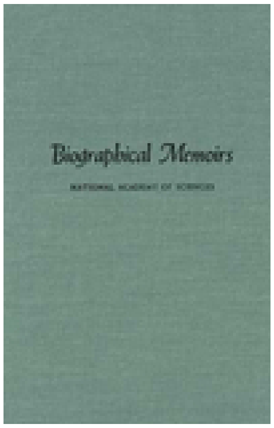

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Preface

The *Biographical Memoirs* is a series of volumes containing the biographies of deceased members of the National Academy of Sciences and bibliographies of their published scientific works. Each biographical essay has been written by a fellow member of the Academy familiar with the professional career of the deceased, with only occasional exceptions. These volumes, therefore, provide a record of the lives and work of some of the most distinguished leaders of American science as witnessed and interpreted by their colleagues and peers.

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, and supported by private and public funds, 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.

Biographical Memoirs

VOLUME XLV

Harold Delos Babcock

January 24, 1882-April 8, 1968

by Ira S. Bowen*

Harold Delos Babcock came from a family whose members have made many contributions to science. Harold himself was elected to the National Academy of Sciences in 1933; his brother Ernest B., a biologist, was elected in 1946; and his son Horace W., an astronomer, was elected in 1954.

Harold Babcock was born January 24, 1882, in Edgerton, Wisconsin, a town of 2,000 inhabitants, twenty-five miles south of Madison. He was the youngest of seven children of Emilus W. and Mary Eliza (Brown) Babcock. His father's ancestry is traced to James Badcock (later spelled Babcock), who was born in England in 1614 and settled in Rhode Island in 1642. His mother's grandparents, German and English, traveled on a raft down the Ohio River from Pittsburgh to Cincinnati to build a home there, about 1800.

Harold's father owned and operated a general store in Edgerton and a farm nearby. The environment, while isolated, was wholesome. Family life was busy and congenial. The father and mother and an older sister had all been teachers. With their help Harold learned to read before reaching school age, and he acquired a lifelong love of music. From an old

* The Academy is indebted to Horace W. Babcock, son of Harold D. Babcock, for his assistance in the final preparation of this memoir for publication after the death of the author.

book, *Natural Philosophy,* and a copy of S. P. Thompson's *Elementary Lessons in Electricity and Magnesium,* he developed an early interest in these subjects, performing many experiments in static electricity and constructing a simple telegraph. A pinhole camera and a few photographic plates, a reward for obtaining a subscription to the *Youth's Companion,* introduced him to photographic techniques.

Harold's health was never robust, possibly because of an attack of rheumatic fever in early youth. He attended public school and had completed one year of high school when, in 1896, the family, except for the two oldest sons, moved to Los Angeles.

In Los Angeles Harold entered the only high school then operating and continued for four and a half years until February 1901. In addition to the usual mathematics, physics, and chemistry, the extra year and a half spent at the school enabled him to take four years of Latin and a year each of German and Greek, as well as short courses in geometry and astronomy. He engaged in dramatics and was president of the school literary society. At the high school he came under gifted teachers who aroused his interest in physics and chemistry. During these years he carried out at home such experimental work in these subjects and in photography as meager equipment would permit.

Marconi's successes in radio communications impressed young Babcock greatly. In 1900 he used the facilities of the physics laboratory of the Los Angeles High School to demonstrate the transmission of radio signals over a distance of one hundred feet. The discharge of a high-voltage condenser was used to produce the signal, which was received by a "coherer" patterned after Marconi's apparatus. Years later Babcock built his own apparatus for receiving early radio broadcasts and in 1940 received his license as an amateur radio operator. The familiarity with electronics obtained in these early studies proved useful in much later work.

In August 1901 Babcock enrolled in the College of Electrical Engineering at the University of California in Berkeley. He found his chief interest in physics and had an opportunity for unscheduled laboratory study in electrical measurements and spectroscopy under the guidance of Professors W. J. Raymond and E. P. Lewis. The death of Babcock's father and his own illness delayed the completion of the requirements for the B.S. degree until 1906. The degree was conferred in absentia the following year.

Summer vacations during these college years brought diverse experiences. In 1903 Babcock was a member of a party making reconnaissance surveys for new construction by the Pacific Electric Railroad. In 1904 he accompanied Dr. H. M. Hall of the Department of Biology of the university on a five hundred-mile botanical collecting expedition in the southern High Sierra. For two months, in the tradition of John Muir, the party lived in magnificent scenery in regions remote from settlements and often without trails. Several peaks, including Olancha Peak and Mount Whitney, were climbed. The exhilaration of this experience was lasting.

In July 1906 Babcock received an appointment as laboratory assistant at the National Bureau of Standards. At the bureau he and Edward B. Rosa made an extensive study of the instability of laboratory standards of electrical resistance. They found the cause to be fluctuations in atmospheric humidity, a result at first disputed but later confirmed by the corresponding bureaus in England and Germany.

Babcock was united in marriage to Mary G. Henderson in 1907. To this union one son, Horace, was born in 1912.

After a few months of teaching physics at the University of California, in 1908, Babcock was invited by George E. Hale to join the staff of the Mount Wilson Observatory of the Carnegie Institution of Washington; this he did on February 1, 1909. He continued this connection with the observatory for the remainder of his active life.

At Mount Wilson, Babcock's first assignment was the photography of selected star fields at the Newtonian focus of the newly completed sixty-inch telescope as part of Professor J. C. Kapteyn's program for the study of the structure and kinematics of the Galaxy. The plates obtained provided some of the first evidence for interstellar absorption of light. Later Babcock collaborated with Walter S. Adams in a spectroscopic program using the 60-inch telescope. Very high-dispersion spectrograms of seven of the brightest stars and some five hundred spectrograms of fainter stars at lower dispersions were obtained.

In 1896 Zeeman had discovered that spectral lines emitted in a strong magnetic field were split into three or more components, the width of the pattern of lines being proportional to the strength of the field. Twelve years later Hale observed with the sixty-foot solar tower telescope on Mount Wilson the same splitting of the lines coming from sun spots. Obviously, the Zeeman effect provided a powerful tool for the study of magnetism in astronomical bodies. The number of components into which a line is split, however, and the ratio of the width of the pattern to the magnetic field, vary from line to line and from chemical element to chemical element. Extensive laboratory work was obviously required before the method could be applied to astronomical studies. Using newly developed equipment that provided fields of up to 35,000 gauss, Babcock made detailed observations of the Zeeman patterns in vanadium and chromium, two elements whose lines are prominent in the solar spectrum.

With the development of atomic structure theory and, in particular, of the vector model early in the 1920s, it became possible to predict the structures of the complicated Zeeman patterns and their widths as functions of the strength of the magnetic field and certain atomic constants. At that time Babcock returned to the problem and from very precise measures of the width of the Zeeman patterns and of the magnetic fields was able to obtain one of the most accurate values then available for *e/m* or the ratio of the charge of the electron to its mass.

During the first decades of the present century, spectroscopy experienced a tremendous development. This was especially so in astronomy, since it was realized that spectroscopy held the key to many problems, including chemical compositions, temperatures, pressures, radial velocities, and magnetic fields of astronomical bodies. Progress in most of these problems depended on very precise measurement of wavelengths, and this in turn required accurate and easily reproducible standards for comparison. The publication of Rowland's *Preliminary Table of Solar Spectrum Wavelengths* between 1895 and 1897 provided the first such set of standards. They were used as the basis for both laboratory and astronomical observation of spectra for the next quarter century. However, by the second decade of the present century it became evident that these Rowland wavelengths were not only too large by 0.0036 percent, but had erratic fluctuations of up to 0.03 to 0.04 A throughout the range from 3000 to 7330 A.

One of the first problems here was to find a new source, preferably a laboratory source, that produced a large number of lines well distributed through the ordinarily observed spectral range and the wavelengths of whose lines remained constant under all ordinary conditions of operation. The first criterion, a satisfactory distribution of lines throughout the spectrum, was satisfied by an electric arc between iron electrodes. Beginning in 1914, Charles E. St. John of the observatory staff and Babcock carried out an extensive study of the second criterion, the constancy of the wavelength of the light emitted from various points in such an arc under different conditions of operation using the most precise techniques available, namely, a Fabry-Perot interferometer combined with a grating. In general, they found substantial wavelength shifts of many of the lines, depending on operating conditions and the position in the arc from which the light originated. However, by carefully defining the operating conditions and the location of the source of the light, they were able to achieve highly reproducible results. The specifications for the arc that they set up were later adopted officially by the International Astronomical Union for the source of the iron wavelength standards.

Using this standard source, St. John and Babcock proceeded in 1921 to the measurements of the wavelengths of the lines emitted. Later, in 1927, Babcock repeated many of the measurements and measured additional lines. For adoption as an official wavelength standard, the rules of the International Astronomical Union required that at least three independent observers agree on the wavelength of a line within certain very close limits. The values by St. John and Babcock were used as one of these three measures.

Because of the important role that these studies played in the establishment of the basic wavelength standards, Babcock was asked in 1925 and again in 1928 to serve as president of the Commission des Etalons du Longueur d'Onde et des Tables de Spectres Solaires of the International Astronomical Union.

Using the same precision techniques, St. John and Babcock then investigated certain pressure-sensitive lines in the solar spectrum to measure the pressure in the photosphere. They also studied the constancy of the wavelengths of both terrestrial and solar lines in the solar spectrum as a means of detecting motions in the earth's and the sun's atmospheres.

Having established the necessary wavelength standards and investigated the constancy of the wavelengths of the lines in the solar spectrum, St. John and Babcock, with the assistance of

Charlotte E. Moore, Louise M. Ware, and Edward F. Adams, carried out a revision of Rowland's table of solar wavelengths. All wavelengths were related to the new wavelength standards. A large fraction of the lines that had not been classified as to chemical element in Rowland's list were classified on the basis of later laboratory work. Temperature classifications and excitation potentials were added when available. The tables were extended from Rowland's limit in the red at 7730 Å to 10,218 Å on the basis of new observations. A total of some 22,000 solar and terrestrial lines were listed. The volume containing these results was published in 1928 and at once became the basis for many investigations of both the sun and other stars.

In 1947 Babcock and Charlotte Moore published a second volume repeating many of the earlier infrared measurements and extending them to 13,500 Å in the far infrared. The following year Babcock, with Miss Moore and Mary F. Coffeen, extended the observations of the solar spectrum in the ultraviolet to 2935 Å and increased the accuracy and detail of the measurements from 3063 Å to the former limit.

These observations of the solar spectrum included a large number of sharp lines originating in the earth's atmosphere, especially in the red and infrared. Many of these lines were caused by absorption by the oxygen molecule. Observations made when the sun was near the horizon and the light passed through some hundreds of kilometers of air brought out many very faint lines that could not have been observed otherwise. In 1927, G. H. Dieke and Babcock published the wavelengths of these lines and their classification as bands of the oxygen molecule, each of whose atoms was considered to be of mass 16. A little more than a year later Giauque and Johnson of the University of California noted that several of the faint bands could be explained best as arising from an $^{16}O^{18}O$ molecule. Babcock again went over the observational data and listed a number of hitherto unclassified lines. Some of these proved to be missing $^{16}O^{18}O$ lines, while some of the faintest were caused by the $^{16}O^{17}O$ molecule. From measures of the relative intensities of the lines of $^{16}O^{16}O$, $^{16}O^{17}O$, and $^{16}O^{18}O$, Babcock was able to estimate the relative abundances of the ^{16}O, ^{17}O, and ^{18}O isotopes. Likewise Birge and Babcock were able to fix the relative masses of the ^{16}O and ^{18}O isotopes from the constants of the band structures. Some twenty years later, Babcock and Louise Herzberg, using new measurements, made new precision determinations of the constants of the $^{16}O^{16}O$, $^{16}O^{17}O$, and $^{16}O^{18}O$ molecules.

This discovery that oxygen had isotopes of mass seventeen and eighteen and that ordinary oxygen was a mixture of these with the predominant isotope of mass sixteen had a fundamental impact on the atomic weight systems as determined from chemical analyses and from mass-spectrograph observations. Because of the procedures used, the chemical system was relative to the average weight of all isotopes of oxygen, while the mass-spectrograph system was relative to the mass of the ^{16}O atom alone. Since the basic assumption of both systems was that the atomic weight of oxygen was exactly sixteen, it became necessary to shift one or the other system to a new base to bring them into agreement.

