointments

1922-1925	Intern and Assistant Resident Surgeon, The Johns Hopkins Hospital
1925-1926	Resident Surgeon, Vanderbilt University Hospital
1925-1927	Instructor in Surgery, Vanderbilt University School of Medicine
1928-1930	Assistant Professor of Surgery, Vanderbilt University School of Medicine
1930-1938	Associate Professor of Surgery, Vanderbilt University School of Medicine
1938-1941	Professor of Surgery, Vanderbilt University School of Medicine
1941-1964	Professor of Surgery and Director, Department of Surgery, The Johns Hopkins University School of Medicine
1941-1964	Surgeon-in-Chief, The Johns Hopkins Hospital
1955-1964	Chairman, Medical Board, The Johns Hopkins Hospital
1964	Professor Emeritus of Surgery, The Johns Hopkins School of Medicine
1964	Surgeon-in-Chief Emeritus, The Johns Hopkins Hospital

Professional and Honorary Societies

Allen O. Whipple Surgical Society, Honorary Member

American Association for Thoracic Surgery (Council, 1944-1948; President, 1950; Senior Member, 1954-1964)

American Board of Surgery (Founders Group)

American Clinical and Climatological Association, Honorary Member

American College of Cardiology, Honorary Fellow

American College of Surgeons (Board of Regents, 1941-1953; President-Elect, 1953-1954; President, 1954-1955)

American Heart Association

American Medical Association

American Society for Clinical Investigation

American Surgical Association (President, 1956; Council, 1957-1964)

Blalock Society

Board of Thoracic Surgery (Founders Group)

Buffalo Surgical Society, Honorary Member

Chicago Surgical Society, Honorary Member

Halsted Society (Senior Member, 1956-1964)

International Society of Surgery

International Surgical Group

National Academy of Sciences, Member

Royal Society of Medicine, Honorary Fellow

Society of Clinical Surgery (President, 1950-1952)

Society of University Surgeons, Honorary Member

Society for Vascular Surgery, Charter Member (President, 1951-1952)

Southern Surgical Association (Secretary, 1943-1948; President, 1949; Council, 1950-1954)

Foreign Societies

1946	Academie de Chirurgie, Foreign Associate
1955	Academie de Medecine
1957	Academie Royale de Medecine de Belgique, Membre Honoraire Etranger
1955	Academie des Sciences, Institute de France, Associe Etranger

1951	Argentine Academy of Surgery, Foreign Corresponding Member
1955	Association of Surgeons of Great Britain and Ireland, Honorary Fellow
1947	Belgian Society of Surgery, Honorary Foreign Member
1947	British Cardiac Society, Honorary Member
1944	Colegio Brasileiro de Cirurgioes, Foreign
1947	Greek Surgical Society, Honorary Fellow
	Harvey Society, Honorary
1952	Horse Shoe Club (Great Britain), Honorary Member
1957	James IV Association of Surgeons
1950	Academie Nationale de M6decine (Correspondant Etranger)
1948	Royal Academy of Medicine of Belgium, Foreign Correspondent
1954	Royal College of Surgeons of Edinburgh, Honorary Fellow
1947	Royal College of Surgeons of England, Honorary Fellow
1952	Royal Faculty of Physicians and Surgeons of Glasgow, Honorary Fellow
1953	Sociedad Argentina de Cardiologia, Honorary Member
1946	Sociedade Brasileira de Cardiologia, Honorary
1947	Society of Thoracic Surgeons of Great Britain and Ireland, Honorary Member

Committees

	National Research Council, Division of Medical Sciences
1939-1951	Medical Fellowship Board
1940-1952	Committee on Surgery
1940-1946	Subcommittee on Shock
1946-1956	Board of Directors, National Society for Medical Research (Vice-President, 1956-1964)
1950-1952	Advisory Council, Life Insurance Medical Research Fund
1952-1964	Committee on the John J. Carty Fund, National Academy of Sciences

1956-1964	Comite de Patronage Scientifique de l'Hopital Chirurgical de Monaco
1957-1960	Board of Scientific Counselors, National Heart Institute, National Institutes of Health
1958-1964	Medical Advisory Council of Medical International Cooperation (Medico)
1959-1960	Committee of Consultants on Medical Research to the Senate Appropriations Committee
1961-1964	Vanderbilt Medical Committee of Visitors
1963-1964	Maryland Judicial Selection Council, Inc.

Editorships

1936-1964	Associate Editor, *Surgery*
1939-1964	Editorial Board, *Archives of Surgery*
1942-1953	Consulting Editorial Board, *Surgery, Gynecology and Obstetrics*
1945-1949	Editorial Board, *The American Heart Journal*
1946-1964	Advisory Editorial *Board, Journal of Thoracic Surgery*
1948-1951	Editorial Committee, *Annual Review of Medicine*
1955-1964	Editorial Board, *The American Surgeon*

Awards

1940	Research Medal, Southern Medical Association
1941	Gordon Wilson Medal
1947	Chevalier de la Republique Francaise, Ordre National de la Legion d'Honneur
1947	Charles Mickle Fellowship
1948	Passano Award
1949	Rene Leriche Award
1950	Matas Award
1953	American Medical Association Distinguished Service Award
1954	International Feltrinelli Prize for Medicine
1954	Lasker Award
1955	Roswell Park Medal
1956	National Order of Merit "Carlos J. Finlay" (Officer's Degree, Government of Cuba)
1959	Holland Society of New York, Potomac Branch Award
1959	Gairdner Award

1960	Modern Medicine Award for Distinguished Achievement
1960	The Johns Hopkins Hospital Distinguished Service Award
1960	Alumni Merit Award, University of Georgia
1965	Dr. Blalock received a posthumous award of the Henry Jacob Bigelow Medal at the meeting of the Johns Hopkins Medical and Surgical Association on February 27, 1965. He had been engaged in preparation of his address for the presentation of that medal in Boston when he was interrupted by his final illness.

Selected Bibliography*

1924 A clinical study of biliary tract disease. J. Am. Med. Assoc., 83:2057-60.

A statistical study of eight hundred and eighty-eight cases of biliary tract disease. Johns Hopkins Hosp. Bull., 35:391-409.

1925 With Samuel J. Growe. The treatment of chronic middle ear suppuration. Arch. Otolaryngol., 1:267-71.

With Tinsley R. Harrison and Charles P. Wilson. The effects of changes in hydrogen ion concentration on the blood flow of morphinized dogs. J. Clin. Invest., 1:547-68.

The incidence of mastoid disease in the United States. South. Med. J., 18:621.

1926 With Samuel J. Crowe. The recurrent laryngeal nerves in dogs. Experimental studies. Arch. Surg., 12:95-116.

With Tinsley R. Harrison. Cardiac output in pneumonia in the dog. J. Clin. Invest., 2:435-55.

With Tinsley R. Harrison and Charles P. Wilson. Partial tracheal obstruction. An experimental study on the effects on the circulation and respiration of morphinized dogs. Arch. Surg., 13:81-95.

With Tinsley R. Harrison. Oxygen lack and cardiac output. Some clinical considerations. J. Am. Med. Assoc., 87:1984-87.

1927 A rubber mask for determination of oxygen consumption of the dog. J. Lab. Clin. Med., 12:378-79.

With Tinsley R. Harrison. The regulation of circulation. V. The effect of anemia and hemorrhage on the cardiac output of dogs. Am. J. Physiol., 80:157-68.

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* A complete bibliography of the works of Alfred Blalock is available from the Archives of the National Academy of Sciences.

With Tinsley R. Harrison. The regulation of circulation. VI. The effects of severe anoxemia of short duration on the cardiac output of morphinized dogs and trained unnarcotized dogs. Am. J. Physiol., 80:169-78.

Cardiac output in the dog during ether anesthesia. I. The effect of ether anesthesia on the cardiac output. Arch. Surg., 14:732-51.

Cardiac output in the dog during ether anesthesia. II. The effect of the injection of alkali on the cardiac output of the anesthetized dog. Arch. Surg., 14:921-33.

Cardiac output in the dog during ether anesthesia. II. The effect of therapeutic amounts of digitalis on cardiac output of the anesthetized dog. Arch. Surg., 14:978-90.

With Tinsley R. Harrison. The effects of thyroidectomy and thyroid feeding on the cardiac output. Surg. Gynecol. Obstet., 44:617-26.

Mechanism and treatment of experimental shock. I. Shock following hemorrhage. Arch. Surg., 15:762-98.

With Tinsley R. Harrison, Cobb Pilcher, and Charles P. Wilson. The regulation of circulation. VIII. The relative importance of nervous, endocrine and vascular regulation in the response of the cardiac output to anoxemia. Am. J. Physiol., 83:284-301.

1928 The effects of ether, chloroform, and ethyl chloride anesthetics on the minute cardiac output and blood pressure. An experimental study. Surg. Gynecol. Obstet., 46:72-78.

With Barney Brooks and G. S. Johnson. Ligation of the terminal abdominal aorta. An experimental study. Arch. Surg., 17:794-812.

1929 With G. V. Anrep and M. Hammouda. The distribution of the blood in the coronary blood vessels. J. Physiol., 67:87-96.

With Cobb Pilcher and Tinsley R. Harrison. Blood-flow through edematous extremities. An experimental study of the effects on the gaseous content and the volume-flow of blood of the injection of saline solution. Am. J. Physiol., 89:589-95.

With Hubert B. Bradburn. Trauma to central nervous system. Its effects on cardiac output and blood pressure. An experimental study. Arch. Surg., 19:725-34.

Oxygen content of blood in patients with varicose veins. Arch. Surg., 19:898-905.

With Hubert B. Bradburn. The relationship of changes in blood-flow through an extremity to (1) changes in temperature of tissues, (2) differences in oxygen content of the arterial and venous blood, and (3) cardiac output. Am. J. Physiol., 91:115-22.

1930

With Hubert B. Bradburn. Distribution of the blood in shock. The oxygen content of the venous blood from different localities in shock produced by hemorrhage, by histamine and by trauma. Arch. Surg., 20:26-38.

With P. N. Harris and Bentley Cox. Observation upon the exteriorized appendix of the dog. Surg. Gynecol. Obstet., 50:572-77.

Experimental shock. The cause of the low blood pressure produced by muscle injury to dogs. Arch. Surg., 20:959-96.

With John C. Burch and Tinsley R. Harrison. A comparison of the effects of hemorrhage under ether anesthesia and under spinal anesthesia. Arch. Surg., 21:693-97.

Observations upon the blood flow through skeletal muscle by the use of the hot wire anemometer. Am. J. Physiol., 95:554-60.

1931

Trauma to the intestines. The importance of the local loss of fluid in the production of low blood pressure. Arch. Surg., 22: 314-24.

Experimental shock. IV. The probable cause for the reduction in the blood pressure following mild trauma to an extremity. Arch. Surg., 22:598-609.

Experimental shock. VII. The importance of the local loss of fluid in the production of the low blood pressure after burns. Arch. Surg., 22:610-16.

With J. W. Beard. Experimental shock. VIII. The composition of the fluid that escapes from the blood stream after mild trauma to an extremity, after trauma to the intestines, and after burns. Arch. Surg., 22:617-25.

With George S. Johnson. Experimental shock. IX. A study of the effects of the loss of whole blood, of blood plasma and of red blood cells. Arch. Surg., 22:626-37.

With P. N. Harris. Experimental shock. X. Observations on the water content of the tissues of the body after trauma and after hemorrhage. Arch. Surg., 22:638-48.

Stab wound of heart. Ann. Surg., 93:1278-79.

With J. W. Beard and Virginia Butler. A study of the effects of division of the cervical esophagus of the dog. Surg. Gynecol. Obstet., 53:169-75.

With Virginia Butler and J. W. Beard. Experimental shock. XI. A study of the alterations in the volume of blood and in the water content of blood and of muscle that are produced by histamine. Arch. Surg., 23:848-54.

With G. S. Johnson. Experimental shock. XII. A study of the effects of hemorrhage, of trauma to muscles, of trauma to the intestines, of burns and of histamine on the cardiac output and on blood pressure of dogs. Arch. Surg., 23:855-63.

With J. W. Beard and G. S. Johnson. Experimental shock. A study of its production and treatment. J. Am. Med. Assoc., 97: 1794-97.

1932 With J. W. Beard. Intravenous injections. A study of the composition of the blood during continuous trauma to the intestines when no fluid is injected and when fluid is injected continuously. J. Clin. Invest., 11:249-65.

With J. W. Beard and Charles Thuss. Intravenous injections. A study of the effects on the composition of the blood of the injection of various fluids into dogs with normal and with low blood pressures. J. Clin. Invest., 11:267-90.

With J. W. Beard, Harwell Wilson, and B. M. Weinstein. A study of the effects of hemorrhage, trauma, histamine and spinal anesthesia on the composition of the blood when no fluids are injected and when fluids are introduced intravenously. J. Clin. Invest., 11:291-309.

With J. W. Beard. The effects on the composition of the blood of the subcutaneous injection of normal salt solution into normal dogs and into dogs subjected to intestinal trauma. Graded hemorrhages and histamine injection. J. Clin. Invest., 11: 311-25.

The effects of complete occlusion of the thoracic aorta. An experimental study. J. Thorac. Surg., 2:69-76.

1933 With J. W. Beard and Harwell Wilson. Effects on composition of blood of physiologic solution of sodium chloride when introduced by intraperitoneal injection and by stomach tube in the presence of low blood pressure. Arch. Surg., 26:122-33.

With Harwell Wilson, B. M. Weinstein, and J. W. Beard. Loss of protein from the blood stream. Effects of the injection of solution of pituitary and of epinephrine. Arch. Surg., 26:330-34.

Exposure of the heart to atmospheric pressure. Effects on the cardiac output and blood pressure. Arch. Surg., 26:516-21.

With W. M. Raymond. Studies on bronchial occlusion by the method of Adams and Livingstone. Surg. Gynecol. Obstet., 56:779-81.

With R. A. Daniel, Jr., and S. E. Upchurch. The absorption from traumatized muscles. Surg. Gynecol. Obstet., 56:1017-20.

Peritonitis. Effects on the administration of salt solution on the amount of fluid that accumulates in the peritoneal cavity. Arch. Surg., 26:1098-1102.

Effects of primary shock on cardiac output and blood pressure. Proc. Soc. Exp. Biol. Med. , 31:36-37.

1934 With G. V. Anrep and A. Samaan. The effect of muscular contraction upon the blood flow in the skeletal muscle. Proc. R. Soc. Ser. B, 114:223-45.

Shock. Further studies with particular reference to the effects of hemorrhage. Arch. Surg., 29:837-57.

Influence of exposure to cold and of deprivation of food and water on the development of shock. Arch. Surg., 29:1055-68.

1935 Experimental studies on the effects of the perforation of peptic ulcers. Surg. Gynecol. Obstet., 61:20-26.

The effect of total pneumonectomy on the position of the esophagus. Proc. Soc. Exp. Biol. Med., 32:1552-55.

With Sidney Burwell. Thoracic duct lymph pressure in concretio cordis. An experimental study. J. Lab. Clin. Med., 21:296-97.

With Gunnar Nystrom. Contributions to the technic of pulmonary embolectomy. J. Thorac. Surg., 5:169-88.

1936 With R. S. Cunningham and C. S. Robinson. The experimental production of chylothorax by occlusion of the superior vena cava. Ann. Surg., 104:359-64.

With Morton F. Mason. Observations on the blood flow and gaseous metabolism of the liver of unanesthetized dogs. Am. J. Physiol., 117:328-34.

With Tinsley R. Harrison and Morton F. Mason. Effects on blood pressure of injection of kidney extracts of dogs with renal hypertension. Proc. Soc. Exp. Biol. Med., 35:38-40.

1937 With Sanford E. Levy. Fractionation of the output of the heart and the oxygen consumption of normal unanesthetized dogs. Am. J. Physiol., 118:368-71.

With Sanford E. Levy, Morton F. Mason, and Tinsley R. Harrison. The effects of ureteral occlusion on the blood flow and oxygen consumption of the kidneys of unanesthetized dogs. Surgery, 1:238-42.

With Morton F. Mason and Tinsley R. Harrison. The direct determination of the renal blood flow and renal oxygen consumption of the unanesthetized dog. Am. J. Physiol., 118:667-76.

With Sanford E. Levy. The effect of hemorrhage, intestinal trauma and histamine on the partition of the blood stream. Am. J. Physiol., 118:734-38.

With C. S. Robinson, R. S. Cunningham, and Mary E. Gray. Experimental studies on lymphatic blockage. Arch. Surg., 34: 1049-71.

With Morton F. Mason and Ray Evers. Renal oxygen utilization of dogs with experimental hypertension. Proc. Soc. Exp. Biol. Med., 36:819-20.

With Sanford E. Levy. The effects of obstruction of the common bile duct on the portal blood flow and oxygen consumption. Surgery, 2:33-36.

With C. S. Robinson, R. S. Cunningham, Mary E. Gray, and B. Carl Rogers. Chylous effusions produced by experimental ligation of the superior vena cava. Chemical and cytologic studies. Arch. Pathol., 24:303-14.

With Tinsley R. Harrison, Morton F. Mason, and John R. Williams, Jr. Relation of kidneys to blood pressure. Effects of extracts of kidneys of normal dogs and of dogs with renal hypertension on blood pressure of rats. Arch. Int. Med., 60:1058-68.

1938 With C. Sidney Burwell. Chronic constrictive pericarditis. Physiologic and pathologic considerations. J. Am. Med. Assoc., 110: 265-71.

With Sanford E. Levy and Rudolph A. Light. The blood flow and oxygen consumption of the kidney in experimental renal hypertension. Am. J. Physiol., 122:38-42.

With Sanford E. Levy. The effects of unilateral nephrectomy on the renal blood flow and oxygen consumption of unanesthetized dogs. Am. J. Physiol., 122:609-13.

With Sanford E. Levy. Experimental attempts to prevent or abolish the hypertension that is associated with renal ischemia. Surgery, 3:899-903.

With Sanford E. Levy and Charles S. Robinson. The effect of altering the renal blood pressure and blood flow on the glomerular filtration of a transplanted kidney in unanesthetized dogs. Am. J. Physiol., 123:383-87.

1939 With Sanford E. Levy. A method for transplanting the adrenal gland of the dog with reestablishment of its blood supply. Report of observations. Ann. Surg., 109:84-98.

With Sanford E. Levy. Gradual complete occlusion of the coeliac axis, the superior and inferior mesenteric arteries with survival of animals: Effects of ischemia on blood pressure. Surgery, 5:175-78.

With Ralph D. Cressman. Experimental traumatic shock. Further studies with particular reference to the role of the nervous system. Surg. Gynecol. Obstet., 68:278-87.

With Ralph D. Cressman. Experimental hypertension. Effects of kieselguhr injection and of splanchnic stimulation. Proc. Soc. Exp. Biol. Med., 40:258-60.

With Sanford E. Levy and Ralph D. Cressman. Experimental hypertension. The effects of unilateral renal ischemia combined with intestinal ischemia on the arterial blood pressure. J. Exp. Med., 69:833-46.

With Sanford E. Levy. Experimental observations on the effects of connecting by suture the left main pulmonary artery to the systemic circulation. J. Thorac. Surg., 8:525-30.

With Morton F. Mason, Hugh J. Morgan, and S. S. Riven. Myasthenia gravis and tumors of the thymic region. Ann. Surg., 110: 554-61.

1940 With George O. Wood and Morton F. Mason. Studies on the effects of the inhalation of a high concentration of oxygen in experimental shock. Surgery, 8:247-56.

With Morton F. Mason and C. Sidney Robinson. Studies on the renal arterial blood pressure and the metabolism of kidney tissue in experimental hypertension. J. Exp. Med., 72:289-99.

With A. S. Minot. Plasma loss in severe dehydration, shock and other conditions as affected by therapy. Ann. Surg., 112: 557-67.

1941 With James R. Dawson and Ralph D. Cressman. Experimental hypertension and pregnancy in dogs. Am. J. Pathol., 17:31-38.

With Morton F. Mason. Blood and blood substitutes in the prevention and treatment of shock: With particular reference to their uses in warfare. Ann. Surg., 133:657-76.

With George O. Wood. Effects of uncomplicated hemoconcentration (erythrocytosis) with particular reference to shock. Arch. Surg., 42:1019-25.

With Morton F. Mason. A comparison of the effects of heat and those of cold in the prevention and treatment of shock. Arch. Surg., 42:1054-59.

Tumors of the thymic region and myasthenia gravis. Am. J. Surg., 54:149-50.

With Sidney Burwell. Chronic pericardial disease. Report of twenty-eight cases of constrictive pericarditis. Surg. Gynecol. Obstet., 73:433-61.

With A. McGehee Harvey, Frank R. Ford, and Joseph L. Lilienthal. The treatment of myasthenia gravis by removal of the thymus gland. Preliminary report. J. Am. Med. Assoc., 117:1529-33.

1942 With Mark M. Ravitch. An evaluation of blood and blood substitutes. Surg. Gynecol. Obstet., 74:348-52.

A comparison of the effects of local application of heat and cold in the prevention and treatment of experimental traumatic shock. Surgery, 11:356-59.

With George Duncan. The uniform production of experimental shock by crush injury. Possible relationship to clinical crush syndrome. Ann. Surg., 115:684-97.

With George W. Duncan. Shock produced by crush injury. Effects of the administration of plasma and the local application of cold. Arch. Surg., 45:183-94.

1943 Effects of lowering temperature of an injured extremity to which a tourniquet has been applied. Arch. Surg., 46:167-70.

Effects of morphine in experimental shock due to hemorrhage. Arch. Surg., 47:326-28.

1944 With Edwards A. Park. The surgical treatment of experimental coarctation (atresia) of the aorta. Ann. Surg., 119:445-56.

A comparison of the effects of continuous and of intermittent application of a tourniquet to a traumatized extremity. Arch. Surg., 48:489-90.

Thymectomy in the treatment of myasthenia gravis. Report of twenty cases. J. Thorac. Surg., 13:316-39.

With William P. Longmire, Jr., and George W. Duncan. The use of venous tourniquets as an aid to the diagnosis of incipient traumatic shock. Surg. Gynecol. Obstet., 79:434-37.

The utilization of oxygen by the brain in traumatic shock. Arch. Surg., 49:167-69.

1945 With Helen B. Taussig. The surgical treatment of malformations of the heart in which there is pulmonary stenosis or pulmonary atresia. J. Am. Med. Assoc., 128:189-202.

1946 Operative closure of the patent ductus arteriosus. Surg. Gynecol. Obstet., 82:113-14.

Effects of an artificial ductus arteriosus on experimental cyanosis and anoxemia. Arch. Surg., 52:247-52.

The surgical treatment of congenital pulmonic stenosis. Ann. Surg., 124:879-87.

1947 The use of shunt or bypass operations in the treatment of certain circulatory disorders including portal hypertension and pulmonic stenosis (Churchill Lecture). Ann. Surg., 125:129-41.

With Helen B. Taussig. Observations on the volume of the pulmonary circulation and its importance in the production of cyanosis and polycythemia. Am. Heart J., 33:413-19.

The technique of the creation of an artificial ductus arteriosus in the treatment of pulmonic stenosis. J. Thorac. Surg., 16: 244-54.

1948 With C. Rollins Hanlon. Complete transposition of the aorta and the pulmonary artery. Experimental observations on venous shunts as corrective procedures. Ann. Surg., 127:385-97.

With C. Rollins Hanlon. Interatrial septal defect. Its experimental production under direct vision without interruption of the circulation. Surg. Gynecol. Obstet., 87:183-87.

With R. J. Bing, J. C. Handelsman, J. Campbell, and H. Griswold. The surgical treatment and the physiopathology of coarctation of the aorta. Ann. Surg., 128:803-24.

1949 With Mark M. Ravitch. Aspiration of blood from pericardium in treatment of acute cardiac tamponade after injury. Further experience, with report of cases. Arch. Surg., 58:463-77.

1950 With C. Rollins Hanlon. The surgical treatment of complete transposition of the aorta and the pulmonary artery. Surg. Gynecol. Obstet., 90:1-15.

With Henry T. Bahnson. Aortic vascular rings encountered in the surgical treatment of congenital pulmonic stenosis. Ann. Surg., 133:356-62.

With Richard C. Clay. Congenital arteriovenous fistulas in the mandible. Surg. Gynecol. Obstet., 90:543-46.

With Richard F. Kieffer, Jr. Valvulotomy for the relief of congenital valvular pulmonic stenosis with intact ventricular septum. Report of nineteen operations by the Brock method. Ann. Surg., 132:496-516.

With Ray Heimbecker and Vivien Thomas. Experimental reversal of capillary blood flow. Circulation, 4:116-19.

1953 With Henry T. Bahnson and Robert D. Sloan. Splenic-portal venography. A technique utilizing percutaneous injection of radiopaque material into the spleen. Johns Hopkins Hosp. Bull., 92:331-45.

With Jerome Harold Kay and Vivien Thomas. The experimental production of high interventricular septal defects. Surg. Gynecol. Obstet., 96:529-35.

1954 With Thomas N. P. Johns. Mitral insufficiency: The experimental use of a mobile polyvinyl sponge prosthesis. Ann. Surg., 140:335-41.

1955 Our obligations and opportunities (Presidential address, American College of Surgeons, November 19, 1954). Bull. Am. Coil. Surg., 40:83-86.

1956 The nature of discovery (Presidential address, American Surgical Association). Ann. Surg., 144:289-303.

With Robert A. Gaertner. Experimental coarctation of the ascending aorta. Surgery, 40:712-17.

1957 With David C. Sabiston, Jr., and Jean P. Fauteux. An experimental study of the fate of arterial implants in the left ventricular myocardium. Ann. Surg., 145:927-38.

1958 With David C. Sabiston, Jr. Experimental ligation of the internal mammary artery and its effect on coronary occlusion. Surgery, 43:906-12.

With David C. Sabiston, Jr. Physiologic and anatomic determinants of coronary blood flow and their relationship to myocardial revascularization. Surgery, 44:406-23.

1959 With David C. Sabiston, Jr., J. L. Talbert, and L. H. Riley. Maintenance of the heart beat by perfusion of the coronary circulation with gaseous oxygen. Ann. Surg., 150:361-70.

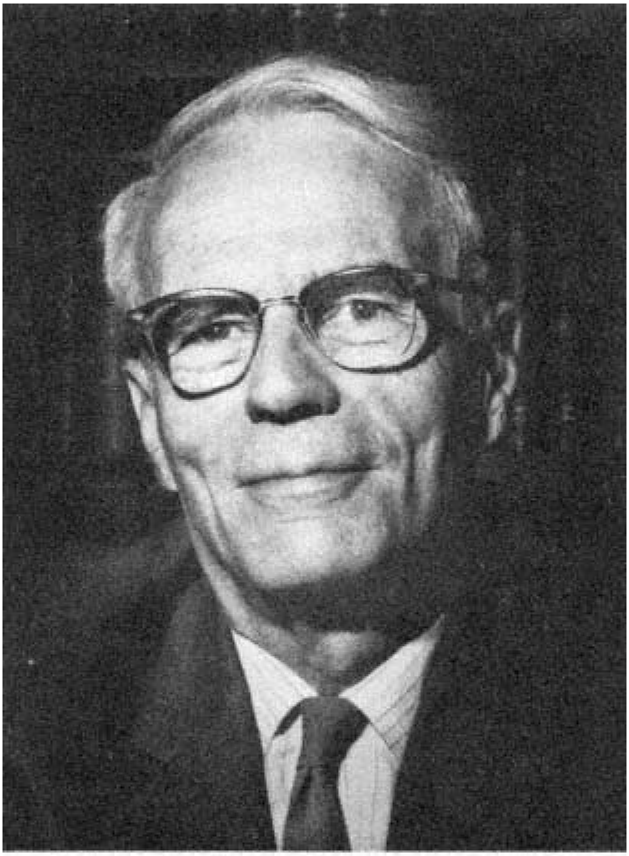
1961 With James L. Talbert, Lee H. Riley, Jr., and David C. Sabiston, Jr. An evaluation of gaseous oxygen perfusion as a method for maintaining renal viability during periods of complete ischemia. Surg. Gynecol. Obstet., 112:593-99.

With David C. Sabiston, Jr. Coronary thromboendarterectomy for angina pectoris. Postgrad. Med., 29:439-50.

1963 With David C. Sabiston, Jr., and Gary W. Archer. Fate of cells in passage through lymphatics and lymph nodes. Ann. Surg., 158:570-80.

1964 With Lazar J. Greenfield. Effect of low molecular weight dextran on survival following hemorrhagic shock. Surgery, 55:684-86.

Reminiscence: Shock after thirty-four years. Rev. Surg., 21:231-34.



Ira S. Bowen

Ira Sprague Bowen

December 21, 1898-February 6, 1973

by Horace W. Babcock

Ira Sprague Brown was one of the outstanding physicists and astronomers of the twentieth century. He was gifted with exceptional physical insight and with a compelling concern for fundamentals from which he seldom permitted himself to be diverted. As a pioneer in ultraviolet spectroscopy he discovered, with R. A. Millikan, evidence that led to the concept of electron spin in the vector model of the atom. He solved the long-standing mystery of the "nebulium" lines in the spectra of gaseous nebulae, showing that they were "forbidden" lines of ordinary elements. He was a master of applied optics who was responsible for successful completion of the 200-inch Hale Telescope and for many ingenious devices or optical systems that contributed enormously to mankind's observations of the universe.

Bowen was director of the Mount Wilson and Palomar Observatories for eighteen years. Here he took the lead in developing a major organization for research and education while at the same time closely supervising details of observatory operations. On a wider scale, he accomplished much to broaden the opportunities for astronomers generally and to increase the number and efficiency of astronomical facilities.

FAMILY BACKGROUND AND SCHOOLING

The Bowen family traces its beginning in New England to Richard Bowen, who left Wales and settled in Rehoboth, Massachusetts in 1643. During the Revolutionary War some members of the family were Tories and were forced to emigrate to Canada. Later they returned to Washington County, New York. Ira Bowen's great-grandfather, Aaron Bowen, pioneered in Steuben County, in the western part of the state. His grandfather, William H. Bowen, grew up on a farm in this region and married Juliza Cotton, whose family was likewise of New England origin and had pioneered in the same section of the state. After spending his early years on the farm, Ira's father, James H. Bowen, received his education at the local high school and at the Geneseo State Normal. He then became a preacher in the Wesleyan Methodist Church, a small denomination with fundamentalist doctrines and strict codes of conduct. James Bowen married Philinda Sprague, who had grown up in the same rural community of Haskinsville in Steuben County and had completed her education at the Geneseo State Normal.

Ira was born December 21, 1898 at Seneca Falls, New York, where his father was at the time pastor of the local church. Two years later the family, including Ira's older brother, Ward, moved to Millview, a small village in Sullivan County, Pennsylvania. While Ira was quite young, his father became business agent of the Wesleyan Methodist Church; the resulting responsibilities required frequent moves between Houghton and Syracuse, with the result that from 1905 to 1908 Ira did not attend school but was taught at home by his mother, who was a licensed teacher in New York State. Following the death of his father in 1908, the boy's education was continued at Houghton Wesleyan Methodist Seminary, where his mother had obtained a position as a teacher. She later became principal of the high school department.

During his high school years, Ira (or Ike, as he was known to his friends) took considerable interest in popular science as represented by *Popular Mechanics* and *Scientific American.* He also played with lenses, wires, and batteries to the extent permitted by the very limited family finances. He graduated from the high school in 1915 as valedictorian of a class of seventeen.

The first three years of Ike Bowen's college courses were in the junior college that formed part of Houghton Seminary. All of the courses in mathematics, physics, and astronomy were taught by the president, J. S. Luckey, who was a most effective teacher and who was largely responsible for the unusually high scholastic standards at the school. For these three years Bowen had charge of the laboratory of the high school physics course; the income earned in this way was used to pay his tuition.

His early interest in science deepened during Bowen's first college years. It was no doubt stimulated by the ingenuity required to devise suitable experiments with the limited equipment available, as well as by the formal courses. Following a connection established by Luckey, Bowen transferred to Oberlin College for his senior year and received the A.B. degree in June 1919. While at Oberlin he came under the direction of Professor S. R. Williams, whose sympathetic collaboration with his students in research projects was responsible for the continuation of many of these students in advanced study and research. In a project of this sort, Bowen studied the magnetic and magnetomechanical properties of samples of manganese steel supplied by Sir Robert Hadfield, with whom he eventually published the results in the *Proceedings of the Royal Society*. During this year he also assisted in one of the general physics laboratories and gave some time to the Students Army Training Corps, in which he had enlisted before the end of World War I.

In the fall of 1919, having been awarded a scholarship, Bowen took up graduate studies at the University of Chicago. In the two years that he remained there he attended all of the very comprehensive group of courses given by A. A. Michelson on classical physics and R. A. Millikan on modern physics, as well as many other courses in the department. These contacts, and the involvement in a major physics department during a period of extraordinary progress, undoubtedly had a deep and lasting influence. In later life Bowen insisted that research should be aimed incisively at a well-defined, fundamental problem; he was intent on understanding the basic physics and had little patience with mere data-gathering programs, which he characterized as "weather-bureau-type" activity.

RESEARCH AND TEACHING

At about the time of Bowen's arrival at the University of Chicago, Millikan's laboratory assistant, Dr. Ishida, announced his intention of leaving the University and returning to Japan. Bowen immediately accepted the offer of this position, which he took up on January 1, 1920. His first duties were to assist Ishida in the completion of his measurement of the viscosities of several gases by the oil-drop method. Upon Ishida's departure, however, Bowen was transferred to spectroscopic studies in the extreme ultraviolet using the vacuum spectrograph that had been developed by R. A. Sawyer and G. D. Shallenberger under Millikan's direction. At about this time significant improvements were introduced in the methods of ruling diffraction gratings, permitting extension of the shortward limit observable in the laboratory to about 150 angstroms. In the winter of 1920 and 1921 Bowen systematically photographed, in this newly available region, the spectra of most of the first twenty elements of the periodic table. The results were published jointly with Millikan in 1924. Many interesting surprises occurred in this first survey of the new region, such as the discovery that chemically pure aluminum and magnesium electrodes gave practically identical spectra in the region between 300 A and 1200 A. At first the investigators even considered attributing this finding to some transmutation of one element into another by the powerful condensed spark that was used. But more reflection and investigation showed that these common lines were due to oxygen, always present on the surface of these easily oxidizable metals. The difference in behavior in the new region and in the spectral regions previously explored results from the presence of all the strong lines of these metals in the older, long wavelength range.

In 1921 George E. Hale persuaded Millikan to move to the California Institute of Technology as chairman of its executive council and director of the Norman Bridge Laboratory of Physics, then nearing completion. Arrangements were made for Bowen also to make the move and to continue as Millikan's assistant in the new physics group at Caltech. One of the inducements offered by Hale was the proximity of the emergent scientific school to the Mount Wilson Observatory of the Carnegie Institution of Washington, where the largest telescopes in the world were being used by an active staff in a variety of investigations in astrophysics and cosmology. More specifically, Hale promised Millikan that diffraction gratings would be provided from the new ruling machine that had just gone into operation at the Pasadena headquarters of the Observatory.

During the first year after the move to Caltech, Bowen taught a course in general physics, using a lecture room in Throop Hall because the Norman Bridge Laboratory was still under construction. He also participated with Millikan in research on cosmic rays. The program involved the design and use of instruments carried to high altitudes by sounding balloons, the actual flights being made from San Antonio, Texas. The researchers obtained the first record from sounding balloons of cosmic rays and found definite evidence for an increase of intensity with altitude. Aerial observations had already been made by Hess and Kolhorster, but because they used manned balloons they were limited to lower altitudes. Bowen also participated with R. M. Otis in measurements of cosmic-ray intensity in the High Sierra of California. They used detectors that were lowered into the waters of mountain lakes at altitudes of some 12,500 feet, such that the water shielded the instruments from local radioactivity of the rocks. A love of the mountains stayed with Bowen all his life, but his principal research interests lay in spectroscopy, to which he soon returned.

With the completion of the physics laboratory, apparatus could be assembled for the continuation of the ultraviolet studies. An exceptionally fine grating was indeed provided by J. A. Anderson of the Mount Wilson Observatory. This grating gave much higher resolution than had hitherto been obtained in this region and made possible the studies of the fine structure of many lines in the extreme ultraviolet that were carried out by Bowen with the vacuum spectrograph in 1923 and 1924.

At about this time, Paschen and R. H. Fowler almost simultaneously made their analyses of highly ionized Al III and Si IV, and Bohr published his discussion of penetrating and nonpenetrating orbits. Applying these results to their new data, Bowen and Millikan found it possible to make an analysis of B III. From further studies made early in 1924 they were able to show that the so-called regular and irregular doublet laws, developed earlier for X-ray spectra, applied equally well to optical spectra when isoelectronic sequences (series of ions of the same electronic structure but differing nuclear charge) were used. This discovery at once made possible a direct correlation between optical and X-ray spectra and therefore between the atomic-structure formalisms developed from these two types of spectra. The results of this correlation constituted part of the evidence that later resulted in the introduction of the important concept of the spinning electron by Uhlenbeck and Goudsmit.

The doublet laws provided a very powerful tool for the analysis of highly ionized atoms. In 1925 and 1926 Bowen and Millikan applied these laws to the analysis of their new data and were able to obtain partial analyses of Be I, Be II, B II, B III, C III, C IV, P III, P IV, P V, S IV, S V, S VI, C1 V, CI VI, C1 VII, C1 VIII, Y III, and Zr IV. In this research, the heavier part of the load fell on Bowen, who produced and measured the spectrograms and analyzed the data. Millikan was exceedingly busy with the administration of the Institute and of the Norman Bridge Laboratory, as well as with a variety of other research efforts. He would occasionally drop in to keep in touch. When Bowen was ready, he would say to Millikan, "I've got an article. How about coming around tonight?"* Millikan would appear at about nine o'clock in Bowen's office, and the two would work until midnight writing the paper.

For several years after coming to Caltech, Bowen held the title of instructor and research assistant to the director of the Norman Bridge Laboratory of Physics. His teaching assignment was to instruct one of the undergraduate sections of twenty students in physics. In 1924 the practice was initiated of assigning the top men of the sophomore class to section A, the honor section, and Bowen was given this section. Much later he commented that "I never had quite such a run for my money. In the section were Ed McMillan, Robley Evans, and several others who later became heads of departments or university presidents. Keeping ahead of that group took quite some time."†

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* Interview with Charles Weiner, Center for the History of Physics, American Institute of Physics.

† *Ibid.*

Bowen continued with undergraduate teaching in physics until 1929, when he took over the teaching of graduate courses in optics and spectroscopy. He became assistant professor of physics in 1926, associate professor in 1928, and professor in 1931.

Under the pressures of research and teaching, Bowen found little time to proceed with the formal requirements for the Ph.D. degree, although he finally received it in 1926, by which time he had already published some twenty articles. Language examinations were required, and partly for this reason he took a month's vacation in the summer of 1925, spending some of the time reading Sommerfeld's *Atombau und Spectrallinien* in German. (He had already passed the French examination.) His thesis, somewhat surprisingly, was on the subject of "The Ratio of Heat Losses by Conduction and by Evaporation from Any Water Surface." This came about because Bowen had been assigned to guide the thesis work of another graduate student, an older man who had been with the weather bureau and who proposed to do a thesis on evaporation but later lost interest. Bowen's interest grew to the extent that he worked out a formula for the ratio of heat lost by evaporation and by conduction to the air, showing that this ratio can be determined uniquely from the temperature of the air, the temperature of the water, and the humidity. This quantity, known as the Bowen ratio, is to be found in the literature of meteorology and has been of use in oceanography. His ratio method is now commonly used to measure the evaporation from plant, soil, and water surfaces. As Bowen said later, "When I got ready to take my degree, that was the paper that was going to press, so it became my thesis."* His subject was undoubtedly a novel one for the faculty pundits—including P. Epstein, R. C. Tolman, and Millikan—who sat on his examining committee.

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* *Ibid.*

In the middle 1920's the vector model of the atom to account for complex spectra was developed by Russell, Saunders, Pauli, Hund, and others. Bowen applied this theory to the analysis of the more complex spectra of the elements in the first row of the periodic table, using again the data accumulated from the use of the high-resolution spectrograph. It was thus possible for him in 1926 to fix the low terms of C II, N III, O IV, N II, O III, F IV, 0 II, F III, F II, and F I. This, as it turned out, was preliminary to his most outstanding discovery, the identification of the so-called "nebulium lines" in the spectra of galactic nebulae. These two bright green lines had been a puzzle to spectroscopists since their discovery by Huggins some sixty years earlier. In parallel with the bright yellow line in the spectrum of the sun's corona, which had been attributed to an unknown element (helium) before the element was discovered on earth, it had been conjectured that nebulium was also an unknown but real element. By 1920, however, spectroscopy in the X-ray region had established the sequence of light elements. It was clear that there was no room here for an unknown, while the very strong nebulium lines could hardly be due to a rare element at the heavy end of the periodic table. Spectroscopists were generally aware of the problem and were alert to any leads that might provide a solution.

H. N. Russell of Princeton was knowledgeable about these matters. In 1927 the text of the classic *Astronomy* by Russell, Dugan, and Stewart appeared, in which Russell made the suggestion that "The nebular lines may be emitted only in a gas of very low density. This would happen, for example, if it took a relatively long time for an atom to get into the right state to emit them, and if a collision with another atom in this interval prevented the completion of the process. In such a case, it might require a great thickness of the very rarefied gas to emit these lines strongly enough to be visible" [p. 838].

Bowen bought the two volumes of *Astronomy* and thus became aware of Russell's summary. Later he related that one evening he came home from the laboratory at about nine o'clock and while preparing for bed was thinking about the energy levels of O II and O III and the "forbidden transitions." According to the theory, there was no way for the atom to get from the D or F states to the S (lowest or ground) state except through collisions. In a very rare gas, as in a nebula, the rate of collisions was insignificant. What, then, happens to these atoms? Are they stuck forever in the D and F states? Then it occurred to Bowen that, given enough time, perhaps the atoms can, in fact, make the "forbidden" jumps, although at a low rate.

Bowen quickly dressed and returned to his office. Since all the data on the energy levels were available in his records, it was easy for him to take the differences and to compute the wavelengths of the forbidden lines in a matter of minutes. There they were, correct to a hundredth of an angstrom! "I worked until midnight and had the answer when I went home,"* he said. The "nebulium" lines were in fact due to forbidden transitions between low-lying energy levels of singly and doubly ionized oxygen. The lines were intense because of the immense volume of gas at low pressure in the nebulae. The name "nebulium" could be laid to rest. The solution to the problem was widely acclaimed and brought well-deserved recognition to its author.

The initial discovery explained half a dozen of the strongest lines in the spectra of gaseous nebulae, but there were many other fainter lines that required years of work by Bowen and others; some were regular permitted lines of hydrogen and helium, but many were fainter forbidden lines of various elements. Bowen continued the work for years,

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* Interview with Charles Weiner.

bent on identifying the elements that might provide an explanation of the fainter lines and on solving the larger problem of determining the relative abundance of elements in the gaseous nebulae.

Bowen soon became interested in the possibility of fluorescence in nebulae. He noticed that the wavelength of the strong resonance lines of ionized helium (He II) in the 300-400 A region coincided within one- or two-hundredths of an angstrom with certain lines of O III. The fascinating possibility occurred to him that ultraviolet radiation from helium, in passing through a rarefied gas containing O III, could be expected to selectively populate certain energy levels in O III; this could give rise to peculiar enhancement of specific emission lines of the latter element.

In 1934 Bowen received a letter from W. H. Wright, director of the Lick Observatory, who was one of the chief observers of nebular spectra. New data from the ultraviolet were just becoming available. Wright mentioned that he had new nebular lines in the 3100-3300 A region and that the intensities of some were quite abnormal. In response to Wright's inquiry about the strange line intensities, Bowen was able to write back with the explanation. He had lacked the data until that time but had the solution in the form of the fluorescence mechanism. It is interesting to note that Wright's new ultraviolet data were made possible by the aluminum coating recently applied to the mirror of the 36-inch Crossley reflector. The great superiority of aluminium compared to silver as a reflective coating for telescope mirrors had resulted in the development by John Strong in Pasadena of the method for evaporative coating in a high vacuum; this development, closely related to the 200-inch telescope project, was quickly adopted for all large telescope mirrors.

Bowen accepted Wright's invitation to spend the summer term of 1938 at the Lick Observatory as a Morrison Associate.

With Arthur B. Wyse, he observed the spectra of gaseous nebulae. This was his first real observational work in astronomy. The work benefited from new, much faster panchromatic photographic emulsions that had just become available from the Eastman Research Laboratories, and the observers were able to discover numerous new emission lines. For many of these lines, Bowen had the identifications from his laboratory studies. The spectrograms made by Bowen and Wyse carried intensity calibrations, so that they were able to obtain quantitative results on line intensities. In this way they showed that the composition of the gaseous nebulae, i.e., the relative abundance of the elements, is about the same as that of the sun and stars. This statement includes the finding that hydrogen is by far the most abundant element, which Russell had already established for the sun.

In later years Bowen carried heavy responsibilities for administration, so he found little time for research. Nevertheless, he continued some work on the spectra of gaseous nebulae. With the large grating spectrograph at the coude focus of the 200-inch telescope, he made very significant improvements in the precision of the wavelengths of nebular lines, primarily because the resolution and dispersion of this instrument were far superior to those of the laboratory and observatory spectrographs used earlier for the ultraviolet studies from which the term differences were derived. He published this work in 1955, as well as a definitive contribution with L. H. Aller and R. Minkowski on the uniquely rich spectrum (263 lines) of the gaseous nebula NGC 7027.

In the course of his observational work, Bowen was impressed with the very long exposure times often required to obtain direct photographs or spectrograms of faint objects. It was known that the photographic emulsion, generally designed for exposure times of a fraction of a second, does not maintain a reciprocity between light intensity and exposure time for exposures measured in hours. He originated a pre-exposure procedure of baking plates for a specified time at an elevated temperature, showing that for many emulsions this would significantly increase the effective speed for long exposures. Astronomers were quick to adopt the baking technique, which often cuts exposure times to 50 percent or less; it has for years been standard procedure at major observatories.

The "image slicer" is an optical device originated by Bowen to improve efficiency in recording the spectra of stars or nebulae. Because the image of such an object may be large compared to the width of the spectrograph slit, much light may be lost. The image slicer, consisting of an array of several tiny, carefully shaped mirrors, effectively cuts the image into a series of narrow strips that are optically transposed into a single narrow strip that enters the slit with little loss.

THE 200-INCH TELESCOPE PROJECT

Mention has already been made of the activity in astronomy and astrophysics that was so prominent in the scientific life of Pasadena in 1921 when Bowen arrived from Chicago. The Carnegie Institution's 60-inch reflector had been in operation on Mount Wilson for thirteen years, the 100-inch Hooker telescope for three years. The researches of Kapteyn and of Shapley had opened new vistas on the structure of our Galaxy. Using the 100-inch, Hubble was soon to clinch the concept of a universe populated by countless galaxies like our own; the evidence for the expanding universe was on the horizon. Meanwhile, stellar spectroscopy was flourishing. This wave of progress was due in large measure to the quality and size of the Mount Wilson telescopes and to the excellent observing conditions provided by the site. While the optical quality of the mirrors was attributable to the skill of G. W. Ritchey, much of the telescopes' success, and in particular their mechanical design, was due to the Mount Wilson engineer and astronomer Francis G. Pease. It was clear that this sequence of large productive telescopes should not be allowed to end with the 100-inch. Pease went on to promote the design of a 300-inch telescope, for which in 1921 he produced drawings and a scale model introducing the concept of a large "horseshoe" for the main bearing of the polar axle. In 1928 George E. Hale, at that time honorary director of the Mount Wilson Observatory, successfully launched the project to build a 200-inch telescope and obtained from Rockefeller sources the funding for this great optical instrument that was destined to be installed on Palomar Mountain. As we shall see, the project was to be completed by Ike Bowen twenty-two years later.

The design of the 200-inch telescope was conducted at Caltech with the close collaboration of astronomers and engineers of the Carnegie Institution's Mount Wilson Observatory over a period of several years, beginning about 1930. The project was guided by the Observatory Council and by a Policy Committee of which Bowen was a member. His knowledge of optics and his aptitude for instrumentation were invaluable here, and, not surprisingly, his responsibilities rapidly increased as the work progressed. Among the important decisions in which he participated were the choice of the focal ratio of the primary mirror (f/3.3); the specification of a thin-section, ribbed disk of borosilicate glass; and the adoption of the Serrurier truss and of the horseshoe mounting with hydrostatic bearings. His influence was also strong in applications of the Schmidt camera, both for use in spectrographs and for sky-survey instruments such as the 18-inch and 48-inch wide-angle telescopes at Palomar. Indeed, the basic parameters of the 48-inch were due to him. Remarkable success was achieved with this telescope because the aperture, focal length, field size, and correcting-plate material were so specified that three crucial quantities-size of the typical stellar "seeing disk," optical aberrations, and limiting resolution of the emulsion (plate grain)-were equated, each being about 30 microns on the plate.

THE WAR YEARS

The completion of the 200-inch and 48-inch telescopes was delayed by the entry of the United States into World War II; this brought to a halt all work on the Palomar Observatory and resulted in drastic shifts in the activity of all concerned. Bowen accepted responsibility for exterior ballistics on the Caltech ordnance rocket project. This organization, which grew to large size and had an important impact on military operations, was headed by physicist Charles C. Lauritsen, with W. A. Fowler second in command. In close collaboration with military services, it was concerned with all phases of design, development, testing, and production of solid-fuel rockets for immediate use in the war. Bowen organized the photographic section and for nearly four years guided and participated in the field work and analysis needed to provide precise data on acceleration, stability, trajectory, blast effects, and other parameters. Thousands of rocket tests were monitored from the ground and from the air. On other wartime projects not connected with rockets, Bowen contributed to the development of high explosive devices by inventing cameras capable of cinematography at unprecedented rates. He also collaborated in experiments for measuring the transparency of seawater and the penetration of sunlight in the ocean.

In August 1945 Vannevar Bush, who headed the wartime Office of Scientific Research and Development, travelled from Washington to witness the explosion of the first nuclear bomb (the Trinity Test) in New Mexico; he continued on to the West Coast and, in his other capacity as president of the Carnegie Institution, stopped in Pasadena to tell Bowen that he had been appointed director of the Mount Wilson Observatory, to succeed Walter S. Adams on January 1, 1946. Thus began another and very different phase of Bowen's career, in which he effectively completed the transition from physicist to astronomer, and from researcher to director and administrator.

OBSERVATORY ADMINISTRATION

For the Carnegie Institution of Washington and the California Institute of Technology, the end of the war brought urgency to the matter of reorganizing and renewing their peacetime effort in astronomy. Bowen was responsible to the two institutions, and in 1948 he was appointed director of the combined Mount Wilson and Palomar Observatories. Most of the staff members were of an older generation. Hale, Pease, and Sinclair Smith had died in 1938. John A. Anderson, executive officer of the 200-inch telescope project, was in poor health and nearing retirement. The unfinished 200 inch mirror was in the optical shop in Pasadena. To Bowen fell the crucial task of guiding to completion the telescope project and of staffing and commissioning the Palomar Observatory. Further, a graduate school of astronomy had to be established at Caltech. It was necessary to plan and guide the main research programs to utilize to best advantage the new facilities that would soon be available, to encourage the application to astronomy of recent advances in nuclear physics, and to exploit the gains that were being made in technology.

To promote cross-fertilization of the two fields—stellar spectroscopy and nuclear physics—Bowen initiated a series of informal evening gatherings at his home overlooking lower Eaton Canyon. From time to time the group included such physicists as L. Blitzer, L. Davis, W. A. Fowler, R. B. King, C. C. Lauritsen, T. Lauritsen, H. P. Robertson, S. Rubin, and astronomers W. Baade, H. W. Babcock, P. W.

Merrill, R. Minkowski, R. Sanford, and 0. C. Wilson. If one can judge from the later contributions of some of the participants, the discussions that developed at these meetings were highly productive.

The main task for the 200-inch telescope was clearly defined: to extend the earlier investigations of the distribution of galaxies in space to the most extreme limits that could be reached and to measure velocities or redshifts in order to refine and extend the velocity-distance relation. The development of more precise methods for the photometry of very faint galaxies was a formidable task, but one that had to be faced. New and better spectrographs had to be provided. The wide-angle, 48-inch Schmidt telescope would be an essential companion instrument for survey purposes and for the study of clusters of galaxies.

To Bowen it was clear that research in stellar astronomy was ready to enter a quantitative phase. Much was known about the classification of spectra, and wavelength measurements could be made accurately, but the measurement of line intensity (equivalent width) was a difficult and generally inexact art. Yet line intensities were the keys to the abundance of the elements in stars, nebulae, and interstellar clouds. It was now evident that great advances were to be made in stellar structure, stellar evolution, and the study of nuclear reactions that produce heavy elements in stars. At many universities and research centers there would be theoreticians and interpreters eagerly demanding quantitative observational data; the instruments at Mount Wilson and at Palomar Mountain must be effectively used to help meet this need.

Bowen himself guided the final stages of polishing and figuring the 200-inch mirror. Such a mirror is extremely sensitive to the functioning of its support system, being subject to flexure that varies as the fourth power of the diameter and inversely as the square of the thickness. Of necessity, testing of the mirror in the optical shop on the Caltech campus had to be done with the mirror on edge. There was concern that upon being placed face up in the telescope, the outer rim of the mirror might sag, giving the figure a turned down edge. To avoid the possibility of having to refigure the whole mirror, it had been decided to leave an optically high zone some eighteen inches in width around the outer edge; after installation in the telescope, the mirror would be tested on stars, and the outer zone polished down to the extent required. This procedure was indeed followed after the mirror was placed in the telescope in 1948. The rather lengthy process was one of successive approximations. Bowen, using a Hartmann screen in front of the mirror, photographed the Hartmann patterns of bright stars. He then measured the plates in Pasadena and derived the results in terms of high and low areas of the mirror surface. Then the mirror on its support system was lowered to the floor of the dome where Donald O. Hendrix, the optician, carefully polished down the high areas, using a simple mechanism with small tools. After several iterations that required many months, the figure had been brought to a very satisfactory level such that 80 percent of the light of a star was concentrated within a circle 50 microns in diameter. The mirror was then aluminized, and the telescope was placed in regular service for observations at the prime focus and Cassegrain focus, beginning in 1950. The successful completion of the 200-inch Hale Telescope was undoubtedly one of Bowen's major achievements and one in which he inwardly took great and justifiable pride.

Observations at the coude focus awaited the construction of a large grating spectrograph. For this, the design was evolved by Bowen from the prototype developed by Adams and T. Dunham, Jr., for the 100-inch Hooker telescope on Mount Wilson. Bowen specified a very long focus (30-foot) collimator, to minimize losses at the slit. The resulting

12-inch beam demanded a larger diffraction grating than could be produced on a single blank. He was able to devise a method of mounting and adjusting four identical plane gratings that could be used as a composite. For the four interchangeable Schmidt cameras he introduced the "twice through" correcting plate, to be positioned just in front of the gratings. The spectrograph went into operation most successfully in 1951.

When the 48-inch Schmidt telescope was completed in 1948, the test photographs that were made demonstrated remarkable gains in astronomical photography. On each 14-inch square plate, 6.5 degrees on a side, were recorded vast numbers of stars and galaxies to the faint limiting magnitude of 20.3, with exposure times of only 12 minutes in the blue and 48 minutes in the red. (With more modern plates the limiting magnitude is substantially fainter.) Faint filamentary features such as supernova remnants, gaseous nebulae, and clusters of galaxies formerly beyond reach were now recordable with ease. The Schmidt camera of appropriate size and at a good site had outmoded all earlier sky survey instruments.

Bowen at once came under strong pressure from certain aggressive staff members to let them put the 48-inch to use for their own researches. He was convinced, however, that the telescope should first be used exclusively to complete a survey of the sky for the general good of astronomy. The region from the north pole to declination -30° could be photographed from Palomar Mountain on about 900 plates. With financial assistance from the National Geographic Society, Bowen organized the Palomar Sky Survey. Each of the 900 fields was to be photographed under good conditions on red and blue plates in immediate succession. Glass copies and paper prints produced under close quality control would be made available at cost to other observatories and research centers throughout the world. The Sky Survey was successfully carried through between 1949 and 1957 as a result of Bowen's firm administrative control. R. Minkowski supervised the work and personally approved the plates to be accepted. By 1979, 322 complete sets of prints and twenty glass copies of the Survey had been distributed worldwide.

The Palomar Sky Survey effectively enlarged manyfold the volume of the observed universe and formed the basis for several important catalogs and for almost countless research articles. It led to the optical identification of large numbers of radio sources and to catalogs of planetary nebulae, of supernova remnants, of galaxies, and of clusters of galaxies.

Following World War II the multiplier phototube revolutionized astronomical photometry for individual stars. The next step—the development of' image tubes, wherein the high quantum efficiency and other advantages of the photocathode might be applied to the recording and photometry of two-dimensional sky fields—was one that held great promise. Commercial television camera tubes were not suited to the low light levels encountered in astronomy, but if a relatively simple, reliable image tube could be developed, it would have wide application at many observatories. With the enthusiastic support of Bush, Bowen took the initiative in organizing the Carnegie Image Tube Committee, with Merle Tuve as chairman. The Committee, working with industrial laboratories and observatories over an interval of several years, developed a successful sealed, magnetically focused image tube that was produced in some quantity by the Radio Corporation of America; such tubes were widely adopted for use and remain to this day the instrument of choice in several systems. They provide the astronomer with a convenient image-amplifying device having a quantum efficiency of the order of 20 percent as compared to less than 1 percent for the photographic plate.

The ruling engine mentioned earlier had been superseded at Mount Wilson by a newer, more precise machine, and beginning in 1950 this machine began to produce sizeable gratings of a quality not available before. Many of these gratings were put to use at Mount Wilson and Palomar; dozens of others, some of large size and very high quality, were sold at cost or given away to various observatories and physical laboratories on a worldwide basis. This distribution was characteristic of Bowen's policy to broaden opportunities for the advancement of science. Not only were gratings given away to scientific groups, but the complete technology of ruling gratings at the Mount Wilson Observatory was freely disclosed in detail to three scientific companies interested in commercial production.

Bowen faced and solved many challenging problems during his tenure as director of the combined Mount Wilson and Palomar Observatories. The basic pattern for the proposed organization had been sketched years before by officers of Caltech and of the Carnegie Institution, and the plan was understood when Bowen agreed to take the directorship. It remained for him, in consultation with Caltech President Lee DuBridge and Carnegie Institution President Bush, to formalize the agreement for unified operation of the two observatories that was adopted by the trustees of both institutions in 1948. It was not possible for the observatory organization to have a separate corporate existence; ownership of the respective facilities was to be maintained by the two sponsoring institutions, and they required separate budgets. The director would be equally responsible to the two presidents. Research was to be conducted by one integrated scientific staff, together with guest investigators who would be invited to come from outside institutions. There was emphasis on education and on opportunities for young astronomers through research fellowships. Such an organization, with dual spon sorship, is rare, if not unique, in American science. It was no easy task for Bowen, over a period of eighteen years, to maintain a balance between the interests of the two institutions, however harmonious they were at the start.

Working with the Caltech administration, in 1948 Bowen expanded the academic group responsible for instruction in astronomy at Caltech; this group became part of the Division of Physics, Mathematics, and Astronomy, which was administratively separate from the Mount Wilson and Palomar Observatories. J. L. Greenstein accepted an invitation to come from the University of Chicago to join H. P. Robertson and F. Zwicky, with a dual appointment as professor of astronomy and staff member of the Observatories.

One of Bowen's principal aims as director of the Mount Wilson and Palomar Observatories was to ensure that the facilities, and especially the 200-inch telescope, would be administered and used at the highest level of efficiency and productivity. Practically the entire load of administration was carried by him personally, with a very minimum of assistance. He called on staff members for advice but only rarely requested that they perform special tasks, and then after careful consideration. Every effort was made to provide each astronomer with maximum time and freedom for research with support for long-term programs. In return, Bowen took it for granted that others would match his extraordinary capacity for work. Various phases of the rather complex observatory operations were conducted according to policies that he developed and applied uniformly. Some individuals from outside the organization occasionally found it difficult to understand and to adapt to what may have seemed to them rather rigid rules.

An accomplishment, perhaps insufficiently appreciated, resulted from Bowen's efforts to create observation opportunities for astronomers not connected with major observa tories. This was particularly important for many in other parts of the United States who lacked not only large instruments but also the good observing conditions that prevailed on parts of the West Coast. The guest investigator program of the Mount Wilson and Palomar Observatories, which Bowen developed and administered with great care, gave substantial opportunities to many, but it fell short of meeting the general needs. This became increasingly evident with the growth of the science during the 1950's, and more so after the opening of the space age. The answer was to promote the construction of more large telescopes at good sites by other organizations or agencies. This became one of Bowen's main interests, and to this cause he gave generously of his time and energy. Even before 1950, for example, he strongly supported the 120-inch telescope project of the University of California. This support included the transfer of much technical and engineering information; he also lent the services of Hendrix, the optician, to the Lick Observatory to oversee and advise on the figuring of the 120-inch primary mirror.

During the formative period of the National Science Foundation and, somewhat later, the creation of the major research facility that became the Kitt Peak National Observatory, Bowen's advice was frequently requested by Bush, Robert R. McMath, and many others. In his service on the National Astronomical Observatory Advisory Panel, he wisely insisted that the several sponsoring universities should be involved in early decisions as to specifications for the basic instrumental facilities of the new observatory.