In 1923 Babcock used the interferometer techniques that he had developed to make the first precise measurement of the wavelength of the brightest but as yet unidentified line in the spectrum of the aurora—the well-known "green line." He achieved at least a one hundred-fold increase in accuracy, showing that the line's wavelength was 5577.350 Å and that its width was less than 0.035 Å. This led to its identification as due to a forbidden transition in the oxygen atom.

Starting in 1912, a very large engine for ruling diffraction gratings was designed and constructed at the Mount Wilson Observatory under the direction of J. A. Anderson. Because of the friction and flexure inherent in such a large engine, it never succeeded in ruling the very large gratings for which it was designed. When the 200-inch telescope project was initiated, in 1928, Anderson was made its executive officer, and Babcock was asked to take charge of the ruling of gratings. A careful review of the program indicated that for the ruling of small and moderate size gratings (up to 10 x 7 inches), a smaller engine would have a much larger probability of success. Under Babcock's direction, such an engine was designed and constructed between 1928 and 1932 by Francis G. Pease, Edgar C. Nichols, Clement Jacomini, and Elmer Prall. In the course of this development much attention was given to the selection and the shaping of the ruling diamond in order to control the exact shape of the grooves ruled. With the proper groove shape, it is possible to throw most of the incident light into one order of the spectrum. A still further increase in grating efficiency was achieved by Babcock by shifting from speculum metal (an alloy of tin and copper) to a coat of aluminum evaporated onto glass as a ruling surface. These procedures were so successful that the gratings he produced had a higher efficiency than a prism train of the same dispersive power. Moreover, when ruled on aluminum evaporated onto Pyrex blanks, the gratings had a sensitivity to temperature only about one twenty-fifth of that of prisms. Because of these advantages, all prisms in the spectrographs at Mount Wilson were replaced with Babcock's gratings, and noteworthy improvements were achieved in resolving power, speed, and stability.

On Babcock's retirement from regular duties at the observatory on February 1, 1948, he was asked to continue the supervision of the ruling engine for another year. By the end of that year the ruling engine was in regular production of grating up to 6 x 7.5 inches, which approached closely the capacity of the engine. The gratings produced in this and the following year met the needs of the large spectrographs of the 200-inch Hale telescope.

After Hale's discovery of magnetic fields in sunspots in 1908, Hale and a number of collaborators had attempted for many years to detect and measure the general magnetic field of the sun, but were never able to achieve conclusive results. Babcock began work on the problem in 1938 using a Lummer plate, which provided somewhat higher resolution than had been used in the earlier studies. The photographs obtained, however, failed to yield a definite answer.

Shortly after World War II, Babcock and his son Horace attacked the problem again, using new optical and electronic techniques that had been developed since the earlier studies. The Babcocks achieved not only unambiguous measures of the field, but were able to push the sensitivity to the point that it was possible to scan the sun's surface rapidly and plot the detailed distribution of the intensity of the field over the surface. A program was then set up for producing daily maps of this distribution of the field. In the course of these observations it was found that this general magnetic field of the sun reverses with the eleven-year period of the sunspot cycle.

Babcock participated in the Mount Wilson Observatory expeditions to observe solar eclipses in 1918, 1923, 1930, and 1932.

In World War I Babcock served in the Research Information Service of the National Research Council. Later he engaged in supersonic research that was part of an antisubmarine effort. In World War II he served as consultant on a number of projects and produced special ruled surfaces for the Manhattan District, the U.S. atomic bomb project.

In summary, Harold Babcock's scientific life was devoted to pushing the precision of measurements and of techniques to the furthest possible limits. In spectroscopy this resulted in a set of standards that are basic to most spectroscopic measures in both astronomy and physics. His accurate measurements of the oxygen bands provided the basis for the discovery by Giauque and Johnson of the isotopes of oxygen of mass seventeen and eighteen, which required a major revision of the atomic weight system. His development of precision techniques made possible the final solution of the problem of the general magnetic field of the sun and the ruling of the first large gratings of high efficiency for astronomy.

Harold Babcock's results were never published until they had been carefully considered and were fully established on a sound basis. Underlying this patience and thoroughness was an unusual awareness of nature. As Gerald E. Kron remarked in 1953, when presenting to him the Bruce Medal of the Astronomical Society of the Pacific, Babcock was a person with a high degree of interest in his environment and in people. He sought to understand and appreciate in depth the elements of nature that he encountered, on whatever scale, and he had the ability to transmit this appreciation, especially to younger associates.

Always considerate of his colleagues, he unobtrusively accomplished many kindnesses for them and their families, especially in later years. He died suddenly on April 8, 1968.

Babcock was a member of the American Association for the Advancement of Science and received its Pacific Division Prize in 1929. He was a member of the American Physical Society, the American Astronomical Society, and the Astronomical Society of the Pacific and was an Associate of the Royal Astronomical Society. The University of California conferred the honorary LL.D. degree on him in 1957.

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KEY TO ABBREVIATIONS

Astrophys. J. = Astrophysical Journal

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

J. Inst. Metals = Journal of the Institute of Metals

J. Opt. Soc. Am. = Journal of the Optical Society of America

Phys. Rev. = Physical Review

Phys. Soc. London Opt. Soc. = Physical Society of London Optical Society

Popular Astron. = Popular Astronomy

Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences

Publ. Am. Astron. Soc. = Publications of the American Astronomical Society

Publ. Astron. Soc. Pacific = Publications of the Astronomical Society of the Pacific

Trans. Internat. Astron. Union = Transactions of the International Astronomical Union

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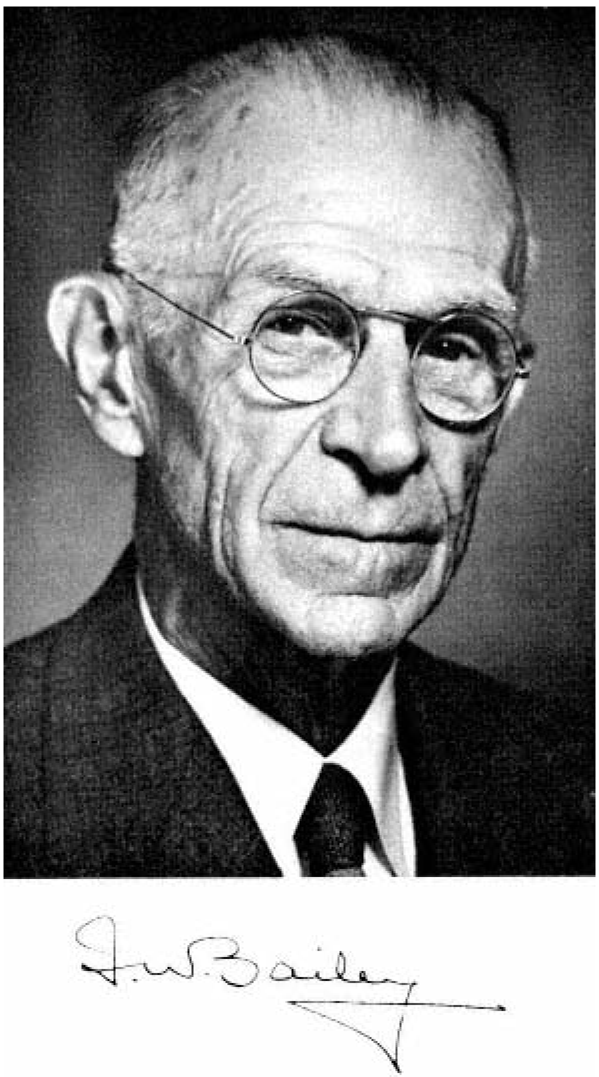

Irving Widmer Bailey

August 15, 1884-May 16, 1967

by Ralph H. Wetmore

Irving Bailey died on May 16, 1967, in his eighty-third year. He had had symptoms of a cardiac disability for some years, but no serious problem had arisen until he was subjected to a subterminal coronary occlusion in the laboratory one morning.

His early arrival that day was as usual. For the fifty-eight years of his successive appointments at Harvard University, the institution at which he had spent his entire professional life, he was always the first or one of the first to arrive each morning. His hours were long, and his concentration on the task at hand was complete. This intentness of purpose along with a natural ingenuity and mechanical ability permitted him in those depression years between the two world wars to master his field. He became nationally and internationally recognized as expert in all aspects of morphological botany, ranging from cytology to anatomy, from evolutionary trends to phylogeny and taxonomy, from organic chemistry to wood structure and wood technology, and from silviculture to preservation of forests. Newer instrumentation and improved quantitative methods have extended the margins of the wide range of knowledge that emanated from I. W. Bailey's laboratories, but little modification has become evident as yet in those fundamental principles and those patterns of organization and function in plants to which he gave his full attention.

To understand how much Irving Bailey's entire life was influenced by his early life, to envision the man and his chosen field of endeavor, one must have a view of his early years. The only son of Solon Irving Bailey and Ruth Elaine (Poulter) Bailey was born in Tilton, New Hampshire, on August 15, 1884. His father, at that time thirty years of age, was Headmaster of Tilton Academy. He had been awarded his B.A. degree at Boston University in 1881 and had already shown a predisposition toward astronomy, initiated when he was twelve years of age in his excitement, it is said, over the last great display of the Leonid meteors on November 13 and 14, 1866.

In the year of Irving's birth his father, then teaching a course in elementary astronomy at Tilton Academy, had written to President Eliot of Harvard University asking about possible opportunities to pursue studies in astronomy at that institution. President Eliot turned the letter over to Professor Edward C. Pickering, Director of the Harvard Astronomical Observatory, who was forced to write Solon Bailey that no opening then existed. Three years later, however, Mr. Bailey again wrote Director Pickering that, since he understood the observatory had received sizable funds in the intervening years, he was still hopeful of pursuing graduate study there. Pickering's reply intimated that Bailey might begin his studies for the master's degree if it was also possible for him to act as an unpaid assistant in the observatory.

Enrolled as a degree candidate in 1887, Solon Bailey agreed to spend about forty hours weekly as an assistant. Within a few weeks, Pickering found Bailey's work so satisfactory that he began paying him a small salary and recommended that he be given course credit toward his degree, which he obtained in 1888. He was at once appointed to head a two-year expedition to Peru to find a site and make plans for a high mountain observatory for study and photography of stars in both southern and northern hemispheres under as favorable meteorological conditions as possible.

Pickering and Bailey chose a part of the Andes near the town of Chosica as promising for temporary study, this area being reasonably near the city of Lima with its port facilities at nearby Calleo. Careful planning enabled Solon Bailey with his wife and their three and one-half year-old son to arrive in San Francisco and embark for Peru on February 2, 1889, aboard the S.S. *San Jose* of the Pacific Mail Company. They were joined in Panama by Solon Bailey's brother, a professional photographer. It is stated that the accompanying baggage and material for building a planned, prefabricated observatory and living quarters for the family and staff comprised one hundred units, all of which had to be landed at Calleo and eventually transported over an eight-mile mountainous trail. At times, the trail had to be specially constructed to permit movement of equipment to a then-unnamed peak—later named Mount Harvard—above Chosica, some 16,500 feet above sea level. The next two months were spent in the selection of an exact site, in moving equipment and material, and in setting up the buildings and apparatus. The ensuing winter and early spring—May to October—were very satisfying to the astronomers. On the four-year-old Irving this all made a lasting impression; he had no time to be lonesome. Memories of these experiences persisted throughout his life and indeed enriched both his teaching and research.

Irving's father and uncle utilized the following rainy and foggy season to explore for and select a more favorable site, both inland and at high altitude, for a permanent observatory station. Their reports resulted in Pickering's choosing a site above Arequipa at an altitude of more than 8,000 feet above sea level.

The task of dismantling and moving buildings and equipment, and the valuable photographs and data not yet sent to Cambridge, was carried to a successful conclusion just before William Pickering, brother of the director, arrived in January 1891, with his staff to operate the station. The Bailey's with young Irving were able to leave on May 9, sailing through the Strait of Magellan on their way to Europe before returning to Cambridge.