Bowen's advice was sought by astronomers, telescope engineers, and instrument designers worldwide who visited Mount Wilson and Palomar Mountain for consultation, to inspect the instruments in detail, and to obtain plans and drawings from the engineering group. Many of the innovative features of the 200-inch Hale Telescope are to be found in other large telescopes subsequently constructed: the list includes the 6-meter reflector of the U.S.S.R., the Kitt Peak 84-inch and 150-inch telescopes, the 158-inch telescope of the Cerro Tololo Interamerican Observatory, the 153-inch Anglo-Australian telescope, the 140-inch telescope of the European Southern Observatory, and others too numerous to mention. Complete engineering drawings of the 48-inch Schmidt telescope at Palomar were given to other observatories, so that three or more near-duplicates are now productive in various parts of the world.

Among Bowen's later contributions are authoritative studies of the optical design of large reflectors and of spectrographs. In these articles, he showed that five meters is about the largest single passive primary mirror that can feasibly be constructed and supported, and he emphasized the extreme importance of selecting a site with excellent seeing for any large telescope. He went on to optimize the optical design of three very modern instruments that have been constructed since his retirement as director in 1964. These are the 60-inch reflector at Palomar Mountain (1969), the 40-inch Swope telescope (1970), and the 100-inch (2.5-meter) Irenee du Pont telescope of the Carnegie Institution's Las Campanas Observatory in Chile. The performance of this last instrument is especially noteworthy, for it yields at the Cassegrain focus a field of critically good definition, 2.1 degrees in diameter, with seeing-limited images over the whole of a single glass plate 50 centimeters on a side. Bowen and Vaughan accomplished this by adding a Gascoigne correcting lens to a Ritchey-Chretien system and by adopting a moderate concave bending of the plate. This highly successful design climaxed Bowen's contributions to the evolution of the wide-field, general-purpose telescope.

The classic treatment on the design of stellar spectro graphs for maximum efficiency is to be found in Bowen's 1962 article (see bibliography). He further improved spectrographs by devising several ingenious adaptations of the Schmidt camera, using solid or semisolid camera optics. Typically, only two days before his unexpected death, Ike discussed at lunch with several staff members a new spectrograph that they hoped he could design.

Bowen's work was characterized by penetrating physical insight, thoroughness, and integrity; it was generally held that when he provided the answer to a problem, that answer was right. Associates came to appreciate his inner enthusiasm and his satisfaction with solid results, but these qualities never blossomed into exuberance. Ike could be firm in his insistence on adhering to principles and procedures that had proved to be correct, but he was a most considerate and unselfish individual who held the deep respect and friendship of those who knew him well.

He was elected to the National Academy of Sciences in 1936.

Ira Bowen and Mary Jane Howard were married in 1929; there were no children. Mary Bowen pursued a career as a child psychologist. With her husband, she provided warm hospitality to numerous gatherings at their home in Altadena. Bowen himself read widely in history, especially the history of physics and astronomy, and he was a collector of rare and early editions of scientific books. He also had a substantial collection of ancient coins.

Many honors came to Ira Bowen during his lifetime. In the words of Caryl Haskins, "These were the formal tributes to a life of extraordinary service to science and to scientific organization and administration. But perhaps the most permanent of all will be the living inspiration, both professional and personal, that he brought to three generations of colleagues and students and associates, and their living regard and attachment for him and for his wife Mary."*

The author has had the benefit of biographical notes provided by the National Academy of Sciences, of a transcript of interviews from the American Institute of Physics, and of articles written by L. H. Aller, J. L. Greenstein, C. P. Haskins, A. McKellar, O. C. Wilson, and A. H. Vaughan.

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* *Yearbook, American Philosophical Society, 1973:* 117.

HONORS AND DISTINCTIONS

Degrees

A.B., Oberlin College, 1919
Ph.D., California Institute of Technology, 1926
Sc.D. (honorary), Oberlin College, 1948
Ph.D. (honorary), University of Lund, 1950
Sc.D. (honorary), Princeton University, 1953

Professional Appointments

Morrison Research Associate, Lick Observatory, 1938-1939
Director, Mount Wilson Observatory, 1946-1948
Director, Mount Wilson and Palomar Observatories, 1948-1964
National Astronomical Observatory Advisory Panel, 1953-1957

Professional And Honorary Societies

National Academy of Sciences, 1936
American Academy of Arts and Sciences, 1939
American Philosophical Society, 1940
Royal Astronomical Society, London (Associate), 1946-1973
Astronomical Society of the Pacific, President, 1948

Awards

Draper Medal, National Academy of Sciences, 1942
Potts Medal, Franklin Institute, 1946
Rumford Premium, American Academy of Arts and Sciences, 1949
Ives Medal, Optical Society of America, 1952
Catherine Wolf Bruce Gold Medal, Astronomical Society of the Pacific, 1957
Distinguished Service Staff Member, Carnegie Institution of Washington, 1964-1973
Henry Norris Russell Lecturer, American Astronomical Society, 1964
Gold Medalist and George Darwin Lecturer, Royal Astronomical Society, 1966

Bibliography

1921 With Sir Robert Hadfield and S. R. Williams. The magnetic mechanical analysis of manganese steel. Proc. R. Astron. Soc., 98:297-302.

With R. A. Millikan and R. A. Sawyer. The vacuum spark spectra in the extreme ultra-violet of carbon, iron, and nickel. Astrophys. J., 53:150-60.

1924 With R. A. Millikan. Extreme ultra-violet spectra. Phys. Rev., 23:1-34.

With R. A. Millikan. The series spectra of the stripped boron atom (B III). Proc. Natl. Acad. Sci. USA, 10: 199-203.

With R. A. Millikan. The fine structure of the nitrogen, oxygen, and fluorine lines in the extreme ultra-violet. Philos. Mag., 48:259-64.

With R. A. Millikan. The assignment of lines and term values in beryllium II and carbon IV. Nature, 114:380.

With R. A. Millikan. The extension of the X-ray doublet laws into the field of optics. Phys. Rev., 24:209-22.

With R. A. Millikan. Some conspicuous successes of the Bohr atom and a serious difficulty. Phys. Rev., 24:223-28.

1925 With R. A. Millikan. The significance of the discovery of the X-ray laws in the field of optics. Proc. Natl. Acad. Sci. USA, 11: 119-22.

With R. A. Millikan. A possible reconciliation of Bohr's interpenetration ideas with Sommerfeld's relativistic treatment of electron orbits. Philos. Mag., 49:923-35.

With R. A. Millikan. The series spectra of the stripped atoms of phosphorus (P V), sulphur (S VI), and chlorine (Cl VII). Phys. Rev., 25:295-305.

With R. A. Millikan. The series spectra of two-valence-electron and of three-valence-electron systems. Nature, 115:423.

With R. A. Millikan. The series spectra of two-valence-electron atoms of phosphorus (P IV), sulphur (S V) and chlorine (Cl VI). Phys. Rev., 25:591-99.

With R. A. Millikan. The series spectra of three-valence-electron

atoms of phosphorus (P III), sulphur (S IV) and chlorine (Cl V). Phys. Rev., 25:600-605.
With R. A. Millikan. New light on two-electron jumps. Proc. Natl. Acad. Sci. USA, 11:329-34.
With R. A. Millikan. Relations of PP' groups in atoms of the same electronic structure. Phys. Rev., 26:150-64.
With R. A. Millikan. Series spectra of the two-valence-electron atoms of boron (B II) and carbon (C III). Phys. Rev., 26:310-18.
1926 With R. A. Millikan. Stripped oxygen O VI, the PP' group in O V and new aluminum lines in the extreme ultra-violet. Phys. Rev., 27:144-49.
With R. A. Millikan. High frequency rays of cosmic origin I. Sounding balloon observations at extreme altitudes. Phys. Rev., 27:353-61.
The ratio of heat losses by conduction and by evaporation from any water surface. Phys. Rev., 27:779-87.
Vacuum spectroscopy. J. Opt. Soc. Am., 13:89-93.
With R. A. Millikan. Series spectra of beryllium, Be I and Be II. Phys. Rev., 28:256-58.
With S. B. Ingram. Wave-length standards in the extreme ultraviolet spectra of carbon, nitrogen, oxygen, and aluminum. Phys. Rev. 28:444-48.
With R. A. Millikan. The ionization potential of O II. Nature, 118:410.
With R. A. Millikan. Stripped yttrium (Y III) and zirconium (Zr IV). Phys. Rev., 28:923-26.
1927 The series spectra of boron, carbon, nitrogen, oxygen, and fluorine. Phys. Rev., 29:321-47.
Series spectra of ionized phosphorus P II. Phys. Rev., 29:510-12.
With R. A. Millikan. Energy relationships and ionization potentials of atoms of the first row of the periodic table in all stages of ionization. Proc. Natl. Acad. Sci. USA, 13:531-34.
With R. A. Millikan. Spectral relationships of lines arising from the atoms of the first row of the periodic table. Philos. Mag., 4:561-80.
The origin of the nebulium spectrum. Nature, 120:473.

The origin of the chief nebular lines. Publ. Astron. Soc. Pac., 39:295-97.

1928 The origin of the nebular lines and the structure of the planetary nebulae. Astrophys. J., 67:1-15.

The life of atomic states and the intensity of spectral lines. Proc. Natl. Acad. Sci. USA, 14:30-32.

Series spectra of chlorine, Cl II, Cl III, Cl IV, Cl V, and of Si II, P III, and S IV. Phys. Rev., 31:34-38.

Series spectra of potassium and calcium. Phys. Rev., 31:497-502.

Series spectrum of sodium Na II. Phys. Rev., 31:967-68.

With D. H. Menzel. Forbidden lines in the flash spectrum. Publ. Astron. Soc. Pac., 40:332-40.

1929 The presence of sulphur in the gaseous nebulae. Nature, 123:450.

With H. N. Russell. Is there argon in the corona? Astrophys. J., 69:196-208.

Additional lines in the spectra of C II and N II. Phys. Rev., 34:534-36.

1930 With R. A. Millikan. The significance of recent cosmic ray experiments. Proc. Natl. Acad. Sci. USA, 16:421-25.

The presence of neutral oxygen in the gaseous nebulae. Phys. Rev., 36:600-601.

1931 Spectrum of doubly ionized carbon C III. Phys. Rev., 38:128-32.

With R. A. Millikan. Similarity between cosmic rays and gamma rays. Nature, 128:582-83.

1932 Spectra of two- and three-valence-electron atoms, Si II, P III, S IV, P IV, and S V. Phys. Rev., 39:8-15.

Ionization of air by gamma rays as a function of pressure and collecting field. Phys. Rev., 41:24-31.

1933 With R. A. Millikan. Cosmic-ray intensities in the stratosphere. Phys. Rev., 43:695-700.

With R. A. Millikan and H. V. Neher. New high-altitude study of cosmic-ray bands and a new determination of their total energy content. Phys. Rev., 44:246-52.

The aberrations of the concave grating at large angles of incidence. J. Opt. Soc. Am., 23:313-15.

1934 The spectrum of fluorine, F II, F III, F IV. Phys. Rev., 45:82-86.

The spectrum of chlorine, Cl III, Cl IV, Cl V. Phys. Rev., 45:401-4.

The path of a secondary cosmic-ray particle in the earth's magnetic field. Phys. Rev., 45:349-51.

The presence of neon in the nebulae. Publ. Astron. Soc. Pac., 46:145-46.

The excitation of the permitted O III nebular lines. Publ. Astron. Soc. Pac., 46:146-48.

The chemical composition of the nebulae. Publ. Astron. Soc. Pac., 46:186-87.

The singlet lines of Cl IV. Phys. Rev., 46:377.

With R. A. Millikan and H. V. Neher. Very high altitude survey of the effect of latitude upon cosmic-ray intensities and an attempt at a general interpretation of cosmic-ray phenomena. Phys. Rev., 46:641-52. Also in: Papers and Discussions of the International Conference on Physics, London, 1:206-24.

Spectra of potassium, K IV and K V, and of calcium, Ca V and Ca VI. Phys. Rev., 46:791-92.

1935 The spectrum and composition of the gaseous nebulae. Astrophys. J., 81:1-16.

The low terms in Mn V and Fe VI. Phys. Rev., 47:924-25.

The extreme ultra-violet in astronomical sources. In: *Zeeman Verhandlungen,* pp. 55-62. The Hague: Martinus Nijhoff.

1936 The galactic nebulae. Scientia, 59:77-86.

Forbidden lines. Rev. Mod. Phys., 8:55-81.

With R. A. Millikan, S. A. Korff, and H. V. Neher. The latitude effect in cosmic rays at altitudes up to 29,000 feet. Phys. Rev., 50:579-81.

1937 With Everett F. Cox. Ionization of air by gamma-rays as a function of pressure and collecting field II. Phys. Rev., 51:232-34.

With R. A. Millikan and H. V. Neher. Measurement of nuclear absorption of electrons by the atmosphere up to 101° electron volts. Nature, 140:23.

With R. A. Millikan and H. V. Neher. The influence of the earth's magnetic field on cosmic-ray intensities up to the top of the atmosphere. Phys. Rev., 52:80-88.

The low terms in Cr III, Cr IV, Mn IV, and Fe V. Phys. Rev., 52:1153-56.

1938 With R. A. Millikan and H. V. Neher. New evidence as to the nature of the incoming cosmic rays, their absorbability in the atmosphere, and the secondary character of the penetrating rays found in such abundance at sea level and below. Phys. Rev., 53:217-23.

With R. A. Millikan and H. V. Neher. New light on the nature and the origin of the incoming cosmic rays. Phys. Rev., 53: 855-61.

The low terms in Co VI. Phys. Rev., 53:889-90.

The image-slicer, a device for reducing the loss of light at the slit of a stellar spectrograph. Astrophys. J.; 88:113-24.

With A. B. Wyse. Hypersensitization and reciprocity failure. Publ. Astron. Soc. Pac., 50:305.

With A. B. Wyse. New lines in the spectra of the gaseous nebulae. Publ. Astron. Soc. Pac., 50:348-49.

With R. Minkowski. Effect of collisions on the intensities of nebular lines. Nature, 142:1079-80.

1939 With B. Edlen. Forbidden lines of Fe VII in the spectrum of nova RR Pictoris (1925). Nature, 143:374.

With A. B. Wyse. The spectra and chemical composition of the gaseous nebulae NGC 6572, 7027, 7762. Lick Obs. Bull., 19:1-16.

1940 With L. T. Clark. Hypersensitization and reciprocity failure of photographic plates. J. Opt. Soc. Am., 30:508-10.

1946 Metric photography: Field equipment and operations. In: *Field Testing of Rockets,* pp. 47-89. Pasadena: California Institute of Technology.

With F. A. Jenkins. Transparency of ocean water. J. Opt. Soc. Am., 36:617-23.

Survey of the year's work at Mount Wilson. Publ. Astron. Soc. Pac., 58:329-40.

1947 With P. Swings. Relative intensities of the coronal and other forbidden lines. Astrophys. J., 105:92-95.

Excitation by line coincidence. Publ. Astron. Soc. Pac., 59:196-98.

Limiting visual magnitude. Publ. Astron. Soc. Pac., 59:253-56.

1948 Survey of the year's work at Mount Wilson. Publ. Astron. Soc. Pac., 60:5-17.

The abundance of oxygen in the sun. Rev. Mod. Phys., 20:109-12.

Survey of the year's work at Mount Wilson and Palomar Observatories. Publ. Astron. Soc. Pac. 60:353-65.

The telescope at work. In: *Palomar, June 3, 1948.* San Francisco: Grabhorn Press. Reprinted in: Griffith Obs., 12:121-23.

1949 The award of the Bruce Medal to Sir Harold Spencer Jones. Publ. Astron. Soc. Pac., 61:61-62.

Survey of the year's work at the Mount Wilson and Palomar Observatories. Publ. Astron. Soc. Pac. 61:243-53.

1950 The 200-inch Hale Telescope. Bull. Am. Acad. Arts Sci., 3(4):2-3.

Final adjustments and tests of the Hale Telescope. Publ. Astron. Soc. Pac., 62:91-97.
1951 Survey of the year's work at the Mount Wilson and Palomar Observatories. Publ. Astron. Soc. Pac., 63:5-16.
Palomar Observatory. Sci. Mon., 73:141-49.
With Paul Merrill. The spectrum of RS Ophiuichi in May 1951. Publ. Astron. Soc. Pac., 63:255-56.
With Paul Merrill. Forbidden lines in the spectrum of MWC 300. Publ. Astron. Soc. Pac., 63:295-96.
1952 The spectrographic equipment of the 200-inch Hale Telescope. Astrophys. J., 116:1-7.
Optical problems at the Palomar Observatory. J. Opt. Soc. Am., 42:795-800.
Some new tools of the astronomer. Observatory, 72:129-37.
Mount Wilson and Palomar Observatories (Reports of Observatories 1951-52). Astron. J., 57:184-85.
1953 Mount Wilson and Palomar Observatories (Reports of Observatories 1951-52). Astron. J., 58:259-61.
1954 The 200-inch Hale Telescope. Trans. Int. Astron. Un., 8:750-54.
Mount Wilson and Palomar Observatories (Reports of Observatories 1953-54). Astron. J., 59:353-56.
Edwin P. Hubble, 1889-1953. Science, 119:204.
1955 Wavelengths of forbidden nebular lines. Astron. J., 121:306-11.
Mount Wilson and Palomar Observatories (Reports of Observatories 1954-55) Astron. J., 60:296-99.
Astronomical spectrographs: Past, present, and future. In: *Vistas in Astronomy,* ed. Arthur Beer, vol. 1, pp. 400-406, London: Pergamon Press.
With L. H. Aller and R. Minkowski. The spectrum of NGC 7027. Astron. J., 122:62-71.

1956 Optics. Smithson. Contrib. Astrophys., 1:1-3.

Mount Wilson and Palomar Observatories (Reports of Observatories 1955-56). Astron. J., 61:336-41.

1957 Instrumentation at Mount Wilson and Palomar Observatories. Publ. Astron. Soc. Pac., 69:377-84.

1958 The Universe from Palomar. Griffith Obs., 22:69-78.

Astronomy in a changing world. In: *Frontiers in Science,* ed. E. Hutchings, Jr., pp. 289-94. New York: Basic Books.

1960 John August Anderson, 1876-1959. Publ. Astron. Soc. Pac., 72:94-96.

John A. Anderson, astronomer and physicist. Science, 131:649-50.

Wavelengths of forbidden nebular lines II. Astrophys. J., 132:1-17.

The 200-inch Hale Telescope. In: *Stars and Stellar Systems,* ed. G. P. Kuiper and B. M. Middlehurst, vol. 1, *Telescopes,* pp. 1-15. Chicago: Univ. of Chicago Press.

Schmidt Cameras. In: *Stars and Stellar Systems,* ed. G. P. Kuiper and B. M. Middlehurst, vol. 1, *Telescopes,* pp. 43-61. Chicago: Univ. of Chicago Press.

1961 Problems in future telescope design. Publ. Astron. Soc. Pac., 73:114-24.

1962 John August Anderson. In: *Biographical Memoirs,* 36:1-18. New York, Columbia Univ. Press for the National Academy of Sciences.

Robert Raynolds McMath (1891-1962). Yearb. Am. Philos. Soc., pp. 149-53.

Spectrographs. In: *Stars and Stellar Systems,* ed. W. A. Hiltner, vol. 2, *Astronomical Techniques,* pp. 34-62, Chicago: Univ. of Chicago Press.

1963 With L. H. Aller and O. C. Wilson. The spectrum of NGC 7027. Astrophys. J., 138:1013-17.

1964 Explorations with the Hale Telescope. Science, 145:1391-98.

Le choix de sites d'observatoires astronomiques (site testing), informal discussions. Int. Astron. Un. Symp. no. 19, ed. J. Rosch pp. 15-34. Paris: Gauthier-Villars.

Telescopes. Astron. J., 169:816-25.

1965 With James B. Kaler and Lawrence H. Aller. Spectrophotometric studies of gaseous nebulae. IV. The Orion Nebula. Astrophys. J., 141:912-22.

1966 With Lawrence H. Aller and James B. Kaler. Spectrophotometric studies of gaseous nebulae. VII. The ring planetary NGC 7662. Astrophys. J., 144:291-304.

Optimum thermal effects for large domes. In: *The Construction of Large Telescopes,* ed. D. L. Crawford, pp. 170-74. New York: Academic Press.

Control of thermal effects. In: *The Construction of Large Telescopes,* ed. D. L. Crawford, pp. 60-65. New York: Academic Press.

Statement of aims and limitations of the program. In: *The Construction of Large Telescopes,* ed. D. L. Crawford, pp. 4-7. New York: Academic Press.

With Bruce Rule. The Palomar 60-inch photometric telescope. Sky Telesc., 32:185-87.

1967 Future tools of the astronomer. (Darwin lecture.) Q. J. R. Astron. Soc., 8:9-22.

Astronomical optics. In: *Annual Review of Astronomy and Astrophysics,* ed. Leo Goldberg, David Layzer, and John G. Phillips, vol. 5, pp. 45-66. Palo Alto: Annual Reviews.

1968 Comments on accuracy and on optical tests and adjustments of the 200-inch Hale Telescope. In: *Symposium on Support and Testing of Large Telescope Mirrors,* pp. 8-9; 98-101. Tucson: Kitt Peak National Observatory and Univ. of Arizona.

1969 With E. W. Denison and M. Schmidt. An image tube spectrograph for the Hale 200-inch Telescope. In: *Advances in Electronics and Electron Physics,* ed. L. Marton, vol. 28, pp. 767-71. New York: Academic Press.

1972 National Geographic Society-Palomar Sky Atlas. In: *National Geographic Society Research Reports, 1955-1960 Projects,* pp. 27-32. National Geographic: Wash., D.C.

Astronomical instruments in the twentieth century. In: *Legacy of George Ellery Hale,* ed. Helen Wright, Joan N. Warnow, and Charles Weiner , pp. 239-49. Cambridge, Mass.: MIT Press.

1973 With Arthur H. Vaughan, Jr. The optical design of the 40-inch telescope and of the Irenee DuPont telescope at Las Campanas Observatory, Chile. Appl. Opt., 12:1430-34.

Milton Lasell Humason. Q. J. R. Astron. Soc., 14:235-37.

With Arthur H. Vaughan, Jr. "Nonobjective" gratings. Publ. Astron. Soc. Pac., 85:174-76.

In Press Telescopes and observational techniques. In: *Enciclopedia del Novecento,* ed. Guiseppe Bedeschi, vol. 6. Rome: Istituto Della Enciclopedia Italiana.

Ralph E. Cleland

Ralph Erskine Cleland

October 20, 1892-June 11, 1971

by Erich Steiner

Much of the work that led to the establishment of genetics as a distinct biological discipline involved studies of *Oenothera,* the evening primrose. The concept of mutation, which remains central to the dogma of genetics, had its origin in the observations of Hugo deVries on *Oenothera.* deVries's mutation theory was challenged, however, when it became clear that *Oenothera* exhibited a breeding behavior that did not conform to that of' other organisms. It took some thirty years before the genetic nature of *Oenothera* was fully explained. Ralph Cleland made a major contribution to the solution of this long-puzzling problem through his discovery of chromosomal ring formation at meiosis and the subsequent proof that it is the physical basis of the atypical breeding behavior of *Oenothera.*

Ralph Erskine Cleland was born in LeClaire, Iowa on October 20, 1892, the first child of Charles Samuel and Edith Collins Cleland. The family was of Scotch-Irish ancestry on both sides. Ralph's father, who spent his childhood on a farm in Minnesota, was a minister of the United Presbyterian Church; his mother came from a family of farmers in Ohio. When Ralph was one-and-a-half years old, his father accepted a call from a church in downtown Philadelphia, the pulpit of which he was to occupy for forty-five years. Charles

Cleland became active in denominational affairs, serving a term as moderator and spending many years as secretary of the Foreign Mission Board, an assignment that led him to travel extensively in Africa, the Middle East, and India.

Ralph grew up in a low-income, urban neighborhood where his playmates were largely children of factory workers. Ralph's father, however, insisted that the children spend their summers at a country cottage in Lehigh County, Pennsylvania. It was here that Ralph developed his interest in botany and natural history, spending much of his time roaming through the fields and woods.

In Philadelphia, Ralph attended Central High School where he was enrolled in the "classical" course. He considered his high school education unusual in that it was to some extent the equivalent of a college program. The curriculum was broadly liberal arts, the courses rigorous, and he was taught by men with recognized standing in their disciplines. He entered the University of Pennsylvania with a four-year scholarship, receiving advanced credit for some of his high school work. He selected classics as his major and history as a minor, but he also took several courses in botany. During his undergraduate years Ralph engaged in a number of extra-curricular affairs, including participation in plays, debating, sports, the editorial board of the yearbook, and membership in the literary society. He believed that the latter activity contributed in particular to his social development during the college years. Undergraduate honors included prizes in Greek and botany and election to Phi Beta Kappa.

In addition to his college studies and activities, he worked in the social programs of his father's church, an experience that impressed upon him the damaging social effects of alcohol and led him to become a lifelong teetotaler.

Upon graduation from the University, Cleland was offered an assistantship in the Department of Botany, even though his course preparation in biology was minimal. Nevertheless, by working summers, and with the aid of a Harrison Fellowship for two years, he was able to complete the requirements for the Ph.D. in three years. His doctoral research was carried out under the direction of Professor B. M. Davis, whose interests centered on cytological studies of the algae and the cytogenetics of *Oenothera.* Cleland chose to work on the cytological life history of a red alga, *Nemalion multifidum.* As a result, he spent his summers at Woods Hole where he was able to extend his acquaintance with other biologists. In July of 1918, upon mailing his thesis to be published, he returned home to find an order to report for induction into the military service. After five weeks of training, he was sent to France with a field artillery unit. Shortly after his arrival abroad, he was hospitalized with influenza. By the time he recovered, the armistice had been signed and he returned to the United States to be discharged from the army in April 1919.

Ralph Cleland's research career had its origin in a set of fortuitous circumstances. Shortly after his discharge from the army, he obtained an appointment as an instructor in biology at Goucher College, to begin in the fall of 1919. Without a commitment for several months, Cleland offered to assist Professor B. M. Davis with his research. Dr. Davis was at the moment interested in the cytology of the hybrid between a diploid and tetraploid race *of Oenothera* and turned the problem over to Cleland. Cleland chose *Oenothera franciscana,* a strain that happened to be at hand, for determining the best methods of fixation and staining. The study *of Oe. franciscana* proved to be of greater interest than a mere test of technique, since the cytological preparations revealed that four of the fourteen chromosomes regularly formed a closed circle at meiosis. This observation led to a series of studies by Cleland that were of major importance in elucidating the puzzling genetic behavior of *Oenothera,* a problem that had remained largely unsolved since it first came to light as a result of studies of the genus by Hugo deVries at the turn of the century.

Hugo deVries, a Dutch plant physiologist, initiated studies of variation in *Oenothera* in 1885, hoping to attack problems of evolution through an experimental approach. His results led to the publication in 1900 of *The Mutation Theory,** in which he proposed that evolution of new species occurred through sudden and spontaneous changes in one or more hereditary characters. Evidence for his theory came largely from observations of *Oenothera;* it was quickly apparent that the concept of mutation could only be valid if the strains alleged to be undergoing mutation were pure species. Were the strains of hybrid nature, then such variations, which deVries called mutations, could simply be recombinants. The problem arose because strains of *Oenothera* bred true when self-pollinated, but behaved as hybrids when outcrossed. It was thus essential to establish the purity of the *Oenothera* species before the mutation concept could be considered valid and thus significant for evolutionary theory.

The contradictory behavior of *Oenothera* remained a puzzle that attracted a great many investigators in the early 1900's, but their efforts met with little success until the meticulous genetic analysis of Otto Renner, published in 1917, which demonstrated that many of the oenotheras were permanent heterozygotes persisting in this condition because of balanced lethal factors. Ralph Cleland's study of *Oenothera franciscana* appeared in 1922 and was the first step toward the explanation of the physical basis of the mechanism revealed by Renner's brilliant analysis. The curious fact is that

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* H. deVries, *Die Mutationstheorie* (Leipzig: Von Veit; vol. 1, 1901; vol. 2, 1903); *The Mutation Theory* (English translation, Chicago: Open Court; vol. 1, 1909; vol. 2, 1910).

numerous cytological studies of *Oenothera* had been carried out during the previous two decades, yet no one had recognized that the formation of chromosome rings at meiosis was an unusual and constant feature of most of the *Oenothera* strains under investigation.

The paper on *Oenothera franciscana* does not place emphasis on the discovery of chromosomal ring formation; more attention is focused on the evidence for the purity of *Oenotherafranciscana* and the general importance of species purity for the *Oenothera* problem. The next paper published by Cleland appeared in the *American Naturalist* in 1923 and had an entirely different orientation. In the interim Cleland had examined the meiotic division in several other strains of *Oenothera* and discovered that each had a characteristic chromosomal configuration involving circles of various sizes. He noted that the adjacent members in a circle of chromosomes appear to go to opposite poles, an arrangement not likely to depend purely on chance. Further, if one assumes that homologous chromosomes go to opposite poles, then circle formation could explain the genetic results of Shull, who had concluded that all the genes in *Oenothera* belong to a single linkage group.

Cleland's studies of the following years extended the number of *Oenothera* strains examined. He established that a wide range of chromosomal configurations occur and each remains constant for a particular strain. Moreover, it was recognized that some mechanism must exist to give the regular arrangement of the chromosomes in the circle that leads to alternate segregation at the time of the division. Nevertheless, at this time Cleland still believed that the chromosomes in the circle were unpaired; thus he failed to recognize the cause of circle formation.

Not until the 1926 paper on meiosis in *Oenothera biennis* and *Oe. biennis sulfurea* did Cleland specifically cite Renner's work and utilize the balanced lethal concept to explain the genetic behavior of *Oe. biennis.* It is clear that by this time Cleland was fully aware of the direct relationship between the unique chromosomal situation and the atypical genetic behavior *of Oenothera;* a coherent hypothesis still remained to be developed, however.

The award of a Guggenheim Fellowship made it possible for Cleland, accompanied by his recent bride, Elizabeth, to spend the summers of 1927 and 1928 as well as the intervening academic year in Germany in collaborative efforts with Friederich Oehlkers, Otto Renner, and Hugo deVries. A major study aimed at correlating the chromosome configurations in various races *of Oenothera* and their hybrids with their breeding behavior was undertaken with Dr. Oehlkers. In this project Cleland assumed responsibility for the cytological work, while Oehlkers carried out the genetic studies. The preliminary results of the work were reported in the *American Naturalist* in 1929; this was followed by a full account in 1930 in the *Jahrbuch fur Wissenschaftliche Botanik.* These articles presented convincing evidence that races of *Oenothera* exhibiting a circle of fourteen chromosomes at meiosis transmitted the genes in single groups; the hybrids, on the other hand, showed diverse configurations at meiosis, but with each hybrid constant in its configuration. Further, the number of linkage groups was shown to be precisely correlated with the number of pairs and/or circles of chromosomes at meiosis. Here was rigorous proof of the correlation between gene and chromosome behavior. While the breeding behavior of *Oenothera* could now be understood in terms of its unique chromosomal mechanism, questions regarding the nature and distribution of the chromosomes still needed to be answered. Cleland did not offer an explanation for the formation of the chromosomal circles. He continued to consider the chromosomes within a circle as essentially unpaired and as examples of telosynapsis (association of the ends of chromosomes), a concept that later proved to be incorrect.

The explanation for circle formation was first suggested by John Belling in a 1927 paper dealing with various types of chromosome configurations observed in *Datura stramonium.*[*] Among these was a circle of four chromosomes, the members of which could be identified morphologically and thus recognized as nonhomologous. Belling reasoned that an exchange between two nonhomologous chromosomes had occurred, and that such a plant, possessing two original chromosomes and two that had interchanged segments, would exhibit a circle of four chromosomes as a result of their pairing requirements. The concept of segmental interchange[†] was the element needed to solve the last major question of the *Oenothera* behavior.

Further, the concept of segmental interchange led Cleland to recognize that chromosome configuration could serve as an index of similarity in segmental arrangement, pairs or small circles of chromosomes indicating that two complexes are identical or similar while a circle of fourteen chromosomes signifies the greatest degree of dissimilarity between the two complexes. This suggested that chromosomal end arrangement might be useful as a measure of phylogenetic relationship and thus lead to an understanding of the evolutionary history of the group. The genus *Oenothera* had long been a problem for taxonomists because of the large number of intergrading forms that hybridize freely. Cytogenetic analysis showed promise for identifying phylogenetic groupings, which then might be a basis for a more satisfactory taxonomic treatment of the genus.

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[*] J. Belling, "The Attachment of Chromosomes at the Reduction Division in Flowering Plants," *Journal of Genetics,* 18(1927): 177-205.

[†] More commonly called reciprocal translocation.

A study of collections from natural populations throughout California initiated this new direction of Cleland's work, which was to become the main thrust of his research for the remainder of his career. Following study of the California races, the work expanded into a cytogenetic analysis of large numbers of *Oenothera* collections from throughout North America; the objective was to understand the genetic and chromosomal structure of natural populations and thereby trace their evolution.

In 1938 Cleland left Goucher College to assume the chairmanship of the Department of Botany at Indiana University. Here his research program on *Oenothera* population structure gained momentum with the support of the Rockefeller Foundation and with the aid of various research associates and graduate students.

The progress of these studies was reported in a series of papers beginning in 1940 with "Analysis of Wild American Races of *Oenothera (Onagra),"* in which the various phylogenetic groups were identified and described in a provisional way. The subsequent work by Cleland and his associates concentrated on the cytogenetic analysis of over 300 collections and led to a more precise characterization of the different groups and their evolution. These studies, which extended over nearly thirty years, were summarized and reviewed in Cleland's book, *Oenothera: Cytogenetics and Evolution,* completed shortly before his death.

While the main thrust of Cleland's research in the latter years took the direction of *Oenothera* population studies, he also published a number of papers on such more-or-less unrelated genetic topics in *Oenothera* as chromosome structure and behavior, incompatibility factors, and the inheritance of cruciate petals. A number of these studies were carried out during his retirement years. It is noteworthy that Cleland's scientific contributions extended over a period of fifty years.

His papers were consistently significant. As new problems arose, he attacked them with success, focusing on the significant and avoiding the trivial.

Cleland's distinction in research brought him national recognition and subsequent election to leadership positions in various professional scientific societies as well as numerous honors and awards. He served on many national committees dealing with scientific matters. In 1950 he assumed the deanship of the Graduate School of Indiana University, at the same time continuing as chairman of the Department of Botany. He held both positions until his retirement from administrative duties in 1958. Cleland was a member of the National Academy of Sciences, the American Philosophical Society, and the American Academy of Arts and Sciences. He served as president of the Genetics Society, The Botanical Society of America, the American Society of Naturalists, and the Indiana Academy of Science. Other honors included the first John F. Lewis Award of the American Philosophical Society and the Golden Jubilee Merit Citation of the Botanical Society. He was a corresponding member of the Deutsche Botanische Gesellschaft and an honorary member of the Genetics Society of Japan and the Botanical Society of Korea. Cleland held honorary degrees from Hanover College, the University of Pennsylvania, and Indiana University.

Cleland possessed an unusual combination of personal characteristics that undoubtedly played an important role in the professional achievement he attained. In some respects he seemed a shy man, yet he possessed a quiet self-assurance that enabled him to present his research conclusions effectively at a scientific meeting, even as a young scientist whose work was just getting under way. In the early years, when he undoubtedly carried a relatively heavy teaching load, he devoted his summers to research, and to growing, gathering, and preparing material for study throughout the academic year. His consistently significant research output was undoubtedly the result, not only of long hours, but of a persistence and steady application to the task at hand. Nevertheless, he was always available to his students; he never gave evidence of resentment or impatience at being interrupted. His calm, unruffled personality enabled him to shift from one activity to another with a minimal lag effect, using his time efficiently. In later years, because of his involvement in national scientific affairs, he traveled to Washington on a regular schedule and was often away from his laboratory as much as he was there. He arranged his teaching schedule to adapt to these demands, not infrequently returning to campus only moments before his lecture.

Cleland had a high regard for the academic way of life, and particularly for creative scholarship. The fact that his three sons all chose academic careers was a great satisfaction to him. He was a conscientious teacher who set high standards for his students. He willingly taught at the introductory as well as at the graduate level. Although not a charismatic speaker, he was nevertheless articulate, and his lectures were well organized, accurate, and up-to-date.

While many high achievers in science frequently have fragile egos requiring continuous nurture, Cleland obtained his satisfactions from an inner conviction that his contributions were significant and sound. He was essentially a modest person, readily approachable, lacking any trace of an exaggerated sense of self-importance. He was always willing to do a menial task when it was expedient. His concern and consideration for others were shown not only in his personal relationships but also in his support of programs that would contribute to the well-being of the community as a whole. He remained an active church member throughout his life. While on occasion his students may have considered him a bit straight-laced because of his opposition to smoking and alco hol, he nevertheless held their respect and admiration. Ralph and Elizabeth Cleland lived without ostentation. Their home radiated a warm and comfortable atmosphere. They both had a positive, optimistic, and cheerful attitude toward life with an enthusiastic involvement in the affairs of the University, the community, and the world as a whole. This persisted even after Mrs. Cleland became an invalid.

Cleland had a deep interest in music. For many years he was a regular member of a group that assembled in Alfred Kinsey's home to listen to music in a serious way. Whenever possible he attended the concerts and was proud of the quality of the musical offerings at Indiana University. It was thus highly appropriate that the memorial service following his death took the form of a concert.

After retiring from administrative posts at the age of sixty-five, Cleland returned to teaching until complete retirement at seventy. Subsequently he continued his research, pursuing various problems that earlier had lower priority in his research program. Ralph Cleland was the last survivor of the investigators who had played a major role in the unravelling of the *Oenothera* problem and who had had direct contact with most of the prominent workers in the field initiated by Hugo deVries. It was thus particularly important that he write a book reviewing the massive *Oenothera* literature and providing an up-to-date critical summary of the *Oenothera* work. In typical fashion, the project moved ahead on schedule and the manuscript was completed only a few days before he was stricken with a heart attack in his laboratory.

HONORS AND DISTINCTIONS

Degrees

A.B., University of Pennsylvania, 1915; M.S., 1916; Ph.D., 1919

Sc.D. (honorary), Indiana University, 1970

LI.D., Hanover College, 1957

Academic Positions

Goucher College: Instructor of Biology (1919-1920); Assistant Professor (1920-1923); Associate Professor (1923-1930); Professor (1930-1938); Chairman of Department (1937-1938)

Indiana University: Professor and Chairman, Botany Department (1938-1958); Dean of Graduate School 1950-1958)

Instructor, University of Michigan, summer 1920

Instructor, Marine Biological Laboratory, summer 1925

Editorial Positions

Editor, *Plant Cytology, Biological Abstracts,* 1925-1972

Trustee, *Biological Abstracts,* 1943-1948

Editor-in-Chief, *American Journal of Botany,* 1940-1946

Editorial Board, *American Journal of Botany,* 1946-1953

Awards

Phi Beta Kappa

Sigma Xi

First John F. Lewis Award, American Philosophical Society, 1937

Golden Jubilee Merit Citation, Botanical Society of America, 1956

Guggenheim Fellowship, 1927-1928; Renewed, 1928

Professional And Honorary Affiliations

Fellow, American Association for the Advancement of Science (Council at various times; Vice-President and Chairman of Section G, 1944)

Fellow, Indiana Academy of Science (President, 1959)

Fellow, American Academy of Arts and Sciences

Member, Botanical Society of America (President, 1947)

Member, Genetics Society of America (Vice-President, 1955; President, 1956)

Member, American Society of Naturalists (Secretary, 1938-1940; President, 1942)

Member, Society for Study of Evolution

Member, International Society for Cell Biology

Member, American Philosophical Society

Member, National Academy of Sciences

Honorary Foreign Member, Genetics Society of Japan

Honorary Life Member, Botanical Society of Korea

Corresponding Member, Deutsche Botanische Gesellschaft

Organizing Committee, Member of Governing Board, and first Chairman (1948-1949), American Institute of Biological Sciences

Chairman, Division of Biology and Agriculture, National Research Council, 1948-1951

Chairman or Member of many NRC committees, including: UNESCO Committee; Maize Committee; Kimber Award Committee; Agricultural Board; NRC, NSF, and Fulbright Fellowship panels or boards; Pacific Science Board; Advisory Committee, Office of Scientific Personnel (Chairman); Advisory Committee, International Organizations and Programs, Office of Foreign Secretary

Member, Advisory Committee to Selective Service, 1951-1953

Member, U.S. National Commission for UNESCO, 1958-1960

Consultant, National Science Foundation, 1952-1959

Secretary-Treasurer, Association of Graduate Schools of the Association of American Universities, 1955-1958

Chairman, American Delegation to 7th International Botanical Congress, Stockholm, 1951

Member, American Delegation to 9th International Genetics Congress, Bellagio, 1953

Member, American Delegation to General Assembly, International Union of Biological Sciences (IUBS), Nice, 1953

Vice-President, IUBS, 1953-1959

President, Genetics Section, 8th International Botanical Congress, Paris, 1954

Sent with Farrington Daniels by the National Academy of Sciences to Southeast Asia as "Scientific Ambassador," 1960; visited thirteen countries during three-month trip. Also served as consultant for the Asia Foundation

Bibliography

1917 A new Erythrotrichia from Woods Hole. Rhodora, 20:144-45.
1919 The cytology and life history of *Nemalion multifidum.* Ann. Bot., 33:323-52.
1922 The reduction division in the pollen mother cells of *Oenothera franciscana.* Am. J. Bot., 9:391-413.
1923 Chromosome arrangements during meiosis in certain oenotheras. Am. Nat., 57:562-66.
1924 Meiosis in pollen mother cells of *Oenothera franciscana sulfurea.* Bot. Gaz., 77:149-70.
1925 Chromosome behavior during meiosis in the pollen mother cells of certain oenotheras. Am. Nat., 59:475-79.
1926 Meiosis in the pollen mother cells of *Oenothera biennis* and *Oenothera biennis sulfurea.* Genetics, 11:127-62.
Cytological study of meiosis in anthers of *Oenothera muricata.* Bot. Gaz., 82:55-70.
1928 The genetics of *Oenothera* in relation to chromosome behavior, with special reference to certain hybrids. Z. Induk. Abstamm. Vererbungsl., Suppl. Bd., 1:554-67.
1929 Meiosis in the pollen mother cells of the oenotheras, and its probable bearing upon certain genetical problems. Proc. Int. Congr. Plant Sci., 1:317-31.

Chromosome behavior in the pollen mother cells of several strains of *Oenothera lamarckiana* . Z. Induk. Abstamm. Vererbungsl., 51: 126-45.

With F. Oehlkers. New evidence bearing upon the problem of the cytological basis for genetical peculiarities in the oenotheras. Am. Nat., 63:497-510.

Die Zytologie der *Oenothera*-Gruppe *biennis* in ihrem Verhältnis zur Vererbungslehre. Tübinger Naturwiss. Abhandlungen, 12: 50-55.

1930 With A. F. Blakeslee. Interaction between complexes as evidence for segmental interchange in *Oenothera.* Proc. Natl. Acad. Sci. USA, 16:183-89.

With A. F. Blakeslee. Circle formation in *Datura* and *Oenothera.* Proc. Natl. Acad. Sci. USA, 16:177-83.

With F. Oehlkers. Erblichkeit und Zytologie verschiedener Oenotheren und ihrer Kreuzungen. Jahrb. Wiss. Bot., 73:1-124.

1931 Cytological evidence of genetical relationships in *Oenothera.* Am. J. Bot., 18:629-40.

The probable origin of *Oenothera rubricalyx* "Afterglow" on the basis of the segmental interchange theory. Proc. Natl. Acad. Sci. USA, 17:437-40.

With A. F. Blakeslee. Segmental interchange, the basis of chromosomal attachments in *Oenothera.* Cytologia, 2:175-233.

1932 Further data bearing upon circle formation in *Oenothera,* its cause and genetical effect. Genetics, 17:572-602.

1933 Predictions as to chromosome configuration, as evidence for segmental interchange in *Oenothera.* Am. Nat., 67:407-18.

With O. Renner. Zur Genetik und Cytologie der *Oenothera chicaginensis* und ihrer Abkömmlinge. Z. Induk. Abstamm. Vererbungsl., 66:275-318.

1934 With W. H. Brittingham. A contribution to an understanding of crossing over within chromosome rings in *Oenothera.* Genetics, 19:62-72.

1935 Chromosome configurations in *Oenothera (grandiflora* x *lamarckiana).* Am. Nat., 69:466-68.

Cytotaxonomic studies on certain oenotheras from California. Proc. Am. Philos. Soc., 75:339-429.

Hugo deVries, 1848-1935. J. Hered., 26:289-97.

1936 Hugo deVries. Proc. Am. Philos. Soc., 76:240-50.

Peculiarities of chromosome and breeding behavior in the evening primrose. Teach. Biol., 6:119-24.

Some aspects of the cytogenetics of *Oenothera.* Bot. Rev., 2:316-48.

1937 Species relationships in *Onagra.* Proc. Am. Philos. Soc., 77:477-542.

William Harding Longley. Science, 85:400-401.

1940 David M. Mottier. Science, 91:423-24.

Analysis of wild American races of *Oenothera (Onagra).* Genetics, 25:636-44.

1942 The origin of ^{b}*decipiens* from the complexes of *Oenothera lamarckiana* and its bearing upon the phylogenetic significance of similarities in segmental arrangements. Genetics, 27:55-83.

1943 John Muirhead Macfarlane. Yearb. Am. Philos. Soc., 1943:408-11.

1944 The problem of species in *Oenothera.* Am. Nat., 78:5-28.

1946 With M. Newcomb. The growth of *Oenothera* plants from embryos cultured *in vitro.* Proc. Indiana Acad. Sci., 55:36.

With M. Newcomb. Aseptic cultivation of excised plant embryos. Science, 104:329-30.

1947 Possible advantages of cooperation between societies in publication. Science, 105:567-68.

1949 Phylogenetic relationships in *Oenothera.* Proc. 8th Int. Congr. Genetics. Hereditas SV: 173-88.

A botanical nonconformist. Sci. Mon., 68:35-41.

1950 Studies in *Oenothera* cytogenetics and phylogeny. Introduction and general summary. Indiana Univ. Publ. Sci. Ser., 16:5-9.

The origin of *neo-acuens.* Indiana Univ. Publ. Sci. Ser., 16:73-81.

With B. L. Hammond. Analysis of segmental arrangements in certain races of *Oenothera.* Indiana Univ. Publ. Sci. Ser., 16:10-72.

With L. B. Preer and L. H. Geckler. The nature and relationships of taxonomic entities in the North American euoenotheras. Indiana Univ. Publ. Sci. Ser., 16:218-54.

1951 Extra diminutive chromosomes in *Oenothera.* Evolution, 5:165-76.

1952 With K. B. Raper, R. E. Buchanan, P. R. Burkholder, et al. Culture collections of microorganisms. Science 116:179-80.

1954 Evolution of the euoenotheras: the strigosas. Proc. 9th Int. Congr. Genetics. Caryologia 6 (Suppl.): 1139-41.

Evolution of the North American euoenotheras: the strigosas. Proc. Am. Philos. Soc., 98:189-213.

Charles Elmer Allen. Yearb. Am. Philos. Soc., 1954:392-93.

1955 In union there is strength. Hanover Forum, 1:26-47.

The use of material. Science, 121:519-23.

1956 Cytology: the study of the cell. Am. J. Bot., 43:870-81.

The International Union of Biological Sciences. Am. Inst. Biol. Sci. Bull., 6:10-11.

Analysis of trends in biological literature-plant sciences (Address at 30th anniversary celebration of Biological Abstracts). Bio. Abstr., 30:2459-62.

1957 Chromosome structure in *Oenothera* and its effect on the evolution of the genus. Cytologia, 22 (Suppl.):5-19.

The International Genetics Symposia in Japan. Am. Inst. Biol. Sci. Bull., 7:23-24. Biological sciences. Science, 125:943.

Bradley Moore Davis (1871-1957). Yearb. Am. Philos. Soc., 1957: 113-17.

The distinctive characteristics of American higher education. In: *Conference on Education and Student Life in the U.S.,* pp. 1-10. Ann Arbor, Mich.: Conference Board of Associated Research Councils.

1958 Bradley Moore Davis. Genetics, 43:1-2.

The evolution of the North American oenotheras of the *biennis* group. Planta, 51:378-98.

1959 Professional and educational problems of concern to biology and medicine. Q. Bull. Indiana Univ. Med. Cent., 21:50-56.

1960 A case history of evolution. Proc. Indiana Acad. Sci., 69:51-64.

The S-factor situation in a small sample of an *Oenothera (Raimannia) heterophylla* population. Z. Vererbungsl., 91:303-11.

1961 Otto Renner (1883-1961). Yearb. Am. Philos. Soc., 1961:166-71.

Supply and demand in relation to the Ph.D. Plant. Sci. Bull., 7:1-3.

1962 Plastid behavior in North American euoenotheras. Planta, 57:699-712.

1963 With B. B. Hyde. Evidence of relationship between extra diminutive chromosomes in geographically remote races of *Oenothera.* Am. J. Bot., 50:179-85.

1964 The AIBS in retrospect. Bioscience, 14(3):6.

The evolutionary history of the North American evening primroses of the *biennis* group. Proc. Am. Philos. Soc., 108:88-98.

1966 Otto Renner 1883-1961. Genetics, 53:1-6.

Review, *Natural Features of Indiana.* Indiana Mag. Hist., 62:345-46.

1967 Further evidence bearing upon the origin of extra diminutive chromosomes in *Oenothera hookeri.* Evolution, 21:341-43.

The origin of closed circles of five chromosomes in *Oenothera.* Am. J. Bot., 54:993-97.

1968 Science: boon or bane? Proc. Indiana Acad. Sci., 77:84-93.

Cytogenetic studies on *Oenothera,* subgenus *Raimannia.* Jpn. J. Genet., 43:329-34.

1970 The missing petal character in *Oenothera* and its relation to the cruciate character. Am. J. Bot., 57:850-55.

1972 *Oenothera: Cytogenetics and Evolution.* New York: Academic Press.

W. D. Coolidge

Photograph courtesy of General Electric Research Laboratory

William David Coolidge

October 23, 1873-February 3, 1975

by C. G. Suits

Tungsten, x-rays, and Coolidge form a trinity that has left an indelible impression upon our life and times. The key word in this triad is Coolidge, for his work brought the element tungsten from laboratory obscurity to the center of the industrial stage and gave the X-ray a central role in the progress of medicine throughout the world.

William David Coolidge was born in Hudson, Massachusetts, near Boston, on October 23, 1873, and he died on February 3, 1975 in Schenectady, New York. His father, Albert Edward, was a shoemaker by occupation, but he supplemented his income by running a farm of seven acres. His mother, Martha Alice, was a dressmaker in her spare time.

Will attended a grade school about a mile from town, where one teacher presided over the six grades. He was a good student and was liked by his classmates. After school each day, as an only child of his parents, he had a regular routine of farm chores. This, however, left room for fishing (summer and winter), baseball, hiking, skating, and primitive skiing. Photography became a lifelong hobby, and during this period he built a basement darkroom and constructed his own camera, including the shutter.

After grade school Will attended Hudson High School where, in due time, he graduated valedictorian in his class of thirteen. En route, he quit school for a while and took a job in a local factory manufacturing rubber garments. After a few months he decided that this was not a very good idea, and he went back to school, where he caught up with his class without difficulty. He had assumed that, with very limited family financial resources, he would not be going to college at the end of the school year. His plans changed when a friend who had been impressed by his scholastic record and his mechanical and electrical aptitudes suggested that he might be able to obtain a state scholarship for MIT. He applied, the grant was awarded, and in the fall of 1891 he went to Boston to continue his studies.

At the time MIT was "Boston Tech" and consisted of three buildings that accommodated 1,200 students. The period was one of growing interest in science and engineering, and the opportunities for engineering graduates were numerous in industry. Except for the Military Academy at West Point, MIT was the only institution of learning then offering an engineering degree.

Will enrolled in electrical engineering, which included some chemistry and mathematics and a modicum of literature, modern languages, and philosophy, in addition to professional engineering courses. In the chemistry course he came under the instruction of Professor Willis R. Whitney, which turned out to be the start of a long and happy relationship. Will was an excellent student, especially in his laboratory assignments and in his practical shop work, and the shops at Boston Tech were better than anything he had ever seen before. To see what industry was like, he spent the summer between his junior and senior years at the East Pittsburgh plant of Westinghouse Electric. Illness kept him out of school for a year, so he graduated with the class of 1896.

By this time he sensed that engineering practice was not exactly what he wanted; he had a greater interest in his science studies and the research orientation of his laboratory work. He therefore took a position as an assistant in physics at MIT. During the year he became aware of the possibility of obtaining a fellowship that would permit graduate study in Europe. He applied and obtained a grant for the following year, and he selected Leipzig for graduate work, influenced by the counsel of Professor Whitney, who had done graduate work there, and by the presence of Professor Paul Drude at that institution. The scholarship would not cover all of the costs of European graduate study, but Will was able to obtain a loan from a friend.

Will arrived in Liepzig well in advance of the fall term, and he audited the physics lectures of Professor Gustav Wiedemann, who advised him not to formally register until the fall term in October. In the interim, he set out to improve his German by talking to German students at every opportunity, by avoiding contacts with English and American students, and even by attending German church services. He lived with a German family who gave him a constant opportunity to talk in German. All of this—board and room—cost $20.00 a month!

When the October term started, Will developed close relationships with Drude and Wiedemann. Both were interested in his research and often dropped in to see him and to discuss their progress. During vacations Will took short trips to Italy and Bavaria, where he covered every tourist opportunity at very low cost, taking photographs that he developed in an improvised darkroom in Leipzig.

In Will's second year at Leipzig, he became lecture assistant to Drude, which helped his finances and provided a new experience. Looking ahead to the time when he might finish his doctorate, he wrote to MIT concerning a teaching position there. Meanwhile his research had progressed well, and he started to assemble his dissertation, which was published later in *Annalen der Physik.*

One day during that winter, the celebrated Professor Wilhelm C. Roentgen visited Leipzig and the Physikalishes Institute. Drude's assistant—Coolidge—had a chance to talk with Roentgen and was much impressed by the experience. Will didn't know it at the time, but his later research would serve to provide the major embodiment for the practical usefulness of Roentgen's X-ray discovery.

Later in the second school year, Will decided that, with luck, he might complete his dissertation and tackle his doctoral examinations, the basic requirements for a degree, by late summer. In July he received high marks in all of his examinations and was awarded the doctorate summa cum laude.

His application for an MIT teaching position coincided with an opening in the Physics Department, so Will Coolidge was back in Boston for the fall term in 1899. The following year he became a research assistant to Professor Arthur A. Noyes of the Chemistry Department, where, to his surprise, he remained for five years. In an adjacent laboratory, he became reacquainted with Dr. Whitney, who was then commuting to Schenectady during the formative years of the new General Electric Research Laboratory there. To Coolidge's complete surprise, Whitney offered him a job. He visited GE and accepted the offer in 1905.

The new Research Laboratory was located in an ancient building in the Schenectady plant, and at that time the total employment was about thirty, including several MIT graduates. The new laboratory's growth rate was limited by the availability of people of the quality Whitney wanted. At that early period, persuading a university scientist that he might have a career in industrial research was not accomplished easily nor often. Whitney, however, was developing an academic atmosphere, including weekly research meetings where members reported upon their work and occasionally heard talks by invited scientists. The laboratory was also achieving a modicum of credibility and prestige in its industrial setting because of the success of its early work. Dr. Whitney's improvements in the lamp filament were coming to market at about the time Dr. Coolidge joined the laboratory. This lamp—called the GEM lamp—was about three times more efficient than Edison's lamp, and it alone more than paid for the company's investment in the Research Laboratory up to that time.

Dr. Coolidge was devoted to his research work, but not to the exclusion of social contacts. Letters to his parents at that time made it clear that he was enjoying his friendships with Dr. and Mrs. Whitney and numerous colleagues in the Research Laboratory, and that he had met a number of young ladies. One of these ladies was especially attractive, and on December 30, 1908 he married Ethel Woodward, the daughter of the president of a local bank, in Granville. A daughter, Elizabeth, and a son, Lawrence, were born to this marriage. Early in 1915 Ethel became seriously ill and died at the hospital in February of that year. Dorothy Elizabeth MacHaffie, a graduate nurse from Ellis Hospital, was engaged by Will to help his mother with the two children at home. Dorothy was a charming person and, about a year later, she and Will were married.

Lamp research and experimentation were proceeding apace in the U.S. and in Europe during that period, and it is not surprising that Coolidge caught some of the excitement. Welsbach, of gas mantle fame, produced a lamp with a filament of osmium. The powdered metal was extruded with a binder, then sintered and mounted in the bulb. The resulting lamp was extremely fragile. The same process was used with tantalum powder with similar results. Just and Hanaman, in Vienna, used the same process to produce a tungsten filament. The resulting lamp showed greatly improved light production, but the problem of brittleness remained.

Dr. Coolidge first got into the lamp filament problem by way of tantalum, but he quickly switched to tungsten. Meanwhile, General Electric purchased rights under the Just and Hanaman patent, and Dr. Whitney himself started making tungsten filaments by that method. Coolidge found that these sintered filaments would lose some of their extreme brittleness if they were passed through a rolling mill with heated rolls. This was the first clue that suggested that tungsten was not necessarily brittle under all physical circumstances. Coolidge's observation was a very important "foot in the door." After three more years of painstaking research on this intractable metal, a process was developed by means of which tungsten was made sufficiently ductile at room temperatures to permit drawing through diamond dies. Close control of working temperatures, of tungsten powder grain size, and of trace metal additions, particularly thorium, contributed to the final successful result.

Lamps made with ductile tungsten filaments appeared on the market in 1911, and they have dominated the lighting industry ever since. All of the numerous alternative lamp filament processes were abandoned. Needless to say, Whitney, Coolidge, and the new Research Laboratory gained great stature as a result of this work.

Another very important happening at about this time was the occasion, in 1909, when Irving Langmuir joined the new laboratory. He came from Göttingen by way of Stevens Institute, and his doctoral thesis had concerned heat transfer in gases at high temperatures. The lamp filament involved such processes, and Langmuir soon set up experiments that showed that the light output of Coolidge's new lamp could be doubled if inert gas replaced the high vacuum. This gas-filled lamp with a ductile tungsten filament was about ten times more efficient than Edison's lamp, and it soon became the standard of the world for indoor lighting. At about this time, Coolidge was appointed assistant director of the Research Laboratory. In 1914 he was awarded the Rumford Medal of the American Academy of Arts and Sciences, the first of a long series of medals and honors that marked his career (see appended list.)

The availability of tungsten as a workable metal was a new fact of industrial life that came from Coolidge's work, and the application to the incandescent filament was only the first use of this remarkable metal. Tungsten exhibits the highest melting point in the periodic table, extremely low vapor pressure, great mechanical strength, and many other unusual properties. Its application to a great variety of industrial uses proceeded apace. Because of its high melting point and good electrical conductivity, Coolidge explored its use as an electrical contact for switching devices. At that time plantinum was a favored material for electric contacts in telegraph keys, relays, and small control equipment. It was questionable whether tungsten would be suitable for this purpose because, unlike platinum, it oxidizes readily at high temperatures. For many types of contacts, however, tungsten performed very well and showed much greater contact life than platinum. Coolidge made a trip to Dayton to show the new contacts to Charles Kettering, who became very enthusiastic about tungsten for auto ignition contacts. Ever since, tungsten has been the material of choice for this application.

Roentgen had announced his discovery of X-rays in 1895, and this important event created worldwide interest, especially among medical men who saw the X-ray as a possible diagnostic tool. While Coolidge was still at Boston Tech, he worked with Dr. F. H. Williams, one of the pioneers in the medical application of the new tube, and Coolidge retained an interest in X-rays when he came to Schenectady in 1905. Perhaps it was the success of the replacement of platinum with tungsten in contacts that kindled a new interest in the X-ray tube, which then employed a platinum anode.

The early X-ray tube was full of gas and its operation was very erratic, even in the hands of a skilled practitioner. As Coolidge got into the X-ray tube study, he found that the three principal parts—the cathode, the anode, and the "vacuum" environment—were all sources of erratic performance. The gas was required to produce ions, which produced electrons by bombardment of a cold aluminum cathode. Langmuir was then in the midst of a comprehensive study of electron thermionic emission, and he found that he could get controllable electron emission from one of Coolidge's hot tungsten filaments in the complete absence of gas, in other words at high vacuum. Coolidge immediately installed a heated tungsten filament in an X-ray tube with a tungsten disk anode. This tube was heated and outgassed until all evidence of gas ionization disappeared. The tube became the first stable and controllable X-ray generator for medical and dental use, and it rapidly replaced the gas-filled tubes in this country and throughout the world.

Dr. Coolidge was in touch with many physicians and radiologists during the progress of his X-ray studies, and one of them, Dr. Lewis G. Cole of New York, was the first to have his office equipped with the new tube. He was extremely enthusiastic about the performance of this tube, and he soon sponsored a dinner in a New York hotel where Coolidge demonstrated the new tube to a large group of prominent radiologists. At this dinner, Dr. Cole christened the new generator the "Coolidge Tube," which was later adopted by the General Electric Company as the product name, and it has since been used widely by the medical and dental professions.

The success of the Coolidge Tube brought much recognition and many new honors to its inventor. It greatly expanded the use of X-rays, not only in dentistry and medicine, where therapeutic as well as diagnostic applications grew, but in industry, where they were being used increasingly for nondestructive testing. For many years following the introduction of the Coolidge Tube, Coolidge himself was in the midst of continuing refinement of this generator: to very high voltages for deep therapy applications, to higher power for industrial use, and to finer definition for improved diagnostics. To the end of his career he retained an intense interest in X-rays and their applications.

In 1917 it became evident that the involvement in World War I by the U.S. was unavoidable. The GE Research Laboratory and Dr. Whitney became increasingly concerned with the possible role they could play in such an event, and development of a submarine detection system was an obvious challenge. Allied shipping was being sunk at a far greater rate than it could be replaced, and some solution of this problem was urgently needed. The depth bomb was an effective weapon *if* the submarine could be located, which was the key problem.

Prior to the entry of the U.S. into the war, the GE Research Laboratory became involved in war work through the Naval Consulting Board, on which Dr. Whitney served. A joint attack on the problem of submarine detection was planned involving GE, the Submarine Signalling Company, and Western Electric. An experimental station was set up on the Mohawk River, near where the GE Research and Development Center was located years later. Coolidge soon found that sealed rubber binaural listening tubes provided excellent range of about two miles with an azimuth sensitivity of about five degrees. This device went into service on U.S. and British vessels as the "C" Tube—for Coolidge. A later version, the

"K" tube, developed a range of ten miles with an azimuth sensitivity often degrees. These devices permitted submarine chasers to clear the Mediterranean of submarines in the spring and summer of 1918 and were an important factor in the final outcome of the war. The Coolidge tube was adapted to a field X-ray unit for use in World War I, and it became a major medical tool in field hospitals, where many practitioners became acquainted with it for the first time.

In the period following World War I, the Research Laboratory under Whitney grew in stature and influence, both within the company and in the scientific community. Langmuir's work on electron emission and surface chemistry found many important applications, including radio broadcasting and reception. Albert Hull was one of three scientists (with Debye and Scherrer) to develop X-ray diffraction in crystalline materials. His studies of gas-filled electron tubes helped open up the field of industrial electronics. Coolidge continued to expand the usefulness of X-rays by the development of million-volt, high-power generators for medical therapeutic work and multiple industrial uses. The year 1932 was an important year for the laboratory, for Coolidge became director upon the retirement of Whitney, and in the same year Langmuir became the first American industrial scientist to win the Nobel Prize.

By the time World War II broke out, the appreciation of the role of science and technology in the national defense establishment was well developed, and through the leadership of Dr. Vannevar Bush, a massive national research and development program was mounted to aid the war effort. The Office of Scientific Research and Development identified the areas of opportunity; organized the effort in university, industrial, and government laboratories; and provided the necessary financial backing. Coolidge became involved in the atomic bomb investigation from the begin ning as a member of President Roosevelt's Advisory Committee on Uranium. In 1940 Dr. A. O. Nier of the University of Minnesota and Drs. K. H. Kingdon and H. C. Pollock of the GE Research Laboratory isolated U235 for the first time, and showed that it was the fissionable isotope. This author became a member of Division 13 of the NDRC (microwave radar) and chairman of Division 15 (radio and radar countermeasures), and both subjects became active areas for the participation of the GE Research Laboratory in the war effort.

Coolidge had planned to retire about the time World War II began in Europe, but because of the pressure of wartime work he agreed to stay on beyond his normal retirement. At the war's conclusion he resumed his plans for retirement, and he proposed that I succeed to his position, which I did on January 1, 1945. In retirement, Coolidge retained an active interest in X-ray research. He continued to receive recognition in the form of awards and medals for the impressive work of his career, even through his one-hundreth birthday, and he continued the photography hobby that dated from his boyhood in Massachusetts.

Although some of the milestones in Will Coolidge's remarkable career have been suggested above, this biography would be incomplete without words of appreciation for his personal qualities, which were equally impressive. Kindness and thoughtfulness in dealing with friends and associates were attributes that were deeply imbedded in his nature. I doubt if anyone ever heard him raise his voice in anger. His modesty was almost embarrassing, and he always viewed the accomplishments of his associates more generously than they themselves. He was greatly beloved by everyone who was privileged to be associated with him, and in the world of science, including medical science, he was regarded with deep reverence, as evidenced by the unprecedented award from the University of Zurich of a Doctorate of Medicine.

Will Coolidge was blessed with remarkable health throughout his very active lifetime, and he retained a keen mind into his late nineties. He died on February 3, 1975, at the age of one-hundred-and-one. We revere his memory.

REFERENCES

Baxter, James Phinney III. *Scientist Against Time.* Boston: Little, Brown, 1946.

Birr, K. A. *Pioneering in Industrial Research.* Washington, D.C.: Public Affairs Press, 1957.

Broderick, John. *Willis Rodney Whitney.* Albany, N.Y.: Ft. Orange Press, 1945.

Coolidge, William D. "Autobiography." Typescript. N.Y.: Center for the History of Physics, American Physical Society.

Hammond, John Winthrop. *Men and Volts.* Boston: Lippincott, 1941.

Hawkins, Lawrence A. *Adventures into the Unknown.* N.Y.: William Morrow, 1950.

Liebhafsky, Herman. *William David Coolidge.* N.Y.: John Wiley & Sons, 1974.

Miller, John Anderson, *Yankee Scientist.* Schenectady, N.Y.: Mohawk Development Service, 1963.

Suits, C. Guy. *Speaking of Research.* N.Y.: John Wiley & Sons, 1965.

Suits, C. Guy, and Way, Harold E., eds. *The Collected Works of Irving Langmuir.* N.Y.: Pergamon Press, 1962. 12 vols.

Westervelt, Virginia Veeder. *The World Was His Laboratory: The Story of Willis R. Whitney.* N.Y.: Julian Messner, 1964.

HONORS AND DISTINCTIONS

Medals and Awards

1914

Rumford Medal, American Academy of Arts and Sciences, for his invention of ductile tungsten.

1926

Howard N. Potts Medal, the Franklin Institute, in consideration of the originality and ingenuity shown in the development of a vacuum tube that has simplified and revolutionized the production of X-rays.

Louis Edward Levy Gold Medal, the Franklin Institute, for his paper on "The Production of High Voltage Cathode Rays Outside the Generating Tube."

1927

Gold Medal, the American College of Radiology, in recognition of his contribution to radiology and the science of medicine.

Hughes Medal, the Royal Society, London, for his work on the X-rays and the development of highly efficient apparatus for their production.

Edison Medal, the American Institute of Electrical Engineers, for his contributions to the incandescent electric lighting and the X-ray arts.

1932

Washington Award, the Western Society of Engineers, in recognition of devoted, unselfish, and preeminent service in advancing human progress.

1937

John Scott Award, the City Trusts of the City of Philadelphia, based on his application of a new principle in X-ray tubes.

1939

Faraday Medal, the Institution of Electrical Engineers of England, for notable scientific or industrial achievement in electrical engineering.

1940

Modern Pioneer Award, the National Manufacturer's Association, awarded to Dr. Coolidge as "A Modern Pioneer."

1942

Duddell Medal (18th), the Physical Society of England, in recognition of his invention of the Coolidge X-ray tube.

Orden al Merito, the Chilean Government, for his many services to civilization.

1944

Franklin Medal, the Franklin Institute, in recognition of his contributions to the welfare of humanity, especially in the field of the manufacture of ductile tungsten and in the field of improved apparatus for the production and control of X-rays.

1952

K. C. Li Medal and Award (first recipient), Columbia University, for meritorious achievement in advancing the science of tungsten.

1953

Henry Spenadel Award, the First District Dental Society, for distinguished and significant contributions to dentistry.

1963

Roentgen Medal, the Society of the Friends of the German Roentgen Museum, to individuals of Germany and other countries who have helped in the advancement and dissemination of Roentgen's discovery in both the scientific and practical aspects; or who have been of especial service to the German Roentgen Museum.

1972

Power-Life Award, Power Engineering Society of the IEEE, for his contributions to the science of X-rays, the medical profession, and the welfare of humanity.

1973

Schenectady Patroonship

Climax Molybdenum Wedgwood Medallion, for pioneering work leading to the invention of ductile tungsten and molybdenum.

William D. Coolidge Award, the American Association of Physicists in Medicine

Honorary Degrees

Doctor of Science, Union College, June 1927

Doctor of Science, Lehigh University, June 1927

Doctor of Medicine, University of Zurich, September 1937

Doctor of Laws, Ursinus College, October 1942

Doctor Honoris Causa, University of Sao Paulo, November 1945

Doctor Honoris Causa, National School of Engineering, University of Brazil, November 1945

Doctor of Science, Catholic University of Chile, November 1945

Doctor of Engineering, Indiana Technical College, May 1947

Society Memberships

National Academy of Sciences

American Academy of Arts and Sciences

Washington Academy of Science (Vice-President, 1931)

American Association for the Advancement of Science

American Chemical Society (Emeritus Status)

American Electrochemical Society
American Institute of Electrical Engineers (Fellow)
American Physical Society
American Institute of Chemists
Sigma Xi
American Philosophical Society
Edison Pioneers
Eta Kappa Nu (Eminent Member)

Honorary Memberships

The American Roentgen Ray Society
The American Radium Society
The Radiological Society of North America
American College of Radiology
The Roentgen Society, of England
Société de Radiologie Médicale, de France
Nordisk Förening för Medicinisk Radiologi, Scandinavia
The Pan-American Medical Association
Société FranÇaise des Electriciens
Medical Society of the County of Schenectady
The Dental Society of the State of New York
The Franklin Institute
Brazilian Institute for Study of Tuberculosis
Brazilian Society of Medical Radiology
Paulista Medical Association
Chilean Society of Radiology
Faculty of Physical and Mathematical Sciences of the University of Chile
Faculty of Biological and Medical Sciences of the University of Chile
Argentine Electrotechnical Association
Sociedad Peruana de Radiologia
Sociedad Argentina de Radiologia
American Academy of the History of Dentistry
Odontological Society of Lyon, France

Corresponding Memberships

Brazilian Academy of Science
National Academy of Exact Physical and Natural Sciences of Lima
Société FranÇaise des Electriciens

Selected Bibliography

Dr. Coolidge published more than seventy papers. Some of the more important are listed below.

1903 With A. A. Noyes. Electrical conductivity of aqueous solutions at high temperatures. Proc. Am. Acad. Arts Sci., 39(7):163-219.

1908 With A. A. Noyes. The electrical conductivity of aqueous solutions. II. Original apparatus and method. Conductivity and ionization of NaCI and KCI up to 306 degrees. Carnegie Inst. Washington Publ., 63:9-55.

The electrical conductivity of aqueous solutions. III. Later modifications of the apparatus and method. Carnegie Inst. Washington Publ., 63:59-67.

1910 Ductile tungsten. Trans. Am. Inst. Electr. Eng., 29, part 2:961-65.

1914 A powerful Roentgen ray tube with a pure electron discharge. Phys. Rev., 2d series 2, no. 6:409-30.

1925 Modern X-ray tube development. J. Franklin Inst., 199:619-48.

High voltage cathode rays outside the generating tube. Science, 62:441-42.

1926 With C. N. Moore. Some experiments with high voltage cathode rays outside the generating tube. J. Franklin Inst., 202:721-34.

1931 With L. E. Dempster and H. E. Tanis, Jr. High voltage cathode ray and X-ray tubes and their operation. Physics 1(4):230-44.

1932 With C. N. Moore. Experimental study of cathode rays outside of the generating tube. Congress International d'Electricite, Section 1, Rapport no. 19, 18 pp. Also in: Gen. Electr. Rev., 35(8):413-17.

1942 The role of science institutions in our civilization. Science, 96(2497):411-17.

1945 A plea for more fundamental research effort. Science, 119(3082): 110-11.

PATENTS

Dr. Coolidge received eighty-three U.S. patents. Some of the more important are listed below.

1909	935,463. Dies and Die Supports.
1912	1,026,382. Metal Filaments.
	1,026,383. Metal Filaments.
	1,026,384. Metal Filaments.
1913	1,082,933. Ductile Tungsten.
1915	1,153,290. X-Ray Targets.
1917	1,211,092. X-Ray Tubes.
	1,211,376. Electron Discharge.
	1,215,116. X-Ray Apparatus.
1925	1,529,344. X-Ray Apparatus.
	1,541,627. X-Ray Apparatus.
	1,543,654. X-Ray Apparatus.
1939	2,181,724. Electrostatic Machines.

Alfred E. Emerson

Alfred Edwards Emerson

December 31, 1896-October 3, 1976

by Edward O. Wilson
and
Charles D. Michener

Something about ants, termites, and other social insects attracts generalists, scholars who begin with a deep interest in basic entomology, or else acquire it, and who restlessly probe far beyond into such fields as evolutionary theory, biogeography, the history of science, and philosophy while conducting otherwise ordinary research. In the eighteenth century it was René Antoine Ferchault de Réaumur; in the nineteenth century, John Lubbock, Auguste Forel, and that famous amateur myrmecologist, Charles Darwin. In our own time William Morton Wheeler has been followed by Karl von Frisch, Caryl P. Haskins, and Theodore D. Schneirla.

Into the last group must be placed Alfred E. Emerson. Until his death he was the leading authority on termites, a restless technical expert who contributed massively to their classification, anatomy, and biogeography. He was also an important contributor to modern ecology, one of the synthesizers of the 1940's and 1950's who brought the large quantities of new data on adaptation, physiology, behavior, and distribution into line with the emerging principles of the "New Synthesis" of evolutionary theory. He was a biogeographer of importance; his detailed knowledge of the world distribution of genera and species of termites helped bring insects into the mainstream of general theory in biogeog.

raphy. And not least, Emerson developed the concept of the superorganism to its extreme degree on the basis of his knowledge of the workings of termite colonies; in the course of this effort he helped to establish the importance of behavioral traits in classification and phylogenetic reconstructions.

Alfred Emerson was born in Ithaca, New York on December 31, 1896, the youngest of four children of a Cornell professor of classical archeology. He moved with his family to Chicago in 1905 when his father became curator of antiquities at the Art Institute of the University of Chicago. His mother was a professional concert pianist and instructor in the history of music at the University of Chicago; his brother and two sisters all enjoyed successful academic careers. One sister, Gertrude, became editor of *Asia Magazine,* settled in India, and was responsible for drawing Emerson into a friendship with Indira Gandhi later in his life.

In the midst of this rich early cultural environment, with its emphasis on the humanities, Emerson flirted briefly with the idea of a career in music. Then, while a student at the Interlaken School in Rolling Prairie, Indiana (1910-1914), he built and ran the school poultry farm—the first odd circumstance in a train of events that led to his career as an entomologist. He was to become the family's scientific "mutant," as he later described himself. Upon reaching college age in 1914, he went to Cornell University with the intention of specializing in poultry science. But the courses were too elementary and dull, causing him to try out the beginning course in each science department of the university in turn. When the time came to choose a major subject in his junior year, Emerson picked entomology, principally—as he once said—because the Department of Entomology was at that time the best of its kind in the world. He became the personal friend of John H. Comstock and his wife, Anna Botsford Comstock, as well as of James G. Needham, all large figures in the history of the field. The Cornell entomologists stressed depth of training and detailed expertise in individual groups of insects, and Emerson clearly benefited from this experience through all of his subsequent research. At the same time he formed a close friendship with another student, the herpetologist Karl P. Schmidt, who later became curator-in-chief of zoology at the Chicago Natural History Museum and a fellow member of the National Academy of Sciences.

While at Cornell Emerson met his first wife, Winifred Jelliffe, the daughter of Smith Ely Jelliffe, a leading psychiatrist. The couple became engaged in 1918, and soon afterward Emerson left for nine months service in the army (discharged in December, he did not see combat). Next Emerson made a trip to the New York Zoological Station at Kartabo, British Guiana, where, at the suggestion of William Beebe, he began studying termites, and thus began his life's work.

In 1920, as Emerson completed his M.A. at Cornell, he married Winifred and took her on his second trip to Kartabo. A third expedition to British Guiana followed in 1924 and then a six-month sojourn on Barro Colorado Island, Panama, in 1935. The termite collections that Emerson assembled and the experience he obtained during these early visits to the American tropics were a rich source of data and ideas on which he drew during the rest of his life.

In 1921 Emerson accepted an instructorship at the University of Pittsburgh. After completing the requirements for a Ph.D. at Cornell in 1925, he held a Guggenheim Fellowship in 1925 and 1926 and then an associate professorship at the University of Chicago. There he stayed for the remainder of his professional career. The new associations that he formed at Chicago were decisive in the broadening of his interests and the achievement of theoretical contributions in ecology and behavior.

During the 1920's the Emerson's had two children: Helena, who became the wife of Eugene Wilkening, a professor of sociology at the University of Wisconsin; and William Jelliffe Emerson, employed in the Department of Anatomy of the University of Chicago. In 1949, following a summer in which Alfred served as a visiting professor at the University of California, Berkeley, Winifred died suddenly from the effects of a heart defect acquired during childhood. In 1950 Emerson married Eleanor Fish, whom he had known for years and with whom he had collaborated on a children's book, *Termite City* (1937). Those of us who knew this couple in later years were impressed by the closeness and warmth of their marriage.

By his own testimony, Alfred Emerson's principal contribution to science was the more than one hundred articles that added vastly to our knowledge of the systematics, phylogeny, distribution, and natural history of termites around the world. In fact, he may well have been the most productive researcher on this subject who ever lived. By 1969, 1,914 species of termites had been described by termitologists. Emerson's collection, which was donated to the American Museum of Natural History, contained about one million specimens representing 1,745 species, or 91 percent of the known world fauna. No less than 80 percent of the species are represented by primary type specimens. His personal library on termites is virtually complete to the late 1960's, constituting an important bequest to future investigators. Emerson remained active right through the later years of his life, as evidenced by his excellent review of the Mastotermitidae (1965), description of the first Mesozoic termite (1967), reviews of the fossil Kalotermitidae (1965) and Rhinotermiti dae (1971), and analysis (with Kumar Krishna) of the rare and little known Serritermitidae. Emerson's carefully researched and cautious studies are the most authoritative sources of information on the classification and evolutionary biology of the termites. His monographs on the termites of Kartabo and the Belgian Congo (Zaire) and Cameroon remain after many years the most valuable field guides for entire tropical faunas. They are so well written and illustrated as to be useful to anyone with an elementary knowledge of entomology.

In 1949 Emerson coauthored the major synthetic work on ecology to that time, *Principles of Animal Ecology,* an influential textbook known lightly among students and other biologists as the "The Great AEPPS"—after the initials of the authors' last names (W. C. Allee, A. E. Emerson, Orlando Park, Thomas Park, and Karl P. Schmidt). This massive work collected much of what was known about animal ecology at that time, making full use of current evolutionary theory and the still fragmentary principles of population biology. Emerson's main contribution was to summarize knowledge of the social insects, demonstrating with numerous examples the diverse and often bizarre ways that features of social behavior adapt species to particular challenges in the environment. In general, *Principles of Animal Ecology* stimulated a great deal of rigorous research in ecology and helped set the stage for the surge in population and community ecology that occurred during the 1950's and 1960's. (A commentary on Emerson's eminence as an ecologist was published by T. Park in 1967 *[Bulletin of the Ecological Society of America,* 48: 104-7].) Emerson's scholarly treatment of the social insects was the best since the monographs by W. M. Wheeler twenty years previously, and they helped to keep these creatures in the midst of developments in the major topics of ecology and the remainder of evolutionary theory.

On top of Emerson's cumulative work concerning termites, his most striking single contribution, in the opinion of many, was his use of behavioral traits as taxonomic characters. Emerson referred to the structure of termite nests as "frozen behavior" that could be weighed and sketched with the same reliability and quantity of information as many anatomical traits. He showed that certain species of *Apicotermes* can be distinguished more readily by the architecture of their nests than by the anatomy of the termites themselves. His case seems exceptionally strong today, because termites have relatively complex, stereotyped behavior, and as subsequent investigators have shown, these insects use nest structure to regulate the microclimate of the colony. It is fair to say that what Konrad Lorenz and other vertebrate ethologists did for the use of behavior in bird systematics, Emerson helped to accomplish for the use of behavior in the systematics of termites and other social insects.

Alfred Emerson is also well known for his espousal of the superorganism concept, in which the castes and functions of the insect colony are compared with the anatomical and physiological features of single organisms. This method of analogy, first put in concrete form by Wheeler and highly popular in the first half of the century, was perhaps carried to its extreme by Emerson. He saw in the social insects the exemplification of "dynamic homeostasis," which he believed to be a new unifying principle of evolutionary theory. This part of Emerson's thought has had relatively little impact, principally because during the period of his most assertive articles (1952-1958) the pendulum had begun to swing away from holistic conceptualization and toward piecemeal, experimental analysis of individual physiological mechanisms and patterns of behavior. But at the very least, however much out of focus, and even during this period of its waning, the superorganism concept remained a stimulating distant goal toward which many younger entomologists felt themselves to be working.

In spite of being a hard-working scholar in an exacting specialty, Emerson was a gregarious man, exceptionally generous with his energies and time. He was friendly not only with his intellectually gifted associates, but also with less gifted persons with whom he willingly discussed everyday topics. He wrote long letters of advice and encouragement to younger entomologists, never displaying the protectiveness or hardening of opinion that afflicts some established scientists. In different years he served as president of the Ecological Society of America, the Society for the Study of Evolution, and the Society for Systematic Entomology and was a vice-president of the Entomological Society of America. Among his honors were an honorary D.Sc. from Michigan State University in 1961, received after his service as a distinguished visiting professor in 1960, and the Eminent Ecologist Award for 1967 from the Ecological Society of America. He was elected to the National Academy of Sciences in 1962.

On Sunday, October 3, 1976, Alfred Emerson died of a heart attack near his summer home at Huletts Landing, on Lake George, New York. He will be remembered for the magnitude and rigor of his scholarship, his uncompromising and lifelong devotion to science, his interest in the relevance of science to humane learning, and, especially by those who knew him best, the largeness and generosity of his spirit.

Bibliography

1917 A willy-nilly stepmother and other disasters. Nat. Stud. Rev., 13: 198-99.

1919 Termites of Kartabo. Bull. N.Y. Zool. Soc., 22:75-77.

1923 A new termite from the Juan Fernandez Islands. In: *The Natural History of the Juan Fernandez and Easter Island,* ed. C. Skottsberg, vol. 3, pp. 392-94. Uppsala, Sweden: Almqvist & Wiksells AB.

1924 Social evolution among insects. Radio Publ. no. 8, Univ. of Pittsburgh, pp. 23-29.

1925 The termites of Kartabo, Bartica District, British Guiana. Zoologica, 6:291-459.

The jungle laboratory of tropical biology conducted by the University of Pittsburgh. Science, 61 (1576):281.

1926 Development of a soldier of *Nasutitermes (Constrictotermes) cavifrons* (Holmgren) and its phylogenetic significance . Zoologica, 7: 69-100.

1927 The phylogenetic origin of the sterile castes of termites. Anat. Rec., 37:148-49(A).

Invertebrates. Classroom Teacher, 9:345-412.

A pioneer student of the ants. New Repub., 50:25-26.

1928 Termites of the Belgian Congo and the Cameroon. Bull. Am. Mus. Nat. Hist., 57:401-574.

Le développement des soldats termites. Ann. Sci. Nat., 10 sér., 11:261-84.

Polymorphism among the castes of termites. Anat. Rec., 41: 48-49(A).

With R. T. Hance. *Laboratory Experiments in General Zoology.* Pittsburgh: Univ. of Pittsburgh Press. 64 pp.

1929 Ecological relationships between termites and termitophiles in British Guiana. Tenth International Congress of Zoology, p. 1008. Budapest, Hungary: Stephaneum.

Termite problems in California, pp. 12-14; The social life of termites, pp. 24-30. In: *Report on the Symposium on Termite Problems of the Termite Investigations Committee* , September 2-13, 1929. San Francisco: Termite Investigations Committee. 42 pp.

Communication among termites. In: *Transactions of the Fourth International Congress of Entomology,* (Ithaca, New York), vol. 2, pp. 722-27. Naumburg, Germany: Gottfr. Pätz.

With R. C. Simpson. Apparatus for the detection of substratum communication among termites. Science, 69(1799):648-49.

1931 *Second Year College Sequence in Botany, Zoology, and Physiology. Syllabus for Zoology.* Chicago: Univ. of Chicago Bookstore. 45 pp.

With M. C. Coulter, O. Park, T. Park, and K. P. Schmidt. Invertebrates, pp. 35-80; Animals in relation to their environment, pp. 312-22; Animal geography, pp. 322-24; Animal society, pp. 337-39. In: *Introductory General Course in the Biological Sciences Syllabus.* Chicago: Univ. of Chicago Bookstore.

1933 A revision of the genera of fossil and recent Termopsinae (Isoptera). Univ. Calif. Publ. Entomol., 6:165-96.

The mechanism of tandem behavior following the colonizing flight of termites. Anat. Rec., 57:61-62 (A).

Conditioned behavior among termites. Psyche, 40:125-29.

Protective coloration. Ecology, 14:407.

1934 Biology of termites. Ecology, 15:204-5.

Termitophile distribution and quantitative characters as indicators of physiological speciation in British Guiana termites (Isoptera). Bull. Ecol. Soc. Am., 15:40-41(A).

Entomological apparatus. Ecology, 15:444.

The ecology of insects. Ecology, 15:444-45.

1935 Symbiosis between roaches and protozoa. Ecology, 16:116-17.

Termitophile distribution and quantitative characters as indicators of physiological speciation in British Guiana termites (Isoptera). Ann. Entomol. Soc. Am., 28:369-95.

1936 Termites. Ecology, 17:302-3.

Distribution of termites. Science, 83(2157):410-11.

A marine estuary. Ecology, 17:170.

Mayflies. Ecology, 17:303.

Insects of shade-trees. Ecology, 17:303.

Evolution and speciation. Ecology, 17:532-34.

1937 Termite nests-a study of the phylogeny of behavior. Science, 85:56(A).

The preparation of scientific papers. Ecology, 18:154-55.

Speciation. Ecology, 18:153-54.

Life of the termite. Sci. Dig., 2:55-58.

Termite nests-a study of the phylogeny of behavior. Anat. Rec., 70:137(A). The living world. Ecology, 18:441-42.

Ecological animal geography. Ecology, 18:541-42.

Termite architecture. Nat. Hist., 39:241-48.

The termite problem. Nat. Hist., 39:249-54.

Termite nests-a study of the phylogeny of behavior. Entomol. News, 48:20.

With Eleanor Fish. *Termite City.* Chicago: Rand McNally. 127 pp.

1938 The origin of species. Ecology, 19:152-54.
Termite nests-a study of the phylogeny of behavior. Ecol. Monogr., 8:247-84.
Termites-insect architects. Sci. Dig., 3:52-56.
1939 Social coordination and the superorganism. Am. Midi. Nat., 21:182-209. (Reprinted in: *Plant and Animal Communities,* ed. T. Just. Notre Dame, Ind.: Notre Dame Univ. Press. 255 pp.)
Populations of social insects. Ecol. Monogr., 9:287-300.
Animal societies viewed as super-organisms. Sci. Dig., 4:96.
Animal ecology. Ecology, 20:426.
A laboratory guide to the classification and ecology of animals. Ecology, 20:427.
Ants and men. Ecology, 20:427.
1940 Review of J. Huxley, *The New Systematics.* Bot. Gaz., 102:412.
Objects of the Society for the Study of Speciation. Mimeographed. 16 pp.
A critical review of J. Huxley, *The New Systematics.* Mimeographed. pp. 17-29.
1941 Taxonomy and ecology. Ecology, 22:213-15.
Biological sociology. Denison Univ. Bull. J. Sci. Lab., 36:146-55.
The Society for the Study of Speciation. Am. Nat., 75:86-89.
With Bertha M. Parker. *Insect Societies.* Evanston, Ill.: Row, Peterson and Co. 36 pp.
1942 Ecology and evolution. Chron. Bot., 7:151-52.
Basic comparisons of human and insect societies. Biol. Symp., 8:163-76.
The relations of a relict South African termite (Isoptera, Hodotermitidae, *Stolotermes).* Am. Mus. Novit., 1187:1-12.
The dynamics of evolution. Ecology, 23:493.
The modern naturalist. Transylvania Coll. Bull., 15:71-77.

1943 Ecology, evolution and society. Am. Nat., 77:97-118.

Review of O. Park. A study of Neotropical Pselaphidae. Northwestern Univ. Alumni News, 22:1.

Taxonomy and ecology of Pselaphid beetles. Ecology, 24:132.

Kalotermes milleri, a new species of termite from the Florida Keys and Jamaica (Isoptera, Kalotermitidae). Psyche, 50:18-22.

Systematics and speciation. Ecology, 24:412-13. With E. M. Miller. A key to the termites of Florida. Entomol. News, 54:184-87. (Also in: Proc. Florida Acad. Sci., 6:108-9, 1945.)

1944 Australian termites. Ecology, 25:123-24.

Frank Eugene Lutz (obituary). Science, 99:233-34.

1945 Taxonomic categories and population genetics. Entomol. News, 56:14-19.

The Neotropical genus *Syntermes* (Isoptera: Termitidae). Bull. Am. Mus. Nat. Hist. , 83:427-71.

Co-operation and conflict in the balance of nature. Lecture, Nov. 9, 1945, Univ. of Chicago. Mimeographed.

With R. W. Gerard. Extrapolation from the biological to the social. Science, 101:582-85.

1947 The biological basis of social cooperation. Trans. I11. State Acad. Sci., 39:9-18.

Why termites? Sci. Mon., 64:iv; 337-45.

The imago of *Stolotermes africanus* Emerson. J. Entomol. Soc. South. Afr., 9:127-29.

1948 Biological principles of human social integration. Intercul. Ed. News, 9:8-9.

1949 Termite studies in the Belgian Congo. Extrait Deuxième Rapp. Ann., Inst. Rech. Sci. Afr. Centr.: 149-59.

Devescovinid flagellates of termites. V. The genus *Hyperdevescovina* and the genus *Bullanympha,* and undescribed or unrecorded species. Univ. Calif. Publ. Zoology, 45(5):319-422.

With W. C. Allee, O. Park, T. Park, and K. P. Schmidt. *Principles of Animal Ecology.* Philadelphia: W. B. Saunders Co. 837 pp.

1950 Five new genera of termites from South America and Madagascar (Isoptera, Rhinotermitidae, Termitidae). Am. Mus. Novit., 1444:1-15.

Learning from termites. Broadcast no. 28889, Columbia Broadcasting System, CPN. KNX, Los Angeles; KCBS, San Francisco, Jan. 15, 1950.

1951 Termite studies in the Belgian Congo. Deuxième Rapp. Ann. 1949, Inst. Rech. Sci. Afr. Centr.: 149-60.

1952 The biogeography of termites. Bull. Am. Mus. Nat. Hist., 99: 217-25.

The Neotropical genera *Procornitermes* and *Cornitermes* (Isoptera, Termitidae). Bull. Am. Mus. Nat. Hist., 99:475-540.

Social homeostasis and the evolution of a microhabitat. Bull. Ecol. Soc. Am., 33:51(A)

Phylogeny of social behavior as illustrated by the termite genus *Apicotermes.* Bull. Ecol. Soc. Am., 33:66. Also in: Anat. Rec., 113:38(A).

The superorganismic aspects of the society. In: *Structure et Physiologie des Sociétés animales.* Colloq. Int. C. N. R. S., no. 34, pp. 333-54.

1953 The biological foundations of ethics and social progress. In: *Goals of Economic Life,* ed. A. D. Ward, pp. 277-304. New York: Harper and Bros.

The African genus *Apicotermes* (Isoptera: Termitidae). Ann. Mus. R. Congo Belge, sér. 8, Sci. Zool., 17:99-121.

Evolution of social behavior as illustrated by the termite genus *Apicotermes.* Proc. 8th Ann. Meeting North Central States Branch, American Entomological Society, p. 39(A).
1954 The relation of systematics to other fields. Ecology, 35:102-3.
Dynamic homeostasis: a unifying principle in organic, social, and ethical evolution. Sci. Mon., 78:67-85.
Social insects. Ecology, 35:428.
1955 Species—biological species. In: *Encyclopaedia Britannica.* Chicago: Encyclopaedia Britannica.
Geographical origins and dispersions of termite genera. Fieldiana: Zool., 37:465-521.
Sewall Wright. Bull. Alumni Assoc., School of Medicine, Univ. Chicago, 11:19.
Ecology and Evolution (in Japanese). Tokyo: Misuzu Shobo, 248 pp.
With Thomas Park. Warder Clyde Allee: Ecologist and ethologist. Science, 121(3150):686-87.
1956 Regenerative behavior and social homeostasis of termites. Ecology, 37:248-58.
Homeostasis and comparison of systems. In: *Toward a Unified Theory of Human Behavior,* ed. R. R. Grinker, pp. 147-63. New York: Basic Books.
A new species of *Apicotermes* from Katanga. Rev. Zool. Bot. Afr., 53:98-101.
Ethospecies, ethotypes, taxonomy, and evolution of *Apicotermes* and *Allognathotermes* (Isoptera: Termitidae). Am. Mus. Novit., 1771:1-31.
Procedure in taxonomy. Science, 124:1213.
1957 Homology. In: *Encyclopaedia Britannica,* vol. 11, pp. 708-9. Chicago: Encyclopaedia Britannica.
With Frank A. Banks. Five new species and one redescription of the Neotropical genus *Armitermes* Wasmann (Isoptera, Termitidae, Nasutitermitinae). Am. Mus. Novit. 1841:1-17.

1958 Review of P. J. Darlington, Jr., *Zoogeography: The Geographical Distribution of Animals.* Science, 127:969.

Review of C. P. Martin, *Psychology, Evolution and Sex.* Science, 127:1111.

K. P. Schmidt—Herpetologist, ecologist, zoogeographer. Science, 127:1162-63.

The evolution of behavior among social insects. In: *Behavior and Evolution,* ed. Anne Roe and G. G. Simpson, pp. 311-55. New Haven: Yale Univ. Press.

The termite problem. In: *Illustrated Library of the Natural Sciences,* ed. E. M. Weyer, vol. 4, pp. 2798-2807. New York: Simon & Schuster.

1959 Social insects. In: *Encyclopaedia Britannica,* vol. 20, pp. 871-78. Chicago: Encyclopaedia Britannica.

Beebe, (Charles) William. In: *Encyclopaedia Britannica,* vol. 3, p. 309. Chicago: Encyclopaedia Britannica.

The African termite genera *Firmitermes, Hoplognathotermes, Acutidentitermes, Duplidentitermes,* and *Heimitermes* (Termitidae, Termitinae). Am Mus. Novit., 1947:1-42.

Evolution of the structure and behavior of termites with a reexamination of the concepts of vestiges, recapitulation, and caenogenesis. Science, 130:1416-17(A).

"Feedback" in evolution. Science, 129:1580.

1960 New genera of termites related to *Subulitermes* from the Oriental, Malagasy, and Australian regions (Isoptera, Termitidae, Nasutitermitinae). Am. Mus. Novit., 1986:1-28.

New genera on the *Subulitermes* branch of the Nasutitermitinae from the Ethiopian region (Isoptera, Termitidae). Am. Mus. Novit., 1987:1-21.

Six new genera of Termitinae from the Belgian Congo (Isoptera, Termitidae). Am. Mus. Novit. 1988:1-49.

The evolution of adaptation in population systems. In: *Evolution after Darwin,* ed. S. Tax, vol. 1, *The Evolution of Life,* pp. 307-48. Chicago: Univ. of Chicago Press.

Huxley, Sir Julian Sorell. In: *Encyclopaedia Britannica,* vol. 11, p. 947. Chicago: Encyclopaedia Britannica.

Wheeler, William Morton. In: *Encyclopaedia Britannica,* vol. 23, p. 566. Chicago: Encyclopaedia Britannica.

The impact of the theory of evolution on religion. In: *Science Ponders Religion,* ed. Harlow Shapley, pp. 136-46. New York: Appleton-Century-Crofts.

The evolution of life (panel discussion with J. Huxley, D. I. Axelrod, T. Dobzhansky, E. B. Ford, E. Mayr, A. J. Nicholson, E. C. Olson, C. L. Prosser, G. L. Stebbins, and S. Wright). In: *Evolution after Darwin,* ed. S. Tax and C. Callender, vol. 3, *Issues in Evolution,* pp. 107-43. Chicago: Univ. of Chicago Press.

With K. P. Schmidt. Taxonomy. In: *Encyclopaedia Britannica.* Chicago: Encyclopaedia Britannica.

1961 Review of G. G. Simpson, *Principles of Animal Taxonomy.* Science, 133:1589-90.

Vestigial characters of termites and processes of regressive evolution. Evolution, 15:115-31.

1962 Intellectual cooperation among natural scientists. In: *Report and Proceedings, Sino-American Conference on Intellectual Cooperation,* Univ. Washington, 1960, pp. 86, 100, 123-25, 184-92. Seattle: Univ. of Washington.

The impact of Darwin on biology. Acta Biotheor., 15: 175-216.

Vestigial characters, regressive evolution, and recapitulation among termites. In: *Termites in the Humid Tropics* (Proceedings of the New Delhi symposium, 4-12 October 1960), pp. 17-30. Paris: UNESCO.

Human cultural evolution and its relation to organic evolution of termites. In: *Termites in the Humid Tropics* (Proceedings of the New Delhi symposium, 4-12 October 1960), pp. 247-54. Paris: UNESCO.

With K. Krishna. New species of the genus *Glyptotermes* Froggatt from the Papuan, Oriental, Ethiopian, and Neotropical regions (Isoptera, Kalotermitidae). Am. Mus. Novit., 2089:1-65.

1964 Report of the representative to the centennial celebration of the National Academy of Sciences. Bull. Entomol. Soc. Am., 10:42.

Termites (interview). The New Yorker, 18 Jan., pp. 23-24.

1965 A review of the Mastotermitidae (Isoptera) including a new fossil genus from Brazil. Am. Mus. Novit., 2236:1-46.

Human cultural evolution and its relation to organic evolution of insect societies. In: *Social Changes in Developing Areas: A Reinterpretation of Evolutionary Theory,* ed. H. R. Barringer, G. I. Blanksten, and R. W. Mack, pp. 50-67. Cambridge, Mass.: Schenkman Publ. Co. 328 pp.

With F. A. Banks. The Neotropical genus *Labiotermes* (Holmgren): Its phylogeny, distribution, and ecology (Isoptera, Termitidae, Nasutitermitinae). Am. Mus. Novit., 2208:1-33.

With L. H. Throckmorton. Survey of biological research institutions in Japan, 1964. In: *United States-Japan Cooperative Effort,* ed. J. Renirie and N. Neureiter, BioScience, 15:21-24.

1966 Commentary on theological resources from the biological sciences. Zygon, 1:55-56.

Review of W. A. Sands, A revision of the termite subfamily Nasutitermitinae (Isoptera, Termitidae) from the Ethiopian region. Q. Rev. Biol., 41(1):325.

1967 Homeostasis and comparison of systems. In: *Toward a Unified Theory of Human Behavior: An Introduction to General Systems Theory,* ed. R. R. Grinker, 2d ed., pp. 147-63. New York: Basic Books. 380 pp.

Cretaceous insects from Labrador. 3. A new genus and species of termite (Isoptera: Hodotermitidae). Psyche, 74(4):276-89.

1968 Dynamic homeostasis: A unifying principle in organic, social, and ethical evolution. Zygon, 3 (2):129-68.

A revision of the fossil genus *Ulmeriella* (Isoptera, Hodotermitidae, Hodotermitinae). Am. Mus. Novit., 2332:1-22.

La evolución cultural humana y su relación con la evolución orgánica de las sociedades de insectos. In: *Cambios sociales en Regiones en desarrollo. Una Interpretación de la Teoria evolucionista,* ed. H. R. Barringer, G. I. Blanksten, and R. W. Mack, pp. 59-77. Mexico: Editorial Roble. 360 pp.

1969 A revision of the Tertiary fossil species of the Kalotermitidae (Isoptera). Am. Mus. Novit., 2359:1-57.

1971 Tertiary fossil species of the Rhinotermitidae (Isoptera), phylogeny of genera, and reciprocal phylogeny of associated flagellata (Protozoa) and the Staphylinidae (Coleoptera). Bull. Am. Mus. Nat. Hist., 146(3):243-304.

Review of K. Krishna and F. M. Weesner, *The Biology of Termites,* vol. 1. Am. Sci., 53(3):373.

Thomas Elliott Snyder, 1885-1970. Proc. Entomol. Soc. Wash., 73(2):239-42.

A biologist and his times. Science, 172(3984):679.

1972 Review of E. O. Wilson. *The Insect Societies.* Anim. Behav., 20: 821-22.

1973 Some biological antecedents of human purpose. Zygon, 8(3-4): 294-309.

1974 With R. W. Burhoe. Evolutionary aspects of freedom, death, and dignity. Zygon, 9(2):156-82.

1975 With K. Krishna. The termite family Serritermitidae (Isoptera). Am. Mus. Novit., 2570:1-31.

1976 Termite nests—a study of the phylogeny of behavior (reprint of article originally published in 1938); Addendum: nomenclatural changes, pp. 132-33; Changes in the biological basis of termite ethology with more recent bibliography, p. 133. In: *External Constructions by Animals,* ed. Nicholas Collias and E. Collias. Stroudsburg, Pa.: Dowden, Hutchinson and Ross, Benchmark Papers in Behavior.

RW Gerard

Ralph Waldo Gerard

October 7, 1900-February 17, 1974

by Seymour S. Kety

During the middle decades of the twentieth century, study of the nervous system became a major component of biological research, growing from a strong base in morphology and physiology to involve all of the biological and behavioral sciences. It was not by chance that this development coincided with the time and span of Ralph Gerard's scientific career, for he was one of a small number of intellectual leaders who brought it about.

Born in Harvey, Illinois at the beginning of this century, Ralph Gerard was blessed with an uncommon intellectual endowment, a heritage that has traditionally held scholarship and ethics in high regard, and a remarkable father who nurtured his scientific curiosity. His father, Maurice Gerard, who had come to America from Central Europe after receiving a degree in engineering in Britain, was a self-employed consultant to industry. He named his son after Emerson, whom he admired, and saw for him the career in pure science that he had been unable to pursue. From his father, Ralph Gerard also gained an appreciation of mathematics and of chess, showing particular aptitude for the latter, so that in his teens he beat the American champion and the world champion at different times when they were playing simultaneous matches in Chicago.

He completed the four-year course at Chicago's Hyde Park High School in two years, by passing examinations in subjects he already knew or had taught himself outside of class.

He entered the University of Chicago at the age of fifteen, where he took at least one course in every one of the sciences offered and in most of the other disciplines besides. In this way, the natural genius and irrepressible interest that had been stimulated and reinforced by his father was broadened, undoubtedly contributing to his comprehension of many fields as a scientist and his unique ability to compound and integrate that knowledge with ever-widening scope. He was strongly influenced by Julius Stieglitz in chemistry, and by Anton Carlson and Ralph Lillie in physiology and neurophysiology. He received his doctorate in physiology in 1921. Shortly thereafter, he married Margaret Wilson, who had just completed her doctorate in neuroanatomy, and together they finished their medical training at the Rush Medical College in 1925. Margaret Wilson Gerard went on to train in pediatrics and psychiatry and to become an outstanding scholar and practitioner of child psychiatry until her death in 1954.

Ralph Gerard took an internship at the Los Angeles General Hospital, at the end of which he was faced with what he felt was the major career decision of his life. He was offered a much-coveted residency in medicine at the same time that he was awarded a National Research Council fellowship in neurophysiology and neurochemistry. He accepted the research fellowship. In an interesting revelation of his restive nature and lack of complacency, he recalled that decision at times with some misgiving, even after achieving worldwide acclaim as a neurophysiologist.

The research fellowship launched him most propitiously on his career in neuroscience. With A. V. Hill and Otto Meyerhoff, two giants in biophysics and biochemistry, respectively, he carried out pioneering research leading to the recognition that the conduction of the nerve impulse depended on biochemical processes along the nerve. When he returned to the University of Chicago in 1928, he was faced with another major decision. An offer by Carlson of an appointment in physiology was more than matched by Dallas Phemister, who was in the process of establishing the Department of Surgery in the new Medical School there. Again he chose physiology and remained in that department for twenty-five years. His laboratory there, which encompassed all of the promising neurobiological disciplines, trained a large number of graduate students and postdoctoral fellows, several of whom were later to become leaders in neuroscience.

In 1952 Gerard was asked to develop and direct the research laboratories of the Neuropsychiatric Institute of the University of Illinois, and he spent the next two years organizing a multidisciplinary research program that brought neurology and psychiatry more closely together, as well as the fundamental disciplines that sustain them both.

After the death of Dr. Margaret Gerard, he accepted an invitation from Ralph Tyler to join the first group of distinguished fellows of the Center for Advanced Study in the Behavioral Sciences, which had been established adjacent to the campus of Stanford University.

During the preceding sixteen years, while he was engaged in some of his most significant contributions to neurophysiology, he found time to contemplate and address philosophical and social problems that lay beyond neuroscience. He published "The Role of Pure Science" in 1938, "Organism, Society and Science" in 1940, "A Biological Basis for Ethics" and

"Higher Levels of Integration" in 1942, "Extrapolation from the Biological to the Social" in 1945 and, in 1946, "The Biological Basis of Imagination."

At the Center in Palo Alto, Ralph Gerard's interest in the behavioral and social sciences expanded further in what must have been exciting interactions with Anatol Rapoport, Clyde Kluckhohn, Franz Alexander, Paul Lazarsfeld, and Alex Bavelas, from which emerged "Biological and Cultural Evolution" in 1956 and, in 1957, "Problems in the Institutionalization of Higher Education."

In January 1955 he married Leona Bachrach Chalkley, whom he had known since high school when they were captains of opposing debating teams. "Frosty," as he preferred to call her, in addition to being a skilled debater, is an accomplished writer and poet. In the course of the fellowship year at the Center, another salutary event occurred when James Miller invited Gerard to join him and Anatol Rapoport in founding the Mental Health Research Institute at the University of Michigan, and Ralph Gerard spent many hours discussing with Anatol Rapoport the possibility of creating a new and broader consortium of sciences dedicated to the study of behavior. Convinced of the sincerity and commitment of the Commonwealth and University of Michigan to this important concept, he accepted this opportunity and spent more hours with Miller, planning the philosophy and scope of the new research institute.

Ralph and Frosty spent the next nine years at Ann Arbor, where he was professor of neurophysiology and director of the Institute's laboratories. During that time the Mental Health Research Institute grew from imaginatively conceived plans to one of the outstanding behavioral and psychiatric research centers in the nation, with a scope that embraced fundamental neurochemical and physiological research, the behavioral sciences, and information processing. He devoted the bulk of his research efforts during that time to organizing and leading a multifaceted study of schizophrenia, the most important of the mental illnesses in terms of loss of fulfillment to the individual and family and cost to society.

At the age of sixty-four, when some might have thought of retiring, Ralph Gerard instead accepted a new and broader challenge, moving to the new Irvine campus of the University of California as professor of biological sciences and dean of the Graduate Division. This enabled him to revive the imaginative interest in teaching and educational philosophy that earlier had made him one of the most stimulating contributors to the new concepts of undergraduate education introduced by Robert Hutchins shortly after Ralph Gerard's appointment to the University of Chicago. At Irvine he continued his nurture of neuroscience, participating in the establishment of the important Department of Psychobiology. Most of his energies and wisdom, however, were devoted to the wider fields of education, science, society, and the social aspects of medicine.

At the age of seventy he retired to throw himself fully into civic affairs. At the same time his wife and his colleagues became concerned over some changes in his personality and some slowing of his intellectual functions. An intracerebral tumor was discovered, but even for this ominous situation, his remarkable brain found a salutary resolution. The tumor turned out to be a benign meningioma, which was removed successfully and with complete recovery. Frosty has related a remarkable incident at that time that describes his indomitable spirit: "Service for a year on the Orange County Grand Jury meant so much to him that two days following the neurosurgical operation, and while he still remained in the hospital's intensive care unit, he insisted on dictating to me a letter to the Grand Jurors. He wrote them about his feelings when tests had revealed the existence of a massive tumor. If further tests had suggested that the mass was malignant, he would have refused to undergo surgery and merely awaited the end. Instead, a benign tumor was indicated, he had taken his chances, and won. He wanted them to know that he was eager to rejoin them and to carry his share of the load. Two months later he was among them." For the next three-and one-half years Ralph Gerard remained active; he died of coronary insufficiency in 1974.

Ralph Gerard was elected to the National Academy of Sciences and to the American Academy of Arts and Sciences in 1955. He was the recipient of numerous honors and awards. Honorary degrees were bestowed upon him by the Universities of Maryland, Leiden, and St. Andrews, and by Brown University and McGill University. He was awarded the Medal of Charles University and the Order of the White Lion in Prague in 1946, the Stanley Dean Award in 1964, the Alumni Medal of the University of Chicago in 1967, and the Extraordinarius Award of the University of California at Irvine, posthumously. He was a Distinguished Fellow of the American Psychiatric Association and an Honorary Fellow of the Royal Society of Edinburgh. A long list of honorary lectureships in this country and abroad attests to his international esteem and his brilliance as a speaker. He was a consultant to numerous research arms of the federal government, including the Office of Naval Research, the National Institute of Mental Health, and the National Science Foundation, and an advisor to numerous private foundations.

Ralph Gerard was so extraordinary a man that he became a legend during his lifetime. His intellectual power was his outstanding characteristic, expressed early on by scholastic precocity, in mid-career by creative insights and the careful execution of crucial experiments, and at the end of his career in encyclopedic erudition and wisdom. His knowledge of the scientific literature, perceptiveness, and ability to synthesize observations in a great variety of disciplines, coupled with an almost poetic fluency in articulating and crystallizing issues, made him highly regarded as a teacher and as a summarizer of scientific conferences, in which he was undoubtedly one of the world's leaders for several decades. He also possessed a remarkable sense of humor and a comparable fund of anecdotes, which were always to the point. Among his physical characteristics, more striking than his portly figure and bald pate, were his eyes, which have been described as "bright and restless-the visible edge of a keen and probing mind ... eyes that showed by their sparkle not only the excitement of discovery, but also a reflection of profound awe before the intricacy and complexity of the natural order."* These attributes will remain in the memories of his students and colleagues and all who knew him, but his most enduring legacy will be in his contributions to science, and particularly to neuroscience. During the twenty-five years that he devoted to laboratory research, he was responsible for a remarkable number of pioneering insights and discoveries that opened up areas of neurobiological research that are far from exhausted today.

Ralph Gerard aptly described the motivation and significance of his scientific career as a commitment to "the minute experiment and the large picture."† His contributions fulfilled that commitment generously, for they demonstrate his remarkable ability to design and conduct rigorous research that crucially examines a specific hypothesis. They also epitomize his vision, imagination, and courage to perceive the implications of the experimental results to the broad picture

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* The Reverend Edward P. Allen, remarks on the occasion of a memorial convocation, University of California at Irvine, 7 March 1974.

† R. W. Gerard, "The Minute Experiment and the Large Picture," in *The Neurosciences: Paths of Discovery,* ed. F. C. Worden, J. Swazey, and C. Adelman (Cambridge, Mass.: MIT Press, 1975), pp. 456-74.

that would eventually emerge. He was both an architect of neuroscience and a stone carver.

He attributed his enduring interest in the nervous system to a brief encounter with Anton Carlson, his professor of physiology, while a student at the University of Chicago. Gerard successfully defended his unwillingness, on logical grounds, to draw the accepted conclusion from a laboratory experiment that had for years been used to demonstrate the nonfatigability of nerve. Carlson appreciated the wisdom in what a lesser man might have seen as brashness, took a continuing interest in young Gerard, and, several years later, recommended him for the National Research Fellowship he was awarded in 1925.

That fellowship permitted him to participate in A. V. Hill's classical demonstration of heat production by nerve and to make his first major discovery in the delayed heat production that follows a period of stimulation. Gerard described those observations at Hill's suggestion at the International Physiological Congress in Stockholm in 1926 and in his paper on "The Two Phases of Heat Production of Nerve" in 1927. He had found that of the total quantity of heat attributable to a period of stimulation, only 11 percent was released during the stimulation, the much larger moiety being liberated over a period as long as ten minutes immediately following the stimulation.

Although the heat generated in muscular contraction had been demonstrated and measured for a long time, the much smaller amounts associated with nerve conduction had remained elusive. Hill, thirty-three years after this successful demonstration, recounted his many previous unsuccessful attempts and those of others going back to Helmholz's first attempt in 1848, explaining its importance:

> Why did people go on trying to measure the heat production of nerve, in spite of repeated failure? Chiefly, I suppose, in order to settle the

> question of whether the nerve impulse is the sort of physical wave in which the whole of the energy for transmission is impressed on the system at the start . . . . If it could be shown that heat really was produced all along the nerve during transmission, then the purely physical theory of conduction would be untenable. A distributed relay system would be required, with energy derived presumably from chemical change.*

During the second year of his fellowship, Gerard moved to the laboratory of Otto Meyerhoff in Berlin in order to examine some of the chemical processes involved in axonal transmission and the differences he surmised would exist during stimulation and recovery. With the use of specially prepared chambers of small size, he was able to measure the oxygen consumed and the carbon dioxide released by a segment of nerve at rest and during stimulation. He found that whereas the resting oxygen consumption of nerve and muscle were equal, the increase during stimulation in muscle was 8,000 times greater than that achieved in nerve. In addition, he measured the temperature coefficient of the oxygen metabolism in nerve and its respiratory quotient at rest and during stimulation, and found evidence for the development of an oxygen debt in nerve during anoxic stimulation.

The increased oxygen consumption of stimulated nerve was soon challenged as an artifact resulting from unphysiological stimulation rather than the physiological activity that resulted. F. O. Schmitt was able to counter that criticism by demonstrating that the oxygen consumption was correlated with the number of transmitted impulses rather than the amount or intensity of the stimulation. Then, in the summer of 1933, Gerard and H. K. Hartline established that physiological transmission alone accounted for the increased oxygen consumption:

> Hartline and I agreed to test this out on the *Limulus* optic nerve,

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* A. V. Hill, "The Heat Production of Muscle and Nerve, 1848-1914," *Annual Review of Physiology,* 21 (1959): 1-18.

> isolated along with the attached eye. The first attempt, using small Warburg vessels, was clearly far below the required sensitivity; but the problem was solved that same night by threading the optic nerve into a capillary through a Vaseline seal, the eye being outside and the far end being closed with a measuring drop. Two such capillaries in a large closed test tube in a thermostat were arranged so that light could be shined on the eye of either nerve, and each one thus constituted a control for the other. The movement of the index drop was followed with an ocular micrometer minute by minute. The oxygen consumption when "natural" nerve impulses were carried was established, and a valuable microrespirometer became available. Since our time commitments were such that we had less than a week to work together, experiments were continued day and night and neither of us was out of his clothes for the entire period.*

With H. M. Serota he looked for a similar coupling of metabolism to functional activity within the mammalian brain, where, unfortunately, the elegant technique he had used on the optic nerve was inapplicable. Using temperature, the only approach available to them, but which they could measure accurately, they inserted five thermocouples into particular structures by means of a stereotaxic instrument. They recognized that a change in temperature accompanying functional activity at a point within the brain could be the result either of altered metabolism or altered perfusion. They also reasoned that where the temperature of the blood and brain was the same, a sudden increase in temperature was likely to indicate the liberation of metabolic heat. Recording temperature changes and electrical activity, they were able to demonstrate an increase in both in the optic radiations, the lateral geniculate, and the visual cortex upon illumination of the eye. It was not until forty years later that Louis Sokoloff succeeded in conclusively demonstrating the highly localized increased metabolism that accompanies functional activity in the visual system.

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* R. W. Gerard, "The Minute Experiment."

In 1931 Gerard carried out an imaginative series of experiments with D. D. Cook on the phenomenon of axonal degeneration. They reasoned that a nerve degenerates beyond a cut either because that portion is no longer stimulated, or because an important nutrient flow of chemical substances down the fiber is stopped. They tested the first possibility by chronically stimulating a cut sciatic nerve with buried electrodes and observed that the nerve lost its function even more rapidly when stimulated than when at rest. They concluded: "Degeneration of a nerve process isolated from its cell body might be due to lack of impulses conducted by it or of necessary substances spreading along it. The evidence (here) considered favors the second possibility."* This was perhaps the first suggestion, supported by experimental evidence, for the important process of axonal flow, which Paul Weiss was able to demonstrate thirteen years later. In 1951, employing isotopically labelled phosphorous, Ralph Gerard and four collaborators made the first measurements of the flow of a chemical substance, phosphoprotein, down nerve trunks, which occurred at a rate of 3 millimeters per day.

In 1940, with Oscar Sugar, Gerard tackled the controversial subject of regeneration in the transected mammalian spinal cord. Using immature animals, impeccable surgical techniques, and following a suggestion of Cajal by implanting pieces of peripheral nerve to serve as a scaffold on which the sprouts might climb, they provided the first demonstration that functional as well as structural regeneration could take place. Because of the prevailing belief that regeneration was impossible in the mammalian spinal cord, the report received scant attention. In summarizing a conference on the subject thirty years later, Gerard was able to take some satisfaction

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* R. W. Gerard and D. D. Cook, "The Effect of Stimulation on the Degeneration of a Severed Peripheral Nerve," *American Journal of Physiology,* 97 (1931):412-25.

from the new evidence presented, commenting that "Today the question is rather 'how,' not 'if.' "* Expressing a note of optimism, he congratulated William Windle for having kept the spark alive.

In 1933, 1934, and more completely in 1936, Gerard published, with Wade Marshall and Leon Saul, the results of research that opened a new chapter in neurophysiology and made possible the systematic mapping of the mammalian brain by Clinton Woolsey, and of the human brain by Wilder Penfield. Recognizing the power of recently developed tools—the oscilloscope, powerful amplifiers, and sterotaxic instruments for precise localization—they proceeded to explore the cat brain for spontaneous activity in its various regions. Using what they called "evoked potentials," they were able to trace the pathways by which particular sensory stimuli proceeded to the cortex and to follow their ramifications and ripples into quite unexpected regions. With the cooperation of two neurosurgeons, it was possible to demonstrate evoked cortical potentials at the operating table-the first demonstration of what was to become a powerful clinical tool for the diagnosis and further understanding of disturbed cerebral function.

With Benjamin Libet in 1939 and 1941, Gerard published the first experimental observations of steady potentials in the brain and their potential relationship to excitability patterns and the form of brain waves. They also demonstrated that spread of neuronal activity need not necessarily be mediated by the usual synaptic transmission. Caffeine-induced epileptiform waves were shown to travel across a complete transaction of the frog brain. It became apparent that extracellular fields of electric current flow could provide a significant mode of neuronal interaction and synchronization. Addi

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* R. W. Gerard, "Summary of the Paraplegia Conference, Palm Beach, Florida, May 1-3, 1972," R. W. Gerard Collection, University of California at Irvine.

tional studies on isolated frog brain and fragments thereof showed that spontaneous rhythmicity in brain tissue could be a function of localized neuron groups and their immediate ionic environments. As a result of these and his earlier observations, Gerard developed the now generally accepted concept that the electroencephalogram represents the summation of envelopes of slow potentials rather than neuronal spikes.

Although these remarkable contributions were made with the existing Adrian-Bronk concentric electrodes or with other macroelectrodes, Gerard was convinced that a true microelectrode could be developed that might record the physiological activity of individual neurons in the brain. When Judith Graham joined his laboratory as a graduate student a few years later, she began work on that goal by pulling fine glass capillaries. Gerard traced the idea behind this to what was probably his first research project, imaginatively inspired and ingeniously executed while he was an undergraduate at Chicago. From George Bartelmez, the professor of histology, he learned about myofibrils and the continuing controversy over whether they were real or fixation artifacts. He suggested to the professor "that if a quartz needle was moved steadily across a living muscle fiber, the tip would move smoothly if the protoplasm was homogeneous but in a sort of cogwheel fashion if viscous fibrils were embedded in fluid sarcoplasm, and this could be followed by reflecting a beam of light from a mirror attached to the needle."* Bartelmez was enthusiastic and presented Gerard with the original micromanipulator that had been developed in the department by Kite. "It was a museum piece but it still worked and I had a lot of fun learning micromanipulation and that protoplasm was a more complex thing than I

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* R. W. Gerard, "Informal Talk by an Old-timer" (lecture of 30 March 1972), R. W. Gerard Collection, University of California at Irvine.

thought." He did not solve that problem but the experience was invaluable later on to him and to neurophysiology.

Judith Graham was able to draw capillaries down to a diameter of several micra, fill them with a conducting potassium chloride solution, insert them into individual muscle cells, and record the intracellular potential. In their publication of the findings in 1946, Graham reported an average membrane potential of 62 mV but with a large range (4180 mV). They felt that the variation was due largely to the injury of insertion, since the finest electrodes gave smooth penetration under the microscope and also the highest readings.

Gilbert Ling, who was also a graduate student in the laboratory at that time, began to work on the problem and after two years found that it was possible to make finer micropipettes, considerably less than one micron at their tip and with a taper gentle enough that the tip would not break off. These could be inserted into a muscle cell without any indication of injury. Using these, Ling and Gerard were able to report in 1949 a membrane potential (78 + 5 mV) consistently at the high range that was previously obtained. Alan Hodgkin, attending a meeting of the American Physiological Society, was much impressed by these microelectrodes, came to observe their manufacture and application, and asked permission to take one back to Cambridge. There he modified it by increasing the concentration of electrolyte and adding a cathode follower, which made it capable of recording the rapid changes in potential that accompany action spikes in single cells. John Eccles applied the microelectrode to studies of activity of individual units within the spinal cord and brain and Andrew Huxley used it in muscle cells.

It would be difficult to exaggerate the important role that the capillary microelectrode has played in neurophysiology in the thirty years since its development. It made possible the neurophysiological research for which several Nobel Prizes have been awarded, and many of the most exciting advances that have occurred in the past two decades regarding neuronal activity in sleep, sensory processing, voluntary muscular movements, and attention—to name just a few—could not have occurred at the time without it.

Ralph Gerard indicated more than once that he did not consider the development of the microelectrode to be his most important contribution. This may have been because of its technical rather than its conceptual nature. It was the quality of imaginative and prescient conceptualizations, such as those that underlay his research on memory, of which he was most proud.