Two years later, on February 25, 1893, Solon Bailey, with his wife, his eight and one-half year-old son, and two staff members, arrived again in Arequipa, this time for a first five-year sojourn, to replace William Pickering and his staff. By May (autumn), all was in order at the mountain station for productive work. Life at the station was busy around the clock. Because of arrangements made with the Peabody Museum at Harvard, archaeology became a recreational occupation. Investigation of Inca tombs and ruins proved fascinating to Professor Solon Bailey and had a profound and lasting effect on Irving. The latter in those early years accumulated many significant artifacts that he always took great pride in showing, and only in late life did he ultimately turn them over to the Peabody Museum.

The second expedition of the Bailey's to Peru was extended to 1905. In these twelve years, among other happenings, the personnel of the Arequipa Station had been subjected to a local revolution and to temporary isolation and privation. They were out of contact with the world for more than two weeks.

This account of the early life of Irving Bailey during the two expeditions to Peru has been reported in order that the reader may visualize its enduring effect on him. He himself states in his fifty-year Harvard class report, "I now realize that my reactions and activities in college and subsequent to graduation were profoundly influenced by the fact that eight of the first thirteen years of my boyhood were spent in remote parts of South America rather than in the environment of New England. Having no formal and stereotyped education until the age of thirteen, having no playmates of my own race and age, I was forced to rely upon my own resources for interests and activities. . . . " His self-reliance, independence, ability to make decisions, powers of observation, quick reaction time, necessity for considered judgments—all were important characteristics that, if not acquired, were enhanced in his lonely boyhood and served him well throughout his life. Irving Bailey was never timid in his actions; he was, however, never gregarious. He had many acquaintances; he had fewer friends, but to these he was lastingly close.

In early 1897, when Irving was thirteen, he was sent back to Cambridge to begin an orderly pattern of education. From Cambridge Latin School he was admitted to Harvard College in 1903. He received his A.B. degree in 1907, *magna cum laude,* having also earned membership in Phi Beta Kappa. In his fifty-year Harvard class report he also stated, "In college, I browsed around in history, chemistry, geology and meteorology, but it was not until my senior year that speeches of Gifford Pinchot and President Eliot induced me to undertake a career in forestry, particularly owing to the appeal of an out-of-door profession." So he registered in the Division of Forestry of the newly designated Graduate School of Applied Sciences and received his M.F. degree in 1909. During his second year of graduate study he served as assistant in botany in a course conducted by Professor E. C. Jeffrey. Upon obtaining his master's degree he was appointed instructor in forestry, and in this role he studied lumbering and wood-using industries in the course of preparation for his teaching in forestry.

In 1912, Bailey was appointed assistant professor in the School of Forestry, a unit in the recently aggregated Graduate Schools of Applied Sciences at Harvard. Since undergraduate training of foresters ceased with this reorganization, Bailey's teaching obligations were further reduced. In 1914, the status of the Bussey Institution and that of the School of Forestry were again altered, these two being segregated from the other Graduate Schools of Applied Science as codepartments in a new Graduate School for Research in Applied Biology. Both departments were officially housed in the Bussey Institution, though progressively more and more of the activity in forestry, because of the small number of graduate students involved, was conducted at the Harvard Forest in Petersham, some seventy miles west of Boston.

The eminence of the Graduate School for Research in Applied Biology grew until, in the late teens and early twenties, the Bussey Institution had attained both national and international importance. Though Bailey was appointed associate professor of forestry in 1920, his commitments were entirely to research in the interpretive aspects of plant anatomy rather than to silvicultural practices or to economic aspects of forestry.

During the eight years of his assistant professorship, however, the direct lines of the research Bailey was formulating for himself were retarded. In fact, when they had been little more than initiated they were almost put aside by three important sets of circumstances. In the life of a sensitive young man, the first was devastatingly and lastingly disturbing; the other two were exciting and challenging.

The first of these circumstances stemmed from his two years of stimulative graduate study in the laboratory of Professor E. C. Jeffrey. Here he was in close association with other graduate students, especially Edmund W. Sinnott and Arthur J. Eames, who had been fellow undergraduates at Harvard. All three, as was inevitable in Jeffrey's laboratory, had become strong proponents of evolutionary theory. They were finding in Jeffrey's comparative morphological outlook on plants and animals a challenging *modus operandi* for examining those structural variations that could be correlated collectively with the survival and persistence of groups of organisms under naturally altered environmental stresses.

Bailey and Sinnott were engrossed with the problems presented by numerous cases of herbaceous species and contrasting arboreal species placed taxonomically in the same family. Which of these arose first in geological time and conceivably had given rise to the other? Adherents to both points of view existed. Assiduous collecting of data from groups of fossil and living vascular plants provided abundant material for comparative morphological and anatomical studies. Examination of worldwide distributions of woody and herbaceous taxa in geological periods were made. Published results indicated that Jeffrey and his students had convinced themselves that arboreal plants had preceded herbaceous forms. However, despite this agreement as well as agreement that climatic changes had played a significant part in the survival of the short-lived annual herb, Bailey and Sinnott, on the one hand, and Jeffrey and R. E. Torrey, another of his students, on the other, disagreed on the causal relations of the developmental structural changes by which the annual herbs were enabled to complete their life histories and reproduce within a single favorable season. Arguments were not conducted amicably and reasonably in the laboratories, but were spread publicly in the scientific press. The issues should have been resolved, but the Canadian Scottish Jeffrey found it difficult to envision two sides to any question. The controversy between E. C. Jeffrey and Irving Bailey remained unresolved, and the estrangement persisted throughout their lives.

The second circumstance affecting Bailey's research plans arose with the United States' becoming involved in World War I. In 1918 the Federal Aircraft Production Bureau requested and obtained Irving Bailey's services in a tour of duty at Wright Field, Dayton, Ohio. Bailey was placed in charge of the Wood Section of the Materials Engineering Division of Aircraft Production with the challenge of selecting woods for the manufacture of airplanes. He was later cited for his contribution. He resigned in 1919, as soon as possible after the Armistice, and returned to the Bussey Institution.

Bailey had hardly settled into his research routine, however, when the third circumstance arose. He was strongly attracted by a request of his senior colleague, the Dean of the Graduate School for Research in Applied Biology, Professor William Morton Wheeler, the specialist on ants and related insects. Would Bailey accompany him to the American tropics to study the peculiar and interesting relationships between ants and certain groups of tropical plants, the so-called "ant-plants"? The answer was, "Yes!" Bailey therefore spent much of 1920 in the tropics. The ants to be studied lived in hollow pith or comparable cavities in the younger branches of certain species of plants, mostly trees. When disturbed, they attacked all invaders viciously. From this trip to British Guiana there resulted seven papers, some jointly with Professor Wheeler, reporting on the habits of the ants and their methods of obtaining ingress and egress to their domiciles in the appropriated plant cavities. Once Bailey was initiated in a problem, his interest persisted. One can note in his bibliography a paper in a posthumous publication of Wheeler's in 1942—five years after his death—on British Guiana ant-plants with a section contributed by Bailey.

The period of the 1920s was very significant in Irving Bailey's career. He had found himself impelled to move beyond the then-general conception of the training of foresters and of personnel for forest management and for industrial users of forest products. He was anxious to turn his attention to the little-understood basic problems underlying the growth of trees and their anatomical and physiological organization. The enlargement of his outlet as a result of the demands put upon his knowledge during World War I, coupled with his experiences with the exceedingly diversified arboreal flora of the rain forests of tropical America, had further convinced him that the time was overdue for understanding more of trees if the practice in the United States was to be other than to cut down our forests and exploit their products.

Bailey's studies on the cambium, the circumferential growing layer of woody plants, whether tree, shrub, or woody vine, were begun in 1918-1919. This followed an extensive systematic investigation initiated with a graduate student, Walter W. Tupper, of the range of size variations in vascular elements found in different groups of vascular plants, especially gymnosperms and angiosperms. Considering that all cells of the plant are derived from the isodiametric cells of the apical meristem, the queries, of course, were raised by Bailey, "How do the seemingly organized cell differences arise to produce the heterogeneity within a tree?" and "How much of the heterogeneity is hereditary and how much of the development and differentiation of cell types in the individual is influenced by local cellular conditions?"

Bailey's authoritative knowledge was being called upon increasingly outside of Harvard University. In 1926 he was appointed a member of the Committee on Forest Research of the National Academy of Sciences. This committee, formed at the request of the Chief of the United States Forest Service, Colonel W. B. Greeley, was assigned the task of studying the nation's forest resources to assess our future needs in the production of timber, pulp, paper, and other wood products. A supporting grant was provided by the General Education Board of the Rockefeller Foundation. Professor Bailey was given a leading part in this study. He traveled extensively over the United States and Canada and then spent most of a year in Europe investigating the relative significance of laboratories, tree nurseries, and experiment stations. He wanted to determine what was being done domestically in the conservation and replacement of our progressively disappearing forests and what was being pursued constructively in Europe that would help to enlighten a seemingly little-concerned, forest-destroying group of industrial enterprises in the United States.

As a result of these studies, Bailey and Dr. Herman A. Spoehr of the Carnegie Institution of Washington's laboratory at Stanford University published in 1929 a significant small volume, *The Role of Research in the Development of Forestry in North America.* Bailey always considered this study and report one of the most significant efforts in which he had participated. The carefully considered findings should have had a profound influence on the establishment of a national policy aimed at maintaining a balance of reforestation with cutting and the utilization of our forest resources. The recommendations of the percentages of publicly owned forests versus those in private holdings that should be maintained on a sustained yield basis were considered sound, as were the recommended protective measures against forest fires. The authors were convinced that "the existing economic, social and political status in North America was such as to inhibit for many years an extensive application of intensive European silvicultural methods." Moreover, the greater number of species of trees in the forests of the entire United States, with their naturally different responses to different climates, precluded a simple or single policy pattern. The authors believed that the nation needed a number of research establishments in the climatically diversified parts of our country, preferably in proximity to but not limited by the restrictions of larger academic institutions. The stimulation that would arise from proximity to laboratories in the basic sciences was considered to be a *sine qua non* for broad training and tolerant appreciation of natural problems. The organization and physiology of trees was in need of extensive investigation; Bailey's earlier convictions were carried to a larger audience.

The reports of the Committee on Forest Research of the National Academy of Sciences were turned back to a committee of American foresters, but the problems raised still exist more than forty years later, although some enlightened industries and limited government regulation, state and federal, have followed the main directions pointed out by the Bailey and Spoehr report.

Bailey often spoke of the twenties and early thirties as the best years of his scientific life. In 1927 he was named professor of plant anatomy; this time his appointment was as a full-time member of the research staff of the Bussey Institution, not of the Department of Forestry. Essentially, he continued what he had been doing, in the same laboratory, except when on some outside assignment such as the National Academy project noted above. The joint study program and report by Bailey and Herman Spoehr ended in a close friendship, a friendship that led to an annual summer appointment for Bailey over a ten-year period (1930-1940) as research associate of the Carnegie laboratory at Stanford University. For those ten years, Bailey spent two months each summer carrying on his own work, but at Stanford rather than at the Bussey Institution.

Apart from his accepted external commitments, Bailey worked assiduously with one group of graduate students and research fellows, using newly devised techniques and methods to study the vascular cambium of conifers and angiosperms. With a second group he attempted to discover whether variations in vascular patterns and in diversification of the vascular elements within these patterns had evolutionary significance. The evidence suggested tendencies toward structural and histological similarities among members of a family and striking dissimilarities between taxa that the systematists considered only distantly related. Of course, the fundamental questions arose, "Do variations in structure have survival value? If they do, could one find evidence for an evolutionary or phylogenetic system built on vascular structural-functional elements associated in transport and storage? If so, how would this structural-functional system compare with that system conceived and used by taxonomists, a system based primarily on flowers, or reproductive parts?" As the evolution of flowers unquestionably is related closely to systems of pollination, whether by air, by insects, or by other agents, this idea raised the very important question as to whether the angiosperms could have followed the above-mentioned dual evolutionary pattern, or whether the two patterns would prove to have the same survival of the fittest groups. Fortunately, an excellent statistically minded botanist, Dr. F. H. Frost, arrived as a research fellow in the early 1930s with an urge to tackle this large question. Bailey and Frost increased the existing collections of documented samples of woods from different forested areas of the world, studied the samples microscopically, and tabulated their findings in a statistically controlled pattern. By the mid-thirties, it was evident from their investigations that correlations did exist and that a phylogenetic system of families of angiosperms could be arranged and that, in all likelihood, the system could be followed to subfamilies and even genera as more species became available for study. Moreover, the system so envisioned agreed with that in current use by the taxonomists. In fact, as more research fellows joined the Bailey group, it was increasingly evident that anatomical approaches to taxonomic problems oftentimes resolved the difficulties of the more formal taxonomists. It would seem that this phase of the studies in Bailey's laboratory will continue and that wood collections will become adjuncts to many important herbaria.