In his Gregory Lecture on "Physiology and Psychiatry" in 1948, Gerard proposed a concept of memory that was to become the basis of his experimental work in the field and that, today, remains the most plausible and heuristic model that we have:

> All is not over when an impulse flashes across a synapse and on to its destination. It leaves behind ripples in the state of the system. The fate of a later impulse can thus be at least a little influenced by the past history of the neurons involved. . . . Reverberant circuits, in principle, could last indefinitely, but in practice their duration is doubtful ... Perhaps there is a short-lasting active memory, depending on circuits, and a more enduring static one.*

In a later series of ingenious experiments, it was possible to show that by interrupting or confounding the electrical activity of the brain by induced hibernation or electroshock, recently acquired memories would be erased while those that had been formed an hour or more before would persist. He surmised that in a brief critical period following a learning experience, the memory was transformed from a dynamic

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* R. W. Gerard, "Physiology and Psychiatry," *American Journal of Psychiatry,* 106 (1949): 161-73.

representation in electrical activity through some chemical process to a more permanent molecular, physiological, or morphological state. In 1963, with T. J. Chamberlain and P. Halick, he tested this concept in an experiment that had all of the elegance and simplicity of Claude Bernard's demonstration of the site of action of curare. They found that the postural asymmetry produced by a unilateral cerebellar lesion induced in less than one hour a persistent asymmetry in function at the level of the lower motor neuron. In further experiments with G. H. Rothschild, evidence was adduced that this process could be facilitated by an agent that stimulated RNA synthesis and was retarded by drugs that inhibited that process. There have been thousands of experiments carried out since that time with more sophisticated physiological and biochemical techniques and the general thrust of all of the research has been to support the idea that short-term memory is consolidated in long-term memory by means of chemical processes, and that in that process RNA and protein synthesis may play essential roles.

Gerard was also proud of his contributions to psychiatry in recognition of which he was made a distinguished fellow of the American Psychiatric Association. His contributions here were not at the level of the minute experiment but of the large picture. Indeed, he played an important role in fostering the development in psychiatry of critical and judicious scientific approaches. In the Gregory Lecture, he commented on the continuing controversy regarding the genesis of psychoses:

> The constitutionalists and the organicists and the environmentalists and the mentalists too often are quarreling with each other as to which of them has *the* cause. Now, it is obviously useful to find out that schizophrenics have abnormal capillaries in their fingers, that they had abnormal experiences in their childhood, and that they have abnormal individuals as parents or sibs; but one does not exclude the others and no one of them

> can possibly be the whole story . . . . It is never sensible to ask the question "Does heredity or environment determine some characteristic?" It always takes both . . . and the meaningful question which is mostly not asked is a quantitative one.*

In his Academic Lecture at a convocation of the American Psychiatric Association (1955), he developed further the theme of etiologic and typologic diversity in mental illness:

> Many different etiologies may initiate the same train of pathogenic events; many different pathogenic sequences may produce a single pathology; and many different pathologies may still lead to a single symptom . . . . Can it be doubted that mental disease also presents symptoms and even syndromes which may subsume multiple nosologic entities and which are almost certainly based on multiple chains of abnormalities?†

Gerard prepared that lecture just before moving to the Mental Health Research Institute at the University of Michigan. During the eight years he spent there, he organized and directed a large multidisciplinary program that was to examine his concept of the heterogeneity of schizophrenia. On the basis of a small number of objective psychological, physiological, and biochemical measurements, it was found that seven typologies could be distinguished and characterized as well by clinical and behavioral observations.

Ralph Gerard's recognition of the importance of both genetic and environmental components in mental illness and the heterogeneity of the classical syndromes is by this time well established and has become part of the mainstream of modern psychiatry, where it has led to a more open-minded and less doctrinaire examination of plausible hypotheses that are not mutually exclusive.

It has been possible in the foregoing account to delineate

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* *Ibid.*

† R. W. Gerard, "The Academic Lecture: The Biological Roots of Psychiatry," *American Journal of Psychiatry,* 112 (1955):81-90.

some of the peaks of Ralph Gerard's scientific work. There were other peaks as well, and all rose from a high plateau of phenomenal productivity extending over fifty years and more than 500 publications. By far the major portion of his dedication was to neuroscience, which he insisted was a single discipline. In a deep sense, the appreciation and command of biophysics, biochemistry, physiology, psychology, and psychiatry that his own career exemplified offered a clear validation of that thesis. He played an important role in establishing the Society for Neuroscience and at its founding meeting in 1969 he was elected honorary president by unanimous acclaim.

His large picture, however, was even larger than neuroscience. It embraced all of science and human imagination as well, which he once described as the culminating efflorescence of the process of evolution up to the present time. He was a strong exponent of the implications and responsibility of science to society, but a courageous mentor as well of the reciprocal responsibility of society to scientific freedom and growth. In 1952, in an important paper entitled "The Organization of Science," he wrote:

> It bears repetition . . . that the increase in organization is an inexorable trend in evolution; our problem is to fight the diseases and enhance the uses of interrelatedness. The great danger is authoritarianism and conformity. This can blight at any level from the petty bookkeeping practices of too many governmental agencies to the national murder of the free pursuit of truth. The Lysenko story in Russia and the earlier distortions under Hitler deserve the most careful attention by scientists. Although these represent excesses under totalitarian police states, the anlages of similar attitudes are clearly present in our country.*

Ten years earlier, he saw the relationship of science to society in a remarkable perspective which is even more pertinent today:

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* *Annual Review of Physiology,* 14 (1952):1-12.

> These are the New Frontiers of mankind in the illimitable domain of the mind which science penetrates and scholarship consolidates. Change is often uncomfortable but it is exhilarating. Societies like animals must evolve or retrogress. Science, created by the social organism to sensitize itself to a fuller environment, is stirring and shaking the body politic with the birth pangs of the new. Men may suffer on the way —probably a caterpillar does not metamorphose painlessly into a butterfly-and the direction of travel is still obscure. But mankind is on the march somewhere, not vegetating into decadence. Science has brought and will bring men both weal and woe, mostly weal; and it is not destroying and will not destroy, rather it is enhancing those values of human society which we call civilization.*

That understanding and appreciation of science and its salutary role in social evolution was Ralph Gerard's enduring credo.

He died on February 17, 1974, survived by his son James and his wife Leona Bachrach ("Frosty") Gerard. A dedicated, creative, and compassionate person in her own right, she had made the two decades they spent together a happy and mutually enriching experience.

Many of his colleagues and former students have tried to put into words the unique qualities of Ralph Waldo Gerard. Perhaps Lord Adrian, who was his onetime mentor and longtime friend, stated it best: "There were few physiologists or philosophers with his understanding both of experimental techniques and of human aspirations."†

I am greatly indebted to Leona Bachrach Gerard, who shared with me her personal reminiscences, biographical memorabilia, and letters; to Benjamin Libet and Richard Thompson for notes on Gerard's scientific contributions; to Roger B. Berry of the Irvine University Library for complete bibliographies; and to Roxanne Louise Nilan, who compiled the Gerard Microfiche Collection, for access to unpublished documents.

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* R. W. Gerard, "Science at the Celebration," *University of Chicago Magazine,* 34 (1941):12-14.

† Lord Adrian to Leona Gerard, 10 March 1974.

Bibliography

1922 Chemical studies on intestinal intoxication. I. The presence and significance of histamine in an obstructed bowel. J. Biol. Chem., 52:111-24.

The lethal agent in acute intestinal obstruction. J. Am. Med. Assoc., 79:1581-84.

1924 With A. R. Abel, R. W. Backus, and H. Bourquin. Tryptophane and thyroid function. Am. J. Physiol., 73:287-95.

1926 With T. Koppanyi. Studies on spinal cord regeneration in the rat. Am. J. Physiol., 76:211-12.

With A. C. Downing and A. V. Hill. The heat production of nerve. Proc. R. Soc. London, 100B:223-51.

1927 The two phases of heat production of nerve. J. Physiol., 62:349-63.

With A. V. Hill and Y. Zotterman. The effect of frequency of stimulation on the heat production of nerve. J. Physiol., 63:130-43.

Studies on nerve metabolism. I. The influence of oxygen lack on heat production and action current. J. Physiol., 63:289-98.

Studies on nerve metabolism. II. Respiration in oxygen and nitrogen. Am. J. Physiol., 82:381-404.

With O. Meyerhoff. Studies on nerve metabolism. III. Chemismus and intermediarprozess. Biochem. Z., 191:125-46.

The activity of nerve. Science, 66:495-99.

1928 With A. Forbes. A note on action currents and "equilibration" in the cat's peroneal nerve. Am. J. Physiol., 86:178-85.

With A. Forbes. "Fatigue" of the flexion reflex. Am. J. Physiol., 86:186-205.

1929 With J. Wallen. Studies on nerve metabolism. V. Phosphates. Am. J. Physiol., 89:108-20.

With E. G. Holmes. Studies on nerve metabolism. IV. Carbohydrate metabolism of resting mammalian nerve. Biochem. J., 23:738-47.

1930 The response of nerve to oxygen lack. Am. J. Physiol., 92:498-541.

With T. E. Boyd. The effect of prolonged activity on the irritability of medullated nerve. Am. J. Physiol., 92:656-64.

With N. Tupikova. Creatine in medullated nerve. Proc. Soc. Exp. Biol. Med., 27:360-62.

With H. Necheles. The effect of carbon dioxide on nerve. Am. J. Physiol., 93:318-36.

Delayed action potentials in nerve. Am. J. Physiol., 93:337-41.

With E. G. Holmes and E. I. Solomon. Studies on nerve metabolism. VI. The carbohydrate metabolism of active nerve. Am. J. Physiol., 93:342-52.

Further observations on the oxygen consumption of nerve. Proc. Soc. Exp. Biol. Med., 27:1052-55.

With T. P. Feng. Mechanism of nerve asphyxiation: With a note on the nerve sheath as a diffusion barrier. Proc. Soc. Exp. Biol. Med., 27:1073-76.

The oxygen consumption of nerve during activity. Science, 72:195-96.

With S. Gelfan. Studies of single muscle fibers. II. A further analysis of the grading mechanism. Am. J. Physiol., 95:412-16.

1931 Nerve conduction in relation to nerve structure. Q. Rev. Biol., 6:9-83.

With D. D. Cook. The effect of stimulation on the degeneration of a severed peripheral nerve. Am. J. Physiol., 97:412-25.

With N. Tupikova. Creatine in nerve and muscle. Am. J. Physiol., 97:524-25.

With I. S. Falk. Observations on the metabolism of sarcina lutea I. Biol. Bull., 60:213-26.

Observations on the metabolism of sarcina lutea II. Biol. Bull., 60:227-41.

Oxygen diffusion into cells. Biol. Bull., 60:245-68.

With R. R. Grinker. Regenerative possibilities of the central nervous system. Arch. Neurol. Psychiatry, 26:469-84.

Phospho-creatine in relation to nerve activity. Collecting Net, 6:270-71.

1932 With R. J. Schachter. Glucose utilization by brain. Proc. Soc. Exp. Biol. Med., 29:525-28.

With V. P. Tang and S. Tang. Phosphate in lobster nerve. Proc. Soc. Exp. Biol. Med., 29:545-46.

With P. S. Tang. The oxygen tension-oxygen consumption curve of fertilized arbacia eggs. J. Cell. Comp. Physiol., 1:503-13.

With T. H. Chang and M. Shaffer. The influence of ions on nerve respiration. Am. J. Physiol., 101:19.

Nerve metabolism. Physiol. Rev., 12:469-592.

With M. McIntyre. The effect of thyroid feeding on tissue respiration. Am. J. Physiol., 103:225-31.

1933 With E. U. Still. Studies on the physiology of secretin. V. The effects on the respiration of the excised pancreas. Am. J. Physiol., 103:232-34.

With C. W. Baldridge. The extra respiration of phagocytosis. Am. J. Physiol., 103:235-36.

With T. H. Chang. Studies on nerve metabolism. VII. The role of lactic acid. Am. J. Physiol., 104:291-97.

With W. H. Marshall. Nerve conduction velocity and equilibration. Am. J. Physiol., 104:575-85.

With W. H. Marshall. Nerve impulse velocity and fiber diameter. Am. J. Physiol., 104:586-89.

With J. Abrams. The influence of activity on the survival of isolated nerve. Am. J. Physiol., 104:590-93.

With W. H. Marshall and L. J. Saul. Cerebral action potentials. Proc. Soc. Exp. Biol. Med. , 30:1123-25.

With B. B. Rubenstein. Absolute values and temperature coefficients of arbacia egg respiration. Proc. Soc. Exp. Biol. Med., 31:282-83.

With R. A. Cohen. The anoxic recovery of asphyxiated nerve. J. Cell. Comp. Physiol., 3:425-36.

With H. K. Hartline. Der Gaswechsel der "Naturlichen" und "Kunstlichen" Nervenimpulse. Naturwissenschaften, 50:88283.

1934 With O. M. Solandt, D. Y. Solandt, and E. Ross. Methemoglobin and methylene blue as cyanide antagonists. Proc. Soc. Exp. Biol. Med., 31:539-41.

With H. K. Hartline. Respiration due to natural nerve impulses. A method for measuring. J. Cell. Comp. Physiol., 4:141-60.

With B. B. Rubinstein. A note on the respiration of arbacia eggs. J. Gen. Physiol., 17:375-81.

With A. V. Hill, W. O. Fenn, and H. S. Gasser. Physical and chemical changes in nerve during activity. The chemical activity of nerve. Science, 79 (Suppl.):20-26.

With B. B. Rubinstein. Fertilization and the temperature coefficients of oxygen consumption in eggs of arbacia punctulata. J. Gen. Physiol., 17:677-85.

The irritability of a non-medullated nerve. J. Physiol., 83:24-25.

1935 With McK. Cattell. The inhibitory effect of high-frequency stimulation and the excitation state of nerve. J. Physiol., 83:407-15.

With M. Shaffer and T. H. Chang. The influence of blood constituents on oxygen consumption in nerve. Am. J. Physiol., 111:697-709.

With T. H. Chang and M. Shaffer. The influence of electrolytes on respiration in nerve. Am. J. Physiol., 111:681-96.

1936 With G. L. Engel. The phosphorus metabolism of invertebrate nerve. J. Biol. Chem., 112:379-92.

With H. Seroia. Localized thermal changes in brain. Am. J. Physiol., 116:59.

With N. O. Brookens and L. Ectors. Respiration of local brain regions. Technique and applications. Am. J. Physiol., 116:16-17.

With H. W. Magoun. Influence of potassium and calcium on motor discharges. Proc. Soc. Exp. Biol. Med., 34:755-56.

With M. S. Kharasch, R. R. Legault, and A. B. Wilder. Metal catalysts in biologic oxidations. I. The simple system. J. Biol. Chem., 113:537-55.

With M. S. Kharasch, R. R. Legault, and A. B. Wilder. Metal cata lysts in biologic oxidations. II. Tissue inhibitors. J. Biol. Chem., 113:557-69.

With W. H. Marshall and L. J. Saul. Electrical activity of the cat's brain. Arch. Neurol. Psychiatry, 36:675-735.

With F. Offner. A high speed crystal ink writer. Science, 84: 209-10.

Metabolism and excitation. Cold Spring Harbor Symp. Quant. Biol., 4:194-201.

Factors controlling brain potentials. Cold Spring Harbor Symp. Quant. Biol., 4:292-304.

Factors influencing brain potentials. Trans. Am. Neurol. Assoc., 62:55-60.

1937 The metabolism of brain and nerve. Annu. Rev. Biochem., 6:419-44.

With M. B. Cohen. Oxidizing enzymes in brain extracts. Am. J. Physiol., 119:34-47.

With H. Blake. Brain potentials during sleep. Am. J. Physiol., 119:692-703.

With J. Z. Young. Electrical activity of the central nervous system of the frog. Proc. R. Soc. London, 122B:343-52.

With L. Schoen. The role of dicarboxylic acids in brain oxidations. Am. J. Physiol., 119:397-98.

With N. Tupikova. Salt content of neural structures. Am. J. Physiol., 119:414-15.

With R. A. Cohen. Hyperthyroidism and brain oxidations. J. Cell. Comp. Physiol., 10:223-40.

With H. M. Serota. Localized thermal changes in the cat's brain. J. Neurophysiol., 1:115-24.

Brain metabolism and circulation. Proceedings, Association for Research in Nervous Mental Disease, 18:316-45.

1938 With B. Libet. Automaticity of central neurones after nicotine block of synapses. Proc. Soc. Exp. Biol. Med., 38:886-88.

With L. E. Ectors and N. L. Brookens. Autonomic and motor localization in the hypothalamus. Arch. Neurol. Psychiatry, 39:789-98.

The role of pure science. Science, 88:361-68.

Anoxia and neural metabolism. Arch. Neurol. Psychiatry, 40: 985-96.
With N. Tupikova. Creatine in nerve, muscle and brain. J. Cell. Comp. Physiol., 12:325-60.
With O. Sugar. Anoxia and brain potentials. J. Neurophysiol., 1:558-72.
1939 With N. Tupikova. Nerve and muscle phosphates. J. Cell. Comp. Physiol., 13:1-12.
With H. Blake and N. Kleitman. Factors influencing brain potentials during sleep. J. Neurophysiol., 2:48-60.
With B. Libet. Control of the potential rhythm of the isolated frog brain. J. Neurophysiol., 2:153-69.
With H. H. Dubner. Factors controlling brain potentials in the cat. J. Neurophysiol., 2:142-52.
With E. Tokaji. Avitaminosis B, and pigeon brain potentials. Proc. Soc. Exp. Biol. Med., 41:653-58.
Mechanism of action of "shock therapies." Arch. Neurol. Psychiatry, 42:564-65.
1940 With O. Sugar. Spinal cord regeneration in the rat. J. Neurophysiol., 3:1-19.
With B. Libet. The control of normal and "convulsive" brain potentials. Am. J. Psychiatry, 96:1125-51.
Organism, society and science. Sci. Mon., 50:340-50, 403-12, 530-35.
1941 With F. Panimon and M. K. Horwitt. Orthophenanthroline as accelerator of brain tissue oxygen consumption. J. Cell. Comp. Physiol., 17:17-29.
With F. Panimon and M. K. Horwitt. Iron induced oxidations in brain and other tissues. J. Cell. Comp. Physiol., 17:1-16.
With B. Libet. Steady potential fields and neurone activity. J. Neurophysiol., 4:438-55.
The interaction of neurones. Ohio J. Sci., 41:160-72.
Intercellular electric fields and brain function. Schweiz. Med. Wochenschr., 12:555-59.

With J. Tobias. An improved capillary microrespirometer. Proc. Soc. Exp. Biol. Med., 47:531-33.

Science at the celebration. Univ. Chicago Mag., 34:12-14.

1942 With J. Pearce. The respiration of neurones. Am. J. Physiol., 136: 49-65.

A biological basis for ethics. Philos. Sci., 9:92-120.

Higher levels of integration. Science, 95:309-13.

Electrophysiology. Annu. Rev. Physiol., 4:329-58.

1944 With F. P. Simon and M. K. Horwitt. The inhibition of catalyzed oxidations by hemins. J. Biol. Chem., 154:421-25.

1945 With A. E. Emerson. Extrapolation from the biological to the social. Science, 101:582-85.

1946 The biological basis of imagination. Sci. Mon., 62:477-99.

With J. M. Tobias, C. C. Lushbaugh, H. M. Patt, S. Postel, and M. N. Swift. The pathology and therapy with 2, 3-dimercaptopropanol (BAL) of experimental cadmium poisoning. J. Pharmacol. Exp. Ther. (Suppl.), 87:102-18.

With H. M. Patt, J. M. Tobias, M. N. Swift, and S. Postel. Hemodynamics in pulmonary irritant poisoning. Am. J. Physiol., 147: 329-39.

With J. Graham. Membrane potentials and excitation of impaled single muscle fibers. J. Cell. Comp. Physiol., 28:99-117.

Nerve metabolism and function. Ann. N.Y. Acad. Sci., 47:575-600.

With S. Postel, J. M. Tobias, and H. M. Patt. The effect of exercise on mortality of animals poisoned with diphosgene. Proc. Soc. Exp. Biol. Med. , 63:432-36.

1947 With F. P. Simon and A. M. Potts. Metabolism of isolated lung tissue: Normal and in phosgene poisoning. J. Biol. Chem., 167:303-11.

With F. P. Simon and A. M. Potts. Action of cadmium and thiols on tissues and enzymes. Arch. Biochem., 12:283-91.

With L. L. Boyarsky and J. M. Tobias. Nerve conduction after inactivation of choline esterase. Proc. Soc. Exp. Biol. Med., 64:106-8.

The scope of science. Sci. Mon., 64:496-512.

With S. W. Kuffler. The small-nerve motor system to skeletal muscle. J. Neurophysiol., 10:383-94.

Science and the public. Science, 106:23-25.

Anesthetics and cell metabolism. Anesthesiology, 8:453-63.

With R. D. Tschirgi. The carotid-mandibular reflex in acute respiratory failure. Am. J. Physiol., 105:358-64.

1949 With A. M. Potts and F. P. Simon. The mechanism of action of phosgene and diphosgene. Arch. Biochem., 24:329-37.

Physiology and psychiatry. Am. J. Psychiatry, 106:161-73.

With V. B. Brooks and R. E. Ransmeier. Action of anticholinesterases, drugs and intermediates on respiration and electrical activity of the isolated frog brain. Am. J. Physiol., 157:299-316.

With J. M. Tobias, S. Postel, H. M. Patt, C. C. Lushbaugh, and M. N. Swift. Localization of the site of action of a pulmonary irritant, diphosgene. Am. J. Physiol., 158:173-83.

With G. Ling. The normal membrane potential of frog sartorius fibers. J. Cell. Comp. Physiol., 34:383-96.

With G. Ling. The influence of stretch on the membrane potential of the striated muscle fiber. J. Cell. Comp. Physiol., 34:397-405.

With G. Ling. The membrane potential and metabolism of muscle fibers. J. Cell. Comp. Physiol., 34:413-38.

1950 With A. M. Potts, F. P. Simon, J. M. Tobias, S. Postel, M. N. Swift, and H. M. Patt. Distribution and fate of cadmium in the animal body. Arch. Ind. Hyg. Occup. Med., 2:175-88.

A biologist's view of society. Common Cause, 3:630-38.

With G. Ling. Effect of external potassium concentration on the membrane potential of single muscle fibers. Nature, 165: 113-14.

Some aspects of neural growth, regeneration and function. In: *Genetic Neurology,* ed. P. Weiss, pp. 199-207. Chicago: Univ. of Chicago Press.

With R. W. Doty. Nerve conduction without increased oxygen consumption: Action of azide and fluoracetate. Am. J. Physiol., 162:458-68.

Some of the problems concerning digital notions in the central nervous system. In: *Cybernetics,* ed. H. von Foerster, pp. 11-57. New York: Josiah Macy Jr. Foundation.

1951 The physiology of pain. Abnormal neurone states in causalgia and related phenomena. Anesthesiology, 12:1-13.

With A. L. Samuels, L. L. Boyarsky, B. Libet, and M. Brust. Distribution, exchange and migration of phosphate compounds in the nervous system . Am. J. Physiol., 164:1-15.

With L. G. Abood. A hexokinase inhibitor in nerve. Proc. Soc. Exp. Biol. Med., 77:438-41.

1952 The organization of science. Annu. Rev. Physiol., 14:1-12.

With L. G. Abood, J. Banks, and R. D. Tschirgi. Substrate and enzyme distribution in cells and cell fractions of the nervous system. Am. J. Physiol., 168:728-38.

With L. G. Abood. Oxidative esterification of phosphate by neural tissue. Am. J. Physiol., 168:739-41.

With L. G. Abood and S. Ochs. Electrical stimulation of metabolism of homogenates and particulates. Am. J. Physiol., 171:134-39.

With L. G. Abood and R. D. Tschirgi. Spatial and chemical. exchange of phosphate in the resting and active nervous system. In: *Phosphorus Metabolism,* vol. 2, ed. W. D. McElroy and B. Glass, pp. 798-821. Baltimore: Johns Hopkins Press.

1953 Central excitation and inhibition. In: *Cybernetics,* ed. H. von Foerster, Mead, and Teuber, pp. 127-50. New York: Josiah Macy Jr. Foundation.

With H. P. Jenerick. Membrane potential and threshold of single muscle fibers. J. Cell. Comp. Physiol., 42:79-102.

With L. G. Abood. A new group of cytochrome oxidase inhibitors. J. Pharmacol. Exp. Ther., 108:261-73.

1954 With L. G. Abood. Enzyme distribution in isolated particulates of rat peripheral nerve. J. Cell. Comp. Physiol., 43:379-92.

With G. Falk. Effect of micro-injected salts and ATP on the membrane potential and mechanical response of muscle. J. Cell. Comp. Physiol., 43:393-403.

With E. Streicher. Phosphate exchange in brain phospholipids *in vivo* and *in vitro.* Proc. Soc. Exp. Biol. Med., 85:174-77.

With R. E. Ransmeier. Effects of temperature, convulsion, and metabolic factors on rodent memory and EEG. Am. J. Physiol., 179:663-64.

Experiments in microevolution. Science, 120:727-32.

1955 With A. F. Lash and G. Falk. A microelectrode method in the diagnosis of carcinoma of the cervix and endometrium. Am. J. Obstet. Gynecol., 70:354-58.

With L. G. Abood. A phosphorylation defect in the brains of mice susceptible to audiogenic seizure. In: *Biochemistry of the Developing Nervous System,* ed. H. Waelsch, pp. 467-72. New York: Academic Press.

The academic lecture: The biological roots of psychiatry. Am. J. Psychiatry, 112:81-90.

With M. E. Kosman. The effect of adrenaline on a conditioned avoidance response. J. Comp. Physiol. Psychol., 48:506-8.

With E. Sigg and S. Ochs. Effects of the medullary hormones on the somatic nervous system in the cat. Am. J. Physiol., 183:419-26.

1956 With K. Koketsu. Effect of sodium fluoride on nerve-muscle transmission. Am. J. Physiol., 186:278-82.

With C. Kluckhohn and A. Rapoport. Biological and cultural evolution. Behav. Sci., 1:6-34.

1957 The units and concepts of biology. Science, 125:429-33.

With D. G. Marquis, E. L. Kelly, J. G. Miller, and A. Rapoport. Experimental studies of behavioral effects of meprobamate on normal subjects. In: *Meprobamate and Other Agents Used in Mental Disturbances,* ed. Otto Whitelock. Ann. N.Y. Acad. Sci., 67: 701-10.

Problems in the institutionalization of higher education; an analysis based on historical materials. Behav. Sci., 2:134-46.

1958 With E. L. Kelly, J. G. Miller, D. G. Marquis, and L. Uhr. Continued meprobamate and proclorperazine administration. Arch. Neurol. Psychiatry, 80:247-52.

Concepts of biology. Behav. Sci., 3:92-215.

Anxiety and tension (Hermann M. Biggs Lecture). Bull. N.Y. Acad. Med., 34:429-44.

1959 With A. Rabe. The influence of drugs on memory fixation time. Am. Psychol., 14:423.

1960 Physiology from physicians-the nervous system. In: *Disease and the Advancement of Basic Science* (Lowell Lectures), ed. H. K. Beecher, pp. 234-51. Cambridge, Mass.: Harvard Univ. Press.

1961 The program of the Mental Health Research Institute. Behav. Sci., 6:66-71.

The architecture of knowledge and neural functions. In: *Lectures in Experimental Psychiatry,* ed. H. W. Brosin, pp. 147-63. Pittsburgh: Univ. of Pittsburgh Press.

Fixation of experience. In: *Brain Mechanisms and Learning,* ed. A. Delafresnaye, pp. 21-35. Oxford: Blackwell Scientific Publications.

To prevent another world war-truth detection. J. Conflict Resolution, 5:212-18.

1962 With B. Libet. An analysis of some correlates of steady potentials in mammalian cerebral cortex. Electroencephalogr. Clin. Neurophysiol., 14:445-52.

1963 Material basis of memory. J. Verb. Learn. Verb. Behav., 2:22-63.

The nosology of schizophrenia: A cooperative study. Am. J. Psychiatry, 120:16-29.

With T. J. Chamberlain and G. H. Rothschild. Drugs affecting RNA and learning. Proc. Natl. Acad. Sci. USA, 49:918-24.

With T.J. Chamberlain and P. Halick. Fixation of experience in the rat spinal cord. J. Neurophysiol., 26:662-73.

1965 Intelligence, information and education. Science, 148:762-66.

1966 Building better brains. In: *Enzymes in Mental Health,* ed. G. J. Martin and B. Kisch, pp. 194-200. New York: J. B. Lippincott.

1967 Shaping the mind: Computers in education. In: *Applied Science and Technological Progress,* pp. 207-28. Washington, D.C.: U.S. Govt. Print. Off. for the National Academy of Sciences.

1968 With N. B. Mattson. Typology of schizophrenia based on multidisciplinary observational vectors. In: *Recent Advances in Biological Psychiatry,* vol. 10, pp. 507-34. New York: Plenum Press.

The neurophysiology of purposive behavior. In: *Purposive Systems, Proceedings of First Annual Symposium of American Society of Cybernetics,* ed. H. von Foerster, J. D. White, L. J. Peterson, and J. K. Russell, pp. 25-34. New York: Spartan Books.

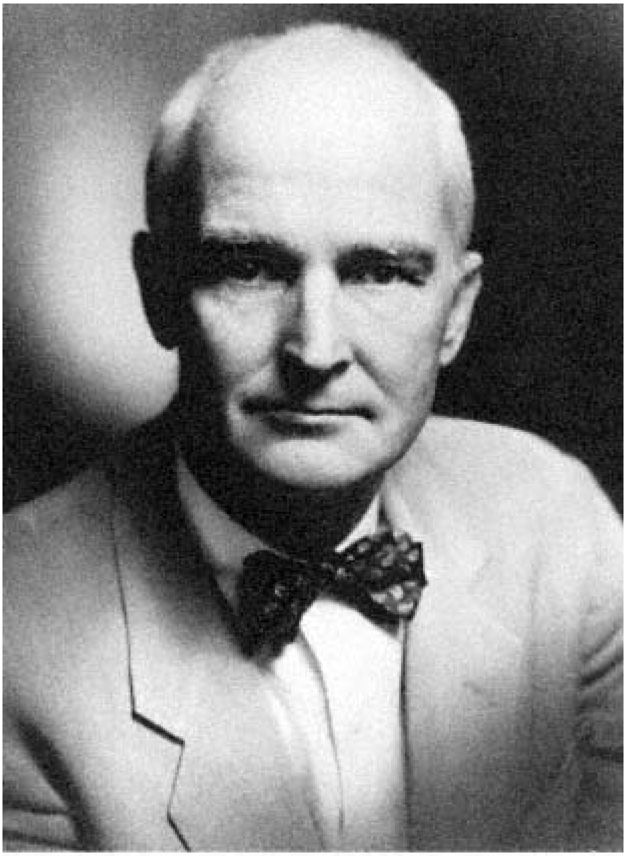
Build thee more stately mansions, O my soul. Recent Adv. Biol. Psychiatry, 10:1-5.

1969 Computer assisted learning: Introduction & general considerations. Proc. Natl. Acad. Sci. USA, 63:573-79.

Hierarchy, entitation & levels. In: *Hierarchical Structures, Proceedings of the Symposium,* ed. L. Whyte, A. G. Wilson, and D. Wilson, pp. 215-28. New York: American Elsevier.

1975 Is the age of heroes ended? In: *The Life and Contributions of Walter Bradford Cannon,* ed. C. McC. Brooks, K. Koisumi, and J. O. Pinkston, pp. 197-208. State Univ. of New York.

The minute experiment and the large picture. In: *The Neurosciences: Paths of Discovery,* ed. F. G. Worden, J. Swazey, and G. Adelman, pp. 456-74. Cambridge, Mass.: MIT Press.



John H. Gibbon Jr.

Photoograph courtesy of Fabian Bachrach

John Heysham Gibbon, Jr.

September 29, 1903 -February 5, 1973

by Harris B Shumacker, Jr.

John Hetsham Gibbon, Jr, or Jack, as he was generally known, was born in Philadelphia, Pennsylvania on September 29, 1903. His mother was Mrs. Marjorie Young Gibbon and his father John Heysham Gibbon, a distinguished, nationally recognized surgeon and professor of surgery at the Jefferson Medical College. His family background is of unusual interest and was undoubtedly of considerable importance in his career.

The first of the Gibbons arrived in Philadelphia from Wiltshire, England in 1684 and, as Jack's sister Marjorie* says, were named prophetically John and Margery. Jack's great-great-grandfather, John Hannum Gibbons, born in Chester County, Pennsylvania and educated in medicine in Edinburgh, was the first American doctor in the direct line of five down to Jack. His son, the first John Heysham Gibbon, born in 1795, dropped the *s*, and the name remained Gibbon thereafter. Though he graduated in medicine from the University of Pennsylvania, he never practiced; instead he became a prominent mineralogist and in 1834 was appointed assayer of the U.S. Mint at Charlotte, North Carolina. His

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* All quotations except those cited specifically as from other sources are from a carefully prepared, delightful family history written for me by Marjorie Battles during the winter of 1979.

second son, Robert, became a practicing physician, as did Robert's two sons, Jack's father and uncle. In addition, through Dr. Gibbon, Sr.'s grandmother, Jack had a greatgreat-great-grandfather who was also a doctor, John Lardner, "Physician of London." A nephew still carries the Gibbon name on in the profession. The family can be proud, indeed, of the heritage of medical service that reached such heights in the accomplishments of John H. Gibbon, Jr.

The only grandparent alive during Jack's life was his maternal grandfather, Samuel B. M. Young, one of our truly outstanding military figures. Born of a prominent Pittsburgh family in 1840, he volunteered upon the outbreak of the Civil War. His promotions from the time of his enlistment in April 1861, from private through the ranks to brigadier general, came about with unbelievable rapidity, within a period of only four years. Following service in Cuba during the war with Spain, he was made a major general and later a lieutenant general. Perhaps the most important post he held was that of the first presidency of the War College in 1902. At the time of his retirement, the Secretary of War, Elihu Root, stated: "There can be no better wish for the Army in the future than that its officers shall remember how distinction and the highest rank have come to this officer, not as a result of self-seeking or political or social influence, but as the result of duty well done."*

This unusual standard of military achievement was upheld by General Young's son, John, and his grandson, Jack, and the lifetime achievements of both, like his, resulted from their own efforts and not from "influence." Jack's father served both in the Spanish-American War and in World War I, during which his assignments included those of consultant in surgery to the American Expeditionary Forces and, ul

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* *New York Herald Tribune,* 2 Sept. 1924.

timately, surgical consultant to the American hospitals in England.

Early during World War II, Jack, too, volunteered for military duty, thus interrupting his practice, teaching, and, of even more significance, his research activities. Invalided home with a herniated disc after he served with distinction in the Pacific with the Pennsylvania Hospital Unit, he took over direction of the surgical service at the Mayo General Hospital in 1945, a post he kept until his discharge at the end of that year.

Jack's father, John Gibbon, Sr., was born in Charlotte, North Carolina in 1871. Following his education in preparatory schools, he attended the Jefferson Medical College, from which he graduated in 1891. He remained closely associated with this institution as well as with the Pennsylvania Hospital through his active years. He was a devoted teacher; a kindly, sympathetic practitioner; and a gentle, careful, skillful operator. He contributed significantly to the clinical surgical literature but, unlike his son, he did no experimental laboratory research. He was honored by being made an officer of a number of professional societies and became first secretary and then president of the American Surgical Association.

In 1901 in San Francisco he married Miss Marjorie Young, whom he had met during the Spanish War at Jefferson Barracks, Missouri. She was one of the "five beautiful Young sisters," daughters of General Young and his wife, Margaret McFadden Young. The new Mrs. Gibbon had been educated in various places according to the location of her father's military assignments. She had a deep love of books and poetry and never stopped reading. It is probable that Jack inherited his fondness for poetry from her. Her experiences were broadened by a year abroad when, following the death of her mother and the marriage of her three older sisters at the turn of the century, she took her ten-year-old sister to Dresden for a year of study. Full of wit, affection, and cordiality, she made for herself a warm and stimulating place among the family's Philadelphia friends, though she often told her children amusing tales of what some of the local people had expected of the "Western Bride."

Jack and his brothers and sister grew up in a happy household, living in Philadelphia during the winter and in summer near Media on beautiful Lynfield Farm, which Jack was to inherit upon his parents' death. It must have been a busy home, with many visitors who often stayed weeks at a time, including "army cousins fattening for West Point, southern cousins coming up for their Philadelphia dental appointments, a White Russian refugee, and a homesick Louisiana bride whom mother had met on a commuter train."

Jack was eighteen months younger than Marjorie, eighteen months older than Sam, and four and a half years older than Robert. He was athletic, very competitive, and at times exhibited an "explosive temper." Excelling his brothers and friends in almost all sports, he was finally overtaken by them in horsemanship. One of the favorite pastimes of the family was chess, a game often begun before dinner, continued between courses, and usually terminated with Jack the winner. This game was one for which his love was never lost. He had great affection and admiration for his parents and enjoyed long talks with his father, whose devotion to his profession and receptiveness to new ideas Jack valued highly. Their major differences lay in the field of politics, his liberalism standing far apart from his father's conservatism. Both parents died in 1956 within a week of one another.

Jack attended the Penn Charter School in Philadelphia, where he was an excellent student. Marjorie says that he returned from summer camp in 1919, just before entering Princeton, an entirely changed person, in large measure because of one of his counselors, Jim Landis, who was later to become the first chairman of SEC. Though he had always been studious, he was now literally "afire" with intellectual interests, keen about literature and philosophy. At the end of his sophomore year, he joined Marjorie, who was taking courses at the Sorbonne, for a summer in Europe. They wandered about free and unrestrained, Jack going along with her "gung-ho" interest in French history, but spending all his spare time reading William James's *Varieties of Religious Experience*. He talked of going to medical school in Edinburgh and of her keeping house there for the two of them. Instead, he returned to Princeton. These first years at Princeton were not entirely happy ones, since he felt too young and immature for real companionship with his classmates, having entered before his sixteenth birthday. A great deal of his time was spent reading and studying. He graduated in 1923 at nineteen.

Towards the end of his first year in the Jefferson Medical College, Jack considered quitting, thinking that something else, perhaps writing, might prove more to his taste. His father made a very strong case for the continuation of his professional education, telling him, "If you don't want to practice you needn't, but you won't write worse for having it." He received his medical degree in 1927.

Though Jack has said and written that his interest in research was stimulated during his internship at the Pennsylvania Hospital, Marjorie feels that the investigative scientific spirit may have been with him since early childhood. As an example, according to one of their mother's stories, she was walking down the street one day holding his little hand when she found that her progress was slowed by his pausing to wave his foot over the curb. She asked, "Jack, what *are* you doing?" He answered: "Well, Mother, if God is everywhere and you can't see Him and you can't hear Him, why can't you feel Him?"

His interest in medical experimentation, however, was first aroused by Dr. Joseph Hayman's clinical studies. Dr. Hayman was looking into the effects of potassium chloride versus sodium chloride in the diet of a severe hypertensive; the patient was unaware of which of the two salts he was served. While taking blood pressures at intervals, Jack came to the exciting realization that contributions of new knowledge could be forthcoming from controlled experimentation.

It is interesting that his initial stimulus came from a physician, in view of the hopes of surgeons that their specialty should be comprised of good physicians who have as their primary therapeutic modality the special capability of operating. This objective certainly underlay Jack Gibbon's professional life. Similarly, the obvious conviction that the best management of surgical disorders requires good basic scientific understanding of them makes his early and continuing interest in physiological and biochemical matters of real significance. It is probably meaningful that during the period from 1930 to 1933, only one of his nine publications appeared in a surgical periodical; the remainder were published in such journals as the *American Journal of Medical Science, the Journal of Clinical Investigation,* the *Proceedings of the Society of Experimental Biology and Medicine,* and the *Archives of Internal Medicine.*

He consulted his father's partner, John B. Flick, surgeon of the Pennsylvania Hospital, concerning the possibility of a career that would ultimately combine research and surgery. John Flick not only assured him that the two were perfectly compatible, but made the fortunate suggestion that he apply for a research fellowship with Dr. Edward B. Churchill at the Harvard Medical School. Jack realized that this would permit him both to find out whether he had any capability for research and whether he liked it. His father offered no objections, provided he continue to recognize the value of balancing research with surgical experience.

He received the appointment and began working with Churchill in February of 1930 in a small laboratory in the

Gate House of the Boston City Hospital. His preceptor suggested that his first research effort be a study of the relationship between pulmonary artery pressure and blood flow in experimentally produced pulmonary arteriovenous fistulas. Stimulated by this project, he proceeded to undertake a number of other investigations that dealt with pulmonary circulation and cardiac function.

A few months after his arrival in Boston, Churchill succeeded to the codirectorship of the West Surgical Service at the Massachusetts General Hospital and moved the laboratory to the top floor of the Bullfinch Building. It was in this institution in February of 1931 that Jack first conceived the idea of developing a mechanism for achieving extracorporeal gaseous exchange and temporarily maintaining body circulation. A patient had developed massive pulmonary embolism following a cholecystectomy. She was taken to the operating room for observation, and Gibbon was assigned the duty of following vital signs. He was to notify his chief when her condition deteriorated to the point where it was felt justifiable to undertake pulmonary embolectomy-an exceedingly risky procedure at that time. This took place early the next morning and, despite Churchill's well-performed operation, ended fatally.

Jack described the development of the idea thus:

> During that long night, helplessly watching the patient struggle for life as her blood became darker and her veins more distended, the idea naturally occurred to me that if it were possible to remove continuously some of the blue blood from the patient's swollen veins, put oxygen into that blood and allow carbon dioxide to escape from it, and then to inject continuously the now-red blood back into the patient's arteries, we might have saved her life. We would have bypassed the obstructing embolus and performed part of the work of the patient's heart and lungs outside the body.*

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* J. H. Gibbon, Jr., "The Development of the Heart-Lung Apparatus," *Review of Surgery,* 27 (1970):231-44.

The conception of what was to prove his life's principal work was only one of two important events of that year in Boston. The other was his marriage to his constant helper in the laboratory, Churchill's technician Mary Hopkinson, affectionately known as Maly, daughter of Charles Hopkinson, one of America's greatest portraitists. It is an extraordinary coincidence that both Jack and his father should have married one of five sisters.

In the spring of 1931 the couple returned to Philadelphia. During the next three and a half years, the mornings were spent practicing surgery and the afternoons working upon a variety of research problems in the laboratories of the University of Pennsylvania's School of Medicine. This period confirmed John Flick's early judgment that it was feasible to combine clinical surgery with research. Though unable to pursue the idea that had captivated his imagination in Boston, Jack was fortunate in many ways. Of particular value were his research opportunities and close association with Eugene M. Landis, who later became professor of physiology at Harvard. A number of important contributions were forthcoming, many carried out conjointly with his wife.

The idea of developing an apparatus for cardiopulmonary bypass remained continuously and vividly in the back of his mind. When he asked Churchill for another year's opportunity to work with him, he was not only awarded a fellowship, but was told that Maly might have a position as his technical assistant. The research plan he had in mind did not by any means meet with universal approval. As a matter of fact, Churchill himself was not enthusiastic though he did not object to its being undertaken. Others, thinking more of his potential academic career, advised him to embark upon less ambitious projects and ones more likely to result in publications in the medical literature. An exception was his friend Eugene Landis, who was particularly helpful and judged that the effort was worth the attempt and might, indeed, prove successful.

The work was begun and the husband-wife team had a very rewarding year. During the experiments the blood had to be rendered noncoagulable and heparin was available as a suitable agent. The lack of a heparin antagonist at that time did not prove a serious handicap. The initial arterial inflow was through the femoral artery and the venous outflow from a superior vena caval catheter introduced through a jugular vein. Exclusion of cardiac function was achieved by pulmonary artery occlusion. The first oxygenator was a revolving cylinder into which the blood withdrawn from the animal was introduced tangentially at the top in the direction of rotation and resulted in a film of blood descending down the inner surface of the nonwettable metal cylinder. It was collected at the bottom through a knife-like edge into a stationary cup that was made of glass and surrounded by a jacket through which warm water could be circulated to avoid chilling the animal. A similar water jacket was utilized in another portion of the circuit and the blood was returned to the animal. The film of blood was exposed to oxygen and it was determined that it took up oxygen and lost carbon dioxide satisfactorily. After a while, to their excitement and joy, it proved possible to sustain the entire cardiorespiratory function of cats for nearly four hours and to demonstrate that the animals could, after the extracorporeal device was discontinued, maintain their own cardiac and pulmonary activity. These results were not reported until 1937.

After the year in Boston the Gibbons returned to Philadelphia in 1935, and the work was continued in the Harrison Research Laboratories of the University of Pennsylvania. Progressively more refined apparatus was developed, and the experiments went better and better, until by 1939 it was possible to report that, after periods varying from twelve to twenty minutes of total substitution of the device for the function of the heart and lungs, four cats had survived indefinitely in healthy condition, and others for varying shorter periods of time.

Though the initial effort had been undertaken with the hope of managing massive pulmonary embolism better, Jack perceived shortly after it was begun that it had far greater potentialities. At the time of his report of these studies to the 1939 meeting of the American Association for Thoracic Surgery, he stated modestly: "It is conceivable that a diseased mitral valve might be exposed to surgical approach under direct vision and that the fields of cardiac and thoracic surgery might be broadened."[*] The presentation was discussed by the guest speaker, Professor Clarence Crafoord of Stockholm, and by Leo Eloesser, who had been president of the organization the preceding year. Eloesser said that the report reminded him of the fantastic tales of Jules Verne, which anticipated seemingly impossible accomplishments that were later realized.[†]

During the next few years, further innovations in the device were made with the idea of supplanting the function of the heart and lungs of larger animals and, eventually, of patients. It was at this time that the investigations had to be stopped because of World War II.

Upon Jack's return to Philadelphia after his military service, he was given an appointment as assistant professor of surgery at Pennsylvania; shortly thereafter, early in 1946, he became director of surgical research at the Jefferson Medical

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[*] J. H. Gibbon, Jr., " The Maintenance of Life During Experimental Occlusion of the Pulmonary Artery Followed by Survival," *Surgery, Gynecology, and Obstetrics, 69* (1939): 602-14.

[†] Unfortunately, having forgotten momentarily about the rules of the Society, Jack had already submitted the paper to *Surgery, Gynecology, and Obstetrics* so that it could not appear in *the Journal of Thoracic Surgery,* and the discussions of Crafoord and Eloesser did not accompany the publication.

College. Here was the opportunity he needed. Adequate research laboratories soon became available, well-equipped with the necessary apparatus and facilities for the best possible care of experimental animals. A staff of eager and hardworking young associates joined him. The work on the heart-lung machine was resumed and progressed more rapidly with the generous assistance of the International Business Machines Corporation and its board chairman, Mr. Thomas J. Watson.

In 1956 Jack received the prized Samuel D. Gross Professorship of Surgery and chairmanship of the Department, posts he held with great distinction until his voluntary retirement in 1967. Under his leadership the Surgical Department at Jefferson became truly outstanding. It was soon the Mecca for intelligent, inquisitive, innovative young surgeons from home and abroad. The residency training program was excellent. Students and house staff were impressed with his gentlemanly straightforward manner, his great ability, and his insistence that thinking fruitfully was even more important than learning factual information. Under his inspiring guidance, all about him wanted to make the most of their capabilities. He was greatly admired for his clarity of thought, careful analysis, sound conclusions and judgments, and open-minded receptiveness.

Though the development of the heart-lung machine constituted his long-term primary interest and was his greatest scientific contribution, he and his colleagues worked upon a variety of other experimental laboratory and clinical problems, and his list of publications is truly a significant one. It includes basic physiological and biochemical studies that assisted considerably in better understanding of cardiac function, pulmonary ventilation, acid-base balance, anesthesia in thoracic procedures, carcinoma of the lung, and other esophageal and pulmonary problems. In addition, not included in his list of published works were numerous valuable discussions of papers presented at meetings of various surgical societies—understanding, pertinent remarks that added to the full appreciation of the potentialities and limitations of the material presented.

Progress in the development of the heart-lung machine was often slow but always steady, and after a period of years it resulted in a highly refined and efficient apparatus. The final model utilized a stationary screen oxygenator. This tedious but rewarding work reached its culmination on 6 May 1953, when Jack was able to perform the first successful open heart procedure upon a patient with the aid of total cardiopulmonary bypass. With the help of his wife and associates, he had by then pursued the investigation from its conception, through its period of gestation and infantile progress, into adult maturity. It was characteristic of him to pass on happily to younger surgeons in clinics about the world the opportunity his contribution provided for developing almost limitless new techniques that have made possible the repair of most of the congenital and acquired cardiac abnormalities. It was also in character that he should have welcomed the various modifications, soon forthcoming, of his extracorporeal device.

Though the principal benefit of the apparatus for extracorporeal circulation and respiration was its unbelievably great extension of our capabilities to deal with hitherto incurable cardiac lesions, it is quite evident that it has had an enormous influence over and beyond these therapeutic advances. Each step forward always has a potential benefit that is more widespread than the contribution itself. It would seem safe to say that none has had a more far-reaching, helpful effect upon medicine, its specialties, and the basic sciences than Gibbon's. The surgical progress that resulted from use of the heart-lung machine has brought about in surgery a renewed emphasis upon the importance of precise anatomic and physiologic diagnosis prior to operation. It has made it increasingly evident that the proper care of patients is a matter of carefully planned teamwork, rather than the responsibility of a single individual. Perhaps more than any other single innovation, it has made surgeons aware of the importance of the many associated physiopathological problems that accompany the disorders they treat. It has certainly been one of the stimulating influences for the study of the total circulation and its component parts, the distribution of blood flow under varying circumstances, the perfusion of organs and tissues, the factors that are related to metabolic and respiratory acidosis, ventricular function, bleeding and clotting abnormalities, disturbances of water and electrolyte balance, and renal function. It has played an important role in instigating further efforts to understand the effects of body and cardiac hypothermia, arrhythmias of the heart, and new and better ways of bringing about cardiac resuscitation, of dealing with heart block and other arrhythmias, and of supporting the inadequately functioning ventricle. Almost certainly nothing has proven a stronger incentive for the establishment of first-rate specialized recovery rooms and intensive care units. It has provided a real stimulus for the development and improvement of helpful monitoring devices. It has underlined the necessity for surgeons to be well grounded in general medicine and in the basic fundamentals and has, furthermore, emphasized in a most convincing way the importance of accurate, gentle, precise, expeditiously performed operative work.

At the same time, the development and utilization of the heart-lung apparatus and the concomitant ever-broadening field of effective cardiac surgery have served as a challenge to adult and pediatric cardiologists and radiologists to make as specific and meaningful diagnoses as possible, based upon careful physiologic and anatomic assessments—and they have come up with assessments hardly dreamed possible only a decade or so earlier.

Over and beyond these influences, the cardiac operations dependent upon the utilization of extracorporeal circulation have played their part in improving doctor-patient relationships, very likely because human beings have a particularly great reverence for their hearts. They have helped physicians and surgeons to become closer to their patients, more aware of their responsibilities to them and their families, and more appreciative of the stresses, hopes, and disappointments that accompany disease and malformation. They have been of considerable influence in making obvious the necessity for the patient and his family to understand the nature of his difficulty, what can be expected of the proposed operation, the risks involved, the reasons why the determination to operate was made, and the alternatives if any exist. They have brought about more sympathetic and intelligent relationships between the profession and those who need professional help.

The laboratory and clinical success with the apparatus for cardiopulmonary bypass did not lessen Jack Gibbon's experimental, clinical, and teaching interests. At the same time, he busied himself with a thousand and one other matters. He was always concerned about political events and movements and never hesitated to express his views bearing upon them or to participate actively when he could support them. He served loyally with all kinds of local, national, and international professional organizations: the Board of Health of the City of Philadelphia, the Surgery Study Section of the United States Public Health Service, the Subcommittee of the Cardiovascular System of the National Research Council, the Committee of the International Exchange of Persons, the Subcommittee on the Cardiovascular System of the American Heart Association, and the Board of Scientific Counselors of the National Heart Institute. He served on the American Board of Surgery and as its vice-chairman. He was on the Conference Committee on Graduate Training in Surgery and chairman of that important group. For years he was active on the Editorial and Advisory Board of the *Annals of Surgery* and its chairman from 1947 to 1957. He belonged to numerous professional organizations and was recognized by election to many important posts.

In addition to various other officerships, he became president of the Laennec Society of Philadelphia, the Pennsylvania Association of Thoracic Surgery, the Philadelphia Academy of Surgery, the College of Physicians of Philadelphia, the Society for Vascular Surgery, and the American Association for Thoracic Surgery. Of especial interest is the fact that no other father and son have been elected to the presidency of the American Surgical Association-an honor widely viewed as the highest which can be awarded an American surgeon. He was on the Board of Governors of the American College of Surgeons for a period of fourteen years and on the Executive Committee of its Program Committee, as well as the Graduate Training Committee. He became a member of the American Academy of Arts and Sciences and, last but not least, of the National Academy of Sciences. He was made an honorary member of the Society of Thoracic Surgery of Great Britain and Ireland and an honorary fellow of the Royal College of Surgeons of England.

Honorary degrees came to him from the University of Buffalo, Princeton University, the University of Pennsylvania, Dickinson College, Jefferson Medical College, and Duke University. He was a visiting professor in a number of universities and delivered many named lectureships. It was natural and deserving that he should have received many honors. Among them were: the John Scott Award of the Board of Directors of City Trusts of Philadelphia; the

Charles Mickle Fellowship of the University of Toronto; the Clarence E. Shaffrey, S. J. Medal of the St. Joseph's College Medical Alumni; the Rudolph Matas Award in Vascular Surgery of Tulane University; the Distinguished Service Award of the Pennsylvania Medical Society; the Strittmatter Award of the Philadelphia County Medical Society; the Philadelphia Award; the Dixon Prize in Medicine of the University of Pennsylvania; and the Gairdner Foundation International Award of the University of Toronto. A special annual lectureship in his name was created by the American College of Surgeons.

He retired from the Chair of Surgery in 1967 and spent the remaining years of his life happily on his beautiful farm near Media where, surrounded by family and friends, he played tennis, swam, and worked in the garden. Reading remained a constant habit. Books, periodicals, and pamphlets were his constant companions—light, heavy, amusing, stimulating, thought-provoking—all sorts. His long-standing passion for portrait painting grew and matured; he devoted progressively more time to his studio with steadily increasing success. Indeed, his work became good enough to prompt a number of commissions. Meanwhile, he traveled extensively, continued to maintain an active part and interest in surgical and educational matters, and remained involved in community and governmental affairs.

It was most appropriate that in 1963, on the tenth anniversary of the first successful open cardiac procedure performed with the aid of the total cardiopulmonary bypass apparatus that he had developed, his portrait, painted by Gardiner Cox, was presented to Jefferson Medical College. It was a happy affair, beginning at home in the rose garden. The presentation itself was made in one of the school's amphitheaters in the presence of his patient, Cecilia Bavolek, then a healthy young lady completely cured of her congenital malformation of the heart, an atrial septal defect, which had made her so seriously ill prior to its repair ten years earlier. It is of interest that when his portrait was hung in McClellan Hall, there were only two portraits of fathers and sons who had been distinguished professors at Jefferson: Samuel D. Gross and his son, and Jack Gibbon and his father.

Almost precisely ten years later, a second portrait was presented during a memorial celebration held at the College of Physicians of Philadelphia, the oldest medical organization in the country, in his honor and in tribute to his life's work. He had died suddenly on February 5, 1973, as he would have wished, while playing a good game of tennis. This affair was held one day before the twentieth anniversary of the operation upon Cecilia Bavolek. This painting was by his father-in-law, Mr. Hopkinson, and the offering remarks were made by Maly, who had donated it. Following an afternoon scientific program, cocktails and dinner, and the bestowal of the portrait, remarks concerning what Jack Gibbon had meant to them were made by three of his close friends and colleagues. One was Clarence Dennis, an early worker in the field of mechanical heart-lung devices, another was Jack's long-time intimate companion, Professor Clarence Crafoord, head of the Thoracic Surgical Service at the Karolinska. It does not seem inappropriate that I close the life story of this extraordinary man by repeating in a slightly different manner the few remarks I was privileged to make on that occasion, since I believe that they represent what his worldwide friends felt about him and what untold thousands of others would have thought of him had they had the privilege of knowing him as well as we.

In essence this is what was said:

It is difficult to put in words what Jack Gibbon meant to us when he meant so very much. He was an inspiration to us all because of the meaningful, innovative contributions he made to the progress of medicine and we are thoroughly persuaded that the most significant of them, the development of the heart-lung machine, will remain forever one of the true milestones in medicine. Rarely, if ever, has one single research effort expanded so much the capacity of surgeons to be of help to the congenitally malformed and to those disabled by acquired lesions.

We think of him as a professor who used his great name and his important position, not for personal gain, but as an opportunity to develop a department that was an intimate, effective teaching unit, one that provided the best of patient care, maintained an overall flavor of original investigation, and attracted young men from here and abroad. He always kept in mind his conviction that research is not only important because of its potential for contributing new knowledge, but also because it creates the ideal atmosphere in which students and young surgeons may mature to their best advantage. The bright young surgeons in this country never had a better friend. He delighted in discovering them and in helping them move ahead.

We remember him as a soldier who made the long journey home from the Southwest Pacific on a stretcher, uncomplaining, and the stimulation he brought to the Mayo General Hospital as chief of the Surgical Service.

We preserve him in our memories as a sympathetic, understanding father and adoring husband who always wanted his wife to occupy the center spot, who was most proud of her early and long-continued collaboration in the heart-lung project, whose relationship with her reflected the admiration, companionship, and deep love he felt for her.

We cannot forget him as a genial host. We see him with attractive and devoted friends at the cocktail hour and dinner, on the tennis court, by the pool, at sheep-dipping time, in long, stimulating conversations, always warm and intimate. We think of him as a friend, the best friend one could ever wish for. We look upon him as a person with a boyish red face and twinkling, vibrant eyes that forever seemed to register a bit of amazement that he should be surrounded by so much that was good, that he had been able to accomplish so much, that so many splendid honors had come his way. We remember him as a man who, despite the seriousness of his work and play, seemed to bubble over with the excitement that characterizes the young at heart. We think of him as a liberal, feeling person who concerned himself deeply with causes and movements he felt were just.

Some men are crushed by the mantle of greatness. Some find it so heavy they must stand tall, erect, arrogant. Jack wore his with easy grace, with no undue pride, but rather with pleasant, somewhat surprised satisfaction. He will always be missed, and remembered, not with sorrow but with joy.

He is survived by his wife, Maly; one son, John; three daughters, Mary, Alice and Marjorie; his sister, Marjorie Battles; and his brother, Samuel.

The National Academy of Sciences did great honor to John H. Gibbon, Jr., by making him a member and, at the same time, did itself honor by having within its midst one of the truly great surgeons of all times.

I could not have written this volume had I not enjoyed for many years an exceedingly close, warm friendship with Jack Gibbon and his wife, Maly—one filled with memorable days of pleasant companionship and hours of unrestrained, stimulating conversation about a thousand and one things of mutual interest. Maly has turned over to me all pertinent material in her possession, such as Jack's last updated curriculum vitae. I am deeply indebted to his sister, Marjorie Battles, who has talked with me, written to me, located various mementos such as newspaper clippings, obituaries, their uncle Robert Gibbon's childhood memories of his mother, and who wrote for my use a most intriguing history of the family with special reference to grandparents, parents, and Jack as a child, an older boy, and an adult. Finally, I wish to thank my wife, Myrtie, affectionately, for her untiring and helpful advice during the preparation of the manuscript.