During this same period the cambium studies had been progressing steadily. Cell division of cambial cells, many times as long as wide, measuring several thousand microns in length in certain species of conifers, is an amazing cytological phenomenon. The two ends of the dividing cell may still be undivided apparently for hours or days after the two daughter nuclei in the middle of the cell are in interphase stage in seemingly separate cells. Bailey demonstrated for the first time how the single cell-layered cambial cylinder, constantly being pushed outward by the new increments of the internally growing and differentiating tissue of wood, which the cambium leaves behind and within itself, is able to increase its circumferential extensibility by a geometrically complex pattern of cell division and cell elongation, thereby increasing the circumference of the expanding stem or root of the plant. Concomitant studies of physiological parameters in cambial cells, by Bailey and a visiting colleague, Professor Conway Zirkle, were equally revealing. Not only might the physical conditions in vacuoles (liquid-containing cavities in the cells' protoplasm), including the acidity, change from season to season; but not infrequently both acidic and basic vacuoles could be found in different compartments of the same cell.

In the early 1930s, capital funds available to the Bussey Institution for Research in Applied Biology had become so reduced that the university had included space for the Bussey staff and students in its plans for the new Biological Laboratories. Bailey moved from the Bussey Institution in Forest Hills to Cambridge in the summer of 1936.

Bailey's cytological studies on living cells of the cambium of many trees were rapidly made possible by a simple microtome technique he devised for producing sections of live cambial tissue for microscopic examination. The growth of cambial derivatives into new xylem or phloem cells caused him to turn his attention from cell division to the structure of the cell wall itself, wherein is reflected the degree of differentiation of manifold types of cells.

The eleven papers on cell wall structure were the joint effort of Professor Bailey and Dr. Thomas Kerr, a research fellow and close associate of Bailey for some years. The deposition of secondary walls, their sculpturing, and their lignification were studied so penetratingly by differential solubility techniques that, for the first time, cell wall chemistry gave evidence of becoming an interpretable subject. Lignin as a basic constituent in the chemistry of higher plants was shown to be deposited in the interfibrillar capillaries of the wall, between and among the macromolecules of cellulose. Electron micrography in recent years has added to and supplemented the Bailey-Kerr model, but in no basic way has modified it. Polarizing microscopic studies, supplemented by x-ray diffraction investigations, gave support to the interpretation set forth by Bailey and Kerr. Bailey's paper "The Walls of Plant Cells" was given at Stanford University in 1939 in a symposium commemorating the one-hundredth anniversary of the cell theory. It was recognized as one of the outstanding papers of the symposium.

With World War II, academic life for I. W. Bailey gave way, at least in part, to wartime and emergency needs. Like a number of botanical staff members at Harvard, Bailey directed his attention to an Army Engineers' camouflage project. The work entailed utilized much of his and others' time and effort until he was asked by the Provost of Harvard University to prepare a broad and soundly conceived plan for the reorganization and closer integration of the nine establishments constituting Harvard's botanical resources. Bailey accepted this responsibility in the full knowledge that the financially competing, mutually independent organizations, built up by strong individualists, were singly and collectively in financial stress. These establishments originally had been neither envisioned nor planned with concern for the instruction of Harvard's undergraduate or, in most cases, of its graduate students.

To this task, Irving Bailey gave two full years; in it, he had the complete and enthusiastic cooperation of his colleagues as well as the support of administrative officials of the university. In 1945, he finally submitted the results of the study with a proposed plan in a confidential report, *Botany and Its Applications at Harvard,* more commonly known as the "Bailey report." This report commanded the respect of all concerned. It was approved by the Harvard Corporation, and its full implementation was initiated promptly.

First came the planning of a recommended new building, to be located in Cambridge near the Biological Laboratories, to house the combined herbaria and their associated libraries and collections. It was constructed in 1953-1954. The great advance came in the fulfillment of the second part of the plan, the creation of two budgetary and administrative botanical areas: the Institute for Research in Experimental and Applied Botany and the Institute for Research in General Plant Morphology. Professor Bailey was appointed chairman of the latter. The university's reorganization of available finances and the new proximity of professorial staff members, now all members of the Department of Biology, were conducive to a common concern for an overall curriculum and a balanced biology staff. The next few years proved not only the wisdom of the Bailey report in resolving the previous botanical dilemma, but also the success of Bailey's first effort in administration. The careful study and consideration, the group discussions, the final decisions that went into his report proved his administrative capacity.

The reorganization brought much acclaim to Bailey, despite a long-drawn-out court case over the interpretation of the university's handling of a recognized trust, the Arnold Arboretum. This action deeply hurt the sensitive Bailey throughout the ten years before the charge against Harvard was finally denied. In some ways, perhaps, the event that meant most to him personally was the honorary degree conferred upon him by his alma mater in 1955, the year of his retirement. The citation accompanying the award was "Your University salutes you for your direction of botanical study and for your accomplishments in searching in the anatomy of plants, for clues to the miracle of growth." That the degree singled out his international scientific attainment—more than any administrative success he may have achieved—meant much to Irving Bailey, how much only his close friends and his family knew.

In 1946, having modestly and quietly enjoyed the success of his administrative venture, Bailey turned back to his research, perforce neglected for some years. When World War II started, he had been contemplating the return to a question on which he had initiated some work in 1915-1916: Are the vesselless angiosperms primitive? Bailey held the conviction, since proven in his laboratory, that the vascular and associated storage and supporting tissues of plants offer comparative documentary evidence of an evolutionary level of attainment of the plant in the overall phylogenetic history of higher plants. He had thus been awaiting an opportunity to return to the joint study of the vesselless genera: *Drimys, Trochodendron,* and *Tetracentron* (reported on by W. P. Thompson and Bailey in 1916 and 1918). He had found added excitement in the discovery, in Fiji, by a Harvard colleague, Dr. A. C. Smith, of what proved to be a new family of angiosperms which they jointly named the Degeneriaceae in 1942. Bailey decided that it was timely to examine the old and continuously baffling issue of the origin of angiosperms. Would an investigation of all available knowledge of both anatomical and floral organizational patterns of all suspected primitive living angiosperms provide adequate information to suggest the probable nature of the primitive angiospermous plant and its flower? Could this information be correlated with evidence from the tracheids-bearing secondary xylem—that is, would this angiosperm prove to have no vessels or, if vessels did exist, would they be the long, tapering, scalariform, primitive elements?

These studies commanded the full attention of the entire personnel of Bailey's laboratory through the late forties and the fifties until his retirement in 1955. In this decade of assiduous application of new approaches and new techniques in detailed studies of developing flowers, mature flowers, and fruits, of younger and older stems, numerous findings were published that added much to the body of available information. Bailey's group reported six woody genera of the so-called "Ranalean complex," that is, the complex of plants directly or less certainly related to the Ranales—the buttercups and magnolias and their putative relatives—which lacked vessels, possessing only long, tapering, scalariform tracheids as water-conducting elements. These six were in addition to the three genera—*Drimys, Trochodendron,* and *Tetracentron*—established earlier as lacking vessels.

An extensive study of floral organization enabled Bailey and his co-workers, Dr. B. G. L. Swamy and Dr. Charlotte Nast, to develop a new concept of the organization of a primitive flower. The floral parts of these plants proved to be even more foliar in nature than Bailey and his associates had conceived them to be on the basis of earlier, inconclusive evidence. Even the carpels with their ovaries, each enclosing a seed or seeds, were clearly foliarlike appendages that had never opened flat as leaves do, but had remained folded on their midribs. In numerous species, however, the carpels were open at the contiguous margins, the seeds being borne on their upper inner surface, and therefore enclosed, being connected with branches of the three carpellary veins by which they were nourished. Pollen grains, upon being transferred by insect or wind to the glandular apex or margins of the open, conduplicately folded carpels, reached the ovules by developing pollen tubes and growing down along the glandular margins or inner surfaces of the folded carpels, or in many species, along the conduplicate or tubular upper part of the carpel. The stamens also in some species were somewhat foliar, bearing the pollen sacs or anthers embedded in the "foliar tissues," also close to branch veins. *Degeneria vitiensis,* the newly discovered species, proved to be nearest to a hypothetical prototype of a primitive flower, but all degrees of transition to modern stamens and carpels could be found in the various genera and species of these vesselless plants and related species, all with primitive vascular systems. As Bailey suggested in critical papers, early or primitive angiosperms must have been vesselless with flowers of component parts resembling leaves that persisted in flowers or other assemblages, because of the added protection given to their reproductive ovules, which were formed within conduplicately folded foliarlike carpels. The pollen was nourished in its growing microgametophyte or pollen tube stages via the inner tissue of the carpel, along or through which it grew. If plants of the "Ranalean complex" were not the most primitive of living angiosperms, Bailey indicated, they represented at least one main evolutionary line of origin and elaboration, providing the angiosperms should prove to be polyphyletic.

All of these exploratory studies were in hand in 1955 when Bailey reached seventy, the mandatory retirement age. In his laboratory on the first floor of the new herbaria building, with his microtome and his microscope and the wood collection around him that Wetmore and he and certain graduate students had built up to approximately 30,000 microscopic slides, he wrote a few articles, as his bibliography between 1955 and 1959 shows, but to those who saw him daily he was restless.

It was not long before he turned back to the old problem of the development and organization of cactus plants, especially those that were leaf-bearing. Were these leaf-bearing species primitive among cacti? As the Cactaceae, the family of cacti, are generally considered to be vesselless, were they among the primitive angiosperms? These questions raised the research flag again and provided Bailey with the kind of problem he liked. He studied the organization of the leaf-bearing cacti using material provided in part by Professor Norman Boke, an authority on cacti, with whom he had discussed the problem in 1959. In fact, Bailey continued these investigations of leaf-bearing cacti until a few months before his death, the last manuscript being published posthumously. He found that the leaf-bearing cacti possessed vessels in the xylem, but that these were not of the primitive, long, scalariform-pitted type. In the remaining cacti, without leaves, vessels do not generally occur. In substance, Bailey concluded with others that the absence of vessels in the Cactaceae is a secondary and derived modification, correlated with their desert habitats. He did convince himself, however, that the leaf-bearing types with vessels were members of the most primitive tribe of cacti.

His task completed, Bailey cleaned and cleared up his laboratory in early 1967. He did not seem restless or disturbed over not having a research job ahead. However, he continued to arrive early every morning as usual for the short time left to him. He had reported that for some years he had had warnings of a heart ailment. Often he had to sit down on the Cambridge Common to rest during his mile walk to the laboratory. Cold weather aggravated the condition.

Early in May 1967 Irving Bailey was stricken with a heart attack in the herbarium building. As soon as he was found, he was moved to the Harvard Infirmary. There he tentatively recovered, but some days later a second attack caused his death, on May 16, 1967.

Irving Bailey reached international recognition early. His meticulous care in drawing conclusions kept him from publishing erroneous results. The present author knows of no published material of Bailey's that has had to be explained or reinterpreted later because of faulty observation or experimentation or inadequate checking. More recent technical improvements and new approaches have extended the horizons he reached. His judicious evaluation of facts and his self-critical habits left no doubt in his mind of the soundness of his findings, for if there were any doubts, no publication would ensue. Moreover, his succinct, direct, simple writing could not be misunderstood. His one hundred forty-three publications may serve as models of scientific prose.