HONORS AND DISTINCTIONS

Degrees

1923	A.B., Princeton University
1927	M.D., Jefferson Medical College

Honorary Degrees

1959	Sc.D., University of Buffalo
1961	Sc.D., Princeton University
1965	Sc.D., University of Pennsylvania
1967	Sc.D., Dickinson College
1969	LL.D., Jefferson Medical College
1970	Sc.D., Duke University

Military Service

1942-1944	Major, M.C., A.U.S.
1945	Lieutenant Colonel, A.U.S.
1945	Mayo General Hospital, Chief of Surgical Service

Hospital and University Appointments

1927-1929	Intern, Pennsylvania Hospital
1930-1931	Research Fellow in Surgery, Harvard Medical School
1931-1932	Fellow in Medicine, School of Medicine, University of Pennsylvania
1931-1937	Assistant Surgeon, Pennsylvania Hospital
1933-1934	Research Fellow in Surgery, Harvard Medical School
1936-1942	Assistant Surgeon, Bryn Mawr Hospital
1936-1942	Harrison Fellow of Surgical Research, School of Medicine, University of Pennsylvania
1937-1950	Surgeon, Pennsylvania Hospital
1945-1946	Assistant Professor of Surgery, University of Pennsylvania
1946-1956	Professor of Surgery and Director of Surgical Research, Jefferson Medical College
1946-1956	Attending Surgeon, Jefferson Medical College Hospital
1950-1967	Consulting Surgeon, Pennsylvania Hospital
1950-1967	Consultant in General Surgery, Veterans Administration Hospital, Philadelphia

1956-1967	Samuel D. Gross Professor of Surgery and Head of the Department, Jefferson Medical College and Hospital
1956-1967	Attending Surgeon-in-Chief, Jefferson Medical College and Hospital
1967-1973	Samuel D. Gross Professor of Surgery, Emeritus, Jefferson Medical College and Hospital

Visiting Professorships and Lectureships

George A. Ball Visiting Professor of Surgery, Indiana University, 1956

Taub Visiting Professor of Surgery, Baylor University, 1959

Visiting Professor of Surgery, Harvard Medical School, 1960

Barney Brooks Visiting Professor of Surgery, Vanderbilt University, 1967

Churchill Lecture, Excelsior Surgical Society, 1956

Harvey Lecture, New York Academy of Medicine, 1958

Conner Memorial Lecture, American Heart Association, 1958

Alvarenga Prize and Lectureship, College of Physicians of Philadelphia, 1962

Arthur Dean Bevan Lecture, Chicago Surgical Society, 1962

Learned Society Memberships

American Association of Arts and Sciences

American Association for Artificial Internal Organs

American Association for Thoracic Surgery (President, 1960-1961; Council, 1949-1954; Representative to American Cancer Society, Advisory Committee on Lung Cancer Case Finding, 1954)

American Cancer Society

American Cancer Society, Philadelphia Division

American College of Surgeons (Fellow; Board of Governors, 1950-1964; Executive Committee of Program Committee; Graduate Training Committee)

American Heart Association

American Medical Association

American Medical Writers Association

American Surgical Association (Recorder, 1947-1952; Vice-President, 1953; President, 1954; Council, 1955-1958)

Association of American Medical Colleges

College of Physicians of Philadelphia (Council, 1954; Vice-President, 1961-1964; President, 1964-1967; Censor, 1967)

Halsted Society

Heart Association of Southeastern Pennsylvania (Research Committee Chairman, 1966; Board of Governors; President, 1958-1959)

International Cardiovascular Society

International Society of Surgery

International Surgical Group

J. Aitken Meigs Medical Society

Laennec Society of Philadelphia, President

Pennsylvania Association for Thoracic Surgery (President, 1963-1964)

Pennsylvania Heart Association (State Research Committee, 1966)

Pennsylvania Public Health Association

Pennsylvania State Medical Society, Commission on Promotion of Medical Research

Pennsylvania Trudeau Society

Philadelphia Academy of Surgery (Vice-President, 1953-1956; President, 1956-1958; Council, 1958-1960)

Philadelphia County Medical Society (Alternate Delegate-at-Large to Pennsylvania Medical Society, 1961-1963)

Pulmonary Neoplasm Research Group of Philadelphia, Medical Advisory Committee

Society of Clinical Surgery (Treasurer; President, 1953-1954)

Society of Surgical Chairmen

Society of University Professors

Society for Vascular Surgery (President, 1964-1965)

World Medical Association

National Academy of Sciences (elected, 1972)

Honorary Memberships

Chicago Surgical Society, 1962

Buffalo Surgical Society, 1966

Society of Thoracic Surgeons of Great Britain and Ireland, 1961

Fellow of the Royal College of Surgeons of England, 1959

Editorial Board Memberships

1947-1973	*Annals of Surgery* (Chairman, 1947-1957)
1959-1963	Circulation Research

Committee Memberships

United States Public Health Service, Surgery Study Section

City of Philadelphia, Board of Health

Committee on International Exchange of Persons

National Heart Institute, Board of Scientific Counselors

Conference Committee on Graduate Training in Surgery (Chairman, 1961-1963)

American Board of Surgery (Examination Committee, 1953-1956; Chairman, 1955-1956; Emeritus Member, 1956-1973)

National Board of Medical Examiners, Examination Committee for Surgery

National Research Council, Subcommittee on Cardiovascular System

American Cancer Society, Advisory Committee on Research of the Therapy of Cancer

American Heart Association, Subcommittee on the Cardiovascular System

Board of City Trusts of Philadelphia, Advisory Committee, John Scott Award

City of Philadelphia, Hospital Survey Committee

The Philadelphia Award, Board of Trustees

The Gairdner Foundation, Awards Committee

Phaler Foundation, Advisory Committee

Awards

1953	John Scott Award, Board of Directors of City Trusts of Philadelphia
1957	Charles Mickle Fellowship, University of Toronto
1957	Clarence E. Shaffrey, S. J. Medal, St. Joseph's College Medical Alumni
1958	Rudolph Matas Award in Vascular Surgery, Tulane University
1959	Distinguished Service Award, International Society of Surgery
1963	Strittmatter Award, Philadelphia County Medical Society
1964	The Philadelphia Award
1965	Research Achievement Award, American Heart Association
1966	Roswell Park Medal
1968	Albert Lasker Clinical Research Award
1972	Dixon Prize in Medicine, University of Pennsylvania

Bibliography

1930 With Richard Middleton. The prognostic value of the initial leukocyte and differential count in lobar pneumonia. Am. J. Med. Sci., 180:31-36.

With Edward D. Churchill. Changes in the pulmonary circulation induced by experimentally produced arteriovenous fistula. Arch. Surg., 21:1188-94.

1931 With Edward D. Churchill. Mechanical influence of the pericardium upon cardiac function. J. Clin. Invest., 10:405-22.

1932 With Mary Hopkinson and Edward D. Churchill. Changes in the circulation produced by gradual occlusion of the pulmonary artery. J. Clin. Invest., 11:543-53.

With Eugene M. Landis. Vasodilatation in the lower extremities in response to immersing the forearms in warm water. J. Clin. Invest., 11:1019-36.

1933 With Eugene M. Landis. The effects of temperature and tissue pressure on the movement of fluid through the human capillary wall. J. Clin. Invest., 12:105-38.

With Eugene M. Landis. Effects of alternate suction and pressure on blood flow to the lower extremities. J. Clin. Invest., 12:925-61.

With Eugene M. Landis. The effects of alternate suction and pressure on circulation in the lower extremities. Proc. Soc. Exp. Biol. Med., 30:593-95.

With Eugene M. Landis. A simple method of producing vasodilatation in the lower extremities, with reference to its usefulness in studies of peripheral vascular disease. Arch. Intern. Med., 52:785-808.

1934 Actinomycosis of abdominal wall. Ann. Surg., 99:861-64.

With John B. Flick. Pericardiectomy for advanced Pick's disease. Arch. Surg., 29:126-37.

1936 With John B. Flick. Total removal of left lung for carcinoma. Ann. Surg., 103:130-34.

With Edward D. Churchill. The physiology of massive pulmonary embolism; experimental study of the changes produced by obstruction to the flow of blood through the pulmonary artery and its lobar branches. Ann. Surg., 104:811-22.

With John B. Flick. The application of thoracoplasty to the treatment of pulmonary tuberculosis. Pa. Med. J., 39:768-72.

Artificial maintenance of the circulation during experimental ocelusion of the pulmonary artery. Arch. Surg., 34:1105-31.

1937 With John B. Flick. Hyperparathyroidism relieved by removal of a parathyroid tumor. Bull. Ayer Clin. Lab. Pa. Hosp., 3:73-78.

1939 Pulmonary embolism; a review of recent contributions. Pa. Med. J., 42:877-80.

With Francis C. Grant and Lawrence M. Weinberger. Anoxia of the central nervous system produced by temporary complete arrest of circulation. Trans. Am. Neurol. Assoc., 65:66-72.

An oxygenator with a large surface-volume ratio. J. Lab. Clin. Med., 24:1192-98.

The immediate effect of scalenotomy upon the size of apical tuberculous cavities. J. Thorac. Surg., 8:633-37.

The maintenance of life during experimental occlusion of the pulmonary artery followed by survival. Surg. Gynecol. Obstet., 69:602-14.

With Harold Smith. Blood chemical aids to surgical therapy. Surg. Clin. North Am., 19:1583-94.

1940 With Lawrence M. Weinberger and Mary H. Gibbon. Temporary arrest of the circulation to the central nervous system. I. Physiologic effects. Arch. Neurol. Psychiatry, 43:615-34.

With Lawrence M. Weinberger and Mary H. Gibbon. Temporary arrest of the circulation to the central nervous system. II. Pathologic effects. Arch. Neurol. Psychiatry, 43:961-86.

1941 With Charles W. Kraul. An efficient oxygenator for blood. J. Lab. Clin. Med., 26:1803-9.

With Clare C. Hodge. Aseptic immediate anastomosis following resection of the colon for carcinoma. Ann. Surg., 114:635-52.

With Clare C. Hodge. The treatment of minor war injuries. Med. Clin. North Am., 25:1829-42.

With Frank F. Allbritten, Jr., and John B. Flick. The use of sulfanilamide in partial and total resection of the lung. J. Thorac. Surg., 11:187-97.

1942 With Mary H. Gibbon and Charles W. Kraul. Experimental pulmonary edema following lobectomy and blood transfusion. J. Thorac. Surg., 12:60-77.

With Mary H. Gibbon. Experimental pulmonary edema following lobectomy and plasma infusion. Surgery, 12:694-704.

1946 With Leslie W. Freeman. The primary closure of decubitus ulcers. Ann. Surg., 124:1148-64.

With Louis H. Clerf, Peter A. Herbut, and John J. DeTuerk. The diagnosis and operability of bronchogenic carcinoma. J. Thorac. Surg., 17:419-27.

1949 With John Y. Templeton III. Experimental reconstruction of cardiac valves by venous and pericardial grafts. Ann. Surg., 129:161-76.

Cancer of the lung (Thomas Dent Mutter lecture). LXI. Trans. Stud. Coil. Physicians Philadelphia, 17:49-60.

With Bernard J. Miller and Frank F. Allbritten, Jr. Blood volume and extracellular fluid changes during thoracic operations. J. Thorac. Surg., 18:605-15.

With Joseph W. Stayman, Jr. Symposium on recent advances in surgical physiology; physiology of cardiac surgery. Surg. Clin. North Am., 29:1731-43.

1950 With Frank F. Allbritten, Jr., Herbert Lipshutz, and Bernard J. Miller. Blood volume changes in tuberculous patients treated by thoracoplasty. J. Thorac. Surg., 19:71-79.

With Franz Goldstein, Frank F. Allbritten, Jr., and Joseph W. Stayman, Jr. The combined manometric determination of oxygen and carbon dioxide in blood, in the presence of low concentrations of ethyl ether. J. Biol. Chem., 182:815-20.

With John E. Healey, Jr. Intrapericardial anatomy in relation to pneumonectomy for pulmonary carcinoma. J. Thorac. Surg., 19:864-74.

John S. Lockwood. Ann. Surg., 132:161.

Controlled respiration in thoracic and upper abdominal operations. Minn. Med., 33:1031-34.

With Frank F. Allbritten, Jr., Joseph W. Stayman, Jr., and James M. Judd. A clinical study of respiratory exchange during prolonged operations with an open thorax. Ann. Surg., 132:611-25.

With Frank F. Allbritten, Jr. Recent advances in the surgical treatment of carcinoma of the esophagus; discussion of esophagogastrostomy. Pa. Med. J., 53:811-16.

With Thomas L. Stokes, Jr. Bronchiectasis. In: *Cyclopedia of Medicine, Surgery and Specialties,* ed. George M. Piersol and Edward L. Bortz, 3d. ed., vol. 2, p. 751. Philadelphia: F. A. Davis.

With Joseph W. Stayman, Jr., and Frank F. Allbritten, Jr. Controlled respiration in thoracic surgery. J. Int. Chir., 10:106-11.

Carcinoma of the lung with an analysis of symptoms and end results in 243 cases. Chicago Med. Soc. Bull., 13:945-48.

1951 With Frank F. Allbritten, Jr., and John Y. Templeton III. Carcinoma of the esophagus and gastric cardia. J. Am. Med. Assoc., 145:1035-40.

Cancer of the esophagus. Med. Rec. Ann., 45:600-602.

With Robert K. Finley, Jr., John Y. Templeton III, and Robert H. Holland. Changes in urine and serum electrolytes and plasma volumes after major intrathoracic operations. J. Thorac. Surg., 22:219-34.

With Bernard J. Miller and Mary H. Gibbon. Recent advances in the development of a mechanical heart and lung apparatus. Ann. Surg., 134:694-708.

With John Y. Templeton III and Frank F. Allbritten, Jr. Symposium on abdominal surgery; total gastrectomy. Surg. Clin. North Am., 31:1713-20.

An extracorporeal circulation for the temporary maintenance of the cardiorespiratory functions. *Fourteenth Congress de la Societé Internationale de Chirurgie,* pp. 984-91. Brussels: Imprimarie Médicale et Scientifique.

With John Y. Templeton III and Robert K. Finley, Jr. Body fluids and electrolytes, blood volume and shock-observations on thiocyanate space, serum electrolytes and acid base equilibrium in patients with intrathoracic disease. Surg. Forum, 2:589-95.

1952 The present status of mechanical heart and lungs. Med. Rec. Ann., 46:872-76.

With Frank F. Allbritten, Jr., Thomas F. Nealon, Jr., and John Y. Templeton III. Symposium on safeguards in surgical diagnosis; the diagnosis of lung cancer. Surg. Clin. North Am., 32:165-772.

The pathogenesis and treatment of pulmonary edema in relation to surgery (annual address for 1946). Trans. Philadelphia Acad. Surg., 37:46-67.

1953 With Bernard J. Miller and Charles Fineberg. Symposium on clinical medicine; an improved mechanical heart and lung apparatus; its use during open cardiotomy in experimental animals. Med. Clin. North Am., 37:1603-24.

With Frank F. Allbritten, Jr., John Y. Templeton III, and Thomas F. Nealon, Jr. Cancer of the lung-an analysis of 532 consecutive cases. Ann. Surg., 138:489-501.

With Bernard J. Miller, Victor F. Greco, Harold C. Cohn, and Frank F. Allbritten, Jr. The use of a vent for the left ventricle as a means of avoiding air embolism to the systemic circulation during open cardiotomy with the maintenance of the cardiores piratory function of animals by a pump oxygenator. Surg. Forum, 4:29-33.

With Bernard J. Miller, Victor F. Greco, Burgess A. Smith, Harold C. Cohn , and Frank F. Allbritten, Jr. The production and repair of interatrial septal defects under direct vision with the assistance of an extracorporeal pump oxygenator circuit. J. Thorac. Surg., 26:598-616.

In memoriam: Samuel Clark Harvey. Ann. Surg., 138:679.

1954 Everett Idris Evans. Ann. Surg., 139:257.

With Thomas F. Nealon, Jr. Cancer of the lung. In: *Seminar,* pp. 20-28. Philadelphia: Merck, Sharp, and Dohme.

The application of a mechanical heart and lung apparatus to cardiac surgery. Minn. Med., 37:171-80.

With Frank F. Allbritten, Jr., John Y. Templeton III, Robert K. Finley, Jr., Jose H. Amadeo, and Daniel W. Lewis. The results of mitral valvotomy for mitral stenosis. Ann. Surg., 139:786-96.

With Bernard J. Miller, Arthur R. C. Dobell, Hans C. Engel, and George B. Voigt. The closure of interventricular septal defects in dogs during open cardiotomy with maintenance of the cardiorespiratory functions by a pump-oxygenator. J. Thorac. Surg., 28:235-40.

With Louis H. Clerf, Peter A. Herbut, and Thomas F. Nealon. The lung. In: *Pennsylvania Cancer Manual. An Up-to-date Guide For the Practicing Physician,* ed. Robert C. Horn, Jr., David W. Clare, W. Kenneth Clark, and Benjamin Schneider, chapter 43. Pittsburgh: The Pittsburgh Press.

1955 With Thomas F. Nealon, Jr., George J. Haupt, and Joyce E. Price. Pulmonary ventilation during open thoracotomy: inflation and deflation time ratios and pressures. J. Thorac. Surg., 30:655-75.

With Thomas L. Stokes, Jr., and John J. McKeown, Jr. Surgical treatment of carcinoma of the lung. Am. J. Surg., 89:484-93.

With George J. Haupt. Symposium on applied physiology in modern surgery; the need for adequate pulmonary ventilation during surgical operations. Surg. Clin. North Am., 35:1553-71.

The education of a surgeon (Presidential address). Ann. Surg., 142:321-28.

1956 With Thomas F. Nealon, Jr., and Victor F. Greco. A modification of Glassman's gastrostomy with results in 18 patients. Ann. Surg., 43:838-44.

With Hans C. Engel. Congenital malformations of the heart and great vessels. In: *Cole's Operative Technique. Specialty Surgery,* p. 119. New York: Appleton-Century-Crofts.

With Thomas F. Nealon, Jr., and Joyce E. Price. Respiratory acidosis of pulmonary ventilation during open thoracotomy—the effect of compression of the lung. Surg. Forum, 7:193-96.

With Thomas F. Nealon, Jr., George J. Haupt, Harold Chase, and Joyce E. Price. Insufficient carbon dioxide absorption requiring increased pulmonary ventilation during operations with open thoracotomy. J. Thorac. Surg., 32:464-80.

1957 With Thomas F. Nealon, Jr. Carcinoma of the lung and tumors of the thorax. In: *Surgery, Principles and Practice,* ed. J. G. Allen, H. N. Harkins, C. A. Moyer, and J. E. Rhoads, p. 1205. Philadelphia: J. B. Lippincott.

With Thomas F. Nealon, Jr., and Harold F. Chase. Factors influencing the adequacy of carbon dioxide absorption in clinical anesthesia. Anesthesiology, 19:75-81.

With John Y. Templeton III and Thomas F. Nealon, Jr. Factors influencing the survival of patients with cancer of the lung. Ann. Surg., 145:637-41.

With Thomas F. Nealon, Jr. Symposium on common operations—refinements in technique. Technique of pneumonectomy. Surg. Clin. North Am., 37:1551-63.

1958 With John Y. Templeton III. Current status of pump oxygenators in cardiac surgery and persistent problems in their use. Prog. Cardiovasc. Dis., 1:56-65.

With John Y. Templeton III. Evaluation of current cardiac surgical procedures. J. Newark Beth Isr. Hosp., 9:87-96.

With Thomas F. Nealon, Jr., and Joyce E. Price. The importance of measuring ventilation during the steady state. Surg. Forum, 8:458-65.

With Richard T. Cathcart, Thomas F. Nealon, Jr., William Fraimow, and Louis J. Hampton. Cardiac output under general anesthesia. The effect of mean endotracheal pressure. Ann. Surg., 148:488-97.

With Thomas F. Nealon, Jr. The effect of position on pulmonary ventilation. J. Thorac. Surg., 36:459-554.

With David C. Schechter and Thomas F. Nealon, Jr. Recipient set for removal of potassium and ammonium from bank blood. Arch. Surg., 77:944-46.

Extracorporeal maintenance of cardiorespiratory functions. *Harvey Lectures,* vol. 53, pp. 186-224. New York: Academic Press.

With David C. Schechter and Thomas F. Nealon, Jr. A simple extracorporeal device for reducing elevated blood ammonia levels; preliminary report. Surgery, 44:892-97.

With David C. Schechter and Thomas F. Nealon, Jr. An ion exchange resin type artificial kidney. Surg. Forum, 9:110-114.

1959 With David C. Schechter and Thomas F. Nealon, Jr. The removal of excessive potassium and ammonium from bank blood prior to transfusion. Surg. Gynecol. Obstet., 108:1-6.

With Thomas F. Nealon, Jr., Richard T. Cathcart, William Fraimow, and Edward D. McLaughlin. The effect of mean endotracheal pressure on the cardiac output of patients undergoing intrathoracic operations. J. Thorac. Surg., 38:449-57.

With Thomas F. Nealon, Jr. Carcinoma of the lung. In: *Current Therapy,* p. 73. Howard, Conn.: W. B. Saunders.

Maintenance of cardiorespiratory functions by extracorporeal circulation (The Lewis A. Conner Memorial Lecture). Circulation, 19:646-56.

With Thomas F. Nealon, Jr., and Joyce E. Price. The effect of compression of the lung on pulmonary ventilation . Surg. Forum, 7:193-96.

1960 With Thomas F. Nealon, Jr. Preoperative and postoperative management in the treatment of neoplasms of the chest. In: *Treat* *ment of Cancer and Allied Diseases,* ed. G. T. Pack and I. M. Ariel, vol. 4, p. 279. New York: Paul B. Hoeber.

With Charles Fineberg and Nicholas P. Foris. Experimental sham coronary endarterectomy with and without coronary artery perfusion. Surgery, 47:160-64.

With Edward D. McLaughlin and Thomas F. Nealon, Jr. Treatment of bank blood by resins. J. Thorac. Surg., 40:602-10.

With Walter F. Ballinger II, John Y. Templeton III, and Thomas F. Nealon, Jr. The complications of esophageal hiatal hernia. Pa. Med. J., 63:51-56.

With George J. Haupt, Rudolph C. Camishion, and John Y. Templeton III. Treatment of malignant pleural effusions by talc poudrage. J. Am. Med. Assoc., 172:918-21.

With Thomas F. Nealon, Jr. Recent advances in surgical research and their clinical applications; the physiological effects of pulmonary ventilation during operations under general anesthesia . Surg. Clin. North Am., 40:1491-1502.

1961 With Thomas F. Nealon, Jr., John Y. Templeton III, and Vincent D. Cuddy. Instrumental perforation of the esophagus. J. Thorac. Surg., 41:75-79.

With Rudolph C. Camishion and John Y. Templeton III. Leiomyoma of the esophagus: review of the literature and report of two cases. Ann. Surg., 153:951-56.

Broncho-esophagology and thoracic surgery. The team effort. Trans. Am. Bronchoesoph. Assoc., 41:19-24.

With Rudolph C. Camishion, Yoshinori Ota, and Vincent D. Cuddy. Pulmonary arterial blood flow through an acutely atelectatic lung. J. Thorac. Surg., 42:599-614.

Anesthesia and pulmonary problems. Surg. Gynecol. Obstet., 112: 223-27.

The road ahead for thoracic surgery (Presidential address). J. Thorac. Surg., 42:141-49.

With Rudolph C. Camishion and Yoshinori Ota. Blood flow through the superior mesenteric artery with antegrade and retrograde perfusion of the aorta. Circulation, 24:900.

1962 With Rudolph C. Camishion and Thomas F. Nealon, Jr. Methods of improving morbidity and mortality rates in standard operations; talc poudrage in the treatment of pleural effusion due to cancer. Surg. Clin. North Am., 42:1521-26.

With Jerome L. Sandler and Thomas F. Nealon, Jr. Clinical experience with a cation exchange resin in the treatment of stored blood. J. Cardiovasc. Surg., 3:94-98.

With Yoshinori Ota and Rudolph C. Camishion. *Dirofilaria imitis* (heart worms) and Depetalonema species as causes of "transfusion reactions" in dogs. Surgery, 51:518-26.

Surgery of the Chest (editor). Philadelphia: W. B. Saunders.

1963 With Rudolph C. Camishion, Leon P. Scicchitano, and Robert Trotta. Blood flow through the superior mesenteric artery during retrograde aortic perfusion. Surgery, 54:651-55.

1965 With Thomas F. Nealon, Jr. The lung, the trachea and the pleura. In: *Management of the Patient with Cancer,* ed. Thomas F. Nealon, Jr., p. 470. Philadelphia: W. B. Saunders.

1966 With Rudolph C. Camishion, Louis Pierucci, and Junzo Iida. Paralysis of the left recurrent laryngeal nerve secondary to mitral valvular disease: report of two cases and review of the literature. Ann. Surg., 163:818-28.

The artificial intracorporeal heart (Presidential address). Surgery, 59:1-5.

1967 The early development of an extracorporeal circulation with an artificial heart and lung. Trans. Am. Soc. Artif. Intern. Organs, 13:77-79.

1969 With David C. Sabiston and Frank C. Spencer, eds. *Surgery of the Chest,* 2d. ed. Philadelphia: W. B. Saunders.

1970 With Thomas F. Nealon, Jr., and Joseph McCloskey. Cáncer de pulm6n: reflexiones y revisión del significado de las metástasis linfaticas. Prensa Med. Argent., 57:1244-47.

The development of the heart-lung apparatus. Rev. Surg., 27:231-44.

1971 Memoir of J. Parsons Schaeffer, 1878-1970. Trans. Stud. Coll. Physicians Philadelphia, 38:249-51.

Chester R. Longwell

Chester Ray Longwell

October 15, 1887-December 15, 1975

by John Rodgers

Chester Longwell, the son of John Kilgore and Julia (Megown) Longwell, was born in 1887 near the settlement of Spalding in northeastern Missouri, in the Mark Twain country, an association that he loved to recall and that colored his speech, his many anecdotes, and perhaps even this way of thinking about life. He did not go to college immediately after high school but spent seven years working, partly at various jobs in the Far West, partly on the farm or teaching school at home in Missouri. He then went to the University of Missouri (Columbia), completing a bachelor's degree (with honors) in 1915 and a master's degree the following year. In 1940 the University honored its by then successful alumnus with an LL.D. degree.

From Missouri he went to Yale as a graduate student in geology, but the First World War interrupted his studies, and he spent two years in the U.S. Army, part of it overseas, emerging as a captain. At the time of his death, more than fifty years later, one of his fellow regimental officers wrote: "His composure under unusual circumstances made all officers of the regiment admire and respect him."* After returning from the war, he completed his graduate work at Yale and was awarded a Ph.D. degree in 1920.

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* Henry Broyce to Mrs. Irene Longwell, 1976.

For his doctoral dissertation Longwell worked for five months (summer and fall of 1919) in the Muddy Mountains and vicinity, then a virtually unknown corner of southern Nevada; indeed in his report on the region he mentions the "strong appeal to the geologist" of an area "practically unmapped" (even topographically) and with "the lure of the unknown." The area was primitive, travel was by mule or horse, water was very scarce, and isolation was the rule. He made his camp where he could, often with local hermits or prospectors or with the Indians of the region; it is characteristic of the man that decades later they would remember him with affection.

During those five months he made major discoveries that opened a new chapter in geological exploration of the Great Basin. By subdividing the Cenozoic deposits of the area, he showed that strong tilting and other deformation, some of it contemporaneous with deposition, produced angular unconformities within the Cenozoic sequence, a new result at the time. Furthermore he showed that the Paleozoic and lower Mesozoic stratigraphic sequence in the southern Great Basin, while it can be matched to some degree with the well-known sequence on the adjacent Colorado Plateau, is much thicker and more complete—in other words, that it is geosynclinal. Perhaps his most spectacular result was the demonstration of large low-angle thrust faults involving this thick Paleozoic-Mesozoic sequence; such faults were then known no nearer than southeastern Idaho and adjacent Wyoming and Utah. Subsequent work, much of which was inspired by Longwell, has made clear that the belt of such thrusting is continuous from southeastern California, through the Muddy Mountains region to Idaho, and indeed far beyond into Canada, always associated, as in Longwell's area, with the zone of westward thickening of the stratigraphic sequence into the geosyncline.

In 1920 Longwell was appointed to the faculty of Yale University and also became a member ("when actually employed") of the U.S. Geological Survey; he retained both connections throughout his career. At Yale he advanced steadily to a professorship in 1929 and was chairman of the Department of Geology for eight years, including the difficult period of the Second World War when the university was in continuous session and leaves were not taken-thus he taught and administered "around the clock" for several years. Except during those years, however, he continued a very active program of field work, mainly, though not entirely, in southern Nevada and vicinity, and he made many additional major contributions to the geology of the region up to and long after his official "retirement" in 1956. He mapped the floor of the Boulder Dam reservoir (Lake Mead) and the Davis Dam reservoir (Lake Mojave) before they were flooded and restudied the Muddy Mountains and other ranges nearby, in particular the high Spring Mountains west of Las Vegas, where the thrust belt he discovered is well displayed.

When he approached retirement, he chose to move to California in order to be closer to his field area and to be able to work there at all times of year, and he continued active field work well into his eighties. Many former students and other younger geologists have testified to their inability to keep up with him during those years. In 1974 they organized a symposium in his honor at a meeting in Las Vegas. Characteristically, Longwell gave an outstanding research paper at that symposium, consolidating the evidence for a major strike-slip fault zone in the Las Vegas Valley, an idea he developed largely in his retirement years.

During his years in New Haven, Longwell did not neglect Connecticut geology. Picking up where his Yale predecessor, Joseph Barrell, had been interrupted by his early death, he demonstrated the contemporaneity and close genetic associa tion of faulting and deposition in the Newark (Triassic) basin of Massachusetts and Connecticut and, by implication, in the other Newark basins, an idea close to one he had demonstrated in his doctoral dissertation for the Cenozoic deposits in southern Nevada. Furthermore, he was one of the first to see the value to structural geologists of geophysical data, especially gravity measurements, and he pursued this subject through a series of related papers. Such work led on to the subject of isostasy, and an article of his was in good part responsible for the rehabilitation of Airy's "roots-of-mountains" concept in crustal structure.

Teaching structural geology naturally led into orogenic theory, and Longwell kept abreast of new syntheses and hypotheses in this field, especially in Europe, although he never attempted a synthesis of his own. He took an active part in the long debate over continental drift and was indeed invited by S. Warren Carey to represent northern-hemisphere skepticism at the Tasmania symposium (1956) that preceded (and helped to trigger) the turning of the tide.

Longwell took an active part in the organizational side of geology. Even before his election to the National Academy of Sciences (1935), he was active in the National Research Council, and he served as chairman of its Division of Geology and Geography for three years. One of his activities as chairman of the Tectonics Committee of this division was to urge and organize the work on the first large tectonic map of the United States, the execution of which was entrusted to Philip B. King, whose small-scale tectonic map of 1932 had stimulated the project. Longwell was also active in the Geological Society of America and was elected its president for 1949. In 1948 he agreed to take over the editorship of the *American Journal of Science—"Silliman's* journal," the oldest scientific periodical in America, but now devoted to the geological sciences. He enlisted the present writer as an assistant, leaving him in charge on departing for California. His insistence on improving standards for publication was influential in maintaining the journal's position as a leading geological journal.

Naturally Longwell taught and worked with a great many graduate students; it is quite remarkable how many of those students themselves came to eminent positions in geology. Already, eight of them have followed Longwell into the National Academy of Sciences; five have been, like him, president of the Geological Society of America; and five have received the highest honor in North American geology, the Penrose Medal. He was also active in teaching elementary geology at Yale, and after Pirsson's death he inherited the "Yale" textbook of *Physical Geology,* which, as Longwell, Knopf, and Flint, dominated the textbook field for a decade or so. Into another textbook, compiled of quotations from original sources by Agar, Flint, and Longwell, he inserted several passages from his favorite Mark Twain. He also edited a popular guide to the geology around Connecticut.

Professor Longwell was married in 1921 to Doris Smith but was divorced in 1931. He was married again in 1935 to Irene Moffat. When he and his family moved to California in 1955, he established himself, appropriately enough, on Mark Twain Street in Palo Alto. In California he was welcomed into the active Geological Survey group at Menlo Park and the faculty at the School of Earth Sciences of Stanford University, which he served as research associate and consulting professor. He retained his activity and his ebullient spirits (and bad puns) to the very end, and many of his California associates celebrated his eighty-eighth birthday with him on 15 October 1975. He died two months later. He is survived by his wife, three children, five grandchildren, and by innumerable former students and friends.

I am deeply appreciative for the help I have received in preparing this memoir from Mrs. Irene Longwell. I have also made use of material gathered by Ward C. Smith, Arthur D. Howard, and Professor Longwell's brother, Dean John Harwood Longwell; I am grateful to them for permitting me to use it.

Bibliography

1921 Geology of the Muddy Mountains, Nevada, with a section to the Grand Wash Cliffs in western Arizona. Am. J. Sci., 5th ser. 1:39-62.

With Everett O. Waters. A practical method for determining dip and strike. Econ. Geol., 16:405-9.

1922 The Muddy Mountain overthrust in southeastern Nevada. J. Geol., 30:63-72.

Notes on the structure of the Triassic rocks in southern Connecticut. Am. J. Sci., 5th ser. 4:223-36.

With H. D. Miser, R. C. Moore, and Sidney Paige. Geology of the Colorado River in southeastern Utah. Geol. Soc. Am. Bull., 33:122-23(A).

1923 With Hugh D. Miser, Raymond C. Moore, Kirk Bryan, and Sidney Paige. Rock formations in the Colorado Plateau of southeastern Utah and northern Arizona. US Geol. Surv. Prof. Pap., 132-A. 23 pp.

Kober's theory of orogeny. Geol. Soc. Am. Bull., 34:231-41.

1924 Thrust faults and flaws in southern Nevada. Geol. Soc. Am. Bull., 35:64-65(A).

1925 Geological significance of isostasy and gravity measurements: a review. Geogr. Rev., 15:123-29.

The pre-Triassic unconformity in southern Nevada. Am. J. Sci., 5th ser. 10:93-106.

Complex structure in the Spring Mountains, Nevada. Geol. Soc. Am. Bull., 36:150(A).

1926 Structural studies in southern Nevada and western Arizona. Geol. Soc. Am. Bull., 37:551-83.

1927 Geophysical problems of general interest: a survey of recent literature. Geogr. Rev., 17:467-75.

1928 Three common types of desert mud-cracks. Am. J. Sci., 5th ser. 15:136-45.

Geology of the Muddy Mountains, Nevada, with a section through the Virgin Range to the Grand Wash Cliffs, Arizona. US Geol. Surv. Bull., 798. 152 pp.

Some physical tests of the displacement hypothesis. In: *Theory of Continental Drift,* pp. 145-57. Tulsa, Okla.: American Association of Petroleum Geologists.

Lessons from the St. Francis Dam. Science, 68:36-37.

The Triassic of Connecticut. Am. J. Sci., 5th ser. 16:259-63.

Herschel's view of isostatic adjustment. Am. J. Sci., 5th ser. 16:451-53.

1929 William North Rice, 1845-1928. Am. J. Sci., 5th ser. 17:100.

Obituary: Thomas Chrowder Chamberlin. Geogr. Rev., 19:164-65.

With William M. Agar and Richard F. Flint. *Geology from Original Sources.* New York: Henry Holt. 527 pp.

With Louis V. Pirsson. *A Textbook of Geology,* part I, *Physical Geology,* 3d rev. ed. New York: John Wiley & Sons. 488 pp.

Origin and history of the "continental nuclei." Geol. Soc. Am. Bull., 40:104(A).

1930 *Outlines of Physical Geology.* New York: John Wiley & Sons. 376 pp.

Some problems of mountain structure and mountain history. Am. J. Sci., 5th ser. 19:419-34.

Faulted fans west of the Sheep Range, southern Nevada. Am. J. Sci., 5th ser. 20:1-13.

The "oscillation theory" of diastrophism. Am. J. Sci., 5th ser. 20:217-20.

1931 Bemerkungen zur Oszillationstheorie über Diastrophismus. Dtsch. Geol. Ges. Z., 83:332-36.

Meteor Crater is not a limestone sink. Science, 73:234-35.

1932 With Adolph Knopf and Richard F. Flint. *A Textbook of Geology,* part *I, Physical Geology.* New York: John Wiley & Sons. 514 pp. (2d ed., 543 pp., 1939; 3d ed., 602 pp., 1948.)

With Edward S. Dana. *Walks and Rides in Central Connecticut and Massachusetts.* New Haven, Conn.: Tuttle, Morehouse, and Taylor. 229 pp.

With Charles Schuchert. Paleozoic deformations of the Hudson Valley region, New York. Am. J. Sci., 5th ser. 23:305-26.

The Muddy Mountain thrust in fact and in fiction. Science, 76:99-100.

1933 With Rudolf Ruedemann, Robert Balk, Arthur Keith, and George W. Bain. *Eastern New York and Western New England,* Guidebook 1, 16th Internat. Geol. Congress, Washington, D.C.: US Geological Survey. 118 pp.

Rotated faults in the Desert Range, Nevada. Geol. Soc. Am. Bull., 44:93(A).

Thrust faults of peculiar type. Geol. Soc. Am. Bull., 44:93(A).

Memorial to John Walter Gregory, 1864-1922. Geol. Soc. Am. Bull., 44:414-15.

Meaning of the term "roches moutonnées." Am. J. Sci., 5th ser. 25:503-4.

1934 With Adolph Knopf and Richard F. Flint. *Outlines of Physical Geol*ogy. New York: John Wiley & Sons. 356 pp. (2d ed., 381 pp., 1941.)

Proposed tectonic map of the United States. Science, 80:427-28.

1935 Is the "roots-of-mountains" concept dead? Am. J. Sci., 5th ser. 29:81-92.

1936 With Carl O. Dunbar. Problems of Pennsylvanian-Permian boun dary in southern Nevada. Am. Assoc. Pet. Geol. Bull., 20: 1198-207.

Geology of the Boulder Reservoir floor, Arizona-Nevada. Geol. Soc. Am. Bull., 47:1393-476.

1937 Sedimentation in relation to faulting. Geol. Soc. Am. Bull., 48:433-41.

Carbonation and carbonitization. Science, 85:333-34.

Geological interpretation of gravity anomalies. Geol. Soc. Am. Proc., 1936:86-87(A).

With John F. Mason and John C. Hazzard. Sequence of Cambrian faunas in the southern Great Basin. Geol. Soc. Am. Proc., 1936: 366-67(A).

1938 The Basin Range problem. Trans. N.Y. Acad. Sci., 2d ser. 1:17-20.

Geologic interpretation of gravity-anomalies in the northeastern United States. Trans. Am. Geophys. Union, 19th Ann. Mtg., part 1, p. 84.

1939 Origin of mountains, or how mountains rise. In: *Frontiers of Geology,* pp. 19-22. New York: Geological Society of America.

Thrust faults of southern Nevada photographed in color. Geol. Soc. Am. Bull., 50:1919(A).

With Charles H. Behre, Jr. New tectonic map of the United States: progress report. Geol. Soc. Am. Bull., 50:2001(A).

1940 Relative roles of geology and geophysics in determination of crustal structure. Trans. Am. Geophys. Union, 21st Ann. Mtg., pp. 783-86. (Also in: Geol. Soc. Am. Bull., 49:1952-53 (A), 1938.)

William Bowie, 1872-1940. Am. J. Sci., 238:889-90.

Problems of orogeny. Geol. Soc. Am. Bull., 51:2001(A).

1941 Geology. In: *Development of the Sciences,* pp. 147-96. New Haven, Conn.: Yale Univ. Press.

Muddy Mountain, Nevada, belt of thrusting restudied. Geol. Soc. Am. Bull. , 52:1920-21(A).
1942 The dating of diastrophic events in southern Nevada. Trans. N.Y. Acad. Sci., 2d ser. 5:21-22.
1943 Geologic interpretation of gravity anomalies in the southern New England-Hudson Valley region. Geol. Soc. Am. Bull., 54: 555-90.
Classification of faults. Am. Assoc. Pet. Geol. Bull., 27:1633-40.
1944 Some thoughts on the evidence for continental drift. Am. J. Sci., 242:218-51.
Further discussion of continental drift. Am. J. Sci., 242:514-15.
The mobility of Greenland. Am. J. Sci., 242:624.
Determinations of geographic coordinates in Greenland. Science, 100:403-4.
Tectonic map of the United States. Am. Assoc. Pet. Geol. Bull., 28:1767-74. (Also in: Geol. Soc. Am. Bull., 50:1918-19(A), 1939.)
1945 The mechanics of orogeny. Am. J. Sci., 243-A:417-47.
Education in geology, how advance it? Science, 101:1-7.
Low-angle normal faults in the Basin-and-Range province. Trans.
Am. Geophys. Union, 26:107-18.
1946 How old is the Colorado? Am. J. Sci., 244:817-35.
Bleaching of red sandstones in belts of deformation. Geol. Soc. Am. Bull., 57:1214(A).
1947 Distribution of oceans and continents. Am. Assoc. Pet. Geol. Bull., 31:1300-1303.

1949 Geologia submarina. Un amplio campo de investigación: Cienc. Invest. 5:54-58.
Structure of the northern Muddy Mountain area, Nevada. Geol. Soc. Am. Bull., 60:923-67.
Structure in the Arizona-Nevada boundary area. Geol. Soc. Am. Bull., 60:1905-6(A).
1950 Tectonic theory viewed from the Basin Ranges. Geol. Soc. Am. Bull., 51:413-33.
Charles Hyde Warren (1876-1950). Am. Philos. Soc. Year Book, 1950:328-33.
1951 Memorial to Edward Bayer Branson, 1877-1950. Geol. Soc. Am. Proc., 1950:85-89. (Also in: Am. Assoc. Pet. Geol. Bull., 35: 1706-10.)
The 1950 silly season. Science, 113:418.
The problem of crustal deformation. Science, 114:2967(A).
Megabreccia developed downslope from large faults. Am. J. Sci., 249:343-55.
Thrust-faulting—what does it mean? Trans. N.Y. Acad. Sci., 2d ser. 14:2-5.
The so-called "temple of Jupiter Serapis." Am. J. Sci., 249:929-31.
1952 Basin and range geology west of the St. George Basin, Utah. Guideb. Geol. Utah, 7:27-42.
Structure of the Muddy Mountains, Nevada. Guideb. Geol. Utah, 7:109-14.
The psychology of continental drift. Adv. Sci. 8:426-28.
Is gravitational sliding important in orogeny? Trans. Am. Geophys. Union, 33:336(A).
Lower limit of the Cambrian in the Cordilleran region. J. Wash. Acad. Sci., 42:209-12.
1953 Was South America formerly joined to Africa? Geogr. Rev., 43: 279-81.

1954 Memorial to Herbert Ernest Gregory (1869-1952). Geol. Soc. Am. Proc., 1953:115-23.

Review of the tectogene concept. Geol. Soc. Am. Bull., 65:1279(A).

Origin of the word *Climate.* Science, 120:355.

History of the lower Colorado River and the Imperial depression. Calif. Div. Mines Bull., 170 (5):53-56.

The AGT and the AGI. J. Geol. Ed. 2:43-48.

1955 With Richard F. Flint. *Introduction to Physical Geology.* New York: John Wiley & Sons. 432 pp. (2d ed., 504 pp., 1962.)

In support of the American Geological Institute. Geophysics, 20: 683-87.

1956 Arthur Keith, 1864-1944. In: *Biographical Memoirs,* 29:190-200. New York: Columbia Univ. Press for the National Academy of Sciences.

1957 The outlook for manpower in geoscience. Geotimes, 1(9):6-7.

1958 Clarence Edward Dutton, 1841-1912. In: *Biographical Memoirs,* 32:132-45. New York: Columbia Univ. Press for the National Academy of Sciences.

My estimate of the continental drift concept. In: *Continental Drift—A Symposium,* pp. 1-12. Univ. of Tasmania Geology Dept., Symposium 2.

1960 Possible explanation of diverse structural patterns in southern Nevada. Am. J. Sci., 258-A: 192-203.

Geologic setting of Lake Mead. US Geol. Surv. Prof. Pap., 295: 11-20.

1962 Restudy of the Arrowhead fault, Muddy Mountains, Nevada. US Geol. Surv. Prof. Pap. 450:D82-D85.

1963 Reconnaissance geology between Lake Mead and Davis Dam, Arizona-Nevada. US Geol. Surv. Prof. Pap. 374-E. 51 pp.

1964 With Reuben J. Ross. Paleotectonic significance of Ordovician sections south of the Las Vegas shear zone. US Geol. Surv. Bull., 1180-C:C88-C93.

1965 With George B. Maxey. Geology in vicinity of Las Vegas, Nevada: Frenchman Mtn. and Spring Range. First Basin and Range Geology Field Conference. Mackay School of Mines, Reno, Nevada, 1955. 24 pp.

Is uniformitarianism necessary? Am. J. Sci., 263:918-19.

With E. H. Pampeyan, Ben Bowyer, and R. J. Roberts. Geology and mineral deposits of Clark County, Nevada . Nev. Bur. Mines Bull., 62. 218 pp.

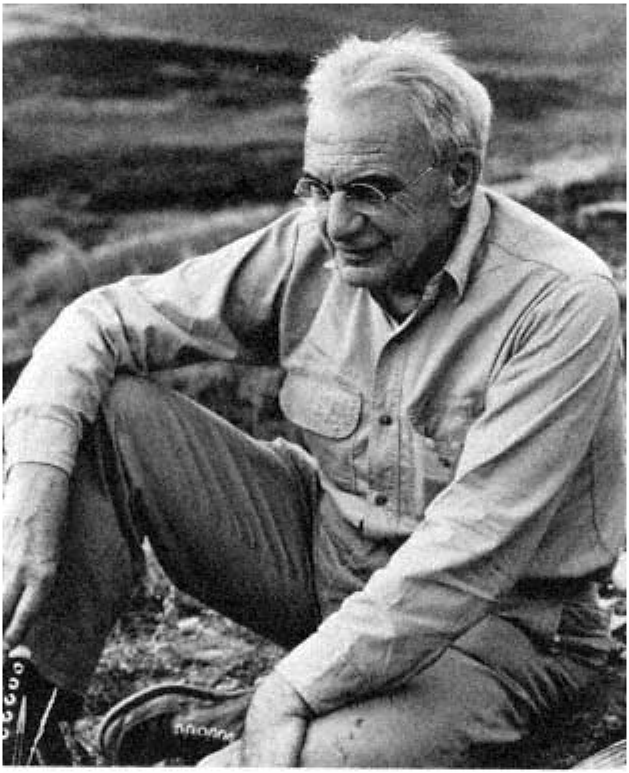
1967 With Michael C. Mound. A new Ordovician formation in Nevada dated by conodonts. Geol. Soc. Am. Bull., 78:405-11.

1971 Measure of lateral movement on Las Vegas shear zone, Nevada. Geol. Soc. Am. Abstr., 3:152.

With R. E. Anderson, Richard L. Armstrong, and others. Significance of K-Ar ages of Tertiary rocks from the Lake Mead region, Nevada-Arizona. Geol. Soc. Am. Bull., 83:273-87.

1974 Large-scale lateral faulting in southern Nevada. Geol. Soc. Am. Abstr., 6:209.

Measure and date of movement on Las Vegas Valley shear zone, Clark County, Nevada. Geol. Soc. Am. Bull., 85:985-89.



Alfred S. Romer

Alfred Sherwood Romer

December 28, 1894-November 5, 1973

by Edwin H. Colbert

Alfred Sherwood Romer was a man of many aspects: a profound scholar whose studies of vertebrate evolution based upon the comparative anatomy of fossils established him throughout the world as an outstanding figure in his field; a gifted teacher who trained several generations of paleontologists and anatomists; an effective administrator who never allowed the burden of office to diminish his research activities; a lucid writer whose books and scientific papers were and are of inestimable value; and a warm person, loved and admired by family, friends, and colleagues. Al, as he was universally known to his friends, lived a full and rewarding life, during which he led and influenced paleontologists, anatomists, and evolutionists in many lands. His absence is keenly felt.

Al Romer was born in White Plains, New York on December 28, 1894, the son of a newspaper man who was editor, and sometimes owner, of several small-town newspapers in Connecticut and New York, and who later worked for the Associated Press. On the paternal side he was descended from Jacob Romer, an emigrant from Zürich who settled among the Dutch residents of the Hudson River Valley about 1725. The Sherwood from whom he derived his middle name, the son of a British soldier, was brought to

Burlington, Vermont by his widowed mother about 1815. As for other forebears, Al has written that he had "a good dash of Scotch-Irish blood . . . . All the rest that I know of were part of the Puritan migration to New England between 1628 and 1640, whose descendants moved on westward to New York State."* Thus Al was a Hudson Valley New Yorker by inheritance and birth, but through his adult life he was a confirmed and enthusiastic New Englander.

At about the time he was ten years old, his parents, who then lived in New York City, were divorced, and he remained with his father. There was a second, unhappy marriage for Al's father, during which young Alfred was, as he says, "in a somewhat miserable situation" for a time.† He was rescued by his paternal grandmother, who lived in White Plains, and there he went to high school. After high school he was entirely on his own; because there was no tradition of a college education in his family, he was not encouraged to apply for entrance to any college or university. During the year after high school he worked as a railroad clerk. Perhaps this experience led to one of his hobbies, that of a railroad buff. In his later years he had an encyclopedic knowledge of American railroads and could rattle on by the hour about various railroad lines—their routes, their histories, and their prime personalities.

After a year of railroading he decided on college, and he obtained a scholarship at Amherst. There he spent four very active and rewarding years, studying hard, while at the same time supporting himself with a variety of jobs. He decided that since he had to work he would not get involved in too many extra-curricular activities, but he did join the college newspaper staff where he became the editor-in-chief. That

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* Alfred Romer to Hugh L. Dryden, 5 June 1961, Archives, National Academy of Sciences.

† *Ibid.*

was all to the good; he got some practical writing experience that was to be most useful to him in later years.

At Amherst Al had a double major in history and German literature, yet in spite of the hours devoted to these subjects there was one course, initially taken to fulfill a requirement, that was to determine the direction of his life. He needed to have a science course, so he opted for evolution, recommended to him by fellow students as "interesting and not too tough."* Part of the course was taught by Frederick Brewster Loomis, a vertebrate paleontologist, and soon after becoming involved in this course Al knew exactly what he wanted to do in life.

Here it may be enlightening to backtrack a bit. When Al was in grade school in Connecticut, he was bitten by his fox terrier, which had become rabid. Al was taken to New York for treatments at a branch of the Pasteur Institute. It was a protracted ordeal, and when Al was not at the Institute receiving injections he stayed with some aunts in Brooklyn. When he was not at either the Institute or at his aunts' house, he spent many hours at the American Museum of Natural History, where he lost his heart to the fossils on display there. When he later heard about fossil vertebrates from Professor Loomis, he understood the significance of his old museum friends. That made his decision.

One of Al's delightful traits was his pixie sense of humor. To hear him tell it, everything he accomplished during his life was the result of some sort of an accident. One would think that he blundered through his world in an aimless way, every now and then bumping into good fortune. If Al could be persuaded to tell about his life history, he would generally begin by recounting how he became a vertebrate paleontologist because he was bitten by a mad dog.

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* *Ibid.*

His training in his now-chosen field was necessarily delayed for a couple of years, because during his senior year at Amherst the United States became involved in the First World War. Al felt the call of duty and joined the American Field Service. Immediately after commencement he went to France, expecting to drive an ambulance; instead, because no ambulances were available, he drove an ammunition truck. In November of that year he joined the U.S. Air Service, where through the months he advanced from the status of a private to the rank of second lieutenant. His service in France culminated with his appointment to a post in command of about five hundred French ladies at a special camp where they were sewing covers on the wings of airplanes. Al's hilarious account of this assignment was just one of the famous Romer stories.

In 1919 he was back in New York, a graduate student at Columbia University with a teaching fellowship, all on the basis of a recommendation from Professor Loomis. There he studied under Professor William King Gregory, who taught on the graduate faculty at Columbia and who at the same time was a curator at the American Museum of Natural History. It should be explained that Professor Gregory had a dual appointment in the two institutions, the result of a longstanding arrangement that had been instituted by Henry Fairfield Osborn in 1891. Graduate students in paleontology at Columbia spent much of their time at the Museum. It was an advantageous arrangement for all concerned; the students had the use of unparalleled collections and instruction, in the case of Gregory, from a man who had a superb knowledge of all of the vertebrates, from fish to man. It was a golden opportunity to study under a man of Gregory's attainments, and Romer made the most of it. Professor Gregory's influence on Romer was inestimable. I have heard Al remark that in his opinion nobody ever had so complete an understanding of the vertebrate skull as did Gregory, and certainly the young Romer benefited from that; he too had a marvelous understanding of the skull.

At Columbia Al enjoyed the advantages not only of studying under some famous teachers—W. K. Gregory, J. H. McGregor, T. H. Morgan, and E. B. Wilson—but also the stimulating companionship of a talented group of fellow students and associates—Charles Camp, G. K. Noble, James Chapin, H. H. Johnson, Franz and Sally Schrader, and A. H. Sturtevant.

The following is another typical Romerian story, taken from a biographical sketch that he prepared for the Academy Archives:

> How I happened to take up a thesis topic is mildly amusing. As soon as I arrived at Columbia, I went to see Gregory, and discovered that I could not take his regular course because of conflict with laboratory teaching. "But," said Gregory, "a few of us are interested in comparative myology, and we're planning to have a special course in that subject. Would you care to join?" I said that I would love to take this, and then went down the hall in search of a dictionary. I thought that myology had something to do with clams, and was pleased to discover that it had to do with muscles. Within a few weeks after I took up the course, I proposed a new theory as to the classification and evolution of limb muscles (which I found held up very nicely after later work on embryology) and was embarked on a thesis which consisted of a consideration of muscle evolution and the probable musculature of primitive fossil amphibians and reptiles.*

This anecdote nicely illustrates one of Romer's endearing qualities; he was serious about his work, but he never took himself too seriously. It also illustrates his remarkable ability to cut through the puzzling aspect of a problem and to resolve its difficulties with an elegant solution. His thesis emerged as a classic paper entitled "The Locomotor Ap

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* *Ibid.*

paratus of Certain Primitive and Mammal-Like Reptiles" (see bibliography, 1922).

After the completion of his graduate course work and his thesis, all within the incredibly short span of two years, Romer was appointed an instructor in the Anatomy Department of the Bellevue Medical School at New York University. He spent two years at Bellevue, teaching histology, embryology, and gross anatomy—courses to which he had never been exposed—all the while working feverishly to keep ahead of his students. During such intervals as he could find within this frenetic schedule, he was continuing his research. One result of such concentrated activity was a spastic colon, for which a regimen of chloreton was prescribed.

Next came an offer from the University of Chicago. The manner in which he obtained his appointment at Chicago is perhaps one of the most amusing of the Romerian stories, and it is here set down in Al's words as recorded in a taped interview made on February 9, 1973.

> They started feeding me some sort of capsules. They probably told me what they were, but I didn't pay any attention . . . . Well, along came the anatomists' meetings that spring, in April. They were out at the University of Chicago, and I went out. God, I was getting sleepy. I tend to go to sleep when people read papers at me, but here, even during ones I was really interested in, I just couldn't stay awake. I was just dopey. Well, they were looking for a vertebrate paleontologist and heard I was in town. So I was invited over to lunch by the chairman of the appropriate department. I was very sleepy, and he started the proposition—could I come up for a quarter, give a course or two—(yawn) I wasn't sure—he went on, could I come out and give a few lectures—(yawn) I wasn't too sure about that either. If I had been awake, I would have jumped at this, but dopey as I was, I didn't jump. Well this blasé attitude apparently was pretty good, because I no sooner got back to New York than I got a letter offering me an assistant professorship, which is one up from instructor . . . . Well, I was sleepier than ever, so I wrote back, "Well, I don't know, this time of the year is pretty late for my boss to get a successor for me, and so forth. Hoping you are the same." And sent it off. Well, a few days later I came to. I had gone to sleep in the middle

> of the morning with my eye on a microscope barrel. I went down and saw the medicine man and said, "Look here, either I've got sleeping sickness, or else it's whatever dope I'm taking." He said, "You damn fool, don't you know what you're taking?" "No." "Chloretone." And the idea was, as you know, it's a nice anesthetic. They thought it might put my large intestine to sleep. Instead, it was putting *me* to sleep, and so they took me off it and I woke up, at which point arrived a telegram offering me an associate professorship. I thought, gee, this has worked out pretty well. I've made two jumps now. Could I play it still further and jump from instructor to full professor? I finally decided not, and signed on the dotted line. So chloretone did it. I don't know if it would work for other people or not.*

While Al was a graduate student at Columbia he spent summers at the Woods Hole Biological Laboratory, and there he met Ruth Hibbard, the younger sister of Dr. Hope Hibbard, a zoologist studying at Woods Hole. When he went to Chicago in 1923 he again encountered Ruth, working as a labor statistician and living in the vicinity of the University. They became friends, they fell in love, and the next fall were married in Columbia, Missouri, where Ruth's father was a professor at the University of Missouri. It was a fortunate and a happy marriage.

One cannot contemplate the career of Al Romer without giving full attention to the contribution Ruth made to that career. She was Al's devoted partner through the years, working closely with him at home and away from home. It must not be thought that Ruth was a self-sacrificing nonentity, subjecting herself in every way to the advancement of Al's career. She was not. Ruth was always a forceful person with her own definite views about the world in which she lived. But she complemented Al in a marvelous fashion; together the two of them cooperated in a mutually advantageous manner. Al fully appreciated Ruth's role in their

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* G. E. Erikson, "Alfred Sherwood Romer" (Proceedings of the Ninetieth Meeting of the American Association of Anatomists), *Anatomical Record,* 189 (1977): 314-24.

partnership; he wrote that their marriage was "the best thing that ever did happen, or could have happened to me."* There are three children: Sally (Mrs. Paul Evans), a librarian at Amherst College; Robert, professor of physics at Amherst College; and James, who lives and works in Providence, Rhode Island. There are seven grandchildren.

Circumstances do affect the directions that our lives follow, a point that Al liked to emphasize in lively tales according to which he just happened, more or less by accident, to develop his career. Of course the favorable circumstances were there; but Al saw his opportunities and developed them with unparalleled acumen and ability. One wonders what direction his life would have taken if he had not gone to Chicago, if he had stayed at Bellevue, or if he had gone to some institution lacking a program in vertebrate paleontology or a collection of fossil vertebrates. He certainly would have become a leading anatomist (as indeed he was) but perhaps an anatomist working more on modern than on extinct animals.

As things turned out, he went to a university that had on hand a fine collection of ancient backboned animals, particularly Permian amphibians and reptiles. These fossils had been amassed by Romer's distinguished predecessor, Samuel Wendell Williston, with the able cooperation of his field and laboratory assistant, Paul Miller, who was still at Chicago when Romer arrived. Al Romer soon became involved with Permian tetrapods, and this field of research remained the dominant center of his scientific effort for the remainder of his life. It is interesting to note that from 1922 through 1924 he was the author of eight anatomically oriented publications. From 1925 through 1935 (which may be taken as the years during which his contributions originated in Chicago) there

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* Romer to Dryden.

were thirty-seven publications, most of which might be characterized as primarily paleontological. This remained true of his subsequent publications. Although Al entered upon a program of research based to a large degree on the Permian collections at Chicago, at the same time he began a vigorous campaign to augment those collections by field work in the Permian sediments of Texas and New Mexico. In 1929 he extended his paleontological horizons by going to South Africa with Paul Miller and making an important collection from the famous Permo-Triassic Karroo beds.

He spent eleven productive years at Chicago, studying, publishing, and teaching. Among other things, he was involved, together with several colleagues, in the presentation of a general education course in science for nonscience students. A text was needed, so the participating professors collaborated on a book, edited by H. H. Newman, entitled *The Nature of the World and of Man,* published in 1926. Al's chapter in this book, "The Evolution of the Vertebrates," was expanded by him into a book, *Man and the Vertebrates,* published in 1933. During the same year the first edition of his invaluable textbook, *Vertebrate Paleontology,* was published. Both of these books have enjoyed well-deserved success in this country and abroad and have appeared during subsequent years as revised editions.

Although Al enjoyed his work and his colleagues at Chicago, he declared that he "had no particular love for midwestern country."* As has been mentioned, he was an enthusiastic New Englander, so in the summer of 1932 the Romer's went to Massachusetts and began to look around in the vicinity of Amherst for a country retreat. The search continued during the following summer, when they were fortunate to find a place completely to their liking in the town

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* *Ibid.*

of Pelham, near Amherst. It was a two-hundred-acre tract of abandoned farmland, mostly grown up into woods, occupied by a dilapidated old house, the earliest section of which had been built in 1740. They bought the property, and through the years it was their much-beloved second home, where they usually spent several months of each year. The house became one of Al's hobbies. Single-handedly he began a long-term project of restoration, eventually resulting in a choice example of a New England colonial farmhouse. Al was a dedicated purist and insisted that everything about the house should be as nearly authentic as possible. For example, he bought an old house that had been condemned because it was on a reservoir site, and he used the woodwork from that house in the restoration of his Pelham home.

The Romer's had contemplated a long trek each summer from Chicago to Pelham and back, but the year after they had purchased their New England place Al was offered a position at Harvard. He was to be professor of zoology and at the same time curator of vertebrate paleontology at the famous Museum of Comparative Zoology (the MCZ as it is known to museum people around the world). It was exactly the type of situation that he had wanted and had never been quite able to achieve at Chicago. The Romer's moved to Cambridge, where they bought a picturesque old home near the MCZ; through the years they graciously entertained hosts of visiting paleontologists and other guests visiting the MCZ. Al divided his time between an office in the Biological Laboratories and another office in the Museum. Al would be busily and happily occupied in Cambridge and in Pelham for just short of forty years.

When Al arrived at Harvard the program in vertebrate paleontology at the MCZ was in a state of desuetude; collections were available but were not being used to any great extent, nor were they being augmented. Romer changed that situation by inaugurating a very active program of research and field work on Permian tetrapods. He continued his collecting activities in Texas, and within a few years the Permian collections at the Museum were well known and well used. A steady outflow of research, recorded in various scientific journals, emanated from the Museum—research by Romer and by his students. It should be emphasized that during his years at Harvard, Professor Romer trained an outstanding cadre of vertebrate paleontologists, anatomists, and vertebrate zoologists, men and women who now hold positions in universities and mUseums across the land, and in some foreign countries as well.

During this earlier years at Harvard, the MCZ was under the directorship of Thomas Barbour, a distinguished herpetologist. It is said that Barbour ruled the Museum as a benevolent autocrat. He was wealthy, and he contributed considerable sums to the Museum from his personal fortune, particularly before the depression of the thirties. Perhaps because of his wealth he lacked understanding of the needs of his subordinates. Consequently, at the time of his death, the staff was woefully underpaid, and the Museum was insufficiently financed.

In the meantime, following the depression and immediately after the war, Romer had attracted worldwide attention to the MCZ with his vigorous program in paleontology. Thus he was appointed director of the Museum, relinquishing his position as director of the Biological Laboratories. He directed the Museum for fifteen years. It meant much time diverted from other activities to administration, yet Al accepted the extra burden, to become a vigorous and imaginative leader of the Museum. During his tenure as director, the endowment of the Museum was increased tenfold, and the salaries of an enlarged staff were brought into line with other university salaries. Along with this financial improvement, there was insistence by Al on a high standard of scientific performance and on high standards for staff appointments, following the procedures in Harvard's regular academic departments. As has been said, Al did not allow this important work to submerge his research productivity; in the continuation of his research he was ably assisted through the years by Nelda Wright.

At Harvard Romer augmented his scientific-literary output with revisions of the texts previously mentioned, as well as by the production of a massive source book, *Osteology of the Reptiles,* which appeared in 1956. A book with this same title had been published years earlier by Romer's predecessor at Chicago, S. W. Williston. But Romer's book is more than an updating of Williston's text (which was perhaps the original intention); it is a completely new, authoritative survey of reptilian osteology, widely used by paleontologists throughout the world. He also wrote books for a more general audience, such as *The Vertebrate Body,* published in 1949, now translated into several foreign languages; an immensely popular work, *The Vertebrate Story,* published in 1959; and *The Procession of Life,* published in 1968.

During the last decade of his life, Al organized and conducted a series of expeditions to Argentina, where he worked in close cooperation with Dr. Rosendo Pascual of La Plata in the collection of a very significant suite of Triassic reptiles. The series of papers on these fossils, some of them published in collaboration with other paleontologists, was one of Romer's last extended research projects.

Al was active in many scientific societies. Perhaps his greatest satisfaction in this connection was his role in the organization of the Society of Vertebrate Paleontology, of which he was the first president in 1940. He was also president of the American Association for the Advancement of Science (1966), the American Society of Zoologists (1951), the

Society of Systematic Zoology (1952), the Society for the Study of Evolution (1953), and the Sixteenth International Zoological Congress, held in Washington in 1963. In addition to these societies he was a member of the American Philosophical Society and the American Academy of Arts and Sciences. He also served on the governing boards of various societies.

Many honors were deservedly bestowed upon Professor Romer. He was elected to the National Academy of Sciences in 1944 and was made a foreign member of the Royal Society in 1969. He was a fellow or foreign member of other British societies and also societies in Germany, Argentina, and India.

He received the Thompson Medal and the Elliot Medal of the National Academy of Sciences in 1956 and 1960, respectively, the Penrose Medal of the Geological Society of America in 1962, the Hayden Geological Award of the Academy of Natural Sciences of Philadelphia in 1962, the Paleontological Society Medal in 1967, the Linnean Society Medal in 1972, and the Wollaston Medal of the Geological Society of London in 1973. He received honorary degrees from Harvard (1949), Amherst (1952), Dartmouth (1959), Buffalo (1960), and Lehigh (1963).

Such honors never went to Al's head. He was about the unstuffiest person who ever lived. His ebullient behavior and breezy conversations, often interspersed with comic songs, were frequently the cause of raised eyebrows among visiting European scientists, who probably expected a man of Al's eminence to act in a thoroughly formal, perhaps even a funereal manner.

Romer published considerably more than two hundred papers and books on a wide variety of subjects within the general fields of vertebrate paleontology, anatomy, and evolution. He was especially interested in those fishes most closely related to the tetrapods, presenting valuable papers on the crossopterygians and lungfishes. His largest and most numerous contributions were, however, on fossil amphibians and reptiles. As has been indicated, his studies of these tetrapods revolved around the Permian amphibians and reptiles found in Texas and adjacent regions. Such fossils afforded opportunities for significant papers on the labyrinthodont amphibians, the stem reptiles known as cotylosaurs, and the early synapsid reptiles known as pelycosaurs.

Particular mention should be made of his monographs, *Review of the Pelycosauria* (published in collaboration with L. I. Price in 1940) and *Review of the Labyrinthodontia* (published in 1947). Among other papers on amphibians were those devoted to the relationships of the labyrinthodonts to stem reptiles; he also published a significant paper in 1939, showing that the branchiosaurs, often separately classified, represented in fact ontogenetic stages in labyrinthodont development. Romer was especially interested in the relationships of the pelycosaurs to the therapsids (the higher "mammal-like reptiles"), and in 1956 he published a revised classification of the therapsids in collaboration with Professor D. M. S. Watson of the University of London. But it was not until the last decade of his life that he did extensive work on the therapsids, contained in his series of papers on the Chanares reptiles from the Triassic beds of Argentina.

Romer published little on the mammals as such (except, of course, in his books), but he was interested in the transition from mammal-like reptiles to mammals. He stoutly maintained at an early date that the crucial prehominid anthropoid, *Australopithecus,* was not a chimpanzee.

Many of his papers were in a sense concerned with comparative anatomy—particularly the anatomy of extinct tetrapods. There were, however, important basic anatomical studies, such as his early papers on limb musculature, his papers on crossopterygian fins and on the foot in early tetra pods, his paper on the transformation of the hyomandibular bone into the tetrapod stapes, his studies of skull structure, his essay on the somatic and visceral duality of the vertebrate, his paper on cartilage as an embryonic adaptation, and the papers on the nature of bone in early vertebrates.

Although Al never pretended to be a geologist, he completed some very important papers on the stratigraphy of the Texas red beds, on the stratigraphic sequence in Argentina, and on Gondwanaland.

Needless to say, the theme of evolution permeates a large proportion of his publications. It was not his practice to write essays on evolution, or to theorize on the mechanisms of evolution, but he did introduce evolutionary principles and considerations in his descriptions and discussions of the fossils with which he was concerned. He felt that this was the most effective manner in which he could discuss larger evolutionary problems.

Romer bequeathed his magnificent personal library covering the fields of vertebrate paleontology, anatomy, and embryology to the Museum of Comparative Zoology, and it is located in his old office, a spacious and comfortable room. The library, a separate facility of the Museum, is kept up to date with current publications in the fields mentioned above, and it is widely used.

Alfred Romer died on November 5, 1973, after a brief illness. He is mourned by all who knew him.

Bibliography

1922 The comparison of mammalian and reptilian coracoids. Anat. Rec., 24:39-47.

The locomotor apparatus of certain primitive and mammal-like reptiles. Bull. Am. Mus. Nat. Hist., 46:517-606.

1923 The ilium in dinosaurs and birds. Bull. Am. Mus. Nat. Hist., 48:141-45.

Crocodilian pelvic muscles and their avian and reptilian homologues. Bull. Am. Mus. Nat. Hist., 48:533-52.

The pelvic musculature of saurischian dinosaurs. Bull. Am. Mus. Nat. Hist., 48:605-17.

1924 Pectoral limb musculature and shoulder-girdle structure in fish and tetrapods. Anat. Rec., 27:119-43.

The lesser trochanter of the mammalian femur. Anat. Rec., 28: 95-102.

A radial exostosis in the fossil canid *Daphoenus.* Am. J. Sci., 8(5): 235-40.

1925 An ophiacodont reptile from the Permian of Kansas. J. Geol., 33:173-82.

Permian amphibian and reptilian remains described as *Stephanospondylus.* J. Geol., 33:447-63.

1926 A Lower Miocene horse, *Anchitherium agatense* (Osborn). Am. J. Sci., 12(5):325-35.

The evolution of the vertebrates. In: *The Nature of the World and of Man,* ed. H. H. Newman, pp. 304-48. Chicago: Univ. of Chicago Press.

Vertebrate faunal horizons in the Texas "red beds." Geol. Soc. Am. Prelim. List, 38:58-59.

1927 The development of the thigh musculature of the chick. J. Morphol. Physiol., 43:347-85.

With A. H. Sutton. A new arctoid carnivore from the Lower Miocene. Am. J. Sci., 14(5):459-64.

Notes on the Permo-Carboniferous reptile *Dimetrodon.* J. Geol., 35: 673-89.

The pelvic musculature of ornithischian dinosaurs. Acta Zool., 8: 225-75.

1928 Among our ancestors. Univ. Chicago Mag., 20:322-26.

A fossil camel recently living in Utah. Science, 68:19-20.

Vertebrate faunal horizons in the Texas Permo-Carboniferous red beds. Univ. Tex. Bull., 2801:67-107.

The notochord in fossil vertebrates. Am. J. Sci., 15(5):432-33.

A skeletal model of the primitive reptile *Seymouria,* and the phylogenetic position of that type. J. Geol., 36:247-260.

Pleistocene mammals of Algeria: Fauna of the Paleolithic station of Mecha-el Arbi. Logan Mus. Bull., 2:79-163.

With F. M. Fryxell. *Paramiatus gurleyi,* a deep-bodied amid fish from the Eocene of Wyoming. Am. J. Sci., 16:519-27.

With J. T. McCormack. A large *Palaeocastor* from the Lower Miocene. Am. J. Sci., 15:58-60.

1929 A fresh skull of an extinct American camel. J. Geol., 37:261-67.

Taxonomy and morphology of some Pennsylvanian amphibians. Bull. Geol. Soc. Am., 40:243(A).

1930 *Australopithecus* not a chimpanzee. Science, 71:482-83.

The Pennsylvanian tetrapods of Linton, Ohio. Bull. Am. Mus. Nat. Hist., 59:77-147.

Fossil hunting in the Karroo, South Africa. Sci. Mon., 31:134-44.

With P. H. Nesbitt. An extinct cane-rat *(Thryonomys Logani,* sp. n.) from the central Sahara. Annu. Mag. Nat. Hist., 6(10):687-90.

1931 With F. Byrne. The pes of *Diadectes:* Notes on the primitive tetrapod limb. Palaeobiologica, 4:25-48.

Surveying Pleistocene fauna. Science, n.s. 74:117(A).

1932 Treasure hunters. Univ. Chicago Mag., 24:149-54.

1933 Pleistocene vertebrates and their bearing on the problem of human antiquity in North America. In: *The American Aborigines,* ed. D. Jenness, pp. 49-83. Toronto: Univ. of Toronto Press.

Eurypterid influence on vertebrate history. Science, n.s. 78:114-17.

Man and the Vertebrates. Chicago: Univ. of Chicago Press. vii + 427 pp.

Vertebrate Paleontology. Chicago: Univ. of Chicago Press. vii + 491 pp.

1934 With B. H. Grove. Fresh versus salt water in early vertebrate evolution. Proc. Geol. Soc. Am. (1933):361-62.

With H. J. Smith. American Carboniferous dipnoans. J. Geol., 42:700-719.

1935 Early history of Texas red beds vertebrates. Bull. Geol. Soc. Am., 46:1597-658.

With B. H. Grove. Environment of the early vertebrates. Am. Midi. Nat., 16:805-62.

Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1934-1935:37-40.

1936 Studies on American Permo-Carboniferous tetrapods. Probl. Paleontol., 1:85-93.

The dipnoan cranial roof. Am. J. Sci., 32(5):241-56.

Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1935-1936:40-41. With H. J. Smith. Dipnoan skull roof. Proc. Geol. Soc. Am. (1935): 393.

1937 The braincase of the Carboniferous crossopterygian *Megalichthys nitidus.* Bull. Mus. Comp. Zool. Harv. Univ., 82:1-73.

New genera and species of pelycosaurian reptiles. Proc. N. Engl. Zool. Club, 16:89-96.

Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1936-1937:41-43.

Man and the Vertebrates. 2d ed. Chicago: Univ. of Chicago Press. ix + 434 pp.

Vertebrates. In: *The World and Man,* ed. F. R. Moulton, pp. 347-96. Garden City, N.Y.: Doubleday, Doran, and Co.

1938 Mammalian remains from some Paleolithic stations in Algeria. Part II. Logan Mus. Bull., 5:165-84.

Review of the Pelycosauria. Proc. Geol. Soc. Am. (1937): 287-88.

Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1937-1938:40-41.

Frederic Brewster Loomis (1873-1937). Proc. Am. Acad. Arts Sci., 73:136-37.

1939 The oldest vertebrate egg. Am. J. Sci., 237:826-29.

Notes on branchiosaurs. Am. J. Sci., 237:748-61.

An amphibian graveyard. Sci. Mon., 49:337-39.

Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1938-1939:37-39.

1940 Fossil collecting in the Texas red beds. Harv. Alum. Bull., 42: 1045-49.

Mirror image comparison of upper and lower jaws in primitive tetrapods. Anat. Rec., 77:175-79.

With L. I. Price. *Review of the Pelycosauria.* Geol. Soc. Am. Spec. Pap., 26. x + 538 pp.

Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1939-1940:38-40.

1941 Vertebrate paleontology. In: *Geology, 1888-1939,* pp. 107-35. New York: Geological Society of America.

Notes on the crossopterygian hyomandibular and braincase. J. Morphol. 69:141-60.
Earliest land vertebrates of this continent. Science, 94:279.
The first land animals. Nat. Hist., 48:236-43.
With R. V. Witter. The skin of the rhachitomous amphibian *Eryops.* Am. J. Sci., 239:822-24.
Man and the Vertebrates. 3d ed. Chicago: Univ. of Chicago Press. viii + 405 pp.
Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1940-1941:30-32.
1942 Notes on certain American Paleozoic fishes. Am. J. Sci., 240: 216-28.
Cartilage, an embryonic adaptation. Am. Nat., 76:394-404.
The development of tetrapod limb musculature-the thigh of *Lacerta.* J. Morphol., 71:251-98.
With T. Edinger. Endocranial casts and brains of living and fossil Amphibia. J. Comp. Neurol., 77:355-89.
With R. V. Witter. *Edops,* a primitive rhachitomous amphibian from the Texas red beds . J. Geol., 50:925-60.
Report on vertebrate paleontology. Annu. Rep. Dir. Mus. Comp. Zool. Harv. Univ., 1941-1942:31-32.
1943 Recent mounts of fossil reptiles and amphibians in the Museum of Comparative Zoology. Bull. Mus. Comp. Zool. Harv. Univ., 92:331-38.
Hunting for Grandpa Bumps. Sci. Mon., 57:94-96.
1944 The Permian cotylosaur *Diadectes tenuitectus.* Am. J. Sci., 242: 139-44.
With L. I. Price. *Stahleckeria lenzii,* a giant Triassic Brazilian dicynodont. Bull. Mus. Comp. Zool. Harv. Univ., 93:465-91.
The development of tetrapod limb musculature-the shoulder region of *Lacerta.* J. Morphol., 74:1-41.
1945 *Vertebrate Paleontology.* 2d ed. Chicago: Univ. of Chicago Press. viii + 687 pp.

The late Carboniferous vertebrate fauna of Kounova (Bohemia) compared with that of the Texas red beds. Am. J. Sci., 243:417-42.

1946 The primitive reptile *Limnoscelis* restudied. Am. J. Sci., 244:149-88.

The early evolution of fishes. Q. Rev. Biol., 21:33-69.

Thomas Barbour, 1884-1946. Soc. Vert. Paleontol. News Bull., 17:24-25.

Thomas Barbour. Anat. Rec., 95:473-75.

1947 Review of the Labyrinthodontia. Bull. Mus. Comp. Zool. Harv. Univ., 99:1-368.

The relationships of the Permian reptile *Protorosaurus.* Am. J. Sci., 245:19-30.

The University. The Agassiz Museum. Harv. Alum. Bull., 49: 619-21.

Louis Agassiz; a hundred years after his coming his influence is still felt. Harv. Alum. Bull., 49:628-31.

1948 Relative growth in pelycosaurian reptiles. R. Soc. S. Afr. Spec. Publ., Robert Broom Commemorative vol., pp. 45-55.

Ichthyosaur ancestors. Am. J. Sci., 246:109-21.

Zoology at Harvard. Bios, 19:7-20.

Advances in the study of organic evolution. Bull. Am. Acad. Arts Sci., 1:2-3.

With H. B. Bigelow and F. M. Carpenter. Hubert Lyman Clark. Harv. Univ. Gaz., 43:100-101.

The fossil mammals of Thomas Farm, Gilchrist County, Florida. Q. J. Fl. Acad. Sci., 10:1-11.

Harvard portraits-A. S. Rorer. Harv. Alum. Bull., 9(50):390.

1949 Louis Agassiz. Sci. Am., 181:48-51.

The color line in fraternities. Atl. Mon., May, pp. 27-31.

Time series and trends in animal evolution. In: *Genetics, Paleontology and Evolution,* ed. G. L. Jepsen, E. Mayr, and G. G. Simpson, pp. 103-20. Princeton, N.J.: Princeton Univ. Press.

The Vertebrate Body. Philadelphia: W. B. Saunders. vii + 643 pp.

1950 The nature and relationships of the Paleozoic microsaurs. Am. J. Sci., 248:628-54.

The Upper Paleozoic Abo Formation and its vertebrate fauna. In: *Guidebook for the Fourth Field Conference of the Society of Vertebrate Paleontology in Northwestern New Mexico,* ed. E. H. Colbert, S. A. Northrop, A. S. Romer, and G. G. Simpson, pp. 48-55. Mimeographed.

1951 *Bison crassicornis* in the late Pleistocene of New England. J. Mammal., 32:230-31.

1952 Late Pennsylvanian and early Permian vertebrates of the Pittsburgh-West Virginia region. Ann. Carnegie Mus., 33:47-110.

Discussion of "The Mesozoic tetrapods of South America," by E. H. Colbert. Bull. Am. Mus. Nat. Hist., 99:250-54.

1954 Aestivation in a Permian lungfish. Breviora, Mus. Comp. Zool., 30:1-8.

1955 Fish origins-fresh or salt water? Deep-Sea Res., 3(Suppl.):2^{6}1-8^{0}.

Herpetichthyes, Amphibioidei, Choanichthyes or Sarcopterygii? Nature, 176:126.

The Vertebrate Body. 2d ed., rev. Philadelphia: W. B. Saunders. viii + 644 pp.

1956 With D. M. S. Watson. A classification of therapsid reptiles. Bull. Mus. Comp. Zool. Harv. Univ., 114:37-89.

The early evolution of land vertebrates. Proc. Am. Philos. Soc., 100:157-67.

Les rapports entre la paléontologie des vertébrés et l'anatomie comparée. In: *Colloques internationaux du Centre National de la Recherche Scientifique 60: Problèmes actuels de paléontologie* (Paris, 18-23 April 1955), pp. 149-59. Paris: Centre National de la Recherche Scientifique.

A Shorter Version of the Second Edition of the Vertebrate Body. 2d ed., abridged. Philadelphia: W. B. Saunders. viii + 486 pp.

Osteology of the Reptiles. Chicago: Univ. of Chicago Press. xxi + 772 pp.

1957 The appendicular skeleton of the Permian embolomerous amphibian *Archeria.* Contrib. Mus. Paleontol. Univ. Mich., 13:103-59.

Origin of the amniote egg. Sci. Mon., 85:57-63.

Amphibians. In: *Treatise on Marine Ecology and Paleoecology,* vol. 2, ed. H. S. Ladd. Geol. Soc. Am. Mem., 67:1011.

1958 An embolomere jaw from the mid-Carboniferous of Nova Scotia. Breviora, Mus. Comp. Zool., 87:1-8.

Darwin and the fossil record. In: *A Century of Darwin,* ed. S. A. Barnett, pp. 130-52. London: William Heinemann Ltd.

The Texas Permian red beds and their vertebrate fauna. In: *Studies on Fossil Vertebrates,* ed. T. S. Westoll, pp. 157-79. London: Univ. of London, The Athlone Press.

The vertebrate as a dual animal-visceral and somatic. Anat. Rec., 132:496(A).

Tetrapod limbs and early tetrapod life. Evolution, 12:365-69.

Phylogeny and behavior with special reference to vertebrate evolution. In: *Behavior and Evolution,* ed. A. Roe and G. G. Simpson, pp. 48-75. New Haven: Yale Univ. Press.

1959 *Vergleichende Anatomie der Wirbeltiere* [The Vertebrate Body], trans. H. Frick and D. Starck. Hamburg and Berlin: Paul Parey. xii + 499 pp.

Louis Agassiz in America. Harv. Alum. Bull., 5(62):209, 224-25.

The Vertebrate Story. Chicago: Univ. of Chicago Press. vii + 437 pp.

Vertebrate paleontology, 1908-1958. J. Paleontol., 33:915-25.

Darwin and the fossil record. Nat. Hist., 68:457-69.

A mounted skeleton of the giant plesiosaur *Kronosaurus.* Breviora, Mus. Comp. Zool., 112:1-14.

Fossil skeleton reconstructed after 100-million-year delay. Discovery, Nov., p. 464.

Centennial of the Agassiz Museum. Newsl. Harv. Found. Adv. Stud., 30 Sept., p. 3.

1960 Vertebrate-bearing continental Triassic strata in Mendoza region, Argentina. Bull. Geol. Soc. Am., 71:1279-94.

McGraw-Hill Encyclopedia of Science and Technology, vols. 1-4, 6-14, articles on: Aistopoda, Amphibia fossils, Animal evolution, Anthracosauria, Batoidea fossils, Bradyodonti, Chimaerae, Cladoselachii, Elasmobranchii fossils, Embolomeri, Hybodontoidea, Ichthyostegalia, Labyrinthodontia, Lepospondyli, Microsauria, Nectridia, Pleuracanthodii, Rachitomi, Selachii fossils, Seymouriamorpha, Stereospondyli, Temnospondyli, and Trematosauria. New York: McGraw-Hill.

Explosive evolution. Zool. Jahrb. Abt. Syst. Oekol. Geogr. Tiere, 88:79-90.

The vertebrate fauna of the New Mexico Permian. In: *Guidebook of Rio Chama Country,* ed. E. C. Beaumont and C. B. Read, pp. 48-54. New Mexico Geol. Soc., Eleventh Field Conference. Socorro, N.M.: New Mexico Bureau of Mines and Mineral Resources.

1961 Palaeozoological evidence of climate. (1) Vertebrates. In: *Descriptive Palaeoclimatology,* ed. A. E. M. Nairn, pp. 183-206. London and New York: Interscience Publishers.

A large ophiacodont pelycosaur from the Pennsylvanian of the Pittsburgh region. Breviora, Mus. Comp. Zool., 144:1-7.

1962 A Cambridge triumvirate. New Engl. Q., 35:104-9.

The fossiliferous Triassic deposits of Ischigualasto, Argentina. Breviora, Mus. Comp. Zool., 156:1-7.

Vertebrate evolution. Copeia, 1962:223-27.

The Vertebrate Body. 3d ed. Philadelphia: W. B. Saunders Co. viii + 627 pp.

The Vertebrate Body. 3d ed., abridged. Philadelphia: W. B. Saunders Co. vii + 475 pp.

Synapsid evolution and dentition. Kon. Vlaamse Acad. Wetensch. Lett. Sch. Kunsten België, Brussels, 1961:9-56.

Darwin and paleontology. Univ. Kans. Sci. Bull., 62(Suppl.):53-61.

With N. E. Wright, T. Edinger, and R. Van Frank. *Bibliography of Fossil Vertebrates Exclusive of North America, 1509-1927.* 2 vols. Geol. Soc. Am. Mem. 87. 1544 pp.

La evolución explosive de los rhynchosaurios del Triasico. Rev. Mus. Argent. Cienc. Nat. Bernardino Rivadavia Inst. Nac. Invest. Cien. Nat. Cienc. Zool., 8:1-14.

1963 The larger embolomerous amphibians of the American Carboniferous. Bull. Mus. Comp. Zool. Harv. Univ., 128:415-54.

International Congress of Zoology, 20-27 August 1963. Science, 140:1113-16.

The "ancient history" of bone. Ann. N. Y. Acad. Sci., 109:168-76.

Foreword. In: *The Intelligence of Louis Agassiz,* ed. Guy Davenport, pp. vii-x. New York: Beacon.

1964 The braincase of the Paleozoic elasmobranch T*amiobatis.* Bull. Mus. Comp. Zool. Harv. Univ., 131:87-105.

The skeleton of the Lower Carboniferous labyrinthodont *Pholidogaster pisciformis.* Bull. Mus. Comp. Zool. Harv. Univ., 131: 129-59.

Vertebrate Paleontologists Society. Geotimes, 8:22-24.

Bone in early vertebrates. In: *Bone Biodynamics,* ed. H. M. Frost, pp. 13-40. Boston: Little, Brown.

Problems in early amphibian history. J. Anim. Morphol. Physiol., 2:1-20.

Memorial. Augusta Hasslock Kemp. J. Paleontol., 38:1008.

Diadectes an amphibian? Copeia, 1964:718-19.

Cope versus Marsh. Syst. Zool., 13:201-7.

Thomas Barbour. Syst. Zool., 13:227-34.

1965 Possible polyphylety of the vertebrate classes. Zool. Jahrb. Abt. Syst. Oekol. Geogr. Tiere, 92:143-56.

1966 The Chañares (Argentina) Triassic reptile fauna. I. Introduction. Breviora, Mus. Comp. Zool., 247:1-14.

With J. A. Jensen. The Chañ ares (Argentina) Triassic reptile fauna. II. Sketch of the geology of the Rio Chañares-Rio Gualo region. Breviora, Mus. Comp. Zool., 252:1-20.

Las capas Triasicas del "Gondwana" en la historia de la evolución de los vertebrados. Rev. Mus. Argent. Cienc. Nat. Paleontol., 1(5): 115-31.

Vertebrate Paleontology. 3d ed. Chicago: Univ. of Chicago Press. ix + 468 pp.

Vergleichende Anatonie der Wirbeltiere. 2d ed., trans. H. Frick. Hamburg and Berlin: Paul Parey. 536 pp.

Letter from the president. Bull. Am. Assoc. Adv. Sci., Sept. 1966. 2 pp.

With J. W. Stovall and L. I. Price. The postcranial skeleton of the giant Permian pelycosaur *Cotylorhynchus romeri.* Bull. Mus. Comp. Zool. Harv. Univ., 135:1-30.

1967 The Chañares (Argentina) Triassic reptile fauna. III. Two new gomphodonts *Massetognathus pascuali* and *M. teruggi.* Breviora, Mus. Comp. Zool., 264:1-25.

Collecting Triassic fossils in Argentina. Harv. Alum. Bull., 69(13): April 22, pp. 14-18.

A letter to council members. Bull. Am. Assoc. Adv. Sci., 12:3-4.

Early reptilian evolution reviewed. Evolution, 21:821-33.

Major steps in vertebrate evolution. Science, 158:1629-37.

George Howard Parker. In: *Biographical Memoirs,* 39:359-90. New York: Columbia Univ. Press for the National Academy of Sciences.

Reply to presentation of Paleontological Society Medal. J. Paleontol. 41:818-19.

With others. *Galeopithecus* Pallas. Bull. Zool. Nomencl., 24:190-91.

1968 *The Vertebrate Body.* 3d ed., abridged. Philadelphia and Tokyo: W. B. Saunders Co. 475 pp.

An ichthyosaur skull from the Cretaceous of Wyoming. Contrib. Geol. Univ. Wyo., 1(7):27-41.

The Procession of Life. London: Weidenfeld & Nicolson. 323 pp.

Foreword. In: Bernhard Peyer, *Comparative Odontology,* trans., ed. Rainer Zangerl, pp. v-vi. Chicago: Univ. of Chicago Press.

Notes and Comments on Vertebrate Paleontology. Chicago: Univ. of Chicago Press. 304 pp.
Vertebrate animals. Summary of vertebrate paleontology. J. Paleontol., 6(42): 1371-73.
De Evolutie van de gewervelde Dieren. Nat. Tech., 10(36):339-47.
With G. Clarke, G. Mead, and J. Welsh. Henry Bryant Bigelow. Harv. Univ. Gaz., 64:91-92.
Fossils and Gondwanaland. Proc. Am. Philos. Soc., 112:335-43.
1969 A temnospondylous labyrinthodont from the Lower Carboniferous. Kirtlandia, 6:1-20.
Vertebrate paleontology and zoology. Biologist, 51:49-53.
Research and science teaching. Webb School Cal. Alum. Bull. (1968):13-15.
The Brazilian Triassic cynodont reptiles *Belesodon* and *Chiniquodon.* Breviora, 332:1-16.
The cranial anatomy of the Permian amphibian *Pantylus.* Breviora, 314:1-37.
The Chañares (Argentina) Triassic reptile fauna. V. A new chiniquodontid cynodont, *Probelesodon lewisii—cynodont* ancestry. Breviora, 333:1-24.
Cynodont reptile with incipient mammalian jaw articulation. Science, 166:881-82.
Vertebrate history with special reference to factors related to cerebellar evolution. In: *Neurobiology of Cerebellar Evolution and Development,* ed. R. Llinas, pp. 1-18. Chicago: Chicago Medical Association.
1970 A new anthracosaurian labyrinthodont, *Proterogyrinus scheelei,* from the Lower Carboniferous. Kirtlandia, 10:1-15.
The Vertebrate Body. 4th ed. Philadelphia: W. B. Saunders. 601 pp.
The Chañ ares (Argentina) Triassic reptile fauna. VI. A chiniquodontid cynodont with an incipient squamosal-dentary jaw articulation. Breviora, 344:1-18.
The Triassic faunal succession and the Gondwanaland problem. UNESCO Gondwana Stratigraphy IUGS Symposium, Buenos Aires, Oct. 1-15, 1967, pp. 375-400. Paris: UNESCO.
Teaching vertebrate paleontology. In: *North American Paleonto*

logical Convention, Chicago, 1969, Proc. A, pp. 39-45. Lawrence, Kans.: Allen Press.

Entwicklungsgeschichte der Tiere [The Procession of Life], 2 volumes. Lausanne: Editions Rencontre. 630 pp.

Topics in therapsid evolution and classification. Bull. Indian Geol. Assoc., 2:15-26.

Bojanus and the anatomy of the turtle. In: L. H. Bojanus, *Anatome Testudinis Europaeae.* Athens, Ohio: Reprinted by the Society for the Study of Amphibians and Reptiles, pp. iii-v.

L'Évolution Animale [The Procession of Life], 2 volumes. Lausanne: Editions Rencontre. 630 pp.

1971 The Chañares (Argentina) Triassic reptile fauna. VIII. A fragmentary skull of a large thecodont *Luperosuchus fractus.* Breviora, 373:1-8.

The Chañares (Argentina) Triassic reptile fauna. IX. The Chañares Formation. Breviora, 377:1-8.

The Chañares (Argentina) Triassic reptile fauna. X. Two new but incompletely known long-limbed pseudosuchians. Breviora, 378:1-10.

The Chañares (Argentina) Triassic reptile fauna. XI. Two new long-snouted thecodonts, *Chañaresuchus* and *Gualosuchus.* Breviora, 379:1-22.

Letter to Science. Curse of Russia is Intourist. Science, 172:326.

Unorthodoxies in reptilian phylogeny. Evolution, 25:103-12.

The Vertebrate Body. 4th ed., abridged. Philadelphia: W. B. Saunders Co. 452 pp.

La Evolución Animal [The Procession of Life]. Barcelona: Ediciones Destino. 630 pp.

1972 The Chañares (Argentina) Triassic reptile fauna. XII. The postcranial skeleton of the thecodont *Chañaresuchus.* Breviora, 385:1-21.

The Chañares (Argentina) Triassic reptile fauna. XIII. An early ornithosuchid pseudosuchian, *Gracilisuchus stripanicicorum,* gen. et sp. nov. Breviora, 389:1-24.

The Chañares (Argentina) Triassic reptile fauna. XIV. *Lewisuchus* *admixtus,* gen. et sp. nov., a further thecodont from the Chañares beds. Breviora, 390:1-13.

The Chañares (Argentina) Triassic reptile fauna, XV. Further remains of the thecodonts *Lagerpeton* and *Lagosuchus.* Breviora, 394:1-7.

The Chañares (Argentina) Triassic reptile fauna. XVI. Thecodont classification. Breviora, 395:1-24.

The Chañares (Argentina) Triassic reptile fauna. XVII. The Chañares gomphodonts. Breviora, 396:1-9.

Tetrapod vertebrates and Gondwanaland. In: *Second Gondwana Symposium, South Africa, 1970, Proceedings and Papers,* pp. 11124. Pretoria: Council for Scientific and Industrial Research.

South American fossil reptiles as evidence of Gondwanaland. Aust. Nat. Hist., 17:206-12.

Skin breathing-primary or secondary? Respir. Physiol. 14:183-92.

L'Evoluzione Animale [The Procession of Life], 2 vols. Milan: Garzanti Editore, 766 pp.

The Procession of Life. Garden City, N.Y.: Doubleday, Anchor Books. 384 pp.

A Carboniferous labyrinthodont amphibian with complete dermal armor. Kirtlandia, 16:1-8.

The Vertebrate Body. 4th ed. Jerusalem: Israel Program for Scientific Translations. 534 pp. (In Hebrew).

The vertebrate as a dual animal-somatic and visceral. In: *Evolutionary Biology,* ed. by T. Dobzhansky, M. Hecht, and W. Steere, vol. 6, pp. 121-56. New York: Appleton-Century-Crofts.

1973 The Chañfares (Argentina) Triassic reptile fauna. XVIII. *Probelesodon minor,* a new species of carnivorous cynodont: family Probainognathidae nov. Breviora, 401:1-4.

The Chañares (Argentina) Triassic reptile fauna. XIX. Postcranial materials of the cynodonts *Probelesodon* and *Probainognathus.* Breviora, 407:1-26.

The Chañares (Argentina) Triassic reptile fauna. XX. Summary. Breviora, 413:1-20.

Middle Triassic tetrapod faunas of South America. In: *Actas IV Congreso Latinamericano de Zoologia,* vol. 2, pp. 1 101-17. Caracas: Univ. of Central Venezuela.

L'origine des classes de vertébrés. Recherche, 33:347-61.

Permian reptiles. In: *Atlas of Palaeobiogeography,* ed. A Hallam, pp. 159-67. Amsterdam: Elsevier.

With R. O. Greep, H. A. Salhanick, W. H. Weston, and C. M. Williams. Frederick Lee Hisaw: Discerning observer known as "Pappy." Harv. Univ. Gaz., 34(68): 10.

Vertebrates and continental connections: An introduction. In: *Implications of Continental Drift to the Earth Sciences,* ed. D. H. Tarling and S. K. Runcorn, vol. 1, pp. 345-49. London and New York: Academic Press.

The origin and evolution of life in the sea. In: *Oceanography, the Last Frontier,* ed. R. C. Vetter, pp. 250-65. New York: Basic Books.

1974 The stratigraphy of the Permian Wichita red beds of Texas. Breviora, 427:1-31.

Aquatic adaptation in reptiles—primary or secondary? Ann. S. Afr. Mus., 64:221-30.

Thomas Barbour. In: *Dictionary of American Biography,* 4(Suppl.): 51-53.

Simpson, George Gaylord. In: *Encyclopaedia Britannica,* 15th ed., pp. 778-79. Chicago: Encyclopaedia Britannica.

1975 *The Vertebrate Story* (Sinhalese translation). London: M. D. Dunasena and Co. 568 pp.

Intercontinental correlations of Triassic Gondwana vertebrate faunas. In: *Third Gondwana Symposium, Canberra, Australia,* ed. K. S. W. Campbell, Canberra, A.C.T., Australia, August 1973, pp. 469-473. Canberra: Australia National Univ. Press.

John C Slater

John Clarke Slater

December 22, 1900-July 25, 1976

by Philip M. Morse

John C. Slater merits commemoration for several reasons. He contributed significantly to the start of the quantum revolution in physics; he was one of the very few American-trained physicists to do so. He was exceptional in that he persisted in exploring atomic, molecular, and solid state theory, while many of his peers were coerced by war, or tempted by novelty, to divert to nuclear mysteries. Not least, his texts and his lectures contributed materially to the rise of the illustrious American generation of physicists of the 1940's and 1950's.*

Slater's background was academic. His father, born in Virginia, was an undergraduate at Harvard, a doctoral student at the University of Chicago, and head of the English Department at the University of Rochester. John enjoyed literature, history, and music throughout his life, but his youthful preoccupations were with things mechanical, chemical, and electrical. His goal was set when a family helper, a college girl, put a name to his interests—physics.

He took a course in high school physics the next year, which he found pedestrian except for the laboratory. Never-

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* See J. C. Slater, *Solid-State and Molecular Theory: A Scientific Biography* (New York: Wiley-Interscience, 1975) for a more detailed account of Slater's life than is possible in this short memoir.

the less, when he entered the University of Rochester in 1917 he took physics courses as well as a curriculum of chemistry and mathematics. As a senior he assisted in the physics laboratory and did his first independent research for a special honors thesis, a measurement of the dependence on pressure of the intensities of the Balmer lines of hydrogen. The curriculum in those days did not extend to modern physics, but he was referred to Bohr's 1913 paper. From it he was able to devise a bit of theory that partially explained his observations.

His thesis must have had some merit, for it helped to get him into Harvard graduate school, with the choice of a fellowship or assistantship. He chose the assistantship, during which he worked for P. W. Bridgeman, collecting data from equipment designed by Bridgeman. He was happy at Harvard, where he found great intellectual stimulation from his teachers and fellow students. He followed Bridgeman's courses in fundamental physics and launched into the new quantum physics with the courses of E. C. Kemble. He completed the work for the Ph.D. in three years by publishing his (1924)* paper "Compressibility of the Alkali Halides," which embodied the thesis work he had done under Bridgeman.

His longtime friend J. H. Van Vleck remembers those days:

> Neither Slater nor I have ever written any papers concerning the relation of philosophy and physics, but I have the feeling that both of us were influenced by Bridgeman. The essence of his philosophy, which is basically pragmatic, is that research physicists should not be distracted to the realm of metaphysics or politics, but should concentrate on explaining observable facts. In practically all of Slater's papers, except for the experimental ones, the emphasis is on making calculations or developing theories that explain observed phenomena.
> We both had the benefit of what I call "operation head start," inasmuch

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* The dates in parentheses or brackets refer to the bibliography appended.

as in 1920-1921 Kemble gave the most mature and sophisticated course in quantum theory given in the United States. Slater has said that the training at that time at Harvard was fully the equal of that in English and European universities. By the summer of 1922 he was thoroughly indoctrinated in the successes and failures of the then quantum theory. One historian of science has referred to that period as the crisis in quantum theory. Slater and I lived in the same dormitory and he and I had many talks about the crisis. Like most American theoretical physicists of his generation, Slater wrote an experimental thesis. However his real heart was in theory and his first publication was not his doctor's thesis, but a note [1924] to *Nature* on "Radiation and Atoms."

Life for a graduate student at that time was in some ways harder, in some ways easier than now. Hardly any graduate student had a car or a wife. There was practically no secretarial help for anyone except senior faculty. Most of the books and journals were in the central [Widener] library rather than the physics building. To work in the laboratory at night required special permission and I remember Slater telling how he had to use a flashlight to get to the fuse box to make his apparatus functional. The respects in which life was better were that there were maids who cleaned the rooms and made up the beds, and especially that there was a dining hall in Memorial Hall with full waiter service three meals a day. The days that Bridgeman had classes were grim, as he insisted on beginning his lectures at 8:40 so he could lecture twice a week for eighty minutes instead of the customary three fifty-minute sessions. Little did Slater or I realize that the particular table of the group to which we belonged would have so many distinguished alumni—among others the mathematicians Franklin (at MIT), Walsh and Widder (at Harvard), the economists Ellis and Chamberlain, Woodward of the Harvard Music Department, and Paul Buck, Pulitzer Prize winner and longtime provost of Harvard.

After receiving his Ph.D., Slater held a Harvard Sheldon Fellowship for study in Europe. He spent a period in Cambridge, England, before going to Copenhagen. In spite of these influences Slater's ideas were very much his own, what formal training he had was in America. His concept of virtual oscillators germinated while he was still here. When Slater reached Copenhagen he explained to Bohr and Kramers his idea that classical radiation fields guided the light quanta, a sort of forerunner of the duality principle. The result of Slater's conversations was the celebrated paper [1924] on "The Quantum Theory of Radiation" in the *Philosophical Magazine,* listed as by Prof. Bohr, Dr. Kramers, and Mr. Slater (actually John already had his Ph.D.). With the prestige of the senior author and the interesting character of the subject matter, this paper produced quite a splash; Slater suddenly became an internationally known name. Interest in the paper subsided with the arrival of quantum mechanics but in recent years it has been recognized that the correct ideas in the article are those of Slater.

It was natural that the Harvard Physics Department annexed Slater to its staff when he returned to America. Though all papers written by theoretical physicists on the eve of the quantum mechanical breakthrough were speculative in nature, two of Slater's speculations were essentially correct. One [1925] was the correlation of the width of a spectral line with the reciprocal lifetime of a stationary state, a sort of precursor of a 1930 paper of Weisskopf and Wigner that demonstrated this quantum mechanically. The other [1925] was concerned with the interpretation of the spectra of hydrogen and ionized helium. In the early days it was customary to use four quantum numbers for the valence electron in the alkali and but three for the hydrogen and ionized helium atoms. The additional quantum number in the alkalis was ascribed to the interior electron shells, something obviously lacking in hydrogenic atoms, where there is only one electron. Uhlenbeck and Goudsmit, in a note to *Naturwissenschaften,* suggested that there were really four quantum numbers in hydrogen but that a fortuitous degeneracy made things look like there are only three. Independently and practically simultaneously, Slater had the same idea. In a second paper Uhlenbeck and Goudsmit suggested that the fourth quantum number be attributed to electron spin, with a gyromagnetic ratio of twice the classical value. Had Slater terminated his paper with a similar suggestion, which could have been made in a sentence, Slater would have shared their honors with them. Instead of talking about a spin doublet, however, he simply spoke abstractly in terms of the duality of states found in the Pauli exclusion principle-perhaps a reflection of Slater's pragmatic approach. His papers of the period, though theoretical, sometimes did not contain a single equation, a reflection of the fact that quantum mechanics had not then achieved analytic form.

The advent of the true quantum mechanics, with the miraculous near simultaneity of the matrix and wave forms, brought a whole new world to the theoretical physicist. Slater, despite being at somewhat of a geographical disadvantage on account of being in America, rapidly absorbed the content of the new discoveries. His first quantum mechanical paper [1927] was one on "Radiation and Absorption on Schroedinger's Theory." Slater preferred to make most of his analyses by means of the Schroedinger wave equation; for that reason he was able to use a variational procedure

> [1928], essentially equivalent to that of Hyleraas, to show that quantum mechanics gives the proper binding energy for the normal helium atom.
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> In 1929 Slater published what I regard as his greatest paper, "The Theory of Complex Spectra," which he wrote just before going to Europe on a Guggenheim Fellowship. In this he introduced the Slater determinants, now universally named for him. Heisenberg and Dirac had independently shown that the complete wave function, inclusive of spin factors, must be antisymmetric if both spin and orbit are permuted, and Pauli had shown how to interpret spin in terms of a factor in the wave function which had only two values. However no one had proposed constructing in explicit detail a determinant whose individual entries included the Pauli spin factor. In retrospect it seems an obvious thing to do, as the determinantal form automatically insures the proper antisymmetry. The fact is that almost two years elapsed between the appearance of Pauli's paper and Slater's. In this article Slater also introduced the so-called Slater F and G parameters, integrals describing the energies of all the states arising from a given configuration as long as interconfiguration interaction is neglected.
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> In addition to other notable papers which Slater wrote in the late 1920's, on such subjects as Hartree's self-consistent field, the quantum mechanical derivation of the Rydberg formula and the best values of atomic shielding constants, he wrote a seminal paper on directing valence. It was published in 1931 but he had been working and talking about the subject before 1930. The idea of directed valence is that by using proper linear combinations of s and p wave functions one can construct wave functions that project out in particular directions like the horns of a cow. From sp^3 hybridization one can, for example, construct the tetrahedral valence properties of the carbon atom so dear to the organic chemist. Linus Pauling had the same idea about the same time; his paper and Slater's were practically simultaneous.
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> These were some of the achievements that resulted in his being elected to the National Academy at the almost unprecedented age of thirty-one. He played a key role in lifting American theoretical physics to high international standing for the first time since Willard Gibbs.*

The writer of the present memoir first met Slater in the late spring of 1930. The occasion was the installation of Karl

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* J. H. Van Vleck, remarks, Slater Memorial Session, American Physical Society, Chicago, 7 February 1977.

Compton as president of MIT. John had just been appointed the new chairman of the Institute's Physics Department; I had just accepted an offer of an assistant professorship in that department. Slater impressed me. Though there was a difference of only three years in our ages, and though at that time he looked more like a freshman than a department head, there seemed to me a decade's difference between us in regard to knowledge and experience in physics. John had twice spent time in Europe, taking significant part in hammering out the implications of the new quantum mechanics, whereas I was scheduled to make my first pilgrimage to Europe in the next year.

It is hard for this generation to appreciate the feeling of inferiority we then, in this country, felt for pure science in Europe. Europe was where new physics was being made and one's ambition was to finish one's education there. A few active centers were being started in this country: Harvard, Princeton, Caltech and Berkeley, as well as a few others. We were just beginning to catch up. What impressed me most, in my first meeting with Slater, was his determination to recast the physics curriculum at MIT so the young physicist would not need to go abroad to finish his education, though he would be able to go abroad to work with equals. We did not know then how important this goal was to be ten years later.

When I came back from my year abroad, things were already well under way. With Slater's active support, N. H. Frank was busy recasting the freshman-sophomore course in physics that every MIT undergraduate had to take. It was a tough course for the time, using calculus from the start. But it meant that, by the end of the senior year, the physics majors would be at least the equals of most graduate students after a year of graduate study. Slater himself concentrated on the senior course in theoretical physics. His text, written with

Frank, *Introduction to Theoretical Physics* (1933), became a classic. The whole inventory of graduate courses was also reorganized and the system of general examinations was restructured to give the student more freedom for research—and Slater inseminated much of that research. Many outstanding theoretical physicists of the 1940's and 1950's got their start from him in the 1930's.

His most tangibly lasting contribution was his books. Between 1933 and 1968 he wrote fourteen books—on chemical physics, on microwaves, and on the quantum theory of atoms, molecules, and solids—an average of one weighty tome every two-and-a-half years. Even the oldest of these is still worth reading. The style of all of them is businesslike and simple, though the subject matter may be complex. Apparently, to John, writing was as easy as breathing. While most of the rest of us had to struggle through successive drafts, Slater typed out his first draft himself; as far as I know it needed little revision. In his office, in the few pauses between administrative duties, he would turn to his typewriter to complete another page or two that he had been mentally composing earlier. Administration of the department took up a good deal of time, more time than he would have preferred. John was a good department head.

In spite of the administrative load, Slater continued to write books, to teach and supervise student research, and to do his own research. During the 1930's his research interests shifted from atomic structure to molecular and solid state structure, as is illustrated by a sampling of his publications. In 1931 he wrote on directed valence and on the quantum theory of the equation of state, in 1934 there was a paper on energy bands in metals, and in 1936 one on ferromagnetism in nickel. In 1937 he was writing on the structure of alloys and on the superconductive state, in 1938 on the structure of insulating crystals. All this while he was building, from scratch, one of the most prestigious physics departments in the country.

Through most of this decade he looked more like an undergraduate than a department head. Many secretaries made that mistake once; they did not do so a second time. But those of us who knew him in the 1930's remember him as friendly, stimulating, never dictatorial but inspirational in his own unique and quiet way. He was entertaining, too, if he was so minded. Some of us still remember how he could render his guests weak with laughter simply by counting, slowly and solemnly, up to forty in Danish.

In 1940 the beginning of World War II disrupted all research, but gave the newly trained American physicists a chance to show that their education could be put to practical use. Compton, with Vannevar Bush's National Defense Research Committee's financing, set up the Radiation Laboratory at MIT to develop microwave radar. At these high frequencies the electromagnetic fields had more in common with sound waves than with the usual wire-born currents, and those familiar with theoretical physics were better acquainted with their behavior than were the electrical engineers of that era. Slater soon joined the staff of the Radiation Laboratory. To quote from his 1975 autobiography:

> I. I. Rabi, one of the moving spirits of the Radiation Laboratory, asked my help in understanding the theory of the magnetron, the power oscillator whose development by the British had made microwave radar practical, but whose workings were very poorly understood. I told Rabi I also didn't know, but would try to find out.
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> The problem is analogous to that of the self-consistent field in an atom, one in which we study the motion of an electron in the presence of the space charge produced by all the other electrons and of the nuclei (which take the place of the electrodes in the magnetron). This problem is too hard to solve all in one step. One proceeds instead by iteration, assuming a form for the space charge, solving for the motion of an electron in that field,

> studying the space charge produced by all the electrons moving in the way just described and demanding that the final space charge be identical with that originally assumed. I resolved to try to carry through such an iterative calculation for the magnetron and see if the result would include not only a time independent space charge, but more interesting, an oscillating, or rather rotating, space charge which would produce the oscillating or rotating electromagnetic field which corresponds to the radio-frequency output of the magnetron. I set to work and after a few weeks I had a satisfactory answer.
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> But it was necessary to go further. The behavior of a magnetron depended very profoundly on the impedance of the output circuit, and yet this was not an ordinary lumped circuit, but a set of wave guides. I had been dealing with the microwave cavity as a real cavity, rather than as a circuit element, and it was clear that the same thing had to be done for the rest of the microwave circuit.*

Based on this work, Slater was able to write the book *Microwave Transmission,* which was the bible of the radar designers for a long time.

In the fall of 1941, John arranged to transfer his work to the Bell Telephone Laboratories (at that time in Manhattan), where an MIT graduate, Jim Fisk, was doing fundamental work on magnetrons. Slater stayed with Fisk's team throughout the war, doing experimental as well as theoretical work on magnetron design. Mervin Kelley, then head of Bell Labs, has stated that John had done more than any other person to provide the understanding requisite to progress in the microwave field.

As the war came to an end, the problems of reconverting laboratories and people to peacetime pursuits loomed large. Slater shared his time between finishing the tasks at Bell Labs and planning the postwar Physics Department at MIT. As he put it:

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* J. C. Slater, *Solid-State and Molecular heory: A Scientific Biography* (New York: Wiley-Interscience, 1975), p. 212.

> During the war practically every member of the MIT Physics Department had been associated with war work one way or another. As the end of the war approached many of them turned back to Cambridge, as I did, full of new plans for the development of physics at the Institute. The war had brought physics to the attention of the public, of industry and government, as had never happened before. In particular, the fields of electronics, as exemplified in radar, and of nuclear structure, as applied in the atomic bomb, were bound to lead to greatly accelerated research and application and greatly increased numbers of students and opportunities for their employment.
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> Important was the matter of having different fields of physics represented in the department. We had never believed in the extreme concentration in one or two specialities which some departments have chosen. We could afford to diversify, partly on account of the large size of the department made necessary by the large teaching load, and partly because we felt it a duty in a technical institution to carry on work in applications of physics which do not attract interest in an arts college. Our work in electronics, X-rays, optics and acoustics was in each case in a field pursued in only a few institutions. Our department was often looked down on by those who felt that no physicist of any imagination would be in any field except nuclear and high-energy physics. And yet in each of these less popular fields our department was looked up to by the industrial leaders as the best department in the country, and we were constantly urged to turn out more students in each of these fields. After the war I felt firmly that this diversity was a good thing and that we should not alter it.*

As a result the Radiation Laboratory was transformed into a peacetime Research Laboratory of Electronics; a Laboratory of Nuclear Science and Engineering was established and the wartime work in acoustics was transferred to a smaller but active Acoustics Laboratory.

John's own research returned to molecular and solid state theory. As the postwar department began to take form, he was able to spend more time in this field. By 1951 he was ready to take a number of important steps. A year earlier he had organized a small research group, called the Solid State

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* *Ibid.,* p. 217.

and Molecular Theory Group (SSMTG), which was the forerunner of the interdepartmental Center for Materials Science and Engineering, established ten years later. "As he put it, 'Our purpose is to take the quantum theory of atoms, molecules and solids out of the semi-empirical form which it has largely followed since the development of wave mechanics and to proceed as rapidly as possible to put it on a quantitative basis.' "*

Having served as departmental head for twenty-one years, he was ready to give up administration and become an Institute Professor, able to devote his energies to research and teaching. For the year 1951-1952 he decided to accept the invitation of Brookhaven National Laboratory on Long Island, taking some of the SSMTG with him.

As a starting point for the work of the group, Slater wrote two papers, into which he distilled his wide experience with the Hartree self-consistent field method as applied to the structure of atoms, molecules, and solids. The first paper (1951), entitled "A Simplification of the Hartree-Fock Method," suggested a way whereby the complications produced by electron exchange can be somewhat simplified. This later came to be called the Xa method. The relationship between this exchange term and the magnetic properties of the material was the subject of the second (1951) paper, "Magnetic Effects and the Hartree-Fock Method."

At the time these papers were written, the Xa method was far too complicated for one to expect to find solutions by the laborious use of desk calculators, as was done in the 1930's. But John could see that the new digital computers, just beginning to be developed, would eventually be able to carry out the needed calculations in a time short enough to make

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* G. Koster, remarks, Slater Memorial Session, American Physical Society, Chicago, 7 February 1977.

the method practical. Professor George Koster has outlined the history of the work:

> I was with the group from the beginning. From the start we worked closely together, discussing the problems we were working on. Slater's object in this intense communication was in part educational. He wanted to teach us the developments in our field before World War II.
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> The attention from the outset was about equally divided between atoms, molecules, and solids, not only because the mathematical methods are closely related but because the physical problems are practically indistinguishable. Slater felt that a calculation of electronic structure should be based on the Hartree-Fock method. This does not give an exact ground state for a system, but one can improve on the Hartree-Fock wave function by taking a linear combination of the ground state and excited states in the form of Slater determinants and varying the coefficients.
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> George Pratt used an early IBM card programmed calculator for his Xa method on the Cu+ ion. From this small beginning our computer use increased until we had our own IBM 709 computer in the 1960's. The greatest activity on atoms came in the late 1950's and early 1960's, with extensive Hartree-Fock calculations, that were applied to the computation of X-ray form factors, hyperfine interaction constants and other properties of interest.*

Work on molecules did not progress as rapidly because of the inherently greater difficulties in computation. With atoms, the Hartree-Fock-Slater potential is spherically symmetrical, but with molecules the potential involves two or more centers; the interaction integrals between wave functions far exceeded the computational capabilities of the time. With the advent of the Whirlwind computer, Alvin Meckler was able to do a landmark calculation of the states of the 02 molecule from infinite atomic separation to the equilibrium separation. To quote Koster again:

> As our computational prowess increased and our programming of more sophisticated computers became better, we were able to use more realistic molecular wave functions and work was done on diatomic mole

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* *Ibid.*

> cules such as LiH, OH and HF; by the early 1960's work was done on H_2O, NH_3, CH_4 and others. It was in the field of solids that the greatest activity was centered, but the overall approach was to be the same, though the approach is more difficult. First, it is well-nigh impossible to obtain a completely self-consistent field, both in the region near a nucleus and in the nearly field-free region between nuclei. Second, the wave function has a quite different form in the two regions. The first attempts neglected self-consistency; they gave satisfactory results for energy bands, but were not adequate in other respects. By about 1965 the method was made self-consistent. It was one of the outstanding accomplishments of the group.
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> During the fifteen-year life of the group some sixty persons were members and thirty-four took doctoral degrees with theses connected with its work. In my report I have been unable to separate the work of Slater from that of the group as a whole. He was part of every aspect of the group's research efforts. But one contribution was solely his. During this period he wrote five books on the quantum theory of atoms, molecules and solids, which have become standard references in the field.*

In 1964 John and his ninety-two-year-old father were both awarded honorary degrees by the University of Rochester.

By 1965 Slater had reached retirement age at MIT. He was offered and accepted a position of research professor at the University of Florida, where the retirement age is seventy. He joined the Quantum Theory Project there, which had been set up by Professor Per-Olov Löwdin, who had been an occasional member of the SSMTG during the late 1950's. As Slater has written: "The Florida Physics Department was a congenial one, with main emphasis on solid state physics, statistical physics and related fields. It reminded me of the MIT department in the days when I had been department head there. It was a far cry from the MIT Physics Department which I was leaving; by then it had been literally captured by the nuclear theorists."†

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* *Ibid.*

† J. C. Slater, *Solid-State and Molecular Theory: A Scientific Biography,* p. 275.

In the friendly and relaxing atmosphere of Gainesville he was able to carry forward his objective of understanding the mechanical, electromagnetic, and chemical properties of matter. Back in the 1920's he had read the prophesy of P. A. M. Dirac, "quantum mechanics can explain most of the phenomena of physics and all of the phenomena of chemistry," and, alone of all the theory's pioneers, he continued to demonstrate its validity in ever greater detail and accuracy. Together with Löwdin and an active group of graduate students and postdoctoral assistants, he extended and perfected the methods he devised in the 1950's and 1960's, using more and more powerful computer programs.

He and his colleagues calculated the compressibilities of various solids (thus coming full circle from his doctoral thesis), the magnetic properties of ferromagnetic and antiferromagnetic materials, the binding energies and magnetic properties of various polyatomic molecules, X-ray absorption in molecules and solids, the relationship between the Xa method and the virial theorem and the use of the Xca method in understanding the catalytic process. He continued to publish books and important papers right up to his death.

Slater married Helen Frankenfeld in 1926. They had three children: Louise Chapin, John Frederick, and Clarke Rothwell. All of them are following academic careers. John was divorced and in 1954 he married Dr. Rose Mooney, a physicist, who moved to Florida with him.

Bibliography

1924 Radiation and atoms. Nature, 113:307-8.
Compressibility of the alkali halides. Phys. Rev., 23:488-500.
With N. Bohr and H. A. Kramers. The quantum theory of radiation. Philos. Mag., 47:785-802.
With N. Bohr and H. A. Kramers. Uber die Quantentheorie der Strahlung. Z. Physik, 24:69-87.
1925 The nature of radiation. Nature, 116:278.
A quantum theory of optical phenomena. Phys. Rev., 25:395-428.
Methods for determining transition probabilities from line absorption. Phys. Rev., 25:783-90.
With G. R. Harrison. Line breadths and absorption probabilities in sodium vapor. Phys. Rev., 26:176-88.
Physically degenerate systems and quantum dynamics. Phys. Rev., 26:419-30.
Interpretation of the hydrogen and helium spectra. Proc. Natl. Acad. Sci. USA, 11:732-38.
1926 Alternating intensities in band lines. Nature, 117:278.
Spinning electrons and the structure of spectra. Nature, 117:587.
A dynamical model for complex atoms. Phys. Rev., 28:291-317.
Measurement of the compressibility of the alkali halides. Proc. Am. Acad. Arts Sci., 61:135-50.
1927 Radiation and absorption on Schroedinger's theory. Proc. Natl. Acad. Sci. USA, 13:7-12.
Action of radiation and perturbations on atoms. Proc. Natl. Acad. Sci. USA, 13:104-11.
The structure of the He atom. 1. Proc. Natl. Acad. Sci. USA, 13: 423-30.
1928 Central fields and Rydberg formulas in wave mechanics. Phys. Rev., 31:333-43.

Light quanta and wave mechanics. Phys. Rev., 31:895-99.
The self-consistent field and the structure of atoms. Phys. Rev., 32:339-48.
The normal state of He. Phys. Rev., 32:349-60.
1929 Physical meaning of wave mechanics. J. Franklin Inst., 207:449-55.
The theory of complex spectra. Phys. Rev., 34:1293-323.
1930 Note on Hartree's method. Phys. Rev., 35:210-11.
Cohesion in monovalent metals. Phys. Rev., 35:509-29.
Atomic shielding constants. Phys. Rev., 36:57-64.
1931 Directed valence in polyatomic molecules. Phys. Rev., 37:481-89.
With J. G. Kirkwood. Van der Waals forces in gases. Phys. Rev., 37:682-97.
The quantum theory of the equation of state. Phys. Rev., 38: 237-42.
Note on the structure of the group XO_3. Phys. Rev., 38:325-29.
Molecular energy levels and valence bonds. Phys. Rev., 38:1109-44.
1932 Note on molecular structure. Phys. Rev., 41:255-57.
Analytic atomic wave functions. Phys. Rev., 42:33-43.
1933 The virial and molecular structure. J. Chem. Phys., 1:687-91.
The electron theory of metallic conduction. Science, 77:595-97.
With N. H. Frank. *Introduction to Theoretical Physics.* New York: McGraw-Hill.
1934 Electronic energy bands in metals. Phys. Rev., 45:794-801.
The electronic structure of metals. Rev. Mod. Phys., 6:209-80.
1935 With H. M. Krutter. The Thomas-Fermi method for metals. Phys. Rev., 47:559-68.

1936 The ferromagnetism of Ni. Phys. Rev., 49:537-45.
The ferromagnetism of Ni. II. Temperature effects. Phys. Rev., 49:931-37.
With E. Rudberg. Theory of inelastic scattering of electrons from solids. Phys. Rev., 50:150-58.
With W. Shockley. Optical absorption by the alkali halides. Phys. Rev., 50:705-19.
1937 Electronic structure of alloys. J. Appl. Phys., 8:385-90.
The nature of the superconducting state. Phys. Rev., 51:195-202.
Damped electron waves in crystals. Phys. Rev., 51:840-46.
Wave functions in a periodic potential. Phys. Rev., 51:846-51.
Theory of ferromagnetism: Lowest energy levels. Phys. Rev., 52:198-214.
The nature of the superconducting state. II. Phys. Rev., 52:214-22.
Charles Elwood Mendenhall. Proc. Am. Acad. Arts Sci., 71:529.
1938 Electrodynamics of ponderable bodies. J. Franklin Inst., 225: 277-87.
Excited energy levels of insulating crystals. Trans. Faraday Soc., 34:828-32.
1939 *Introduction to Chemical Physics.* New York: McGraw-Hill.
1940 Note on Gruneisen's constant for the incompressible metals. Phys. Rev., 57:744-46.
Note on the effect of pressure on the Curie point of iron-nickel alloys. Phys. Rev., 58:54-56.
1941 Theory of the transition in KH_2PO_4. J. Chem. Phys., 9:16-33.
1942 *Microwave Transmission.* New York: McGraw-Hill. (Reissued, New York: Dover Publications, 1959.)

1946 Physics and the wave equation. Bull. Am. Math. Soc., 52:392-400.

With F. Bitter, J. B. Garrison, J. Halpern, E. Maxwell, and C. F. Squire. Superconductivity of lead at 3 cm wave length. Phys. Rev., 70:97-98.

Microwave electronics. Rev. Mod. Phys., 18:441-512.

1947 With N. H. Frank. *Mechanics.* New York: McGraw-Hill.

With N. H. Frank. *Electromagnetism.* New York: McGraw-Hill.

1948 The design of linear accelerators. Rev. Mod. Phys., 20:473-518.

1949 With E. Maxwell and P. M. Marcus. Surface impedance of normal and superconductors at 24,000 megacycles per second. Phys. Rev., 76:1332-47.

Electrons in perturbed periodic lattices. Phys. Rev., 76:1592-1601.

The physics of metals. Phys. Today, 2:6-13.

The MIT International Conference on the Physics of Very Low Temperatures, Sept. 6-10, 1949. Science, 110:465-67.

1950 Structure and polarization of atoms and molecules. Electr. Eng., 69:855-58.

The Lorentz correction in barium titanate. Phys. Rev., 78:748-61.

Microwave Electronics. Princeton, N.J.: D. Van Nostrand.

1951 The electron theory of solids. Am. J. Phys., 19:368-74.

The effects of radiation on materials. J. Appl. Phys., 22:237-56.