In addition to his much appreciated and well deserved honorary degree from his alma mater in 1955, Irving Bailey received honorary doctorates in science from the University of Wisconsin in 1931 and from the University of Syracuse in

1961. He was awarded the Mary Soper Pope Medal of the Cranbook Institute of Science in 1954 as "an outstanding plant anatomist and one of America's truly great botanists." In 1955, a *Festschrift* (published in a special number of the *Journal of the Arnold Arboretum,* volume 36), a collection of papers by colleagues, botanical friends, and former graduate students, was presented to him at a retirement dinner. In 1954, he was honored by Dr. Frans Verdoorn, editor of *Chronica Botanica,* with a specially prepared volume of his own writings, with the enthusiastic approval of his colleagues. As chapter headings the volume was provided with decorative motifs, cleverly designed by one of Bailey's research fellows, Dr. B. G. L. Swamy of India.

Bailey was elected to membership in the National Academy of Sciences in 1929. He was a member of the American Academy of Arts and Sciences and the American Philosophical Society and was a fellow of the American Association for the Advancement of Science. He was a member of the Botanical Society of America (president, 1945), the American Society of Foresters, the International Society of Plant Morphologists, the International Association of Wood Anatomists, the American Society of Naturalists, and others. Outside of the United States, he had been elected to honorary membership in the Swedish Royal Academy of Sciences, in the Linnean Society of London, and in the Indian Botanical Society. He had served as vice president of the Seventh and Eighth International Botanical Congresses. In 1956, Professor Bailey was one of fifty outstanding botanists of the United States to be recognized on the occasion of the Fiftieth Anniversary of the Botanical Society of America, his certificate of merit stating, "plant anatomist and inspiring teacher, for his outstanding contributions on the structure of the cell wall and the histology of the cambium, for his application of anatomy and morphology to problems of evolution of angiosperms." It was Irving Bailey and his colleagues, with their cool, deliberate, fair, and analytical approach to departmental and university affairs, that gave the Department of Biology in the thirties, forties, and early fifties the prestige it had within Harvard University as well as outside.

Within his family, Irving Bailey was truly in his own world. On June 15, 1911, while a young instructor in the School of Forestry, Irving Bailey married Helen Diman Harwood, daughter of a prominent family of Littleton, Massachusetts. Two sons were born to them, Harwood and Solon Irving. Both grew up in Cambridge. Harwood attended the Harvard School of Business Administration and became successful in business in Boston. The younger, Solon, selected the field of architecture and became associated with the well-known Boston firm Shepley, Bullfinch, Richardson and Abbott. Solon participated in planning and supervising certain of the buildings at Harvard University. As both sons married and settled in the greater Boston area, the grandparents found great pleasure in their children and grandchildren. Irving and Helen Bailey had acquired the 45-acre estate of his father on the North River in Norwell, Massachusetts. The large New England white house, situated on a hillock and fronting on a big curve in the tidal North River, was beautiful at all seasons of the year. The river was flanked on both sides by wide acres of marshland that, in turn, graded into shrubs and then into higher, former farmland, now white pine woodland. The marsh in summer, the autumn with its acres of highly colored shrubs and vines against the browning green of the pines all helped the beautiful restful quality of the Bailey summer home.

After his ten summers as an associate at the Carnegie laboratories at Palo Alto, Irving spent at least two months each summer at Norwell. Here he recuperated from the strenuous regime he set for himself in Cambridge. He cleared and made lawns of the seven acres of mixed shrub and marsh border vegetation of the unwooded area surrounding the house. This required not only cutting of bushes, but grubbing out their roots and then making smooth grassed areas where they had been. After the devastating hurricane of 1955, which laid low literally some hundreds of large white pine trees in the quarter mile between the house and the road, Irving made a driveway and flanked it by a picturesque fence made of short sections of white pine trunks for fence posts (12 inches or more in diameter) with sections of the trunks, some 10 feet in length, in between. All of this maintenance work he did himself, except, on occasion, when one of the sons might be available. Moreover, the attractive gardens that Mrs. Bailey maintained were a concern of his as well.

The results of Bailey's summer sojourns at Norwell were obvious. When he returned to Cambridge in the fall, his tanned features, the easy vigorous stride of his tall, lean body showed clearly that the summer had been good for him. The vacations of the sons and their families with the Bailey's, especially the visits of the grandchildren by themselves, the dropping in of their friends, impromptu or planned, gave one a chance to see a happy family at peace.

This curve of the North River had been the site of a shipyard, run by Oliver Cromwell's nephew, about which Irving liked to talk and the remains of whose old forge, anvil, and tools he liked to show. The river was lined with bushes, among them being many beach plums. The fruits of these Irving gathered in the late summer; his friends could always count on jars of beach plum jelly made by Irving himself.

This summer place, some twenty-five miles from Irving's home in Cambridge, was a very necessary part of the Bailey family life. They went there often out of season. The big fireplaces provided heat for the late fall and early spring occupancy. The living room had walls of pine boards more than 2 feet in width, an indication of the size of trees that had grown on the earlier New England homestead; some of these virgin pines still could be found in the woodland, having survived the recent hurricanes. This beautiful site with its house, its huge barn, and its carriage house and woodshed, which had become out-of-door eating and resting places, has been sold since Irving's death. The younger son, Solon, who cherished the place, died very suddenly at age fifty, a short time after his father's death, also of a coronary occlusion.

Irving Bailey is fully remembered. His somewhat formal greeting, offset by his very warm smile, his strong voice, his noticeable New England accent, and his careful enunciation, made his presence known. Although he would seldom accept a speaking commitment, his lectures, whether to a class or to a scientific gathering, given without notes but with his clear voice and superb illustrations—he was a superb craftsman in photomicrography—were impressive. At the age of eighty-two, his vision was excellent, his hearing unimpaired. Men like him, personally deliberate, sound in judgment, industrious, and with the same continuing habits to the very end, admired and liked by students, staff, and administration, are sorely missed, as is Irving Bailey himself.

The author expresses his gratitude to his colleagues, Professor Elso S. Barghoorn, Jr., and Professor Reed C. Rollins, who, like himself, counted themselves among Professor Irving W. Bailey's close friends as well as colleagues. Conferences with them have helped much with sequences of events and questions of interpretation both in discussion and in editing the manuscript.

To Mrs. Howard Mumford Jones and Mrs. Lyle Boyd, authors of *History and Work of the Harvard Observatory* (McGraw-Hill Book Co., New York, 1971), the author would like to indicate how much of the boyhood life of Irving Bailey he obtained from their vivid portrayal of Irving's father's two expeditions to Peru. The isolated mountaintop family life there while they set up and ran a temporary and then a more permanent southern hemisphere

astronomical observatory for Harvard University was both informative and exciting. It provided both gains and losses for a sensitive boy.

To his wife, Olive Hawkins Wetmore, to whose knowledge of the Bailey family and whose cooperative aid the author owes much in the preparation of this article on our close friend of many years.

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KEY TO ABBREVIATIONS

Am. J. Bot. = American Journal of Botany
Ann. Bot. = Annals of Botany (London)
Bot. Gaz. = Botanical Gazette (Chicago)
Chron. Bot. = Chronica Botanica
Forestry Quart. = Forestry Quarterly
J. Arnold Arbor. = Journal of the Arnold Arboretum of Harvard University
J. Forestry = Journal of Forestry
J. Gen. Physiol. = Journal of General Physiology
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Trop. Woods = Tropical Woods

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Edward Wilber Berry

February 10, 1875-September 20, 1945

by Ernst Cloos

Edward Wilber Berry died twenty-nine years ago, and several of his colleagues undertook the writing of his memorial. John B. Reeside, Jr., was assigned it, but he died in 1958. Ralph W. Chaney then accepted the task, but died before he completed it. In 1971 I accepted the assignment because I knew Berry well at Hopkins for fourteen years, admired him greatly, and am close to source material. Lloyd W. Stephenson wrote an excellent account for the Geological Society of America (1946), which I used extensively.

Berry was an extraordinary man who owed his success to inherited abilities and hard work. He is an outstanding example of what an energetic and intelligent man with motivation can achieve if given an opportunity. He graduated from high school, never went to a college or university, became an outstanding geologist and paleontologist, and was elected to the National Academy of Sciences in 1922 at the age of forty-seven, which is early for a geologist. His list of publications includes more than 500 entries and almost 8000 printed pages. He became Professor of Paleontology at Johns Hopkins, then dean of the College of Arts and Sciences and provost of the university. He died at age seventy when he was President of the Geological Society of America.

BEGINNINGS

Edward Wilber Berry was born at Newark, New Jersey, on February 10, 1875. His parents were Abijah Conger Berry and Anna Wilber Berry. Of his father and mother he said, "I don't believe there ever lived a kindlier man. My mother was the much more dominating of the two, with an infinite capacity for sacrifice and love." The family included a younger sister, Winnetta, and a still younger brother, Clinton.

Berry graduated from high school in 1890 and would have liked to go to college, but family finances did not permit it. While he was still in high school his interest in botany led him into the field in the region around Raritan Bay in New Jersey, where he collected and identified fossil plants from Cretaceous clays along the south shore. With a friend, he studied the flora of bogs and swamps, and the boys read U.S. Geological Survey reports and books on botany. He must have studied intensely and thoroughly because even before coming to Baltimore, in 1905, he published about thirty paleobotanical papers displaying considerable knowledge of the subject and skill in illustrating.

After high school Berry worked as office boy for the cotton goods commission house of Denny Poor and Company, and in a few years he became traveling salesman in the southern states. In 1897 he accepted a position as business manager for the *Passaic Daily News* and between 1897 and 1905 became, in turn, managing editor, president, and treasurer. During this time he by no means neglected his primary interests, but intensified his studies in geology, biology, and paleontology. He began writing for publication and worked part-time for the New Jersey Geological Survey (1904-1906) and the Geological Survey of North Carolina (1905-1907). In 1901 he received the Walker Prize of the Boston Society of Natural History.

In 1898 Berry married Mary Willard of Passaic. They had two sons, Edward Willard, born in 1900, and Charles Thompson, born in Baltimore in 1906.

HOPKINS CAREER

At a field conference of geologists of Maryland, New Jersey, and the U.S. Geological Survey, William Bullock Clark, head of the Maryland Geological Survey and also chairman of the geology department at Johns Hopkins, was so impressed by Berry's knowledge and personality that he persuaded him to come to Baltimore. Clark's recommendation of April 3, 1906, to President Remsen describes the beginning at Hopkins as follows: "I recommend the appointment of E. W. Berry as assistant in Paleontology at $500.— a year, the understanding being that he will have charge of our rapidly growing collections in geology, paleontology, and mineralogy and will also aid in the laboratory in paleontology. I know of nothing that will more strengthen the work in geology than the appointment of a capable man like Mr. Berry to take charge of the work outlined. Our collections have gotten beyond our control and have reached that point where they are frequently unavailable for instruction. We have tens of thousands of specimens and with no one who has the time to give to the care of this material. Mr. Berry has special aptitude for such work and is furthermore an experienced paleontologist and for a number of years past has written extensively on paleontological subjects, particularly in the field of paleobotany where he is recognized as an authority on certain portions of the subject. At my urgent request he has been in residence with us here for a year and I am anxious to hold him here if possible. He is a mature man who does not contemplate going forward to the attainment of the Doctor's degree but is a thorough scholar and a most admirable man for us to retain." A second letter, by the Hopkins botanist, Professor Duncan S. Johnson, endorsed this recommendation, and the appointment was made.

This was Berry's opportunity, and he accepted the challenge. In the president's report for 1906 the following item appeared: "Mr. Berry has been engaged in a study of the Potomac floras of New Jersey, Maryland, and North Carolina for the surveys of those states, and is already engaged in the preparation of several interesting articles on the same." He was then not yet listed as faculty and carried no courses, but in 1908 Clark recommended that Berry be made an instructor and the salary doubled. During these first years he listened to geology classes unobtrusively in the back of the room, quietly absorbing information. From 1907 he worked as a staff member of the Maryland Geological Survey. In 1910 Mr. Berry was promoted to associate (now called "assistant professor"), and his name appeared in the university catalog as "Mr. Berry, Paleobotany" and jointly with Clark under Paleontology and General Geology. In April 1913 the chairman recommended promotion to associate professor: "Mr. Berry is carrying on investigations of much moment in his special field of paleobotany, several monographs and more than a score of other significant papers having been issued by him during the past three years. Mr. Berry has rapidly come to the front as the leading paleobotanist in this country and his work has elicited much favorable comment on the part of the leading paleobotanists in Europe." In 1916 Professor Clark urged that Mr. Berry be advanced to Professor of Paleontology because "the influence which he exerts over our students is very pronounced, probably greater than that of any other member of our staff." From then on, he was listed simply as "E. W. Berry, Professor of Paleontology," in contrast to all other members of the faculty, who listed degrees, dates, titles, and, at times, several lines of data. His entry remained unchanged until he became dean. The contrast was striking and very typical of the man, who was no friend of pomp, glitter, and prima donnas.

After the death of W. B. Clark, Berry became the dominating personality in the department. He taught a variety of courses reaching a large number of students who were profoundly affected, and nobody in the department remained untouched.

PROFESSOR AND STUDENTS

Berry was an inspiring teacher, and he "turned on" many students who owe him a great deal. He was intolerant of laziness or mediocrity, and his appraisal of his fellowman was prevailingly by instinct and common sense and rarely inaccurate. He made mistakes, but since he was very kind and warmhearted he was sympathetic to those who tried hard but did not quite succeed. One of his students who was not in his field and was afraid of his oral examination writes, "But what I remember most about him was his exceeding kindness to all graduate students. His action during that oral was that of a gentleman."

Another student writes, "When I was at Hopkins he gave courses in paleobotany, invertebrate paleontology, and on classic European localities. He lectured, for example, on the Paris basin as though he had been all over it, but he was never in France."

Berry gave a whole generation of geology students a feeling for creative research, inspired by his own example. He was never hurried or harassed and was always accessible in his room, seated at his big rolltop desk, on which was placed a board that was used for all his writing. The walls around him were lined with bookshelves and books.

If the function of a graduate professor is not to teach facts and theories but to inculcate a critical attitude toward one's own ideas, as well as those of others, Berry did extremely well.

His Saturday morning seminars were famous and are well remembered by all who ever attended. They lasted four hours and typically began with a critical review of some famous textbook. Deflation of the near great was legendary, and though he was caustic he really intended to amuse and shock his audiences.

Following the review was a report by one of the students on a self-selected topic. The last portion of the seminar was a topical discussion on, for instance, cross-bedding, mudcracks, sorting, or anything else having to do with stratified rocks. Berry tried to keep arguments going. He rarely lectured, but made the students dig things out by themselves. He would demolish an illogical report, but mostly he encouraged the students to criticize each other. He had a remarkable gift for creating interest. Some of the sessions became a bit sterile at times but never for long. He would try to stir things up, but never took the floor for more than a few minutes. He was an excellent blackboard artist. He illustrated all his lectures and could make his fossils quickly come to life by a few deft strokes with the chalk.

Once a month the Berry's invited the students and faculty into their home after dinner. Everybody gathered in the living room or, in later years, in the library, where he would read from a classic work in geology or an outside speaker would tell of his works and travels. Ensuing discussions were at times quite heated because Berry held strong opinions, and if he disagreed he said emphatically what he thought about an idea or the person who proposed it. Nobody was spared, no matter how high a position or scientific reputation he held. He taught his students to examine ideas carefully and never to be afraid to challenge them. Many of his students still appreciate the direction their whole lives were given by E. W. Berry.

SCIENTIFIC PRODUCTION

Berry's scientific output was amazing. There are numerous short notes but also many longer articles and very substantial monographs. The number of entries exceeds 500, most of which are illustrated. The bibliography of Stephenson (1946) lists 1028 figures and 585 plates, but some articles are listed as "illustrated," and numbers are therefore only approximate. Even if many illustrations are photographs, almost all plates also include drawings. The preparation of the illustrations alone must have consumed very much time, and the total for writing and illustrating is large. If one has seen the lack of assistance of those days, one wonders how this was accomplished. When I arrived at Hopkins, in 1931, the Maryland Geological Survey had one secretary-typist, the Department of Geology none. A general assistant mailed publications for the survey and took photographs with an old box camera for publications of the survey. Otherwise the faculty did their own work in their offices. There were no research laboratories for faculty or students. There also was no "Illustration Division"; neither were there National Science Foundation grants or research assistants.

Berry's first publications were mostly notes and brief, illustrated descriptions of fossil plants or localities. They reveal a growing familiarity with the subject as well as with techniques of handling the material, identification, description, terminology, and publication. This was during the time when he was working at the *Passaic Daily News,* where he must also have been successful, judging from his advances within that organization.

In the annual report of the New Jersey Geological Survey for 1904, published in 1906, there are two of Berry's articles. One is "A Brief Sketch of Fossil Plants"; the other is "The Flora of the Cliffwood Clays." Both are most revealing and much above the average for a state survey report. The sketch on fossil plants is of interest to anyone interested in natural science. It is a broad review outlining the relevance of paleobotany from the botanical or biological and from the geological points of view. Berry discusses evolution, definition of a species, and relation of fossil to recent plants and reaches far back into history and the first recognition of fossil plants. The sketch is also a summary on geologic time, evolution, and plant classification and is well illustrated. Considering the background and the author's job this reveals a considerable amount of reading and understanding, not only as a fossil collector but as a scientist and geologist. He wrote the report in 1904 at age thirty-four in his "spare time," which cannot have been plentiful. The second report contains extensive lists of paleobotanical data and species, many of which were named by Berry himself, showing that he must have collected extensively in the field and prepared his material at home.

One may divide Berry's publications into four broad categories. First is a vast number of short, mostly illustrated notes, a page or so long, with descriptions, observations, corrections of nomenclature, and general paleontological inventory. Second are many papers in which the cataloged data are placed in broad geological, historical, and biological context. These articles are good reading, as are the New Jersey report mentioned above, an address to the Philosophical Society on "Tertiary Floras of the Atlantic Gulf Coast," and several articles in the *Scientific Monthly,* such as "Rilly, A Fossil Lake" or "The Mayence Basin, A Chapter of Geologic History." Here Berry's vivid and interesting style is delightful and brings dull subjects to life in an extraordinary way. The "Jurassic Lagoons of Solnhofen" deals with paleontology, general philosophy, history, and geology and should be read by all students who feel that paleontology is a dull subject. There are many similar examples, such as "Far Away and Long Ago," where Patagonia becomes an interesting area and geologic history an important factor in today's discussion of tectonics. Other articles deal with tectonics, continental drift, or the origin of the Andes. The article "Shall We Return to Cataclysmal Geology" is a gem and good reading for all scientists. The third category of Berry's publications includes the large monographs of the U.S. Geological Survey and the Maryland Survey and his contributions to the geology and paleobotany of South America. The latter are the fruit of his travels to South America on several expeditions. The comprehensive works are Berry's major contributions, and his keen appreciation of the meaning of fossil plants led him to see forests and prairies, coastal swamps and steaming jungles, where most geologists saw merely fossil leaves. Finally, there are many short publications in newspapers, such as an article on the contributions of Charles Lyell, editorials, discussions of educational policies at Hopkins, and others.

As can be expected with such a volume of publications, not all are of equal quality or weight, and some of Berry's work has been severely criticized. It has been said that he was too quick in submitting manuscripts for publication. He was aware of that himself and once told a student who referred to the encyclopedia as authority, "That stuff is no good; I wrote it myself." On the other hand, when we were discussing publications needed by younger faculty for advancement, he said, "If a man has something he must say, it will come out, because he is alive. All we want are signs of life."

In spite of the administrative diversion, Berry's production continued, if at a declining rate. The crest, however, is between 1920 and 1930, when he was forty-five to fifty-five. After that the writing focused on larger papers with broader application and scope. One of the most interesting and readable papers, "The Origin of Land Plants," was published in 1945.

DEAN AND PROVOST

In 1929 Berry was appointed Dean of the College of Arts and Sciences as successor to Professor Ames, who became president. Though Dr. Ames did not appoint a provost, Professor Berry served as his right-hand man and adviser, essentially as provost.

The appointment was noted by many, inside and outside of the university, and Berry said in 1929, "Most people may feel that Hopkins took a radical step, making a dean out of an uneducated man. But the truth of the matter is that education need not stop with the end of schooling. After all, it is not what we learn at an institution that is of value to us as much as the attitude we develop toward all learning."

The dean did not organize an administrative apparatus, but dealt with students' needs and problems and with those of the institution in a very direct and uncomplicated way. He was accessible, and nobody remained in the dark about the dean's views, because he expressed them openly and very forcefully.

Berry made many important contributions to university policy and scholarly efforts, largely enforcing fundamental Hopkins philosophy, which was not then and is not now universal in the country.

Two important areas stand out: the educational purpose of the university and college, and the role of athletics at Hopkins.

Berry thought there should be three distinct kinds of colleges: one where rich men's sons spend a pleasant four years in contact with culture; a second one for drifters who need strict supervision and persuasion to find out what they want; and a third one for the rare minority of bright young men who have selected a goal and are willing to work toward it. He felt four years of college are not necessary, because after two years a student is ready for either serious graduate work or for business. For the mature student there should be as few rules as possible and no credit or marking system. A student should be allowed to select his own course and pursue it unhindered.

This system had been introduced as the "Goodnow plan," making it possible for a student to bypass the A.B. degree and to enter a department for graduate work after two years or after making up fundamentals. Berry very forcefully favored and applied this philosophy, which is still working at Hopkins today and has recently been reinforced.

A second concern of the dean was the abolition of intercollegiate and commercialized football. He felt that the university should provide ample facilities for all kinds of sports, indoor and outdoor, with competition among all sorts of intramural groups. All students who are not physically unfit should participate in some form of activity.

The deanship did not slow Berry's teaching or departmental activities, but it took some of his time and reduced his scientific output. When Dr. Bowman became president he appointed him provost and P. S. Macauley secretary. This reduced the time Berry could devote to writing, research, and his students, which was regretted by all. Dr. Bowman leaned heavily on his provost and delegated much of the internal administration to him. President Bowman described the relationship as follows:

"In 1935 when I asked him to associate himself with me as Provost I did so because preliminary discussions of University problems with faculty members showed that he had unusually good judgment and a wide range of interest and experience. He was instinctively inquisitive about men and wanted to know what made them click. He tried to get at the roots of a man. He disliked fine words and undocumented praise. In judging his associates he was apt to be severe. I think his best friends would also say that he was somewhat rough in the expression of his opinions. But when it came to action that affected an individual person he habitually went into reverse and became critical of his own judgment, often proposing a milder course than he himself had advocated at the beginning. He was never a man to spend time on mere amiable discourse about policies or appointments. He kept his eye on the action that would have to be taken and was ready with mature judgment when the time for action came."

As Berry approached retirement, friends of his had a portrait painted, and at its presentation to the university President Bowman summarized the esteem in which the dean and provost was held:

"If I were adequately to express the appreciation of the University for the gift of Dean Berry's portrait, I would be obliged to employ terms as wide and diverse as the procedures and interests of students and colleagues who have know their unique Dean for so many years. This would require me at one stage to sing my words, at another to consult an appropriate committee, and at still another to express myself in the symbols of science and the compositions of art. It is with deep gratitude that I receive and accept on behalf of the Trustees this portrait of our College Dean, our distinguished paleontologist, and our efficient Provost who is known to and deeply respected and beloved by so many generations of Hopkins men."

In 1942 Berry retired as dean and provost and in 1943 as professor. On September 20, 1945, Edward Wilber Berry passed away.

In assembling data for this memorial I received help from many sources. The following former students supplied very important information: Christina L. Balk, A. Lincoln Dryden, James Gilluly, J. Edward Hoffmeister, Shailer S. Philbrick, Vernon E. Scheid, Hobart E. Stocking, Edward H. Watson, and Wendell P. Woodring. The manuscript has been checked by some of these and several Hopkins colleagues. I also received assistance from the Department of Geology at Campbell College, Buies Creek, North Carolina, where the Berry papers are on deposit, and from the archives and library here at Johns Hopkins.