Note on orthogonal atomic orbitals. J. Chem. Phys., 19:220-23.

A simplification of the Hartree-Fock method. Phys. Rev., 81: 385-90.

Magnetic effects and the Hartree-Fock equation. Phys. Rev., 82: 538-41.

Note on superlattices and Brillouin zones. Phys. Rev., 84:179-81.

Effect of chemical combination on the internal conversion in technetium. Phys. Rev., 84:1261-62.
Quantum Theory of Matter. New York: McGraw-Hill.
Determinantal wave functions and magnetic problems. In: *Conference on Quantum-Mechanical Methods in Valence Theory,* pp. 121-26. Washington, D.C.: Office of Naval Research.
Remarks on self-consistent molecular orbitals. In: *Conference on Quantum-Mechanical Methods in Valence Theory,* p. 185. Washington, D.C.: Office of Naval Research.
1952 With J. C. Simons. Electromagnetic resonant behavior of a confocal spheroidal cavity system in the microwave region. J. Appl. Phys., 23:29.
With P. T. Demos and A. F. Kip. The MIT Linear Electron Accelerator. J. Appl. Phys., 23:53.
With J. R. Terrall. Particle dynamics in the linear accelerator. J. Appl. Phys., 23:66.
With L. C. Maier, Jr. Field strength measurements in resonant cavities. J. Appl. Phys., 23:68.
With L. C. Maier, Jr. Determination of field strength in a linear accelerator cavity. J. Appl. Phys., 23:78.
Presentation of Bingham Medal to P. W. Bridgman. J. Colloid Sci., 7:199-202.
High energy accelerators (linear). Annu. Rev. Nucl. Sci., 1:199-206.
A soluble problem in energy bands. Phys. Rev., 87:807-35.
The solid state. Phys. Today, 5:10-15.
1953 A generalized self-consistent field method. Phys. Rev., 91:528-30.
With H. Statz and G. F. Koster. A two-electron example of ferromagnetism. Phys. Rev., 91:1323-41.
An augmented plane-wave method for the periodic potential problem. Phys. Rev., 92:603-8.
With M. M. Saffren. An augmented plane-wave method for the periodic potential problem. II. Phys. Rev., 92:1126-28.
Ferromagnetism and the band theory. Rev. Mod. Phys., 25:199-210.

Work on molecular theory in the Solid State and Molecular Theory Group, MIT. Symposium on Molecular Physics, Nikko, pp. 1-4.

Work on molecular theory in the Solid State and Molecular Theory Group at MIT. Proc. Int. Conf. on Theoretical Physics, Kyoto and Tokyo, pp. 611-21.

Problem of ferromagnetism. Proc. Int. Conf. on Theoretical Physics, Kyoto and Tokyo, pp. 679-93.

Electronic structure of atoms and molecules. Solid State Mol. Theory Group Mass. Inst. Technol. Tech. Rep., 3 (February).

Electronic structure of solids. I. The energy band method. Solid State Mol. Theory Group Mass. Inst. Technol. Tech. Rep., 4 (July).

Electronic structure of solids. II. The perturbed periodic lattice. Solid State Mol. Theory Group Mass. Inst. Technol. Tech. Rep., 5 (December).

1954 With G. F. Koster. Wave functions for impurity levels. Phys. Rev., 94:1392.

With G. F. Koster. Simplified LCAO method for the periodic potential problem. Phys. Rev., 94:1498-524.

With G. F. Koster. Wave functions for impurity levels. Phys. Rev., 95:1167-76.

With G. F. Koster. Simplified impurity calculation. Phys. Rev., 96: 1208-23.

Electronic structure of solids. III. Configuration interaction in solids. Solid State Mol. Theory Group Mass. Inst. Technol. Tech. Rep., 6 (April).

1955 One-electron energies of atoms, molecules, and solids. Phys. Rev., 98:1039-45.

Modern Physics. New York: McGraw-Hill.

1956 Barrier theory of the photoconductivity of lead sulfide. Phys. Rev., 103:1631-44.

The electronic structure of solids. In: *Encyclopedia of Physics,* vol. 19, pp. 1-136. Berlin: Springer-Verlag.

Band theory of bonding in metals. In: *Theory of Alloy Phases, pp.* 1-12. Cleveland, Ohio: Am. Soc. Metals.

With G. W. Mahlman and W. B. Nottingham. Photoconductivity of PbS. In: *Photoconductivity Conference,* pp. 489-508. New York: John Wiley & Sons.

1958 Interaction of waves in crystals. Rev. Mod. Phys., 30:197-222.

1959 Note on the interatomic spacings in the ions I3-, FHF-. Acta Crystallogr., 12:197-200.

Band theory. Phys. Chem. Solids, 8:21-25.

1960 *Quantum Theory of Atomic Structure,* vols. 1 and 2. New York: McGraw-Hill.

1962 With G. F. Koster and J. H. Wood. Symmetry and free electron properties of the gallium energy bands. Phys. Rev., 126: 1307-17.

1963 The electronic structure of atoms: The Hartree-Fock method and correlations. Rev. Mod. Phys., 35:484-87.

Electronic-Structure of Molecules, Quantum Theory of Molecules and Solids, vol. 1. New York: McGraw-Hill.

1964 Energy band calculations by the augmented plane wave method. Adv. Quantum Chem., 1:35-58.

Atomic radii in crystal. J. Chem. Phys., 41:3199-204.

Robert Mulliken of Newburyport. In: *Molecular Orbitals in Chemistry, Physics, and Biology,* pp. 17-20. New York: Academic Press.

1965 Molecular orbital and Heitler-London methods. J. Chem. Phys., 43:S1-S17.

Suggestions from solid state theory regarding molecular calculations. J. Chem. Phys., 43:8228.
Space groups and wave-function symmetry in crystals. Rev. Mod. Phys., 37:68-83.
P. W. Bridgman and high pressure physics. Science, 148:805.
Symmetry and Energy Bands in Crystals, Quantum Theory of Molecules and Solids, vol. 2. New York: McGraw-Hill.
1966 Green's function method in the energy-band problem. Phys. Rev., 145:599-602.
1967 The current state of solid state and molecular theory. Int. J. Quantum Chem., 1S:37-102.
Quantum physics in America between the wars. Int. J. Quantum Chem., 1S: 1-23.
Energy bands and Fermi surfaces. Int. J. Quantum Chem., 1S:523-31.
Correlation problems in solids. Int. J. Quantum Chem., lS:783-89.
Insulators, Semiconductors, and Metals, Quantum Theory of Molecules and Solids, vol. 3. New York: McGraw-Hill.
Introduction to the theory of ferroelectricity. In: *Ferroelectricity,* pp. 1-8. Amsterdam, London, and New York: Elsevier.
1968 Energy-band theory of magnetism. J. Appl. Phys., 39:761-67.
Average energy of states of given multiplicities in atoms. Phys. Rev., 165:655-58.
Exchange in spin-polarized energy bands. Phys. Rev., 165:658-69.
Quantum physics in America between the wars. Phys. Today, 21:43-51.
Energy bands in solids. Phys. Today, 21:61-71.
Quantum Theory of Matter, 2d ed. New York: McGraw-Hill.
Review of the energy band problem with recent results. In: *Energy Bands in Metals and Alloys,* ed. L. H. Bennett and J. T. Waber, pp. 1-17. New York: Gordon and Breach.
Energy bands and the theory of solids. In: *Energy Bands of Solids, Methods in Computational Physics,* vol. 8, ed. B. Alder, S. Fernbach, and M. Rotenberg, pp. 1-20. New York: Academic Press.

1969 The graduate student: Why has he changed? Phys. Today, 22(3): 35-37.

With T. M. Wilson and J. H. Wood. A comparison of several exchange potentials for electrons in the Cu+ ion. Phys. Rev., 179:28-38.

With J. B. Mann, T. M. Wilson, and J. H. Wood. Non-integral occupation numbers in transition atoms. Phys. Rev., 184:672-94.

1970 The self-consistent field for crystals. Int. J. Quantum Chem., 3S: 727-46.

Note on the space part of antisymmetric wave functions in the many-electron problem. Int. J. Quantum Chem., 4:561.

Studies of the statistical exchange approximation in the first transition row atoms and ions: the Mn $+^2$ ion. Los Alamos Scientific Lab Univ. of California, LA-DC-11025. Also: Univ. of Florida Quantum Theory Project Tech. Rep. no. 184.

Present status of the Xa statistical exchange. Solid State Mol. Theory Group Mass. Inst. Technol. Prog. Rep., 71 (July).

1971 With J. H. Wood. Statistical exchange and the total energy of a crystal. Int. J. Quantum Chem., 4S:3-34.

The self-consistent field method for crystals. In: *Computational Methods in Band Theory,* ed. P. M. Marcus, J. F. Janak, and A. R. Williams, pp. 447-57. New York: Plenum.

Treatment of exchange in atomic, molecular and solid state theory. Int. J. Quantum Chem., 5S:403.

Transition probabilities and fractional occupation numbers in atoms. In: *Topics in Modern Physics,* ed. W. E. Brittin and H. Odabasi, pp. 297-303. Boulder: Colorado Associated Univ. Press.

1972 With K. H. Johnson. Self-consistent-field Xa cluster method for polyatomic molecules and solids. Phys. Rev., B5:844.

New methods in the theory of molecules and solids. J. Phys. (Paris), 33(C3): 1-6.

Statistical exchange and the Heisenberg exchange integral. J. Phys. (Paris), 33(C3):7-11.

The Hellman-Feynman and virial theorems in the Xa method. J. Chem. Phys., 57:2389.

Statistical exchange-correlation in the self-consistent field. Adv. Quantum Chem., 6:1-91.

1973 With T. M. Hattox, J. B. Conklin, Jr., and S. B. Trickey. Calculation of the magnetization and total energy of vanadium as a function of lattice parameter. J. Phys. Chem. Solids, 34:1627-38.

The development of quantum mechanics in the period 1924-1926. In: *Wave Mechanics, the First Fifty Years,* ed. W. C. Price, S. S. Chissick, and T. Ravensdale, pp. 19-25. London: Butterworths.

Future prospects for the Xa method. Int. J. Quantum Chem., 7S:533.

1974 *The Self-Consistent Field for Molecules and Solids, Quantum Theory of Molecules and Solids,* vol. 4. New York: McGraw-Hill.

The history of the Xa method. In: *The World of Quantum Chemistry,* Proceedings of the 1st International Congress of Quantum Chemistry, Dordrecht, Netherlands: Reidel.

With Keith H. Johnson. Quantum chemistry and catalysis. Phys. Today, 27(10):34.

Recent improvements in the muffin-tin method. Int. J. Quantum Chem., 8S:81.

1975 *Solid-State and Molecular Theory: A Scientific Biography.* New York: Wiley-Interscience.

Comparison of TFD and X methods for molecules and solids. Int. J. Quantum Chem., 9S:7-21.

1976 With J. W. D. Connolly. Remarks on the overlapping-sphere method for molecular orbitals. Int. J. Quantum Chem., 10S: 141-46.

Power series methods for cellular calculations on atoms, molecules, and solids. In: *Quantum Science,* ed. J. L. Calais, O. Goscinski, J. Linderberg, and Y. Ohrn, pp. 57-93. New York: Plenum Publishing.

S. Timoshenko

Stephen P. Timoshenko

December 23, 1878-May 29, 1972

by C. Richard Soderberg

The major factor of the life of Stephen P. Timoshenko are by now well known. He was born as Stepen Prokofyevich Timoshenko* in the village of Shpotovka in the Ukraine on December 23, 1878. Stephen's father, born a serf, had been brought up in the home of a landowner, who later married Stephen's aunt. His father subsequently received an education as a land surveyor and practiced this profession until he himself became a landowner of some means.

Timoshenko's early life seems to have been a happy one, in pleasant rural surroundings. The concluding decades of the nineteenth century were a period of relative tranquility in Russia, and the educational ideals of the middle class were not much different from, and certainly not inferior to, those of their counterparts in Western Europe. He concluded his secondary education with a gold medal at the *technical realschulet*† in Romny, near Kiev. His father had rented an

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NOTE: The Academy would like to express its gratitude to Dr. J. P. Den Hartog for his help in the preparation of this memoir after the death of C. Richard Soderberg in 1979.

* The spelling of Russian names and terms follows that of E. H. Mansfield and D. H. Young, "Stephen P. Timoshenko," in: *Biographical Memoirs of Fellows of the Royal Society,* vol. 19 (London: The Royal Society, 1973), pp. 679-94.

apartment there in which young Stephen and a friend lived, together with Stephen's grandmother. His outstanding subject appears to have been mathematics. His enjoyment in helping classmates with their studies anticipated his subsequent desire to become a teacher. Timoshenko had already developed an intuition for good teaching, but his early ambition was to become a railway engineer. His language studies were less successful, and his Russian had a strong Ukrainian accent, but he appears to have been well read in the Russian classics.

One of the principal objectives of the *technical realschule* was to prepare for the entrance examinations to institutions of higher learning. In 1896 Timoshenko took the examination to enter the Institute of Engineers of Ways of Communication at St. Petersburg, which he seems to have passed with honor.* The trip to St. Petersburg, on which he was accompanied by his mother, opened his eyes to the outside world and began his indefatigable habit of traveling and visiting cathedrals, harbors, bridges, and the like, which was to continue to the end of his life.

His five years (1896-1901) at the Institute of Ways of Communication were spent in intensive and single-minded studies in the sequence of subjects in engineering. But this period also marked the beginning of the end of the years of political tranquility in Russia, and soon there were many incidents of student unrest. Timoshenko always took the liberal view, but one gets the impression that he also regarded these incidents as obstacles to his own professional development.

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† Professor Tichvinsky informs me that this designation was already used in Russia to indicate preparation for entry into technical universities. In most of Western Europe distinction was made between the science-oriented *realschule* and the humanities-oriented *latinschule.* The *technical realschule,* which came later, was a "normal" school where science received greater emphasis than in a gymnasium.

* To play safe he also applied to the Institute of Civil Engineers, which gave easier examinations. The admission rate was twenty to thirty students from among one thousand who had taken entrance examinations, and only good students dared to take these exams.

He graduated in 1901; before this he had made two trips to Western Europe during vacation periods. These trips stimulated him greatly; they were the beginning of close associations with outstanding professionals, particularly in Germany.

His military service, which started in September 1901, after his second trip abroad, seems to have been a much more constructive experience than that in corresponding systems in most European nations at the time. He did not have to drop his professional contacts completely, and he actually started his career as a teacher at the Institute during this period.

Following his military service, in 1902, he married Alexandra Archangelskaya, a student in medical school and an acquaintance from his student days. At that time, he was serving in the Mechanics Laboratory of the Ways of Communication Institute, where, in addition to his duties in testing of materials, he also participated in supplementary lectures in mathematics given by Professors Stanevich and Bobylev, among others.

This noncompulsory program of studies seems to have been important: it brought him into contact with several young physicists, and he also began to attend the sessions of the Physical Society. It made him aware of an issue in engineering education that has remained important ever since. The engineers needed a much more mature background in science, particularly in mathematics, but the professional mathematicians of the period pursued very abstract lines of thought that often failed to attract the interest of the engineering students. The physicist Aleksey N. Krylov appears to have been one of the first to clarify this situation for Timoshenko. Later in his life, and at a more advanced stage, Felix Klein inspired him the same way. Timoshenko, meanwhile, developed his own utilitarian attitude toward mathematics.

In 1903 Timoshenko was made an instructor in the newly organized St. Petersburg Polytechnic Institute; the following years at this Institute mark the beginning of his creative scientific work. He spent the summers in Europe, mostly in Germany, where he received important inspiration from August Föppl in Munich, Ludwig Prandtl and Felix Klein at Göttingen, and others. In his autobiography,* Timoshenko occasionally criticized some of the lecture courses at the Institute in St. Petersburg, but he mentions several of the outstanding teachers, such as Prince Gagarin, the Director of the Institute, who gave English lessons using results in Love's Theory of Elasticity as exercises, and Viktor Kirpichev, who induced him to read C. Lamé, Bernhard Riemann, F. Grashof, and, perhaps most important, Lord Rayleigh's "Theory of Sound." Kirpichev's influence on Timoshenko was very important; through him he was introduced to the Castigliano theorem and the Rayleigh-Ritz method. These influences finally induced Timoshenko to become a teacher rather than a practicing engineer.

The school year 1904-1905 was much influenced by political turmoil in Russia-the disastrous Japanese War increased student demonstrations and general unrest. The Polytechnic Institute in St. Petersburg was closed, and Timoshenko decided to use the time studying under Prandtl at Göttingen. Prandtl, already a professor at twenty-nine, had contributed to Timoshenko's main subject of interest at the

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* Stephen P. Timoshenko, *As I Remember* (New York: Van Nostrand, 1968). The original of this autobiography was written in Russian after Timoshenko's trip to Russia in 1958. It was published in Paris in 1963. The foreword to the Russian edition was written b) Eugene A. Velchorine, chairman of the Association of Graduates of St. Petersburg Polytechnic Institute. 'he translation into English was by Robert Addis under the guidance of Professors J. M. (ere and I). H. Young of Stanford University. The volume contains a complete listing of linoshenkos publications as of March 1967. It also contains a list of limoshenko's doctoral students in the United States.

time: the buckling of beams. Here Timoshenko made his first creative discovery in connection with the buckling of I-beams, where the torsion of rectangular elements of the section had to be taken into account. But by this time, Prandtl had left the field to concentrate on his epoch-making work in connection with boundary layers in fluid flow. It is remarkable, and to some degree characteristic of Timoshenko's single-minded devotion to his own studies, that he makes only a passing reference to this important event.

The stay in Göttingen was important in many other respects. Felix Klein had succeeded in expressing his conviction of the necessity for strong links between abstract and applied sciences. The School of Philosophy at Göttingen had already established an Institute of Applied Mathematics (Carl Runge), Applied Mechanics (Prandtl), and Electrical Engineering (Simon). The impressions from these developments patterned his attitude toward education in technology and contributed much to his future development as a teacher.

The situation in Russia continued to be characterized by much political unrest, and in the summer of 1906 Timoshenko resumed his studies at Göttingen, extending them to potential theory, thermodynamics, and other areas, while continuing his work on elastic stability and buckling. In the fall of 1906 he was appointed to the Chair of Strength of Materials at the Polytechnic Institute in Kiev. The return to his native Ukraine turned out to be an important part of his career and also influenced his future personal life. He was elected dean of the Division of Structural Engineering in 1909; he never ceased to regret the inevitable interference with his own work that this position brought. Political unrest again began to be felt, however, and his ideas of academic freedom now came under scrutiny. In 1911 this conflict led to his dismissal from the school, together with two other professors. Ten professors then resigned their positions in the spirit of solidarity, so strong were their beliefs in the right of the cause.

These difficulties notwithstanding, his scientific work matured, particularly the ideas of strain energy and the Rayleigh-Ritz method in their application to buckling. In his own lectures, begun in 1907, he had gradually developed the technique of beginning a course with the simplest and most elementary concepts, gradually leading to more complicated and sophisticated methods of analysis. His first textbook appeared in 1911, a year that marked the beginning of a period of more than ten years of uncertainty, anxiety, and hardship. One bright event was his winning the Jourowski Medal in 1911, along with 2,500 gold rubles.*

In the fall of 1911, he went to St. Petersburg and succeeded in getting part-time teaching work, meanwhile continuing his writing. During the summer of 1912, he and his wife decided to use the gold rubles from the Jourowski Prize for a trip abroad. This journey was extended to England, where Timoshenko attended a mathematical congress in Cambridge. He met, for the first time, Lord Rayleigh, A. E. H. Love, Horace Lamb, and Levi Civita, among others. One of the lectures at the congress was given by the young representative from Göttingen, Theodor von Kármán. Timoshenko found himself hampered by lack of fluency in English, a lack he determined to remedy as soon as possible. He did not have a scientific discussion with Lord Rayleigh, chancellor of the University, except as part of the crowd at a reception in the University museum.

Returning to Russia in the fall of 1912, he resumed parttime teaching in several schools in St. Petersburg. He was also invited to teach at the Ways of Communication Institute,

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* This illustrates again the courage and solidarity of the Russian men of science, who awarded their top prize to a man dismissed from his job.

where he took over some of the work in theoretical mechanics from Professor Krylov, who had retired. But events overtook him before he really got started on this program; he did not resume this work until he had emigrated to the United States. During this period he also worked on applications of elastic stability to ship bulkheads as a consultant to the Naval Ministry in St. Petersburg. These activities improved his financial position, but the living conditions in St. Petersburg were difficult, and his health was impaired. During this period he happened to meet Paul Ehrenfest, with whom he formed a lasting friendship. Ehrenfest had come from Germany to Russia in the hope that academic freedom there would offer him better opportunities. He was not successful in establishing himself in Russia, but the two met frequently; Ehrenfest would then enlarge upon the current ideas abroad in physics—relativity and quantum physics—new to Timoshenko at the time.

Timoshenko's period of disgrace with the authorities ended in 1913, when he was confirmed in the position of professor at the Ways of Communication Institute, an appointment later extended to include teaching at the Electrical Engineering Institute of the Polytechnic Institute. In the autumn of 1913 he was asked to reorganize the teaching of strength of materials, succeeding Professor Mitinsky. By the summer of 1914, he considered this task completed and went off with his family on a well-earned vacation at Khapsalw on the Baltic. There he completed the proofreading of *Theory of Elasticity* and also continued his work on elastic stability related to ship structures. This work constitutes the forerunner of his books *Theory of Elasticity, Elastic Stability,* and *Plates and Shells,* published many years later in English. It was during this short vacation that World War I began, and with it the beginning of the end of the old world order.

During the early war years, Timoshenko continued and intensified his consulting work, now directed toward aspects of the war effort—shipbuilding problems for the navy and railroad transportation—along with service to many military committees on technical matters. With the gradual disintegration of the Russian social structure, living conditions worsened. In the spring of the year of the Revolution (1917), he sent his family to the Crimea, later joining them. But even there the events of the Revolution reached them. Subsequently, he left his three children with his family in Kiev while his wife accompanied him to St. Petersburg. But she soon returned to Kiev to rejoin her family. During the Christmas holidays of 1917, he made a trip to Kiev to visit them; this turned out to be his final departure from St. Petersburg.

Kiev was held by the Communists for a short while, but in March 1918 the German army took possession. Things improved under German discipline, and Timoshenko was asked to resume his professorship at the Kiev Polytechnic Institute and to participate in the organization of the Ukrainian Academy of Sciences. Before long, German society itself began to disintegrate, and in 1919 there were rumors of the White Army advancing from the south. It arrived in Kiev at the end of August, and Timoshenko visited Rostov to negotiate further on matters of professional education with the leadership of the White Army. But it soon became apparent that this government lacked the necessary strength, and there was a great deal of mutual suspicion among Timoshenko's colleagues about the old issue of a separate Ukrainian state. During this period Timoshenko was in frequent contact with many of his former students and colleagues, all despairing about the possibility of a return to order in Russia. This led to his decision to leave Russia for good.

Through his contacts in the Ukraine he was encouraged to flee to Yugoslavia, where there seemed to be possibilities for a position. After many adventures he found himself pro fessor at the newly established School of Engineering in Zagreb; eventually he also succeeded in bringing his family there from Kiev. The years in Zagreb he regarded as pleasant in many ways, even though there were perpetual difficulties due to material shortages of all kinds. The stay in Zagreb from 1920 to 1922 also included visits to Western Europe and England, where he renewed his acquaintances with Love, R. V. Southwell, G. I. Taylor, and others. He also became acquainted with Piotr Kapitza during one of his trips to England. On his return to Western Europe he visited Weimar to see his friend Ehrenfest, who now held the Chair of Physics at Levden.

Timoshenko's work in Zagreb required that he lecture in Croatian. His assistants translated his Russian lectures into Croatian, and he started the series by reading them in Croatian. In this process the Timoshenko touch was lost. In the end he decided to deliver his lectures in Russian, using as many Croatian words as possible. Eventually his students could follow him without difficulty. During this period he also found it desirable to study more English. He and his English teacher started the task of translating some of his papers into English and sending them to Professor Love, who had them published in England. Through this process the name of Timoshenko began to be known to workers in applied mechanics. To those of us who heard him lecture in English soon after his arrival in the United States, it became apparent that the Russian-Croatian combination was merely one example of his utilitarian approach to language.

His career in Zagreb came to a sudden end in 1922, when he received "a letter from America from a pupil of mine at the Petersburg Polytechnic, one Zelov," * who was then work

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* Viktor Zelov, whose original Russian name was Tselovalnikov, subsequently became a well-known industrialist in the United States and was founder and president of the Viz Manufacturing Company in Germantown, Pennsylvania.

ing with the Vibration Specialty Company, whose president, Akimoff, was familiar with Timoshenko's work. Timoshenko was offered a position with this company and arrived in Philadelphia alone in June 1922. America depressed and frightened him. His work at the Vibration Specialty Company, although well paid, lacked focus, and his future in the new world did not appear bright. After considerable hesitation he decided to stay, however, and in the fall of 1922 he sent for his wife and youngest child—leaving the other two children in Germany. He wanted them to get a good education, and by this time he knew "that there were no good engineering schools in America."

From the vantage point in Philadelphia, Timoshenko began looking for other jobs, naturally beginning with the well-known engineering schools. He records that he received no reply from any one of these. Eventually, one of his letters reached the engineering group at the Westinghouse Electric Corporation in East Pittsburgh. He was identified by L. S. Jacobsen as the author of many interesting papers in applied mechanics, and it became this writer's pleasure to make the first contact with him. Soon after, Timoshenko received a formal offer to join the Westinghouse Research Laboratory. The details and background of this introduction of Timoshenko into American professional life will be dealt with in the following section.

By 1927 Timoshenko had become well known in the United States; in that year he joined the faculty at the University of Michigan at Ann Arbor. His position was professor of graduate mechanics, and he soon had a large following. The years of his widening influence in applied mechanics had begun. He soon had as many doctoral students as he could handle.

At Michigan, he also had his first opportunity to realize his dream of joining applied and abstract sciences. One of his undertakings was a weekly seminar, in which he could bring together representatives from both camps. This led to a special Summer School of Applied Mechanics; distinguished teachers from universities in the United States and abroad were invited to attend, as well as representatives from industry. In this manner men such as Ludwig Prandtl, Theodore von Kármán, R. V. Southwell, G. I. Taylor, and H. M. Westergaard, among others, were brought together.

He continued his contacts with Westinghouse as a consultant, making frequent trips to East Pittsburgh during the early years. Perhaps his principal efforts related to the publishing of textbooks, the first of their kind in the United States, which were closely related to his earlier Russian books. As always, he spent his summers in Europe, visiting his favored vacation spots in Switzerland and colleagues at various universities.

In 1936 Timoshenko joined the faculty at Stanford University, where L. S. Jacobsen was then professor of mechanical engineering. In 1940 he was elected to the National Academy of Sciences. His retirement came in 1944, but he continued to live in his home in Palo Alto, lecturing at Stanford and continuing the routine of summer trips to Europe. These sunset years were placid and pleasant, but not without sorrows; his wife passed away in 1946. He was joined by his brothers and some of their families, which helped to dispel the loneliness. His traveling schedule was interrupted by World War II, and he did not really share in the scientific revival of that epoch. In 1951 the trustees of Stanford University named a new facility in his honor: The Timoshenko Laboratory for Engineering Mechanics.

In 1958 he returned to Russia for a visit and was royally received—in stark contrast to the early years in the United States when he vainly tried to get in touch with his aged father. There he visited many of the scenes of his early years.

On the whole, he felt that the Revolution had not discarded the gains in his field that he had seen during the czarist years. The immediate result of his trip was a small treatise, *Engineering Education in Russia,* published in 1959.

He then settled down to write his autobiography. It is a remarkable fact that, after nearly fifty years in the United States, he felt it necessary to write this book in Russian. It was later translated into English under the title *As I Remember* and forms a charming and unsophisticated account of a varied life.

In 1945, after the end of the war in Germany, he was driven by military personnel all over West Germany, examined what was left of the German industry and the research laboratories, and reported his findings to Washington.

During his long and productive life, Timoshenko received many honors, meticulously listed by Eugene A. Vetchorine in his foreword to *As I Remember.* He was elected a member of the Ukrainian Academy of Sciences, Kiev (1918); Russian Academy of Sciences, Leningrad-Petersburg (1928); Polish Academy of Technical Sciences, Warsaw (1935); French Academy of Sciences, Paris (1939); National Academy of Sciences, Washington, D.C. (1940); Royal Society, London (1944); and Italian Academy of Sciences, Rome (1948). Honorary doctoral degrees were conferred upon him by Lehigh University, D.Sc. (1936); University of Michigan, D.Eng. (1938); Zurich Technical Institute, D.Eng. (1947); Munich Technical Institute, D.Eng. (1949); Glasgow University, D.Laws (1951); University of Bologna, Sc.D. (1954); Zagreb Polytechnic, D.Eng. (1956); and Turin Polytechnic, Sc.D. (1960).

Beginning with the Jourowski Medal and Prize for his opus on elastic stability in 1911, he received one more award in Russia: the Salov Prize for his article on "Stresses in Rail Type Tracks" in 1945. In the United States he received, among others, the Worcester Reed Warner Medal from the American Society of Mechanical Engineers in 1935; the Lamme Medal from the American Society of Engineering Education in 1939; the Levy Medal from the Franklin Institute in 1944; the Cresson Medal* from the same Institute in 1958; the Grande Médaille from the Association des Ingenieurs-Docteurs in France; the coveted James Watts International Medal from the British Institution of Mechanical Engineers the same year; and the Trasenter Medal from the Association des Ingénieurs Sortis de l'Ecole de Liège in Belgium. He was the first recipient of the Timoshenko Medal, instituted in his honor by the American Society of Mechanical Engineers in 1957, and he received the James Ewing Medal from the British Institution of Civil Engineers in 1963.

THE EARLY YEARS AT WESTINGHOUSE

With Timoshenko's arrival at Westinghouse in 1922, there was assembled in East Pittsburgh a remarkable group of young people, engaged partly to aid in the educational program of the design schools and partly to participate in research in the laboratory or to function as consultants, and sometimes as participants, in the design departments. The growing Westinghouse Club in Wilkinsburg organized evening lectures and seminars, in addition to regular daily lectures on company time. The names of the lecturers now read like a list of "Who's Who"; Timoshenko himself mentions V. Zworykin, Muromtsev, G. B. Karelitz, and J. M. Lessells; participating also were Joseph Slepian, Peters, Fortesque, and many others. Later, through Timoshenko's efforts, 0. G. Tietjens and A. Nadai were added. Already enrolled in the first Mechanical Design School were V. D. Barker, H. D. Else,

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* Accepted in his place by his son Gregory.

L. S. Jacobsen, B. E. James, and J. Ormondroyd; later groups included J. P. Den Hartog, R. E. Peterson, R. B. Smith, R. P. Kroon, M. D. Stone, and many others.

Timoshenko was clearly the key addition. He is sometimes thought of as the sole originator of this intellectual revival, but it is no reflection upon his contributions to observe that it had started before him and covered a range of science far beyond the narrower field of applied mechanics. To those of us who had the fortune to participate, it was our first contact with supremely good teaching and a genuine spirit of creative work in science and technology.

This writer has in his possession a small stack of notebooks, beginning with Timoshenko's "Elementary Course in Elasticity" of 1923-1924, through his "Theory of Elasticity" of 1924-1925, and continuing through a variety of other subjects until his departure from Westinghouse in 1927. But these notebooks also include lectures by Slepian ("Heaviside Operator Calculus and Plasma Physics"), Nadai ("Plasticity"), and others as well. It is clear from looking at these notes now that Timoshenko lost no time in getting back to his favorite occupation of lecturing to attentive and relatively mature students. The notes also show Timoshenko at his charming and effective best as a teacher.

The notes also indicate that after the early years, the lecture series took the form of evening courses at the Westinghouse Club, in which many of the younger people also participated. Examples from the notes are: Ormondroyd on "Graphical Integration," Den Hartog on "Bessel's Functions," Timoshenko on "Ritz' Method," Slepian on "Vector Analysis," and Soderberg on "Critical Speeds."

The intellectual atmosphere at East Pittsburgh during these years was strongly influenced by the breakthroughs in modern physics, naturally of particular interest to the physicists. Many of the international figures in science came to lecture in Pittsburgh, which stimulated an interest in these matters, not least in the group of young engineers. There were many study groups, which, under the guidance of competent mathematicians like Slepian, were introduced to the new ideas on relativity and cosmology, quantum physics, and wave theory. It was part of Timoshenko's single-minded attention to applied mechanics, his first love, that he never played a leading role in these diversions from the main task. Similar remarks about his attitude apply to later epochs, such as when the group at South Philadelphia ran their own lecture series on classical thermodynamics, kinetic theory of gases, statistical mechanics, and other subjects. To us, his young pupils, these experiences nevertheless established directions of interest that we have followed through the rest of our lives.

The notes also hint at a spirit of revival, which one encounters on only a few precious occasions during one's life. Many such periods have come and gone since, but to us, young men of the early twenties, those years in dreary and sooty East Pittsburgh on Turtle Creek have a sheen of their own.

CONCLUSION

On his arrival at East Pittsburgh in 1923, Timoshenko thus entered an intellectual environment that seems to have been made expressly for him and to which he made great contributions. He was in his forties, had a striking appearance, wore a beard, and, to those of us still in our twenties who came under his influence, he was a wise old man with a keen sense of humor. His own recollections of the Hiking Club quite accurately portray the influence he had upon us. It is significant that his first effort in the research laboratory was devoted to the establishment of a Mechanics Section—a literal extension of his earlier observations and experiences in

Russia and Europe. He gradually became the apostle of applied mechanics; with G. M. Eaton and John Lessells he was one of the enthusiastic founders of the Applied Mechanics Division of the American Society of Mechanical Engineers, perhaps one of the most significant contributions to come from that Society.

Only gradually did we come to appreciate the turmoil and anxiety that had been his lot during the preceding years. Under the charming exterior there was a deep-seated disappointment in American culture, which to Timoshenko and his wife seemed crude and uncouth in comparison with their experiences in the Ukrainian countryside and in the cultural circles of Europe. He was still smarting under the effects of the cataclysm of his homeland, which prevented him from reunion with his aged father. Out of these experiences grew a strange love-hate relationship in his feelings toward America, which never left him and sometimes stood in the way of full utilization of his talents. In reading *As I Remember,* one is astonished at the absence of a single word in grateful recognition of his debt to America, which had awarded him such a rare opportunity.*

Americans were accustomed to immigrants who developed an uncritical admiration for their new homeland, often accompanied by bitter hostility toward their place of birth. But to many of us, who also were immigrants with strong cultural roots in the homeland, Timoshenko's attitude was at least understandable. Through the years of turmoil—the late twenties, the depression, and the years just prior to World War II—this attitude of Timoshenko's did not appear to soften. These feelings culminated during his trip to Russia in 1958, when he became, so to speak, reunited with his homeland. But it was largely a reunion with the homeland of his

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* Also noted in the review of *As I Remember by J.* P. Den Hartog, *Science,* 160(1968).

dreams and with the successors to the old institutions. It is significant, as observed earlier, that his accounts of his life following this trip were written in Russian—this from the author of dozens of successful textbooks in English. It is also worth observing that circumstances prevented him from fully sharing in the wave of scientific revival that was part of the World War II scene. The experience of the first epoch of American superiority in scientific and technological developments was somehow denied him.

But whatever there was of bitterness was encased within his innermost being. The principal recollection on the part of those of us who were privileged to know him during those first years on the American scene is that of a man of great wisdom and a keen sense of humor, enhanced by his special version of the English language. This was usually devoid of articles and retained the syntax of Russian, mixed with that of other languages. When confronted with examples of American colloquialisms, his favorite expression was a quizzical "What means this?" Some of us who had the opportunity to accompany him to international conferences—the Congress of Applied Mechanics in Zurich in 1926 is an example—also were privileged to sense the enormous range of his acquaintances in the scientific world, acquired during his years of travel. The initial impression was of a remarkable linguistic versatility on his part, but this impression was tempered when it was discovered that he spoke only one language, modified in the international circles with French or German phrases, depending on the makeup of his audience. His Russian was flawless but always retained the strong Ukrainian accent.

While Timoshenko was well known in professional circles all over the world, the number of people admitted to his innermost sphere of affection was not large. His former students had a special position; so did a small group of his early acquaintances in the United States. One has the impression, however, that real intimacy was reserved for his own family* and his Russian-speaking friends. Among those who might be mentioned are G. B. Karelitz, who passed away in 1940, and Vladimir Zworykin. Professor Leonid M. Tichvinsky, in a personal communication, observed that "Timoshenko was the last person who knew my parents; he was my best man when I married my first wife, coming from Ann Arbor to Pittsburgh for this occasion in 1935." In a later communication Professor Tichvinsky observed that Timoshenko, while leading a comfortable life in the United States, did not accumulate any substantial wealth. He left a modest legacy to be divided among his children. The royalties from all his books were assigned to Stanford University.

In the perspective of more than a half century, Timoshenko's great influence upon applied science and technology in America resulted less from his original, creative discoveries than from his ideals of engineering education, his superb skill as a teacher, and his highly developed pragmatic skill in using fragments of exact solutions for a variety of approximate solutions to difficult problems in applied mechanics. Examples of this are his skillful use of the solutions for beams on elastic foundations to problems such as railroad rails and to details of machinery such as highly stressed dovetail joints. Another example is his frequent use of the Boussinesq solutions to a variety of intractable problems in machine structures. He also clarified the premises of the Rayleigh-Ritz iteration method, extending it to a variety of problems in elasticity and dynamics. Throughout all his

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* He was a member of a remarkable family, which contributed much to his worldwide views and connections. Of his two younger brothers, Serhij (an architect) was Secretary of Transportation in the Ukrainian government in the early 1920's, while Vladimir (an economist) was Secretary of Commerce of the Ukraine and later Chief of Statistics of the AAA under President Franklin D. Roosevelt in Washington and at Stanford University.

work there is a pragmatic attitude toward mathematics, and even to theoretical mechanics, which was regarded as one of his great sources of strength.

This charming pragmatism was new to most of us and seemed of immense value to the practicing engineers. It is well to remember that even elementary computer aids, which we now take for granted, were then many years distant in the future. Iteration procedures had to be worked out with brute strength and untold man-hours. With the development of modern computers and new methods such as the finite-element method, many problems once beyond our reach have now become routine. The pragmatic approach may now seem less essential, but I believe this is only a temporary phase. In any case, the effectiveness and charm of Timoshenko's teaching will always be a treasured memory to his students.

A factual account of the career of Stephen P. Timoshenko has been condensed from his book *As I Remember,* aided by tributes and memoirs by former students and associates, particularly those of Professor D. H. Young of Stanford University (Donovan H. Young, "Stephen P. Timoshenko 1878-1972," *Applied Mechanics Review,* July 1972. 5 pp.) and Professor Chia-Shun Yih of the University of Michigan (Chia-Shun Yih, "Stephen P. Timoshenko: A Portrait in Miniature." A note to the faculty of the College of Engineering, October 30, 1972.). These tributes emphasize his years in American universities.

Since this writer and a few remaining colleagues were part of the group that first became associated with Timoshenko on his arrival, in 1923, at the Westinghouse Electric Corporation in East Pittsburgh, Pennsylvania, we wished to record some of the background and impressions from his early years there. In the preparation of this memoir I have been greatly assisted by the members of the "International Hiking Club" in Wilkinsburg—Professors J. P. Den Hartog, J. Ormondroyd, and L. S. Jacobsen—as well as by other members of the group, specifically R. E. Peterson, M. Stone, and Leonid M. Tichvinsky, whose contributions and criticisms I gratefully acknowledge.

Bibliography

1905 Torsional vibrations of shafts. Proc. St. Petersburg Polytech. Inst., 3:55-106.

Krylov's method for integration of ordinary differential equations. Proc. St. Petersburg Polytech. Inst., 3:397-406.

Formulas for combined stresses from various strength theories. Proc. St. Petersburg Polytech. Inst., 3:415-55.

1906 Lateral buckling of I-beams under the influence of forces acting in the plane of largest rigidity. Proc. St. Petersburg Polytech. Inst., 4:151-219; 5(1-2):3-34, 262-92.

1907 Stresses in a plate with a circular hole. Proc. Kiev Polytech. Inst., 9:95-113.

Secondary stresses caused by rigidity of joints. Proc. St. Petersburg Polytech. Inst., 7:135-44.

Buckling of a bar on an elastic foundation. Proc. St. Petersburg Polytech. Inst., 7:145-57.

Forced vibration of prismatic bars. Proc. Kiev Polytech. Inst. 9: 201-52.

Stability of Plates under Compression. Kiev: Kulzhenko Typography. 60 pp.

1908 On the subject of buckling. Proc. Kiev Polytech. Inst., 8:181-212.

Strength of Materials. Part 1. Kiev: Kiev Polytechnic Institute. 373 PP.

Collection of Problems in Strength of Materials. Kiev: Kiev Polytechnic Institute.

1909 *A Course in Elasticity Theory.* Part 1. Kiev: Kiev Polytechnic Institute.

Stresses in a circular ring compressed by two opposing forces. Proc. Kiev Polytech. Inst., 9:21-37.

1910 Application of normal coordinates in analyzing the bending of bars and plates. Proc. Kiev Polytech. Inst., 10:1-19.

Stability of elastic systems. Proc. Kiev Polytech. Inst., 10:147-67.
Stability of elastic systems. Proc. Kiev Polytech. Inst., 10:375-560.
Einige Stabilitätsprobleme der Elastizitätstheorie. Z. Math. Phys., 58:337-85.
1911 *An Approximate Method for Investigating the Stability of Elastic Systems.* Kiev: St. Vladimir University. 25 pp.
A Course in Strength of Materials. Kiev: Kiev Polytechnic Institute. 517 pp.
Erzwungene Schwingungen prismatischer Stäbe. Z. Math. Phys. 59:163-203.
1912 Problems of strength of steam turbines. J. Soc. Technol. St. Petersburg, 7:266-79.
Effect of impact on a beam. Proc. St. Petersburg Polytech. Inst., 17:407-25.
1913 Bending of bars having a slight curvature. J. Soc. Technol. St. Petersburg, 13:411-14.
Bending of spherical shells. J. Soc. Technol. St. Petersburg, 17: 549-57.
Plotting the deflection curve. J. Soc. Technol. St. Petersburg, 21: 241-42.
Use of stress functions to study flexure and torsion of prismatic bars. St. Petersburg. 21 pp. (Reprinted in: Mem. Inst. Ways Commun., 82:1-21.)
Sur la stabilité des systèmes élastiques. Ann. Ponts Chaussees, 1st part, Memoires et Documents, 9th Ser., 13:496-566; 16:72-132, 373-412.
Zur Frage nach der Wirkung eines Stosses auf einen Balken. Z. Math. Phys., 62:198-209.
1914 Stability of a cylindrical shell. J. Soc. Technol. St. Petersburg, 21:785-92.
Effect of initial curvature on the bending of a rectangular plate. Petrograd. 4 pp. (Reprinted in: Mem. Inst. Ways Commun., 89.)

Etude de la flexion des barres au moyen d'une méthode approximative. Ann. Trav. Publics. Belg., 71 Ser., 219:263-96.

1915 Strength of rails. Proc. Inst. Ways Commun. 42 pp.

Large deflections of circular plates. Petrograd. 10 pp. (Reprinted in: Mem. Inst. Ways Commun., 89.)

Stability of plates with stiffening ribs. Petrograd. (Reprinted in: Mem. Inst. Ways Commun., 89.)

Vibration of rails. Proc. Inst. Electr. Eng., Petrograd, 13.

1916 The effect of clearances between the rail and the tie on bending of rails. Mem. Inst. Ways Commun. 14 pp.

A Course in Elasticity Theory. Part 2. St. Petersburg.

1917 Allowable stresses in railway bridges. Proc. Inst. Ways Commun., 7.

1919 Use of trigonometric series for calculation of suspension bridges. Eng. J., 8:239-43.

1921 Über die Stabilität versteifter Platten. Eisenbau, 12:147-63.

Etude de l'action des charges roulantes sur les rails. Génie Civ., 79:555-56.

On the correction for shear of the differential equation for transverse vibrations of prismatic bars. Philos. Mag., 41:744-46.

1922 *A Course of Statics of Structures.* Parts 1 and 2. Petrograd. 344 pp.

Über dies Biegung der allseitig unterstützten rechteckigen Platte unter Wirkung einer Einzellast. Bauingenieur, 3:51-54.

Beams with loads irregularly distributed. Engineering, 113:196-97.

Calcul des Arcs Elastiques, ed. Ch. Beranger. Paris: Librairie Polytechnique. 77 pp.

On the transverse vibrations of bars of uniform cross-sections. Philos. Mag., 43:125-31.

On the forced vibration of bridges. Philos. Mag., 43:1018-19.

On the distribution of stresses in a circular ring compressed by two forces acting along a diameter. Philos. Mag., 44:1014-19.

On the torsion of a prism, one of the cross-sections of which remains plane. Proc. Lond. Math. Soc., Ser. 2, 20:389-97.

A membrane analogy to flexure. Proc. Lond. Math. Soc., Ser. 2, 20:398-407.

Elasticity of pipe bends. Trans. Am. Soc. Mech. Eng., 44:585-93.

Torsion of crankshafts. Trans. Am. Soc. Mech. Eng., 44:653-67.

Berechnung der Schubspannungen im gebogenen Balken. Z. Angew. Math. Mech. 2:160-61.

1923 Determination of the modulus of elasticity. Mech. Eng., 45:259-60.

The pendulum hardness tester. Engineer, 136:21.

Bending stresses in curved tubes of rectangular cross-section. Trans. Am. Soc. Mech. Eng., 45:135-40.

The bending and torsion of multi-throw crankshafts on many supports. Trans. Am. Soc. Mech. Eng., 45:449-70.

Kippsicherheit des gekrummten Stabes mit kreisförmiger Mittellinie. Z. Angew. Math. Mech., 3:358-62.

1924 Über die Biegung von Staben, die eine kleine anfälngliche Krüimmung haben. In: *Festschrift zum siebzwigsten Geburtstage August Foppls,* pp. 74-81. Berlin: Springer-Verlag.

On stresses in a plate with a circular hole. J. Franklin Inst., 197: 505-16.

An approximate method of solution of two dimensional problems in elasticity. Philos. Mag., 47:1095-104.

Beams without lateral support. Trans. Am. Soc. Civ. Eng., 87: 1247-62.

Deflections of a uniformly loaded circular plate with clamped edges. Sci. Pap. No. 162, Westinghouse Electric and Manufacturing Co.

1925 Analysis of bi-metal thermostats. J. Opt. Soc. Am., 11:233-55.

Properties of matter under high pressure. Mech. Eng., 47:513-15.

With W. Dietz. Stress concentration produced by holes and fillets.

Trans. Am. Soc. Mech. Eng., 47:199-220.

With J. M. Lessells. *Applied Elasticity.* New York: D. Van Nostrand Co.

1926 With R. V. Baud. The strength of gear teeth. Mech. Eng., 48: 1105-9.

Method of analysis of statical and dynamical stresses in rail. In: *Proceedings of the Second International Congress for Applied Mechanics,* pp. 407-18. Zurich and Leipzig: Orell Füssli Verlag.

Stress concentration produced by fillets and holes. In: *Proceedings of the Second Congress for Applied Mechanics,* pp. 419-26. Zurich and Leipzig: Orell Füssli Verlag.

1927 Vibration of bridges. Trans. Am. Soc. Mech. Eng., 49-50(part 2): 53-61.

1928 Vibration problems in engineering. N.Y.: D. Van Nostrand Co.

The stiffness of suspension bridges. Atti Congr. Int. Mat. (Proc. Int. Congr. Math.), Bologna, 6:305-6.

Steifigkeit von Häingebrücken. Z. Angew. Math. Mech., 8:1-10.

1929 Teaching of advanced mechanics in engineering schools. Mech. Eng., 51:609-10.

1930 Vibration problems. In: *Marks' Mechanical Engineering Handbook,* pp. 489-502. New York: McGraw-Hill.

The theory of elasticity. Mech. Eng., 52:494-96.

Recent developments in the application of mechanics to machine design. Mech. Eng., 52:607-10.

Strength of Materials. 2 vols. Princeton, N.J.: D. Van Nostrand Co.

Problems concerning elastic stability in structures. Trans. Am. Soc. Civ. Eng., 94: 1000-1020.

1931 Stability and strength of thin-walled constructions. In: *Proceedings of the Third International Congress for Applied Mechanics,* vol. 3, pp. 3-15. Stockholm: Kungl. Boktryckeriet, P. A. Norstedt and Söner.

Stabilitätsprobleme der Elastizität. In: *Handbuch der physikalischen und technischen Mechanik,* ed. F. Auerbach and W. Hort, vol. 4, part I, pp. 81-145. Leipzig: J. A. Barth.

Festigkeitsprobleme im Maschinenbau. In: *Handbuch der physikalischen und technischen Mechanik,* ed. F. Auerbach and W. Hort, vol. 4, part 1, pp. 146-98. Leipzig: J. A. Barth.

Theory of elasticity. Mech. Eng., 53:889-90.

1932 Stability of plate girders subjected to bending. Congr. Int. Assoc. Bridge Struct. Eng., Zurich, pp. 129-47.

With B. F. Langer. Stresses in railroad track. Trans. Am. Soc. Mech. Eng., 54:277-302.

1933 Working stresses for columns and thin-walled structures. Trans. Am. Soc. Mech. Eng., 55:APM 173-83.

Über die Biegung von Trägerrosten. Z. Angew. Math. Mech., 13: 153-59.

With S. Way. Suspension bridges with a continuous stiffening truss. Publ. Int. Assoc. Bridge Struct. Eng., Zurich, 2:452-66.

1934 The stability of the webs of plate girders. Engineering, 138:207-9.

Theory of Elasticity. New York: McGraw-Hill.

1935 *Elements of Strength of Materials.* Princeton, N.J.: D. Van Nostrand Co.

Buckling of flat curved bars and slightly curved plates. J. Appl. Mech., 2:A17-A20.

1936 *Theory of Elastic Stability.* N.Y.: McGraw-Hill.

1937 With D. H. Young. *Engineering Mechanics.* New York: McGraw-Hill.

1938 Bending of rectangular plates with clamped edges. In: *Proceedings of the Fifth International Congress of Applied Mechanics,* pp. 40-43. New York: John Wiley & Sons.

1940 *Theory of Plates and Shells.* New York: McGraw-Hill.

1941 The forced vibrations of tie-rods. In: *Theodore von Kármán Anniversary Volume, Contributions in Applied Mechanics,* pp. 226-30. Friends of Theodore von Karman.

1942 Teaching dynamics. J. Eng. Educ., 32:463-66.

1943 Theory of suspension bridges. J. Franklin Inst., 235:213-38, 327-49.

1945 Theory of bending torsion and buckling of thin-walled members of open cross-section. J. Franklin Inst., 239:201-19, 249-68, 343-61.

With D. H. Young. *Theory of Structures.* New York: McGraw-Hill.

1947 Stress concentration and fatigue failures. Proc. Inst. Mech. Eng., London, 157:163-69.

1948 With D. H. Young. *Advanced Dynamics.* New York: McGraw-Hill.

1950 D. J. Jourawski and his contribution to theory of structures. In: *Federhofer-Girkmann-Festschrift, Beitrage zur angewandten Mechanik,* pp. 115-23. Vienna: Franz Deutickle Verlag.

1953 History of the development of strength of materials in Russia. Academia Nazionale dei Lincei, Rome, no. 29. 8 pp.

History of Strength of Materials. New York: McGraw-Hill.

1954 Stress concentration in the history of strength of materials. Proc. Soc. Exp. Stress Anal., 12:1-12.

1956 The development of engineering education in Russia. Russian Rev., 15:173-85.

1958 The background of engineering education il Russia. J. Eng. Educ., 49:122-25.

1959 *Engineering Education in Russia.* New York: McGraw-Hill.

1968 *As I Remember* (Autobiography). New York: D. Van Nostrand Co.

RELATED BOOKS

Stephen Timoshenko 60th Anniversary Volume, Contributions to the Mechanics of Solids Dedicated to Stephen Timoshenko by His Friends. New York: The Macmillan Co., 1938.

The Collected Papers of Stephen P. Tmoshenko. New York: McGraw-Hill, 1953.

Richard B. Turner

Richard Baldwin Turner

October 7, 1916-December 22, 1971

by Marshall Gates

Richard B. Turner was born in Minneapolis, Minnesota on October 7, 1916 to Hubert Michael Turner and Jessie Baldwin Turner, both highly cultured people and educators. Their son's entire life and career reflected this heritage. Hubert Turner was born in 1882 in Hillsboro, Illinois. After graduating from the University of Illinois in 1910, he stayed on as an assistant instructor for two years while taking graduate work in mathematics, physics, and electrical engineering. Here he met Jessie Baldwin, a teacher and graduate of the university in botany. They were married in 1912 while both were members of the faculty of the university.

After an interlude in Minneapolis, where Dick was born, the family moved to New Haven in 1918 where Hubert Turner took up an appointment as assistant professor of electrical engineering at the Sheffield Scientific School at Yale. His entire subsequent career was spent at Yale; he became internationally known in the field of electrical communication engineering. He was a member of the American Institute of Electrical Engineers, the International Union of Scientific Radio Telegraphy, the Franklin Institute, the Institute of Radio Engineers, the American Standards Association, and the American Association for the Advancement of Science.

He retired from Yale in 1952 and died of a heart attack while living at the Yale Faculty Club in 1965.

Dick's mother, Jessie Baldwin Turner, was born in Deer Park, Illinois. She maintained her interest in botany in New Haven, where she was a member of the Fern Society and had a collection of over one hundred varieties of ferns. Both parents collected rocks, shells, and stamps and were avid bird watchers. They were widely read and well informed, of high principles, interested in both world and local affairs, and maintained a home in which intellectual values were fostered. Dick's only sibling, Elizabeth, died at age nine, and he was raised as an only child from then on. He spent all his childhood in New Haven, strongly influenced by his parents and the college community. He attended Susan Sheridan Junior High School and New Haven High School, graduating in 1933. He was a bright and inquisitive child, excelling in mathematics, physics, and chemistry. Both parents supplemented and enriched his formal education by tutoring him at home.

Turner was also a talented musician and during these years played the piano, clarinet, and trombone. This talent for and love of music was to remain with him all his life; he particularly enjoyed Dixieland jazz and was adept in this style with the clarinet. While at Harvard he played in the college band. Like most boys of his age, he was keenly interested in athletics, and he also enjoyed sailing and woodworking. In later years he found time to combine the last two avocations, building a sailboat that he and his family greatly enjoyed using. He was also an avid reader, particularly in history, and was editor-in-chief of his school newspaper, *The Sentinel,* his junior and senior years.

In 1934 Turner entered Harvard, graduating in 1938. He remained at Harvard for graduate studies, first under William F. Ross, then with Louis F. Fieser, under whom he com pleted his Ph.D. in 1942. He remained at Harvard for another year working under a National Defense Research Committee contract, then went to the Mayo Clinic in 1943 to work with the group assembled by E. C. Kendall to examine the chemistry of the adrenocorticosteroids. After two years at the Mayo Clinic, Turner in 1945 joined a synthetic group at MIT carrying on a wartime project on the synthesis of antimalarials under the direction of Arthur C. Cope. He remained with this group until 1948, when he returned to Harvard as a research fellow of the American Cancer Society, for the first time working alone or with one or two technicians on problems of his own inception. He remained there until the fall of 1951, when he took a position as assistant professor of chemistry at Rice University, rising through the academic ranks to associate professor (1953) and professor (1956). He remained at Rice for the rest of his career.

In 1952 Turner married Halina Deschko, a native of the Ukraine who had come to this country after World War II. She had graduated from Mt. Holyoke and the Simmons College School of Social Work. They had three children: Richard, Jr., Tamara, and William. Richard, Jr., is an architect and his younger brother, William, is also entering this field; Tamara's professional interests lie in anthropology. Turner was a devoted husband and father and spent much time with his family in spite of the heavy demands of his professional career.

Turner was an unusually able teacher and lecturer. His presentations, both in the classroom and at symposia and meetings, were sharp, incisive, rigorous, and polished, and he was invited to present his work widely both here and abroad.

THE SCIENTIFIC WORK OF R. B. TURNER

Aside from the work he carried out for his Ph.D., Richard Turner's first significant contribution to chemistry came from the two years he spent with Kendall's group at the Mayo Clinic. A series of six papers on the general subject of steroids derived from bile acids appeared in the period from 1946 to 1952. Using ^{9,11}, cholenic acid as a raw material, efficient methods were developed for the preparation of the important 1 -ketocholanic acid and a number of its close relatives and the subsequent degradation of 1 -ketocholanic acid to ll-ketoetiocholanic acid by removal of the bile acid side chain. This work was crucial for the preparation of the first partially synthetic samples of Kendall's compound A for clinical trials. During the course of this work, stereochemical assignments were made to twenty-five bile acid derivatives substituted in ring C.

Turner's interest in steroids and related compounds endured in somewhat modified form throughout his career. He was the first to synthesize C^{14} labeled cholestenone and testosterone (1947), and sporadic publications on a variety of problems related to steroids appeared from then until 1960. During the period from 1954 to 1958, Turner and his coworkers carried out structural studies on the cardiotonic steroid ouabagenin, derived from the glycoside ouabain long used as an arrow poison by the East African Somalis. They were able to locate the remaining uncertain hydroxyl group at C 11 and were able to correlate ouabagenin and strephanthidine, another cardiotonic aglycone, by conversion of both to a common derivative still retaining the hydroxyl group at C14 and the butanolide ring.

A general interest in the structure and synthesis of natural substances, of which the above work in the steroid field was one manifestation, continued to occupy Turner throughout most of his career. Determination of the structure and completion of a synthesis of cassaic acid and a synthesis of phyllocladene were published in the period from 1959 to 1966, and he and his coworkers had begun a synthetic approach to the diterpenoid alkaloids at about this time. At the time of his death, Turner and his coworkers were at work on a synthesis of marasmic acid and had completed construction of the carbon skeleton in its correct stereochemical form. Various syntheses of intermediates used in the extensive antimalarial program carried out during the later stages of World War II comprise the work Turner carried out while in the group headed by Arthur C. Cope in the period from 1945 to 1948. Synthetic work in the naphthoquinone field with Louis F. Fieser formed the subject of his doctoral dissertation as well; these results were published in 1947.

In a penetrating and important early paper, Turner, in collaboration with Dorothy Voitle, established the structure of D^{1}-l-acetyl-2-methylcyclohexene as the *s-cis* conformer; this was done by a study of its uv absorption and that of a number of more rigid , ,-unsaturated ketones and provided an explanation of the lowered extinction coefficients in such substances.

Turner's bibliography contains contributions on a wide variety of subjects (synthesis, structures of natural products, use of radioactive carbon in labeling important hormones, conformational analysis, instrumental methods as probes of structure, stereochemistry, and such diverse biochemical subjects as the mechanism of uptake of radioactive iodine by thyroid tissue and the biochemistry of aldosterone), but by far the most important contribution he and his colleagues made was their use of heats of hydrogenation as a tool to study a variety of problems having to do with the comparative stabilities of a wide variety of olefins, estimates of strain energies and conjugative interactions, conformational problems, and others. This work brought order based on quantitative results to a large and important area of organic chemistry that theretofore had been characterized by conjecture, hypothesis, and speculation.

It is perhaps inappropriate to review this work in detail here, but the following important results and conclusions arose directly from it:

- The question of homoallylic resonance in such substances as norbornadiene, barrelene, and *cis, cis, cis*-1,4,7-cyclononatriene was resolved once and for all. They are devoid of such resonance.
- The relative stabilities of various double-bond isomers of cholestene were established on a quantitative basis (^{2} is the most stable).
- The relative stabilities of *cis* and *trans* isomers of cyclooctene, cyclononene, and cyclodecene were quantitatively determined. In any given pair, the *cis* isomer is more stable, cis-cyclodecene having the lowest enthalpy of hydrogenation of any alkene examined.
- Reliable values for the stabilization energies of a number of theoretically important cyclic polyenes were established. Among those studied were cyclooctatetrene; 1,3,5-cyclooctatriene; azulene; heptafulvene; heptafulvalene and its dihydro derivative, tropone; tropylium ion; and acepleiadylene.
- The relative stabilities of exo and endo olefins in five-, six-, and seven-membered ring systems were determined. In all cases, the endo isomer is the more stable. There had been claims, albeit somewhat ambiguous, that could have been interpreted as indicating a greater stability for the exo isomer in five-membered rings.
- The order of stability for substituted olefins was established unambiguously. This work also showed the importance of planarity for olefin stability; *cis*-di-*t*-butylethylene has the highest enthalpy of hydrogenation of any simple olefin examined, nearly 10 kcal/mole higher than its *trans* isomer.

- The essential correctness of a single most stable conformation for cyclodecane, suggested by Dunitz, was established by the demonstration of the necessity for a transoid conformation in 1,1,4,4-tetramethylcyclodecen-7.
- The well-known stabilizing effect of alkyl groups on carbon-carbon double-bonds was shown to be independent of the nature of the alkyl group and therefore not readily accounted for on steric grounds.
- The strain energies of a number of theoretically interesting small ring compounds were determined. 1,3-Dimethylbicyclo[1.1.0] butane has the remarkably high value of 67 kcal/mole.
- The conjugative stabilization in 2-methyl-1,3,5-hexatriene was shown not to be the result of strengthening of sp^2-sp^2 bonds relative to sp^3-sp^2 bonds, but to be consistent with the resonance hypothesis. By inference, this finding should also apply to 1,3-butadiene and 1,3,5-hexatriene and similar substances.
- The triple-bond strain in cyclic alkynes was determined. It is large in cyclooctyne, about 2.9 kcal/mole in cyclononyne and negligible in ten- and twelve-membered rings.
- Reliable quantitative evidence on strain and conjugative interactions in such substances as the cyclohexadienes and the cycloheptadienes, as well as in the cyclooctatrienes, was provided. These data will have to be taken into account in any discussion of such medium-size ring systems.
- Quantitative evidence for transannular interactions in medium-size rings, both saturated and unsaturated, was accumulated.

In this large and important field Turner perceived the need for reliable quantitative data, selected the substrates to be examined with a keen eye for the significance of the findings, provided the methodology for obtaining them, mea sured the quantities with precision, and interpreted the results with rigor and sophistication.

The importance of this work as a whole attracted much attention, and led to fruitful collaboration with other wellknown chemists both here and abroad. The most extensive such collaboration was between Turner and William von E. Doering. They and their collaborators published five joint papers.

Finally, it is remarkable that nearly 20 percent of Turner's papers were published under his name alone. He was a gifted experimentalist and enjoyed laboratory work. He continued to carry on experimentation with his own hands right up to the time of his death.

Bibliography

1941 With W. F. Ross. Hemoglobin linkage of hemoglobin. II. Analysis of the CO product. The pancreatic digestions of several forms of hemoglobin. J. Biol. Chem., 139:603-10.

1946 With V. R. Mattox, L. L. Engel, B. F. McKenzie, and E. C. Kendall. Steroids derived from bile acids. III. Derivatives of A9''', cholenic acid with substituents at C_3 and C_{12}. J. Biol. Chem., 162:571-84.

With V. R. Mattox, L. L. Engel, B. F. McKenzie, W. F. McGuckin, and E. C. Kendall. Steroids derived from bile acids. IV. 3,9-Epoxy-$D^{9,11}$ cholenic acid and closely related compounds. J. Biol. Chem., 164:569-96.

With V. R. Mattox, L. L. Engel, B. F. McKenzie, and E. C. Kendall. Steroids derived from bile acids. V. Introduction of oxygen at C,. J. Biol. Chem., 166:345-65.

With A. C. Cope. (Dialkylaminomethyl)-2-phenyl-4-quinolinemethanols with 8-amino or hydroxy substituents. J. Am. Chem. Soc., 68:2214-19.

With J. Mills and A. C. Cope. (Dialkylaminomethyl)-8-amino (or hydroxy)-4-quinolinemethanols. J. Am. Chem. Soc., 68: 2220-24.

1947 Radioactive cholestenone. J. Am. Chem. Soc., 69:726-27.

Radioactive testosterone. Science, 106:248.

With L. F. Fieser. Addition of mercapto derivatives to 2-methyl-1,4-naphthoquinone. J. Am. Chem. Soc., 69:2335-38.

With L. F. Fieser. Naphthoquinone acids and ketols. J. Am. Chem. Soc., 69:2338-41.

1948 Structure and synthesis of cardiac genins. Chem. Rev., 43:1-42.

With V. R. Mattox, B. F. McKenzie, L. L. Engel, and E. C. Kendall. Steroids derived from bile acid. VII. Probable stereochemical configuration of some derivatives of the bile acids. J. Biol. Chem., 173:283-94.

1949 With A. C. Cope, H. R. Nace, W. R. Hatchard, W. H. Jones, and M. A. Stahmann. Synthesis of 6-methoxy-8-amino-quinoline derivatives. Ethylenimine rearrangements in attachment of monoalkylamino-alkyl side chains. J. Am. Chem. Soc., 71: 554-61.

1950 Steroids labeled with isotopic carbon: Cholestenone and testosterone. J. Am. Chem. Soc., 72:579-85.

With D. M. Voitle. Preparation of DL-cysteine hydrochloride. J. Am. Chem. Soc., 72:628-29.

Stereochemistry of the peracid oxidation of ketones. J. Am. Chem. Soc., 72:878-82.

With D. M. Voitle. Reaction of 1-acetyl-2-methylcyclohexene with cyclohexanone. J. Am. Chem. Soc., 72:4166-68.

1951 With D. M. Voitle. The structure of 1-acetyl-2-methylcyclohexene; Spectral characteristics of *s-cis-* , ,-unsaturated ketones. J. Am. Chem. Soc., 73:1403-10.

With D. M. Voitle. Epimeric 20-hydroxypregnene derivatives. J. Am. Chem. Soc., 73:2283-86.

1952 Energy differences in the *cis-* and *trans*-decahydronaphthalenes. J. Am. Chem. Soc., 74:2118-19.

Acylation of 17- -hydroxy-20-keto steroids: Compound L diacetate. J. Am. Chem. Soc., 74:4220-21.

Further structural evidence for the reaction product of cholesterol -oxide and methylmagnesium iodide. J. Am. Chem. Soc., 74: 5362-64.

With V. R. Mattox, W. F. McGuckin, and E. C. Kendall. Steroids derived from bile acids. Part XIX. Barbier-Wieland degradation in the l-keto series. J. Am. Chem. Soc., 74:5814-18.

With V. R. Mattox, W. F. McGuckin, E. J. H. Chu, and E. C. Kendall. Steroids derived from bile acids. XX. Degradation of 3a, 9a-epoxy-l 1-oxonorcholanic acid to 3a,9a-epoxy-l 1-oxotiocholanic acid. J. Am. Chem. Soc., 74:5818-19.

1953 With R. B. Woodward. Chemistry of the cinchona alkaloids. In: *The Alkaloids—Chemistry and Physiology,* vol. 3, p. 1. New York: Academic Press.

Stereospecificity in the rearrangement of 17-hydroxy-20-oxo steroids. J. Am. Chem. Soc., 75:3484-88.

With R. P. A. Sneeden. Ouabagenin. I. The relationship between ouabagenin monoacetonide and anhydroouabagenin. J. Am. Chem. Soc., 75:3510-13.

Acylation of 17-hydroxy-20-oxo-steroids. J. Am. Chem. Soc., 75: 3489-92.

With H. R. Nace. Dipole moments and conformations of androstane-3,17-dione and of etiocholane-3,17-dione. J. Am. Chem. Soc., 75:4063-66.

Infrared absorption of some steroid digitonides. J. Am. Chem. Soc., 75:4362-63.

Molecular rotation differences for 17-hydroxy-20-keto steroids. J. Am. Chem. Soc., 75:3604-5.

1954 With R. P. A. Sneeden. Structure of ouabagenin. Chem. Ind., 1954:1235-36.

With H. I. Nachimson, R. H. Benson, J. J. Szafir, H. C. Allen, Jr., and R. V. Talmage. Comparative study of effects of phenothiazine and iodine on thyroid uptake of iodine. Proc. Soc. Exp. Biol. Med., 87:157-62.

Pyrolysis of the Windaus acid. J. Am. Chem. Soc., 76:1390.

1955 With R. P. A. Sneeden. Ouabagenin. II. The hydroxyl groups of the A/B ring system. J. Am. Chem. Soc., 77:130-34.

With R. P. A. Sneeden. Catalytic dehydrogenation of primary and secondary alcohols with platinum and oxygen: Selective dehydrogenation in the steroid series. J. Am. Chem. Soc., 77:190-91.

With R. Anliker. Stereochemistry of the epimeric 17a-methyl-Dhomo-testosterones. Helv. Chim. Acta, 38:411-20.

1956 With R. V. Talmage, R. H. Benson, J. J. Szafir, and H. C. Allen. Excess iodide as the primary cause of the inhibition of thyroid uptake of radioiodine produced by phenothiazine N. F. (green). J. Am. Vet. Med. Assoc., 128:150-51.

With J. A. Meschino. Location of the sixth hydroxyl group in ouabagenin. J. Am. Chem. Soc., 78:5130-31.

With D. E. Nettleton, Jr., and R. Ferebee. Synthetic routes to 3,6-dimethoxyphenanthrene. J. Am. Chem. Soc., 78:5923-27.

1957 With R. L. Anliker, A. S. Lindsey, and D. E. Nettleton, Jr. A synthetic approach to polycyclic hydroaromatic systems related to the 19-nonsteroids. J. Am. Chem. Soc., 79:220-26.

With R. H. Garner. Stability relation of 1-methylcyclopentene and methylenecyclopentane. J. Am. Chem. Soc., 79:253.

With M. Perelman and K. T. Park, Jr. Tracer studies in rearrangements of 17-hydroxy-20-oxo-steroids and observations on the reaction of compound L monoacetate with Al isopropoxide. J. Am. Chem. Soc., 79:1108-14.

With W. R. Meador and R. E. Winkler. Heats of hydrogenation. I. Apparatus and the heats of hydrogenation of bicyclo[2.2.1]heptadiene bicyclo[2.2.2]octane and bicyclo[2.2.2] octadiene. J. Am. Chem. Soc., 79:4116-21.

With W. R. Meador and R. E. Winkler. Heats of hydrogenation. II. Heats of hydrogenation and the acid catalyzed isomerization of unsaturated steroids. J. Am. Chem. Soc., 79:4122-27.

With W. R. Meador, W. v. E. Doering, L. H. Knox, J. R. Mayer, and D. W. Wiley. Heats of hydrogenation. III. Hydrogenation of cycloocta tetraene and of seven-membered nonbenzenoid aromatic compounds. J. Am. Chem. Soc., 79:4127-33.

With W. R. Meador. Heats of hydrogenation. IV. Hydrogenation of *cis* and *trans*-cycloolefins. J. Am. Chem. Soc., 79:4133-36.

1958 With R. H. Garner. Heats of hydrogenation. V. Relative stabilities in exocyclic endocyclic olefin pairs. J. Am. Chem. Soc., 80:1424-30.

With D. E. Nettleton, Jr., and M. Perelman. Heats of hydrogenation. VI. Substituted ethylene. J. Am. Chem. Soc., 80:1430-33.

With J. A. Meschino. Ouabagenin. III. Assignment of the sixth hydroxyl group and a structural correlation with strophanthidin. J. Am. Chem. Soc., 80:4862-65.

1959 Stabilization and strain energies from heats of hydrogenation. Kekule Symposium on Theoretical Organic Chemistry, London, 1958. In: *Theoretical Organic Chemistry,* p. 67. London: Butterworths Scientific Publications.

With E. G. Herzog, K. B. Morin, and A. Riebel. Structure of cassaic acid. Tetrahedron Lett., 1959, 2:7-10.

Thermal evidence for hyperconjugation. Tetrahedron, 5:127-34.

1960 With P. E. Shaw. Total synthesis of a degradation product of phyllocladene. Tetrahedron Lett., 18:24-27.

With H. R. Nace. Dipole moments of androstan-17-one, testan-I 1one, and testane-11,17-dione. J. Org. Chem., 25:1403-4.

With H. Prinzbach and W. v. E. Doering. Heats of hydrogenation. VII. Cycloheptatrienylium (tropylium) ion. J. Am. Chem. Soc., 82:3451-54.

With R. H. Benson. Acetyl group determination using acetic anhydride C'^{4}. Anal. Chem., 32:1464-65.

1961 With R. E. Lee and E. G. Hildenbrand. Trans-decalin-2,9-dicarboxylic acid and related derivatives . J. Org. Chem., 26:4800-4803.

With K. H. Ganshirt. Total synthesis of phyllocladene. Tetrahedron Lett., 1961, 231-33.

1964 With P. Goebel, J. Sicher, and M. Svoboda. Conformation of unsaturated cyclodecane derivatives. Enthalpy of hydrogenation of alkylcyclodecenes and -cyclodecynes. Proc. Chem. Soc. London (July):237-38.

With W. R. Roth, W. B. Bang, P. Goebel, R. L. Sass, and A. P. Yue. Question of homoconjugation in *cis, cis, cis*-1,4,7-cyclononatriene. J. Am. Chem. Soc., 86(15):3178-79.

Heat of hydrogenation of bicyclo[2.2.2]octa-2,5,7-triene. J. Am. Chem. Soc., 86(17):3586-87.

With P. Grafen. 1-Cyano-5-methoxy-2-tetralone. Tetrahedron Lett., 52:3935-37.

1965 With P. Goebel, W. v. E. Doering, and J. F. Coburn. Strain in 1,3-dimethylbicyclo[1,1,0] butane. Tetrahedron Lett., 1965, 997-1002.

1966 With J. Sicher, M. Svoboda, J. Zavada, and P. Goebel. Stereochemical studies. Part 36. An approach to conformational analysis of medium ring compounds, unsaturated 10-membered ring derivatives. Tetrahedron, 22:659-71.

With O. Buchardt, E. Herzog, R. B. Morin, A. Riekl, and J. M. Sanders. Structure and total synthesis of cassaic acid. J. Am. Chem. Soc., 88:1766-75.

With K. W. Ganshirt, P. E. Shaw, and J. D. Tauler. Total synthesis of phyllocladene. J. Am. Chem. Soc., 88:1776-85.

With G. D. Diana, G. E. Fodor, K. Gebert, D. L. Simmons, A. S. Rao, O. Roos, and W. Wirth. Synthetic intermediates related to diterpene alkaloid. J. Am. Chem. Soc., 88:1786-92.

1968 With J. Sicher, M. Svoboda, and B.J. Mallon. Stereochemical studies. 47. Conformational analysis of many-membered ring compounds. Cyclododecenes. J. Chem. Soc., 1968B:441-47.

With R. D. Stipanovic. Cyclohexenone-4-acetic acid derivatives from the addition of diazoacetic ester to beta, gamma unsaturated ketals. J. Org. Chem., 33:3261-63.

With P. Goebel, B. J. Mallon, W. v. E. Doering, J. F. Coburn, and M. Pomerant. Heats of hydrogenation. VIII. Compounds with 3- and 4-membered rings. J. Am. Chem. Soc., 90:4315-22.

With R. B. Miller and J. L. Lin. Stereochemical investigations of methyl epimerization in derivatives of 1-methyl-*trans*-decalin. J. Am. Chem. Soc., 90:6124-30.

With P. Grafen, H. J. Kabbe, O. Roos, G. D. Diana, and T. T. Li. Synthetic approaches to diterpene alkaloids. J. Am. Chem. Soc., 90:6131-35.

With P. M. Lesko. Strain energy in bicyclo[3,3,l]non-1-ene. J. Am. Chem. Soc., 90:6888-89.

1970 With M. Tichy. Heats of hydrogenation of cis-2-methylhexatriene and trans-2-methylhexatriene resonance or sigma bond energy. Proc. Natl. Acad. Sci. USA, 67(4):12A.

With L. P. Cawley, S. C. Sommers, and R. Horton. Biochemistry and physiology of aldosterone. Am. J. Clin. Pathol., 54:311-13.

Chemical properties of aldosterone. Am. J. Clin. Pathol., 54:289-96.

1971 With A. Meyer. Interesting synthesis of 3-methoxy-2,6-dimethylphenethyl alcohol. Tetrahedron, 27:2609-15.

With V. Boekelheide and W. S. Lindsay. Heats of hydrogenation. Acepleiadylene. Tetrahedron, 27 (15):3341-44.

1973 With P. Goebel, B. J. Mallon, and A. D. Jarrett. Heats of hydrogenation. IX. Cyclic acetylenes and some miscellaneous olefins. J. Am. Chem. Soc., 95:790-92.

With S. R. Wilson. Pyrazoline stereochemistry. Use of nuclear magnetic resonance shift reagents with azo-compounds. J. Chem. Soc. Chem. Commun., 1973:557-58.

With S. R. Wilson. Studies in sequiterpene synthesis. Marasmic acid skeleton. J. Org. Chem., 38:2870-73.

With B. J. Mallon, M. Tichy, W. v. E. Doering, W. R. Roth, and G. Schroder. Heats of hydrogenation. X. Conjugative interaction in cyclic dienes and trienes. J. Am. Chem. Soc., 95:8605-10.

D L Webster

David Locke Webster II

November 6, 1888-December 17, 1976

by Paul Kirkpatrick

The life to be reviewed here was that of a professional physicist, an educator, a national servant, a family man, and a keen appreciator of the natural earth-its rock, its air, its water, and its celestial environment. He was avid about his hobbies and always made science out of them by studying them in productive depth. Near the end, he said that he should have specialized in geology rather than physics, but few physicists would second this tardy preference. As with able and versatile men in general, there was a variety of good lives open to Webster; like them all, the path actually chosen was a function of the elaborate complex of unpredictables that we must call "chance."

David Webster was born in Boston, and New England was stamped on his tongue to the end, as any ear for dialect would recognize, but it would be wrong just to pronounce him a New England type except as it was typical of nineteenth—century New Englanders to resist complete uniformity. Webster had such individuality or self-dependence. To his students he was a "character," but that tells nothing precise since characters defy characterization.

FAMILY DATA

Each of Webster's parents was anteceded by at least seven generations of New England ancestors, the regressing lines vanishing at about the mid-seventeenth-century peak of immigration from Britain. All of the names seem English, and Webster has dropped a remark that his ancestors were Puritans from the northeast part of England—Yorkshire, Norfolk, and thereabouts—and that they left England, bound for America, about two jumps ahead of the sheriff. If this reference had any other than a facetious meaning, it should be realized that there must then have been some two hundred and fifty unconnected ancestors in the migratory flow that generated our subject, and it is unlikely that one denigration could fit them all.

The Webster name is best remembered in Massachusetts history because of two individuals. One of them (Daniel) shared a seventeenth-century ancestor with our subject. The other (Noah) was on an unrelated line.

Webster's father, Andrew Gerrish Webster, deprived of a college education by Civil War conditions, was of the type capable of self-education. His recorded description of himself was "Tastes simple—self-contained." His wife, Webster's mother, scorned this modesty and pointed out some of his valued services in the community of Boston, the center of his business interest, which was the tanning and wholesale distribution of leather.

Webster's mother was born Lizzie Florence Briggs in Boston in 1853. The Briggs name had been known in the shipbuilding business for more than two centuries, but in the middle of the nineteenth century steamships had improved to the point where they could drive the windjammers off the oceans. Lizzie's father (Harrison Otis Briggs) gave up the contest, moved his family to England, and got himself a job in a shipyard in Liverpool. There Lizzie got most of her schooling. For reasons unknown, the family returned to America after a dozen years, and Webster picks up the story at that point.

> By the time the family returned to the United States the old square-rigged sailing ships were almost a thing of the past, and my grandfather never had built steamships. So Grandpa Briggs went into a bank instead of going back into shipping. As President of the Bank of the Republic in Boston he had a prominent part of some kind in the reconstruction of the South after the Civil War. He was very definitely not a carpetbagger. His work was entirely altruistic, and just what he did there I am not sure, but I know it was for the benefit of the Southerners. My mother once described her father as having "great sweetness and unselfishness, with dignity and reserve.. . a clear and quick brain, great kindness of heart and a sense of humor, very fond of music, literature, travel, and outdoor sports."*
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> Webster's father left his Boston leather business about 1910 at an age now considered appropriate for retirement, but he had other interests to follow, particularly real estate. He worked until his death at ninety-three. His son has cited evidence that at that age he was still "a keen man." There is also evidence that prolonged physical and mental health were Webster family characteristics, traits borne out by David Locke the second.

CHILDHOOD

Raising the young Webster from his twelve-pound birth weight to his teens was a project shared by numerous loving and unskilled hands. In his last decade of life, the product of their efforts testified: "I grew up practically alone [though he had a beloved elder brother who had been brought along under a different formula]. My childhood was deadly uninteresting. I was not allowed to play with other children because it was feared I might catch germs of one kind or another."†

When first sent to school at age five, he was completely surprised by the discovery of what it was like to play with

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* Personal journal of David Locke Webster II, n.d.

† *Ibid.*

other children. His playmates found that he was under motherly orders to keep his hat on (to avoid fatal pneumonia), so naturally they knocked it off. "It was a completely wrong introduction to dealing with humanity, and I don't think I have ever really recovered from it,* he said in his eighty-seventh year.

Had Webster's career been steered by modern aptitude tests, he would never have become a physicist, for, as he confessed in later life, his most difficult elementary subject was arithmetic. His later high-level aptitude for mathematics first became perceptible in the later courses of algebra and geometry, subjects that he found easy and fun.

Other detested experiences of Webster's schooling phase were compulsory dancing lessons (from about age eight), compulsory piano lessons (beginning at about ten and completely ineffective), and school athletics. He liked bodily activity and suffered from no handicapping physical disabilities, but he was deadly sick of being regimented in every way and came to the point of automatically opposing any new thrust of it.

Another form of systematic observance, which began in early childhood but never paid off to the satisfaction of those who administered it, was religious training. Perhaps it would have been more effective had it not been so competitive. He has written that he was "dragged every Sunday to one church or another . . . [and] . . . all through the Episcopal Sunday School,"† but on weekdays there were other pressures. His scholarly and respected paternal grandfather, a devoted Swedenborgian, bore as much responsibility, by family agreement, for the boy's upbringing as did his mother. In David's early years his mother's time was taken up by "social duties," and he spent most of his time with nursemaids, all Irish

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* *Ibid.*

Catholic, and, it appears, as much concerned with the welfare of his soul as with that of his body.

EDUCATION AND RELATED MATTERS

Such experiences were not without later effect. Early warning about Robert Ingersoll drew him into broadening critical reading. Association with his grandfather led him to appreciate Emanuel Swedenborg as a great scientist considerably ahead of Kant and Laplace on some discoveries. Webster's obligatory Sunday morning studies of the *Episcopal Book of Common Prayer* and the *Creed* made wonderful material for swearing, and he developed an ability at picturesque profanity that stayed with him for life.

Ridiculing religion is a simpler course of action than trying to think it out, and Webster's ironic experiences did not leave him an impious scoffer but a thoughtful agnostic who would sneak attendance at a Catholic mass, to see what it was like, when grounded on some long solo flight. After a sailing or flying near-miss he confessed that he could thank God without believing in him. When required to fill out a "religious preference" blank he would profess agnosticism.

In a later year at Stanford, on his morning walks to the Quad, he developed a good acquaintance with his neighbor, the university chaplain. These peripatetic philosophers wasted little time on trivialities and subsequently the chaplain, an inveterate author, expressed in the frontal pages of a book his gratitude for aid received from "Dr. David Webster, distinguished atheist of Stanford University."

Until he went to a teaching post at the University of Michigan in his twenty-eighth year, Webster had had no experience of public education. His own schooling was in Boston private schools, finishing for Harvard at Noble & Greenough's Classical School. Webster himself wrote "I went from there to Harvard because in those days no one with my

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† Ibid.

background and upbringing would have thought of going to college anywhere else."*

There is nothing to be found in Webster's papers about his undergraduate years at Harvard and almost nothing in the possession of his family. He came through in the usual four years with the much less usual summa cum laude. He seems to have been less than completely satisfied with his record and to have grieved over the presence there of a single C grade.

His mother appealed to the Harvard administration about the disgraceful C and had to be satisfied with the declaration that there was nothing higher that the College could give than a summa cum laude, but if the defeated gladiator would present himself at the president's office, that official would publicly put a wreath of laurel and roses on his brow. It is comforting to know that this record did not denote any complete life switch to middle-of-the-road conformities. It surprises this reviewer of his life to find that the child nonconformist could so abruptly convert to conventional academic ideals of performance and aspiration.

RESEARCH BEGINNINGS

Following graduation Webster went on for the doctorate, working principally under the direction of veteran Professor Theodore Lyman on the optical properties of chlorine gas, a rather unexciting classical field that did not firmly hold his interest beyond the three years of degree work. Phrases like "modern physics" and "atomic physics" were resounding in the halls of science and young searchers and researchers recognized that the old classical fields no longer offered the maxima of either the prizes or the fun. Webster selected the field of X-ray physics, and it was to be the area of his chief research effort for three decades. With his new degree he

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* *Ibid.*

received appointment to an instructorship. He assembled X-ray equipment and went to work on problems of his own choosing.

A brief flashback is necessary here. The three graduate student years were not unmitigated labor: In 1911 Webster met and in 1912 married Anna Cutler Woodman. Little is known about this romance, but he has recorded that he was drawn to her because, unlike most of the girls he knew, she was training herself to do something, to become a teacher. Another strong plus for Anna was that she was just the kind who would like a honeymoon on a sailboat, sharing with him his most beloved avocation. In another year, their family of two girls and two boys started coming.

Back at the research laboratory there were interesting developments. Throughout the first decade of this century, X-rays were used but not understood. Not until 1912 was it uniformly agreed that these rays were waves much like ordinary light and not showers of submicroscopic bullets. As waves, they were in the field of the spectroscopist, but none of his instruments could disperse them or measure their wavelengths. The spectrometer that could do these things had been invented by W. Lawrence Bragg, who used a crystal in place of the familiar prism or grating, and so opened up the science of X-ray spectroscopy.

Webster, with some shop aid, put together an X-ray spectrometer on the Bragg pattern and got started observing the nature of the spectra emitted by the then recently available glass X-ray tubes developed for medical use. He could identify the range of wavelengths the tube emitted when in high voltage operation, and he could measure in an approximate manner the relative output strengths of the different wavelengths he chose to observe.

Before going further with the laboratory data, we interpolate. Experimenters had concluded, before spectrometers came to their aid, that the X-ray power put out by the com mon tubes comprised a wide variety of wavelengths. It was evident that electrons in the evacuated tube were accelerated to high speeds by an applied and measurable voltage but were then abruptly stopped by their impact upon a metal target (in this case, of tungsten). The observed X-rays radiated out from the spot on the target where the electrons. collided, so one had to suppose that the X-rays got their energy from what the electrons surrendered when they were stopped. But it would have been rash to suppose that *all* of the electron kinetic energy reappeared as radiation energy.

Such uncertainties had a serious importance since these were days when the old quantum theory was out on trial. It grew in credibility as it was found applicable to more phenomena. Here was a relatively uncluttered phenomenon involving electrons and a kind of light, a sort of reverse of the phenomenon of photoelectricity, which had been greatly clarified by the application of quantum concepts, particularly the doctrine that light, though demonstrably a kind of wave, dealt out its energy in little mutually exclusive packets. Physicists uncounted had wondered if something of the kind were involved in the X-ray tube. Finding out would require, among other things, quantitative X-ray measurements such as a Bragg spectrometer might facilitate.

Professor William Duane, very senior to Webster in the Harvard science escalator, was well aware of the theoretical problems in the X-ray field and of possible modes of solution. He borrowed Webster's spectrometer and assigned one of his younger men, Franklin C. Hunt, to explore with it the continuous X-ray spectra of tungsten, making careful records of the voltages used to accelerate the electrons. The investigation was a brilliant success, showing that the spectrum was abruptly terminated at its high-frequency end and that the terminal frequency there fitted into the famed Planck-Einstein energy formula, which equates the energy of an electron to that of a radiation particle (or, as we came to say later, to the energy of a *photon).*

Professor Duane promptly reported these findings to a meeting of the American Physical Society and the news went round the world under the title of the law of Duane and Hunt, and so it is still known and described in many a book on many a library shelf.

What the world did not know and found out only very recently* is that the Duane-Hunt experiment had been conceived, nicely performed, and recorded (but not publicized) earlier by David Webster.

Now the writer of this memoir must switch to the first person. I worked beside Webster at Stanford University for more than a decade and talked with him occasionally about scientific matters for three decades more. There was much talk about X-rays, but never did he tell me of his anticipation of Duane and Hunt. I do not know why. I came to know of it only because Webster was a meticulous recorder. In his postmortem effects were an uncounted number of loose-leaf ring binders—certainly between one and two hundred—among which I found his Harvard research notes. They show that he knew exactly what he was doing on March 31, 1915, when he gathered data on the tungsten continuous spectrum, plotted a curve, noted that it terminated on the shortwave side, and calculated therefrom a good value of the Planck constant h. He was aware that he had been scooped, and I do not understand why he did not try to salvage what glory was possible later. Young scientists upward bound are expected to put their best feat forward. Had he been as skilled or as well motivated in the matter of public relations as he was in

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* For a discussion of David Webster's work in this connection, see P. Kirkpatrick, "Confirming the Planck-Einstein equation $h\,v = (\ /2)mv^2$," *American Journal of Physics,* 48(10):803-6.

scientific performance and recording, the law of Duane and Hunt might have been Webster's law all these years.

WORLD WAR I

From Harvard, Webster went in 1917 to an assistant professorship in the University of Michigan, which turned out to be but his entrance vestibule to World War I, the first of two wars in which he was destined to render scientific service. He was caught in the general draft, but found a more useful and attractive occupation in the air service of the Army Signal Reserve Corps. He was not a flyer himself at this stage, but requested and got flight instruction. Here began a personal enthusiasm comparable to that which he had always felt, and possibly inherited, for sailing. His responsibilities started with the testing of flight instruments but progressed rapidly to testing and criticism of the many products of the suddenly created military airplane industry, and also of foreign planes. He has been called the first test pilot in American air service, but he later declined this distinction, since there was then no such recognized title.

His flight instruction took place at Gerstner Field (Louisiana) where he had been sent to have charge of the measuring instruments intended for use in tests of the American modification of the British DH4 airplane. Though aware of his defective hearing and apprehensive about a tendency to airsickness, Webster mastered flying promptly and was told by his French instructor, "Mon Dieu! You fly like ze God Himself!"*

The new American planes were a bitter disappointment, particularly their much-touted Liberty motor, which was replacing the British Rolls-Royce. The ship was entirely disqualified from aerial combat. Webster has written about these trials:

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* Personal journal of David Locke Webster II.

> Then, what really made us boiling mad was to go back to quarters each evening and read in the newspapers that the Liberty motor was doing wonderful work over the lines in France, and that the British and French generals were congratulating our generals on these glorious airplanes when we had the only ones in the world, all six of them, on our hands, more than 6,000 miles from the lines in France. We were too unsophisticated. We should have known that the first casualty in any war is God's truth.*

In spite of these unwelcome findings, Webster stayed with the First World War until Armistice as lieutenant and as captain in the air service of the Army. Nearly all of his work was at Langley Field, Virginia. He remained in the air reserves until 1924.

TO STANFORD UNIVERSITY

With the first war behind him, Webster returned to Michigan but within the year accepted an assistant professorship at Massachusetts Institute of Technology. After a single year at MIT, which at the time was just what the name says, Webster gladly accepted from Stanford University an offer of full professorial status and Physics Department chairmanship. In 1920 Stanford was well known for its unique history and its supposed financial security, but its academic greatness was spotty. Physics was represented by a small, aging faculty, busy at their teaching and little involved in the twentieth century explosion of their discipline. In the Webster appointment Stanford had a young man (thirty-one) of unquestioned keenness, freshly developed in a center of eager scientific progress. His interests, his talents, and his experience showed a seemly balance of instruction, research, and academic citizenship. I, the writer of this account, then a graduate student at Berkeley across the Bay, met the new Stanford hope at interdepartmental physics conferences and recognized the awakening influence.

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* *Ibid.*

STANFORD PROBLEMS AND EVENTS

In taking up the Stanford professorship (which was to run for thirty-four years) Webster was serving an academic employer younger than himself and smaller (2,949 students) than those he had known. The waiting tasks were as large and demanding as such things can be anywhere and left little time to grieve about the lack of a plane or a yacht. There was an atmosphere of good will all around, and his acquaintance with physical sciences other than his own gained the respect of neighboring departments. He had great freedom of action and all the facilities therefor except money.

The Bible almost says that the lack of money is the root of all evil. Among pre-World War II experimental scientists this version had many believers.

One of the things that President Wilbur hoped of Webster was that he might make Physics a significant research department. Webster's own ability in this field seems to have been evident to his Army and academic associates and he had documented it by some fifteen published papers, but his name was not yet highly visible generally. Now it was not only his wish but also his duty to build a creative investigative center at Stanford. Webster has told that in planning for research he scoured the University junkyards to pick up the material that might be converted into instruments for scientific observation and measurement. The construction was often done by the scientist himself with the help of graduate students glad of a chance to earn twenty-five cents an hour. The problem here was to find the twenty-five cents, for there was no research budget as such.

Professors necessarily did things almost incredible to their present-day counterparts. In Webster's first Stanford research, he became carpenter, plumber, lineman, pump cleaner, and freight heaver as occasion required. Fortunately his chief collaborator, Professor P. A. Ross, was well endowed with extra-professorial talents. He was the only glass blower on the Quad, had made telescope mirrors, operated all the then-known machine tools, and liked to make them do tricks beyond the intentions of their designers.

On surveying the instruction going on in his new Department, Webster found it lacking. Students were getting practically no chance to learn about what physicists were calling modern physics, not merely because their teachers were not close followers of twentieth century developments but also because available textbooks were not telling the modern story. Furthermore, Webster criticized the general physics texts then available as being catalogs of facts worth knowing, rather than training manuals for finding out.

So, working with Professor H. W. Farwell of Columbia University and Professor E. R. Drew of Stanford, he produced a new textbook entitled *General Physics for Colleges* in 1923. It was the first in America to give extended and connected treatment to the modern physics, and as such it was valued and adopted. But the insistence on thinking things through, albeit nonmathematically, though popular with the kind of students that *elect* physics, dismayed that greater number who took physics because they had to and who were used to getting grades by memory rather than by understanding.

Books that try to comprehend a rapidly growing field go promptly out of date, and this was perhaps the worst time to do the definitive summary of modern physics. *General Physics* came out in 1923, the year in which Compton confirmed that light comes in particles and Pauli clarified atomic structures with his exclusion principle. In the next year, de Broglie suggested that particles are waves and the arrangements of electrons within atoms became clear. In the year after that, wave mechanics was born. So, in 1926 the second (and last) edition of *General Physics for Colleges* was brought out, and while the ink was drying the wave nature of all matter was proposed and convincingly defended, and Davisson and Germer were experimentally substantiating it for the important case of electrons. In the very next year, Heisenberg declared for uncertainty, shaking the foundations of general philosophies and putting a new one under physics.

THE TEACHING PHYSICIST

As a teaching physicist, Webster worked from a philosophy that has already been indicated. Students must not be allowed to think of physics as an esoteric mystery but rather as a means of understanding the why of what goes on around them and, progressively, of gaining explanations—even quantitative ones—of other phenomena far less commonly observed but pregnant with thrilling implications. Sometimes many questions may be grouped under a similar answer, from which emerges a "law." But in referring problems to laws the teacher must be careful to show that the law is a compact summarizing statement of human observations and not, in itself, a proper object of worship. Webster knew that laws are an enormous convenience, but when he introduced students to Boyle's law or Ohm's law or one of Newton's he was careful not to claim that the usual simple forms were absolutely and in all circumstances correct. In his book the three-letter statement of Ohm's law was carefully hedged with conditions about constancy of temperature, homogeneity, and ambient magnetism. This extreme care about correctness was not found ingratiating by all students. Some teachers can stand up before an advanced class and say, "What I told you last year wasn't quite true." Such methods were not for Webster, and the inquiring and well-motivated minds commended his rigor.

The above might suggest that Webster's lecture style was stiff or pedantically formal. On the contrary, it was conversational, without the meaningless sounds and ungrammatical shortcuts often condoned in such communication. As to the

Webster textbook, users agreed that it was extremely hard to find errors in it, even in the first edition. This writer, an avid critic, never succeeded in finding even one.

Webster was one of a group of physicists of standing who felt that teachers of physics, particularly at college levels, were such accidentally and were often lacking in special training and in opportunities to secure such. Since the American Physical Society did not regard teachers as physicists and elected to ignore their problems, a group including Webster founded the American Association of Physics Teachers in 1930 and solicited a membership that has since built up to nearly ten thousand. He was active in this important organization and in the years 1935 and 1936 served as its president.

At Stanford Webster was employed to be both a teacher and an active researcher in the science of physics, and though he was fond of both activities and performed them expertly, he never recognized any helpful symbiotic relationship between them. In 1957 he wrote:

> Speaking from my own experience, I have found that I could never make really good progress in any research job unless I neglected my undergraduate teaching, letting it coast along with obviously insufficient motive power. Conversely, I could never do much to improve any undergraduate course unless I let the moth and rust have their way with my research apparatus.*

RESEARCH IN X-RAY PHYSICS

Webster's productive researches may be considered in a few separable categories of which the first has to do with X-rays. His pioneer X-ray spectrometer observations have already been cited, and his Bibliography mentions a few other X-ray publications traceable to his brief terms at Harvard, Michigan, and Massachusetts Institute of Technology.

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* *Ibid.*

But ideas developed faster than the possible testing of them, so he carried many of them to California for consideration in the research laboratory he was expected to develop at Stanford.

His personal research efforts on the new job were largely devoted to observation of the bombardment of metallic atoms with electrons and the measurement of the resulting characteristic radiations. If this sounds like a puerile occupation, the reader—even the scientific one—may pardon some amplification. The real purpose of the experiment was to draw out internal information about the atom, that is, about any one of the atoms in a pure sample, let us say, of gold. The collision of an electron with an atom *might* energize the atom, causing it to emit a photon (radiation quantum) of a wavelength peculiar to its species. It was part of the investigator's task to catch and count the special photons, and in Webster's work they were always X-rays. The italicized uncertainty above was necessary because the chance of a productive collision is strongly dependent upon the energy of the bombarding electron. For slow electrons the chance is zero, but with increasing speed that probability abruptly takes on a positive value, and this critical speed or energy is an important datum for the atomic theorist, who is also deeply concerned about how the probability varies with electron energy as bombardment speeds are pushed up.

This dip into atomic science will still leave the lay reader dubious about the usefulness of the early Stanford X-ray investigations, really atomic mechanics investigations in which X-rays were a by-product and a handle. The work was never understood by journalists or by the wives of physicists. Over the perspective of years, one may wonder that it ever succeeded in an era when the directing scientist designed and built his own power supply, cleaned and serviced his own vacuum pumps, and measured his high voltages and his millimicro signal currents with homemade meters. (For a fuller acquaintance with those times, see the Webster Bibliography.)

A more glamorous kind of X-ray research enlivened the Stanford laboratory when Webster and Professor P. A. Ross, about 1925, stepped into the controversial territory of the Compton effect, with clarifying effectiveness.

As a point of information, the Stanford word *klystron,* now heard in all the principal languages, was coined or appropriated after a visit by physicists to the Department of Classical Languages. In ancient Greek it meant something about sea waves, but in the modern definition it has to do with waves of electrons, a klystron being specifically a vacuum tube without a grid but able to control such flow in an advantageous manner utterly new to the world of 1937. Webster did little or none of the inventing but he understood more fundamentally than did its inventor and played a valuable role by elucidating its theory (see Bibliography) and by guiding the poverty-stricken Department through a new phase of contact with University administrators and the extramural world of big industry and manufacture. He lived to see the new idea build buildings for his Department and start its growth to conspicuous world visibility.

WORLD WAR II

Even before Pearl Harbor the war was molding that Department. Foresighted staff members were weighing choices of battle stations for what would be called a physicist's war.

Klystron men were at war work already: the development of the tube had been prompted from the beginning by concern for civilian populations under bomber attack and lacking microwave power for effective radar. Parenthetically we may say here that klystron design, testing, and production did get there on time to play a significant, and perhaps determinative, role in the Battle of Britain and other engagements.

Webster headed the klystron development until its size, commercial commitments, and internally competitive ambitions made it obviously not at home as a subdivision of the small Physics Department. Klystron activity went to Long Island with Sperry Gyroscope Company. To Webster, fed up with klystrons, the Pearl Harbor blitz was timely. His first comment on it was, "Thank God we are in the War before the enemy has had time to destroy all our friends!"* A few days later, he struck out in search of a fitting wartime station. Too old for flying, he considered a few thinking posts and settled into duties as assistant chief of the Army Rocket Research Branch at Aberdeen Proving Grounds in Maryland, and here he served out the duration.

Webster later wrote, "I didn't know anything about rockets, but nobody in America knew much about them; so it was easy to get right up with the best of them."† At that time, the Germans did know much about rockets, particularly large ones, and Aberdeen was developing the bazooka, a tactical weapon carried on the marksman's shoulder; a useful achievement of Webster's group was insuring that this weapon would dispatch its rocket forward instead of sideways into the soldier's head.

In the autumn of 1944, Webster was sent to England and France to judge the relative merits of different rockets. Though a civilian, he went dressed in the costume of a colonel, carrying a card stating that he had the rank of "assimilated" colonel, and furnished with decorations appropriate to that rank to pin on if taken prisoner. At such a point, he would be assimilated into the Army without gambit, a kind of plug for better accommodations for captains. His rocket study began in England and later took him to France and war zones, returning him to Aberdeen for separation in the summer of 1945.

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* Ibid.

† Ibid.

WRITING TASKS

The Webster Bibliography, a part of this memoir, might stand by itself, but a few exceptional items merit individual comment. The *International Critical Tables* was an eight-volume world first as a reference source on the physical properties of all sorts of substances. Spectrum lines were included in its wide coverage, and Webster led the group who did the X-ray spectra. He more or less justified this valued job of dull scholarship by pointing out that in the poverty year of 1929 it required no apparatus or other expense to Stanford.

His airflight competence and enthusiasm had survived World War I, and he had taught classes in "Air Craft Operation" for the Civil Pilot Training Program of the Federal Civil Aeronautics Administration and had come to realize painfully that fliers were still being taught World War I superstitions about the physics of flight and how to cope with its vital problems. Flight training had been cleansed of some of its plain denials of Newton's laws of motion, but not enough in Webster's view, since very few flight instructors had learned to read differential equations of the fourth order, while every airplane understands and promptly obeys two such equations.

Webster bought a 65-horsepower Cub, flew it from California to Washington (more specifically, to College Park, Maryland), and explained that he was the man who could revise their training literature so that it would neither make the trainee dizzy nor the scientist sick. He got the paying job promptly, along with the collaboration of junior authors, and spent the summer of 1940 happily rewriting and flying.

Having been openly critical of some common textbook treatments of electric and magnetic theory, Webster was a natural candidate for membership in American Association of Physics Teachers' committees for review and recommen dation in these fields. It may seem odd that in the twentieth century physics teachers could not all immediately agree upon what should be said about magnets and about electrostatically charged objects, these being matters that have been thought about for millennia. Here questions about experimental truth or mathematical rigor were few and readily answered; but matters of taste, philosophy, historical precedent, and even a little respect for tradition and authority arose to demonstrate that scientists are still humans. The Coulomb's Law Committee report was published in 1950, after two extended summer meetings of committee work. Webster, as human as any, did a great part of the writing and injected a point of view that seems more and more natural and acceptable with the passing years.

If a reader really wants to know what the preceding paragraph has been about, he may well turn to the *Encyclopaedia Britannica* of 1970 and read or browse Webster's fifty-seven-page general article "Electricity," a masterful presentation of a mathematical subject including general relativistic touches without mathematics beyond a little high-school reckoning.

RETIREMENT AND THEREAFTER

His postwar years at Stanford were not Webster's happiest. He has written that upon his return from the war he found himself a misfit at the University. The new University president had replaced him as department executive. His old research quarters had been revised out of recognition and were now occupied by busy younger men with younger problems. He said himself that nuclear research was a young man's game and he had voluntarily written himself out of the klystron empire, even to turning down a piece of the expected royalties.

Ever concerned about the professor's dilemma of serving two masters, Webster now turned from research to teaching and accepted the assignment of putting in order the deplorable engineering and science physics instructional program, which had slipped too much into the hands of unregulated teaching assistants. In this useful job he made himself quite a student reputation. More widely distributed benefit to physics came out of his leading role in the Coulomb's Law Committee discussed earlier.

Following his retirement in 1954, Webster issued a dozen publications of scattered character, including several on relativity matters and some ventures in astrophysics, which were facilitated by a congenial appointment at Ames Research Center, Moffett Field, California, with practically professorial freedom to pursue mathematical research in space sciences. This was a clean slate. Years earlier he had decried the attempts of aging scholars to ignore Nature's cool insistence that they were not permanently productive supermen. He was going to recognize his mental deterioration before other people did and go sail his boat. Now was the time to test such intentions, but the opportunity of an Indian-summer career with old pressures off brought him into a more attractive course and somehow a still productive one. Big modern research organizations, rapidly assembled, are staffed with smart, young, deeply specialized people, who have not taken the time to become broadly educated, even within science. A genial old man who knew so much about so much was a naturally popular consultant, both socially and professionally, in such company.

SAILING AND FLYING

Webster's life included the already mentioned avocational enthusiasms of sailing and flying. The sailing interest was nothing new in the family; his Briggs maternal ancestors had been builders and sailors of Massachusetts ships for two centuries, up to about the Civil War times. The most celebrated of these, the 220-ton Clipper ship *Columbia,* was the first American vessel to double stormy Cape Horn and ply the West Coast waters. She traded in the American Northwest, where she gave her name to a great river, continuing westward thereafter and carrying the flag of the United States around the world for the first time.

Among the Briggs sailing men was a pirate, still spoken of in the family as Uncle Tom. It is recorded of this cousin of Webster's maternal grandfather that he was in and out of English prisons, bearing his fate calmly as a godly man may.

When Webster was ten he and his brother were given a skiff with which they taught themselves to sail by doing it. He later explained that he surpassed preteen playmates in perfecting this art because at that period he didn't give a damn whether he drowned or not. As life took on value, his aquatic instruction continued and he passed the grammar school of seamanship, which was Massachusetts Bay, and the high school, which was Cape Cod.

In the Webster literature are the names of eight wind-borne boats that he owned in whole or in part during his sailing life, and in one of which he and two companions accomplished a round-trip cruise of 2,500 miles, circumnavigating New England, most of Nova Scotia, and slices of New York and Quebec. The closeness of sailing to his heart appears even in the choice of his first wife, Anna.

The only real disappointment about the family move to California was the discovery that the state offered no good cruising for small sailboats. Webster searched and found nothing to meet his Atlantic standards within eight hundred miles, but British Columbia held the family's desires and there they spent several consecutive summers.

Webster was interested in aviation before Kitty Hawk. He wrote, "I always wanted to get up in the air."* Boyhood para chute jumps with a big umbrella ended in crash landings and he remained grounded until World War I, though in the Harvard period he took part in the building of a plane that declined to fly. He has written that World War I gave him an excellent excuse for going aloft and he remembered throughout life the elation of his first solo flight and his half-thinking, half-saying, "My God, here I am at last! *Really flying and in full control!*"*

After military flying his circumstances grounded him for several years, but in the 1930's he was getting nostalgic for the air and took steps. Although a military pilot, experienced in the flying of thirteen plane types of the period, he took his first private pilot's license in 1936 (at the age of fifty-four) and celebrated it by flying under the Golden Gate Bridge. He later acknowledged that that was a dangerous sport in that he might have lost his license for it.

With the talents of a natural seat-of-the-pants flier, Webster brought to the craft his mechanics, mathematics, meteorology, and love of nature. Flying became a fine family activity. Though Anna still preferred boats, his two sons were soon good fliers, relishing the air and finding careers in it. One became a pilot with a commercial airline and the other served as fighter pilot of the U.S. Airforce in World War II. Webster owned five successively more powerful planes. The logbook of one of them came to show at least one landing in each of the forty-eight United States of the time.

IN CONCLUSION

The two strong egos of David and Anna Webster attracted like magnets for a few decades, but a polarity reversal came and brought divorce in 1951. He soon married Olive Ross, a longtime widow of his early X-ray colleague, P. A. Ross. In the nine successful years of this marriage (until Olive's death)

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* *Ibid.*

* *Ibid.*

she rendered him an abundance of human understanding, literary criticism, social guidance, and flight companionship. The dearest friend of his later years was the space scientist, Alberta Alksne, with whom he wrote theoretical papers and toured Australia, New Zealand, and the Barrier Reef.

Webster stopped working at NASA in 1975, when he was eighty-six years old. He was not eager to quit, but years of battling with uremic poisoning had worn him down and he died on December 17, 1976. He retained his curiosity about the world and life to the end, asking, almost at the last, "What's it all about?"

It is not the function of these pages to praise but to recall and commemorate. In summary, David Webster in his thirties was known among physicists of his time as an X-ray man and more particularly as an experimenter rather than as a theorist. This trend of his reputation was an accidental result of his opportunities and no real choice of his own. He was conscious that he had no great gifts of digital dexterity and no kind of apprenticeship in the manual arts of the instrument shop, but at Stanford, in a delicate and budgetless experimental program, any such disadvantages were compensated by his superior understanding of what was being attempted, his mathematical familiarity with its past and presumable future, and his ability to theorize his way out of a dilemma.

In the twenties he was the only possible theorist in the small Department. He came to realize, though none too rapidly, that high-class power in such physics was an essential condition for the future growth and service of a university physics department in either its teaching or its investigative function. In this need he took the strong step of securing the appointment of Felix Bloch (1932), the more to Webster's credit inasmuch as his makeup included a trace of ethnic discrimination.

This would have been an appropriate time to swing the research emphasis of the Department into one of the new productive channels, but Webster preferred to carry on with X-ray observations using more energetic collisions. This simple-sounding extension would have required far bigger budgets than the Department had ever seen; Webster went to the foundations for such support and was turned down. This was his last attempt at major research leadership. William Hansen, meanwhile, pored over cheaper ways to get high-energy electron collisions producing the cavity oscillator, which led to the klystron and to the two-mile linear electron accelerator. In the list of Webster's life achievements the production of Hansen is not the least. This prodigious undergraduate (now long dead) was first Webster's worshipper, then his replacement in advanced lectures, and later his adversary in klystron diplomacy and management.

Webster held the fixed opinion that a university has in its work of teaching and scholarly investigation two separable functions with a degree of competition between them. He felt the dishonesty of spending tuition receipts on the showier activity of research, visible to donors and popular with most of the costly scholars. Opposing this custom in principle, he unavoidably practiced it and confessed in print that he could not serve two competing masters with fairness if he had to divide individual days between them. It was a relief to him that he lived to see research supported in relative abundance from other sources.

Webster never did set his evident capacities and less evident ambitions on any resolute pursuit of maximum professional visibility. He took up the questions of living as they addressed him. His always curious mind was intrigued by the problems of nature and he solved a few. More solutions would have meant more glory, but sometimes it appeared that his payoff was more in the solving than in the solution. It was characteristic that when he visited Hawaii and saw the destructive work of a "tidal wave," he busied himself for two years on tsunami research and determined the effects of certain idealized island forms upon the impacting sea waves. When he learned of the anomalous magnetizations frozen into historic lava flows, it was not long before he was in conference with vulcanologists about causes of the phenomena and their possible use in predicting eruptions.

The impression of Webster's personality was one of strength and gentleness. He was often charming, though certainly with no intent to charm. He had some biases and the grace to conceal them. Though not infallible in dealings with people, he was quite devoid of guile and was irritated by signs of it in others. Since successful diplomacy cannot operate without guile, his had its limits. His judgments of others were confident, but some found his condemnations exaggerated. In general, people liked him warmly and remembered him lastingly. His concern for public opinion was slight and yet detectable.

His memory became richly filled with science items now rapidly becoming historic and with details of personal experiences relevant to many continuing lives. It must always seem a definite human loss when such slowly built files are wiped out without a copy.

David Webster was elected to the National Academy of Sciences in 1923.

Bibliography

1912 On an electromagnetic theory of gravitation. Proc. Am. Acad. Arts Sci., 47:559-81.
On the existence and properties of the ether. Proc. Am. Acad. Arts Sci., 48:509-27.
1913 The theory of the scattering of Röntgen radiation. Philos. Mag., 25:234-41.
1914 The effect of pressure on the absorption of light by bromine and chlorine, and its theoretical significance. Phys. Rev., 4:177-94.
1915 Planck's radiation formula and the classical electrodynamics. Proc. Am. Acad. Arts Sci., 50:129-45.
The intensities of X-ray spectra. Phys. Rev., 5:238-43.
The X-ray spectrum of tungsten at a constant potential. Phys. Rev., 6:56.
Parson's magneton theory of atomic structure. Phys. Rev., 6:54.
1916 The emission quanta of characteristic X-rays. Phys. Rev., 7:403.
The emission quanta of characteristic X-rays. Proc. Natl. Acad. Sci. USA, 2:90-94.
Experiments on the emission quanta of characteristic X-rays. Phys. Rev., 7:599-613.
With H. Clark. A test for X-ray refraction made with monochromatic rays. Phys. Rev., 8:528-53.
Notes on Page's theory of heat radiation. Phys. Rev., 8:66-69.
1917 With H. Clark. The intensities of X-rays of the L series. Proc. Natl. Acad. Sci. USA, 3:181-85.
X-ray emissivity as a function of cathode potential. Phys. Rev., 9:220-25.

The theory of electromagnetic mass of the Parson magneton and other non-spherical systems. Phys. Rev., 9:484-99.

Equations as statements about things. Science, 46:187-89.

1918 The scattering of alpha rays as evidence on the Parson magneton hypothesis. J. Am. Chem. Soc., 40:375-79.

1919 An approximate law of energy distribution in the general X-ray spectrum. Proc. Natl. Acad. Sci. USA, 5:163-66.

The origin of the general radiation spectrum of X-rays. Phys. Rev., 13:303-5.

1920 The intensities of X-rays of the L series. II. The critical potentials of the platinum lines. Proc. Natl. Acad. Sci. USA, 6:26-35.

The physics of flight. J. Franklin Inst., 189:553-80.

Quantum emission phenomena in radiation. Phys. Rev., 16:31-40.

Quantum emission phenomena—radiation. Science, 51:504.

Problems of X-ray emission. Bull. Natl. Res. Council U.S., 1: 427-55.

1921 High-frequency limits of X-ray spectra at different angles from the cathode stream. Phys. Rev., 18:155.

Some X-ray isochromats. Phys. Rev., 18:321-22.

A general survey of the present status of the atomic structure problem. Bull. Natl. Res. Council U.S., 2:336-55.

The present conception of atomic structure. Bull. Natl. Res. Council U.S., 2(part I):335-55.

1922 With H. N. Russell. Note on the masses of the stars. Not. R. Astron. Soc., 82:181-82.

The penetration of cathode rays in molybdenum, and its effect on the X-ray spectrum. Phys. Rev., 19:545-46.

A device for timing ionization currents accurately. Phys. Rev., 19: 546.

1923 With A. E. Hennings. The penetration of cathode rays in molybdenum. Phys. Rev., 21:301-11.

With A. E. Hennings. X-ray isochromats of molybdenum. Phys. Rev., 21:312-25.

With E. R. Drew and H. W. Farwell. *General Physics for Colleges*. New York: D. Appleton-Century Co.

1924 The distribution of energy in the continuous X-ray spectrum. Radiology, 2:214-21.

A possible explanation of tertiary line spectra in X-rays. Proc. Natl. Acad. Sci. USA, 10:186-90.

A possible explanation of tertiary line spectra in X-rays. Phys. Rev., 23:663.

1925 With P. A. Ross. The Compton effect with no box around the tubes. Proc. Natl. Acad. Sci. USA, 11:56-61.

With P. A. Ross. The Compton effect: Evidence on its relation to Duane's box effect. Proc. Natl. Acad. Sci. USA, 11:61-64.

With P. A. Ross. The Compton effect with hard X-rays. Tungsten K series. Proc. Natl. Acad. Sci. USA, 11:224-27.

With P. A. Ross. The Compton effect with hard X-rays. Phys. Rev., 25:714.

With P. A. Ross. The Compton and Duane effects. Phys. Rev., 25:235.

1926 The use of British units in the teaching of mechanics. Bull. Am. Phys. Soc., 1:1-7.

The teaching of physics, with special reference to the teaching of physics to students of agriculture. Bull. Am. Phys. Soc., 1:7-25.

Continuity of the X-ray spectrum at a wave-length twice the shortwave limit. Phys. Rev., 27:638.

1927 Direct and indirect ejection of K electrons by cathode rays. Phys. Rev., 30:365.

Large losses of energy by cathode rays: Ratio of the probabilities of the two types. Phys. Rev., 30:365.

Direct and indirect production of characteristic X-rays. Proc. Natl. Acad. Sci. USA, 13:445-56.

1928 Direct and indirect characteristic X-rays: Their ratio as a function of cathode-ray energy. Proc. Natl. Acad. Sci. USA, 14:330-39.

K-electron ionization by direct impact of cathode rays. Proc. Natl. Acad. Sci. USA, 14:339-44.

K-electron ionization by direct impact of cathode rays. Phys. Rev., 31:1118.

With H. Clark, R. M. Yeatman, and W. W. Hansen. Intensities of K-series X-rays from thin targets. Proc. Natl. Acad. Sci. USA, 14:679-86.

With R. M. Yeatman. The ballistic method of ionization measurement with a quadrant electrometer. J. Opt. Soc. Am. Rev. Sci. Instrum., 17:248-53.

With R. M. Yeatman. Recombination of ions in the chamber of an X-ray spectrometer. Phys. Rev., 32:325.

1929 With W. W. Nicholas and M. Siegbahn. Emission of X-rays, wavelengths and data on absorption limits. In: *International Critical Tables,* vol. 6, pp. 23-49. New York: McGraw-Hill.

With W. W. Hansen. The relation between the continuous and line spectra of X-rays from thin targets. Phys. Rev., 33:635-36.

1931 With H. Clark and W. W. Hansen. Effects of cathode-ray diffusion on intensities in X-ray spectra. Phys. Rev., 37:115-35.

1932 With W. W. Hansen and F. B. Duveneck. Probabilities of K-electron ionization of silver by cathode rays. Phys. Rev., 42:141-42.

With W. W. Hansen and F. B. Duveneck. Measurement of X-ray intensities as functions of voltage, up to 180 kv. Rev. Sci. Instrum., 3:729-49.

1933 With W. W. Hansen and F. B. Duveneck. Relative intensity of the silver K-lines from a thick target as a function of voltage and emergence angle. Phys. Rev., 43:385.

With W. W. Hansen and F. B. Duveneck. Probabilities of K-electron ionization of silver by cathode rays. Phys. Rev., 43:839-58.

With W. W. Hansen and F. B. Duveneck. X-ray line intensities and cathode-ray retardation in thick targets of silver. Phys. Rev., 44:258-64.

With L. T. Pockman and P. Kirkpatrick. Probabilities of L ionizations of Au by cathode rays. Phys. Rev., 44:130-31.

1934 Vacuum-leak hunting with carbon dioxide. Rev. Sci. Instrum., 5:42-43.

Facing reality in the teaching of magnetism. Am. Phys. Teach., 2:7-10.

Current progress in X-ray physics. Science, 79:191-97.

Unscrambling the dielectric constant. Am. Phys. Teach., 2:149-51.

On the teaching of magnetism. Am. Phys. Teach., 2:179-80.

With L. T. Pockman and P. Kirkpatrick. X-ray line intensities in thick targets of nickel. Phys. Rev., 45:151.

1935 With W. W. Hansen and F. B. Duveneck. Errata: Ionization area of He and Bethe's theory. Phys. Rev., 47:699.

With W. W. Hansen and P. Kirkpatrick. Electron optics of a 3000 kv X-ray tube. Phys. Rev., 48:486.

1936 With W. W. Hansen. Electrostatic focusing at relativistic speeds. Rev. Sci. Instrum., 7:17-23.

1938 Contributions of Edwin Herbert Hall to the teaching of physics. Am. Phys. Teach., 6:14-16.

With N. C. Little, F. W. Warburton, M. W. White, S. R. Williams, and W. H. Michener. What is the meter-kilogram-second system of units? Am. Phys. Teach., 6:144-51.

1939 Cathode-ray bunching. J. Appl. Phys., 10:501-9.

Surface currents in deep tidal waters. Science, 90:107-8.

The theory of klystron oscillations. J. Appl. Phys., 10:864-72.

With L. T. Pockman, K. Harworth, and P. Kirkpatrick. Probability of K ionization of nickel by cathode rays. Phys. Rev., 55:682.

1940 The ground school of the Civilian Pilot Training Program. Air Facts, 3:16-27.

Perceptual disorientation during landing of airplane. Science, 92:1-3.

1941 With L. T. Pockman. New techniques for making thin targets. Rev. Sci. Instrum., 12:389-92.

With D. J. Brimm, Jr., and J. R. Cram. *Flight Instructor's Manual,* Civil Aeronautics Bull. no. 5, 3d ed. Civil Aeronautics Administration. 146 pp. Wash., D.C.: U.S. Govt. Print. Off.

With D. J. Brimm, Jr., and J. R. Cram. *Civil Pilot Training Manual,* Civil Aeronautics Bull. no. 23, 2d ed. Civil Aeronautics Administration. 334 pp. Wash., D.C.: U.S. Govt. Print. Off.

1942 Velocity modulation currents. J. Appl. Phys., 13:786-87.

1946 Forces on ferromagnets through which electrons are moving. Am. J. Phys., 14:360-64.

Forces on ferromagnets through which electrons are moving. Phys. Rev., 70:446.

1947 With L. T. Pockman, P. Kirkpatrick, and K. Harworth. The probability of K ionization of nickel by electrons as a function of their energy. Phys. Rev., 71:330-38.

With L. T. Pockman, P. Kirkpatrick, and K. Harworth. Probability of K ionization of nickel by electrons. Phys. Rev., 71:470.

What shall we say about airplanes? Am. J. Phys., 15:228-37.

1950 With W. F. Brown, Jr., N. H. Frank, W. H. Michener, C. C. Murdock, and E. C. Kemble. The teaching of electricity and magnetism at college level. Am. J. Phys., 18:1-25; 69-88.

1951 Masses of carriers in conductors. Phys. Rev., 82:808-9.

1953 Roentgen ray physics. In: *The Science of Radiology,* ed. Otto Glaser, pp. 39-63. Springfield, Ill.: Charles C Thomas.

1957 Reminiscences of the early years of the Association. Am. J. Phys., 25:131-34.

1961 Relativity and parallel wires. Am. J. Phys., 29:841-44.

Relativity of moving circuits and magnets. Am. J. Phys., 29:262-68.

1963 Schiffs charges and currents in rotating matter. Am. J. Phys., 31:590-97.

1967 Dynamical friction. Am. J. Phys., 35:186-93.

1970 Electricity. In: *Encyclopaedia Britannica,* pp. 127-86. Chicago: Encyclopaedia Britannica.

With A. Y. Alksne. Magnetic and electric fields in the magnetosheath. Planet. Space Sci., 18:1203-12.

1972 With A. Y. Alksne and R. C. Whitten. Does Io's ionosphere influence Jupiter's radio bursts? Astrophys. J., 174:685-96.

1973 With R. C. Whitten. Which electromagnetic equations apply in rotating coordinates? Astrophys. Space Sci., 24:323-33.