The memorial by Lloyd William Stephenson, published in *Geological Society of America Proceedings* for 1945, was of great help.

HONORS, DISTINCTIONS, MEMBERSHIPS

New York Academy of Sciences, 1899-1938

American Association for the Advancement of Science, 1900

Torrey Botanical Club (Secretary, 1904; Honorary Life Member, 1937-1945; Editorial Board, 1939-1945)

Geological Society of America (Fellow, 1909; Vice President, 1924; Depositories and Exchange Committee, 1924-1933; President, 1945)

Paleontological Society (President, 1924)

American Philosophical Society, 1919 (Council, 1933-1936)

American Academy of Arts and Sciences, 1921

National Academy of Sciences, 1922 (Marsh Fund Committee, 1933-1937)

Executive Committee, National Research Council, 1929-1932 (Chairman, Committee on Paleobotany; Member, Committee on Quantitative Data of Geological Processes and Committee on Paleoecology)

Board of Control and Editor for Paleobotany and Evolutionary History, *Botanical Abstracts,* 1918-1926

Assistant State Geologist of Maryland, 1917-1942

Senior Geologist, U.S. Geological Survey, 1910-1942

American Society of Naturalists

Société Géologique de France

Paleontologische Gesellschaft

Academia National de Ciencias en Córdoba (Argentina)

Sociadad Geológica del Peru

Washington Academy of Sciences

International Committee Paleobotanische Zeitschrift

Editor Paleobotany, *Biological Abstracts,* 1926-1943

Associate Editor, *American Journal of Science,* 1921-1938

Pan American Geologist, 1922-1939

Member, Research Committee of 100, A.A.A.S.

Member, Committee on Grants, A.A.A.S., 1934

Member, International Commission Paleobotanical Nomenclature, 1930-1945

Walker Prize, Boston Society of Natural History, 1901

Phi Beta Kappa, Alpha Chapter President, 1933-1934

Sigma Xi, Gamma Alpha, Omicron Delta Kappa, Phi Delta Epsilon, Scabbard and Blade

Medal Houra a los Colaboradores de la Instrucción Publica, Republica de Venezuela, 1933

Sc.D., honorary, Lehigh University, 1930

Vice President, Section Paleobotany, Fifth Institute of Botanical Congress, Cambridge, 1930

Mary Clark Thompson Medal for Paleontology (1942), 1945

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KEY TO ABBREVIATIONS

Am. J. Sci. = American Journal of Science
Am. Museum Novitates = American Museum Novitates
Am. Naturalist = American Naturalist
Asa Gray Bull. = Asa Gray Bulletin
Bol. Petról. = Bolétin de Petróleos
Bot. Gaz. = Botanical Gazette
Bull., Can. Geol. Surv. = Bulletin, Canada Geological Survey
Bull. Geol. Soc. Am. = Bulletin of the Geological Society of America
Bull. Torrey Bot. Club = Bulletin of the Torrey Botanical Club
Can. Dep. Mines Geol. Surv. Mem. = Canada Department of Mines, Geological Survey Memoir
Fla. Geol. Surv. = Florida Geological Survey
Geol. Mag. = Geological Magazine
Johns Hopkins Univ. Alumni Mag. = Johns Hopkins University Alumni Magazine
Johns Hopkins Univ. Circ. = Johns Hopkins University Circular
Johns Hopkins Univ. Stud. Geol. = Johns Hopkins University Studies in Geology
J. Geol. = Journal of Geology
J. Paleontol. = Journal of Paleontology
J. Wash. Acad. Sci. = Journal of the Washington Academy of Science
Natur. Hist. = Natural History
N.J. Geol. Surv. Ann. Rept. = New Jersey Geological Survey Annual Report
Pan-Am. Geol. = Pan-American Geologist
Proc. Am. Phil. Soc. = Proceedings of the American Philosophical Society
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Roy. Soc. Can. = Proceedings of the Royal Society of Canada
Proc. U.S. Nat. Museum = Proceedings United States National Museum
Sci. Monthly = Scientific Monthly
Smithsonian Inst. Ann. Rept. = Smithsonian Institution Annual Report
Trans. Roy. Soc. Can. = Transactions of the Royal Society of Canada
U.S. Geol. Surv. Prof. Paper = United States Geological Survey Professional Paper

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Marston T. Bogert

Marston Taylor Bogert

April 18, 1868-March 21, 1954

by Louis P. Hammett

Marston Taylor Bogert was born in Flushing, New York, on April 18, 1868, and died in New York City on March 21, 1954. His long and intensely active professional life was connected intimately and continuously with Columbia University, with the development of organized activity in chemistry in the United States, and with the fostering of international cooperation in science.

Bogert entered Columbia College in 1886, having graduated from the Flushing Institute, a well-known private school. His undergraduate record was remarkable. It included the grade of A in every course; the award of honors in German, Spanish, Italian, botany, and geology; the captainship of a famous freshman crew; the college championship in tennis; honors in football, shot-putting, and pole vaulting; membership as a flute player in the college orchestra; and the presidency of his class in the sophomore and in the senior years.

After Bogert was graduated with the degree of A.B. in 1890 he entered the new Columbia School of Mines in the course of analytical and applied chemistry. His academic record continued to be distinguished, and he obtained the degree of Ph.B. in 1894. When, during this curriculum, it became time for him and seven or eight classmates to take a course in organic chemistry, C. E. Colby, who had taught the course as adjunct professor, became ill, and the school was unable to find a replacement. Consequently, the students were told to teach themselves, and Bogert, who during his career published several hundred scientific papers in organic chemistry, never had any formal instruction in the subject. Neither did he follow the fashion of a trip abroad for a German doctorate. Instead he accepted an appointment to give instruction in organic chemistry at Columbia and advanced there through various grades, attaining the full professorship in 1904. He continued in that position with the title of Professor of Organic Chemistry until he retired in 1939 as Emeritus Professor of Organic Chemistry in Residence.

Bogert arrived at Columbia at a time when, under the leadership of Burgess and of Butler, a small college was in the early stages of an expansion that led to a great university, and his abilities found their opportunity in that expansion. The graduate faculty of pure science had been established in 1892, and Bogert's first research publication, "A New Synthesis in the Quinazoline Group" (with A. H. Gotthelf), appeared in the *Journal of the American Chemical Society* in March of 1900. From then on, brilliant young men flocked to his laboratory to be trained for careers in research and went forth to assume positions of importance in academic life and in the rapidly expanding chemical industry. It is a revealing comment on the times and the place that, while Bogert remained firm in his attachment to synthetic organic chemistry, many of his students refused to be typecast by their doctorates in that field: H. T. Beans became a pioneer in the revolution in instruction in analytical chemistry; J. M. Nelson initiated important advances in what later came to be called physical organic chemistry and in biological chemistry; M. Heidelberger became one of the world's great microbiologists; F. D. Snell founded one of this country's leading chemical consulting firms; G. Scatchard be came a distinguished physical chemist. These men were self-taught in their fields as Bogert had been in his.

Bogert was a polished lecturer, and his teaching evidenced clearly his lifelong love affair with the intricacies of structural organic chemistry. For Columbia University he carried the usual burdens of membership on committees, and he served on the University Council in 1908-1911 and 1916-1919. He was a charter member of the Columbia chapters of Sigma Xi and of Phi Lambda Upsilon. Columbia recognized his accomplishments and his services to the university by an honorary Sc.D. degree in 1929, by the Egleston Medal in 1939, and by the Charles Frederick Chandler Medal in 1949. He had received an honorary LL.D. degree from Clark University in 1909.

Bogert's professional career accompanied a phenomenal growth in chemistry in the United States, and he was an active participant in the parallel development of professional organizations. The American Chemical Society, which now numbers more than 100,000 members, had about one percent of that membership in the mid-nineties when Bogert began to take an active part in its affairs. In 1901 he was chairman of the New York section, and in 1907 and 1908 he was president of the national Society. In the latter position he introduced an organizational reform that established divisions of the Society, each representing a major portion of the field of chemical science and technology. This averted a threatened schism that would have set up separate societies for applied and for pure chemists. He was awarded the Nichols Medal of the New York section in 1905 and the Priestley Medal of the national Society in 1938.

Bogert was elected to the National Academy of Sciences in 1916. He led in the establishment of the Division of Chemistry and Chemical Technology of the National Research Council in 1917 and was its first chairman. In 1898 he became one of one hundred fifty-four charter members of the Chemists' Club of New

York, which has played an important role in the chemical life of metropolitan New York. The dining room of the club where Bogert presided at so many dinner meetings over the years has been named the Bogert Room in his honor. He was a member of the American Philosophical Society, the American Academy of Arts and Sciences, the Washington Academy of Sciences, the National Institute of Social Sciences, the American Association for the Advancement of Science, and the American Association of University Professors. He was a Fellow of the Chemical Society (London) and a Gold Medalist of the American Institute of Chemists; a member of Phi Beta Kappa, of Sigma Xi, and of Phi Lambda Upsilon; and a member of the Century Association, the Cosmos Club, the Royal Societies Club (London), the Hunters' Fraternity of America, and the Chevy Chase Club.

During the First World War Bogert served as a consultant to many government agencies and as a member of numerous boards and committees. He was commissioned lieutenant colonel in the U.S. Army in March 1918 and promoted to colonel in the Chemical Warfare Service in July of that year. He was honorably discharged from the army in May 1919. For the rest of his life his friends referred to him as Colonel, a form of address that he much enjoyed.

From 1900 on, Bogert had been active in the New York section of the British Society of Chemical Industry, and in 1912-1913 he was president of the parent Society. He also lent his support to the New York section of the French Société de Chimie Industrielle. In 1927 he was appointed to give lectures at the Charles University in Prague as the first Carnegie Professor of International Relations. While in Czechoslovakia, he was awarded honorary degrees by the Charles University and by the University of Bratislava and was made a Commander of the Order of the White Lion of Czechoslovakia. Following his participation over many years in international conferences on chemistry, he was elected president of the International Union of Pure and Applied Chemistry in 1938. His devotion and tact were in large measure responsible for the successful reestablishment of that Union after the Second World War, and he himself felt that that was one of his most valuable accomplishments.

The first Bogert to come to this country arrived from Holland in 1663, the year before the British took New Amsterdam from the Dutch. Many of his descendents were prominent citizens of New York, and Marston's father, Henry Augustine Bogert, was a well-known lawyer in that city. He was also a graduate of Columbia College, as were Marston's three brothers. In 1893 Marston married Charlotte E. Hoogland. They had two daughters, Annette H. and Elsie B. The Bogerts's homes in New York City and at Belgrade Lakes, Maine, were happy and friendly ones. Bogert was an active participant in the affairs of the Reformed church.

Bogert was distinguished in appearance, striking in personality, and gifted with wit and eloquence. He was the perfect presiding officer at all public occasions and was constantly in demand for that function.

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KEY TO ABBREVIATIONS

Am. Perfum. Essent. Oil Rev. = American Perfumer and Essential Oil Review

Chem. Eng. News = Chemical Engineering News

Chem. Listy = Chemike Listy

Chem. Rev. = Chemical Reviews

Columbia Univ. Q. = Columbia University Quarterly

Columbia Univ. Sch. Mines Q. = Columbia University School of Mines Quarterly

Collect. Czech. Chem. Commun. = Collection of Czechoslovak Chemical Communications

Color Trade J. = Color Trade Journal

Drug Cosmetic Ind. = Drug and Cosmetic Industry

Ind. Eng. Chem. = Industrial and Engineering Chemistry

Ind. Eng. Chem. (News Ed.) = Industrial and Engineering Chemistry, News Edition

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

J. Chem. Educ. = Journal of Chemical Education

J. Franklin Inst. = Journal of the Franklin Institute

J. Ind. Eng. Chem. = Journal of Industrial and Engineering Chemistry

J. Org. Chem. = Journal of Organic Chemistry

J. Soc. Chem. Ind., London = Journal of the Society of Chemical Industry, London

Orig. Commun. 8th Int. Congr. Appl. Chem. = Original Communications of the 8th

International Congress of Applied Chemistry

Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences

Rec. Trav. Chim. Pays-Bas = Recueil des Travaux Chimiques des Pays-Bas

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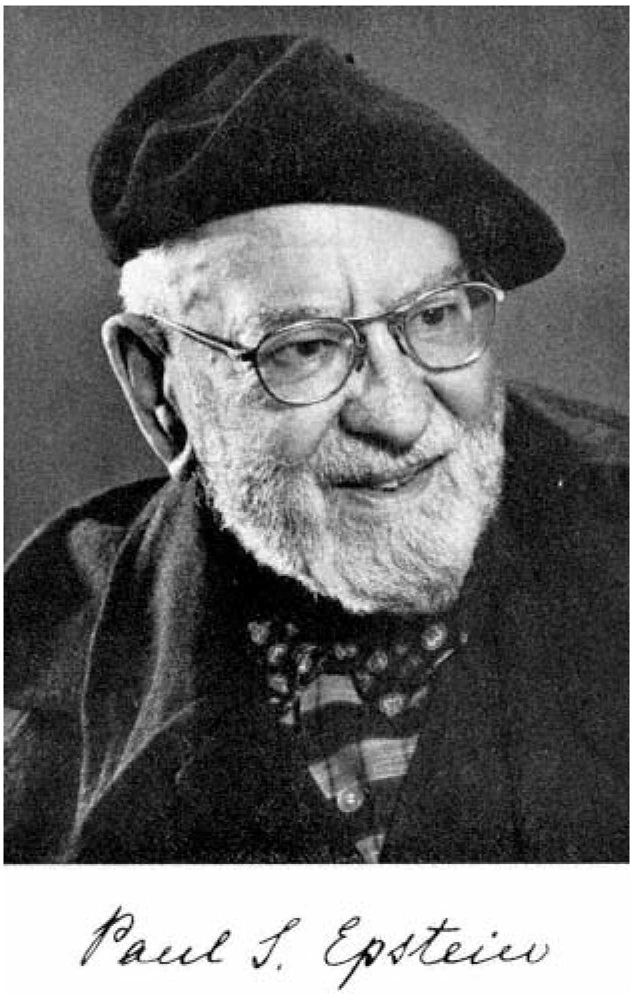

Paul Sophus Epstein

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by Jesse W. M. DuMond

Paul Sophus Epstein was one of the group of prominent and very gifted mathematical physicists whose insight, creative originality, and willingness to abandon accepted classical concepts brought about that veritable revolution in our understanding of nature which may be said to have created "modern physics," i.e., the physics which has been widely accepted during the Twentieth Century. Paul Epstein's name is closely associated with those of that group, such as H. A. Lorentz, Albert Einstein, H. Minkowski, J. J. Thomson, E. Rutherford, A. Sommerfeld, W. C. Röntgen, Max von Laue, Niels Bohr, L. de Broglie, Paul Ehrenfest, and Karl Schwarzschild.

Paul Epstein was born in 1883 in Warsaw, which was then a part of Russia. His parents, Siegmund Simon Epstein, a businessman, and Sarah Sophia (Lurie) Epstein, were of a moderately well-to-do Jewish family. He himself has told how, when he was but four years old, his mother recognized his potential mathematical gifts and predicted that he was going to be a mathematician. After receiving his secondary education in the Humanistic Hochschule of Minsk (Russia), he entered the school of physics and mathematics of the Imperial University of Moscow in 1901. In the third year of his undergraduate studies he started research in experimental physics under Professor Peter N. Lebedew, who in 1901 had become famous for his ex perimental demonstration of the pressure exerted on bodies by light or other electromagnetic radiation, an example of the Einstein principle of the inertia of energy.

After graduation, in 1905, Epstein served as laboratory instructor in physics, first at the Moscow Institute of Agriculture and later at the Imperial University, continuing his research at the same time. In 1909 he obtained his master's degree in physics and was appointed assistant professor (Privatdozent) at the Imperial University. In 1910 he decided to specialize in theoretical physics and obtained a leave of absence to do research under the famous Arnold Sommerfeld at the University of Munich (Germany).

Epstein's early research was in the theory of electromagnetic waves and particularly the theory of their diffraction. Two of his papers of this period were his doctoral thesis (1914), "Diffraction from a Plane Screen," and an article in the German Encyclopedia of Mathematical Sciences (1916), "Special Problems of Diffraction."

At the beginning of the First World War, in 1914, Epstein was at Munich. Being a Russian, he was regarded as an "enemy alien" and was automatically declared a civil prisoner. However, he was interned in a prisoner's camp only for a short time, thanks to the kindly intervention of Professor Sommerfeld. For the duration of the war he was allowed to live privately in Munich with access to the facilities for doing theoretical work and for publishing it, but of course was restricted from leaving Germany.

By 1916 Epstein had become deeply interested in problems of the quantum theory of atomic structure based on classical mechanics, and he shared the early development of this branch of physics with Niels Bohr and Arnold Sommerfeld. His most important paper in this connection was "Zur Theorie des Starkeffektes" (1916). In this paper he computed the electron orbits, atomic energy levels, and splitting of the spectral lines for a hydrogen atom in the presence of a superimposed electric field and compared his theoretical predictions with the experimentally-observed results then available. The dramatic story of the writing of the paper was told by Epstein years later. The story, which follows, was taken from a tape recording of an interview between the historian J. L. Heilbron and Paul S. Epstein on May 25, 1962.

Paul Epstein had been understandably anxious to escape from his captivity as an "enemy alien" in Munich, and to do this he had hopes of finding a position as a theoretical physicist somewhere outside Germany. Two places he had in mind were Leyden and Zurich. But to obtain such a position as the one in Zurich, he must write a habilitationsschrift, that is to say a thesis for becoming Privatdozent. Sommerfeld had just written his famous paper in which, by introducing the principle of relativity into Bohr's theory of atomic orbits, he had arrived at an explanation of the fine structure-splitting, till then unexplained by the simpler Bohr theory. A much more complicated case of line-splitting was known, however, and was as yet completely unaccounted for by any theoretical treatment. This was the effect, observed by Stark in 1913, when an atom is in the presence of an externally-imposed electric field. So Epstein proposed to Sommerfeld that he would tackle this difficult problem as the subject of his habilitationsschrift for Zurich, and Epstein's proposal was accepted at Zurich.

The Stark effect had been well known for three years and in fact, as chance would have it, at the very time of which we are speaking, Wagner, one of Röntgen's assistants in Munich, put on a demonstration of the Stark effect using a so-called "canal ray" tube. This was a vacuum electrical discharge tube in which the negative electrode or cathode was provided with holes. In such a tube most of the positive ions bombarded the cathode and "splashed out" the electrons from it so as to maintain the discharge, but a few of the positive ions would go on through the holes, and these were called "canal rays." Since vacuum technique was in a very primitive stage, the mean free path of an ion in such tubes was short, and the canal ray ions, excited by collisions with other ions, would emit spectral lines of much greater complexity than normal for that atomic species if the electric field in the near vicinity of the cathode were strong. The splitting up of the normally-to-be-expected spectral lines into these complicated spectra was the Stark effect. Wagner's timely demonstration of the effect in Munich was probably done with mercury vapor in the tube, but the theoretical explanation of the effect, even for the simplest atomic species, hydrogen, was difficult enough to be as yet an unsolved problem in terms of Bohr-Sommerfeld quantum theory.

This demonstration stimulated Epstein to start thinking vigorously how he might construct a theory to explain quantitatively the splitting of the line spectra. He had studied generalized mechanics from the French text of P. Appell, and he knew from this a certain theorem of the famous mathematician, Jacoby, furnishing a convenient method of integrating the differential equations of motion for a case such as this.

Now at that time there was a famous mathematician, Karl Schwarzschild, of powerful ability whom P. S. Epstein, as behooved a much younger and less widely known man, in fact only a young Privatdozent, held in great respect and considerable awe. Epstein only saw Sommerfeld infrequently, owing to restrictions imposed on him because of his "enemy alien" status in Munich, but at one of the meetings which he was permitted to attend through Sommerfeld's intervention, the latter told Epstein, "I wrote Schwarzschild that he should work on this article " (meaning the Stark effect). Epstein relates that he "was a little crestfallen, because I regarded this as a stab in the back, since he [Sommerfeld] knew that I was writing about it and," Epstein continued, "Schwarzschild was a mathematician of unbelievable energy; he could do everything in a twinkling; of course I couldn't reproach him [Sommerfeld], but I decided: 'Now I have no prospects unless Schwarzschild should go to heaven.'" Epstein goes on to tell how the next day when he was going to bed he saw his way through what he needed for the solution. He got up at 5 o'clock the next morning and by 10 o'clock he had the formula! And then the same morning he showed his result to Sommerfeld. "And what do you know, the same afternoon he [Sommerfeld] got a letter from Schwarzschild, and Schwarzschild had the wrong formula! It was the same order of magnitude, but didn't agree on the positions of the lines. So Sommerfeld wrote Schwarzschild, 'This morning Epstein brought me the formula of the Stark effect, and this afternoon we got your letter. But Epstein's formula agrees with the observations.'"

When Schwarzschild had first obtained his result, he immediately announced in the Berlin Academy that he would speak about it. He did so, however, *before* having written to Sommerfeld and Epstein, so the report he gave to the Academy before he actually lectured contained his erroneous result. By that time Epstein had already submitted his announcement of *his* result for publication, and it came out dated just one day before Schwarzschild delivered the above-mentioned lecture to the Academy. So Epstein had the priority over Schwarzschild by one day. In his lecture Schwarzschild had apparently corrected his error verbally (undoubtedly giving credit to Epstein for the correction), and when he received the galley or page proof of the printed version he corrected the error and removed all of the discrepancies. Thus Schwarzschild's final published version came out correctly.

In substantially all textbooks and histories of physics the theory of the Stark effect is attributed jointly to Epstein and Schwarzschild.* It is clear, however, that they really solved the

* See, for example, *History of Physics* by Max von Laue, translated by Ralph E. Oesper, Academic Press, Inc., New York, N.Y., 1950.

problem *independently* and that Epstein's solution came first and did indeed correct an error in Schwarzschild's solution. This incident, recounted directly from Epstein's own lips, illustrates dramatically the competitive tensions that existed among this group of European physicists in those early days of the development of the quantum theory of atomic structure.

Paul Epstein's intimate knowledge of those exciting times and gifted scientists at the turn of the century was a source of great inspiration to us younger men who attended his classes in theoretical or mathematical physics a little later after he had come to Caltech (in 1921). I shall never forget his account of von Laue's accidental learning of the hypothesis (first clearly formulated by Ludwig A. Sieber) that crystals are latticework structures of atoms. It seems that von Laue first learned of this when, in hopes of a consultation, he sought out Sommerfeld, who happened to be sitting in a little summer pavilion in one of the gardens of the University of Munich with his student, P. P. Ewald, discussing Ewald's famous thesis in which the idea of the "reciprocal lattice" had emerged as a mathematical device of great power. Von Laue was electrified when he overheard the conversation and grasped the idea of the crystalline atomic lattice. Here, made by Nature herself, was the equivalent of the artificial ruled grating (of Henry Rowland), the ideal tool, perhaps, which might indeed have the appropriate fineness of structure to answer the burning question with which von Laue had been deeply occupied—whether or not the Röntgen rays, discovered 17 years earlier, in 1895, were undulatory in nature and, like visible light waves, capable of being diffracted by a grating or lattice.

By 1900 Haage and Wind had tried to determine, by diffractions of x rays through fine slits, whether Röntgen's radiations were undulatory in nature and, if so, of what order of wavelength. These first results were inconclusive, but later, Walter and Pohl repeated the slit-diffraction experiment with greater refinement. Not until 1912, however, through the good fortune that the microphotometer of P. P. Koch* had just been invented and developed, did it become possible to study quantitatively the slight broadening of the photographically-recorded lines of Walter and Pohl. From this broadening they concluded that the x rays observed had wavelengths of the order of 4 X 10 $^{-9}$ cm.†

Von Laue immediately set two young experimental physicists, Friedrich and Knipping, at the University of Munich the task of trying to see if a beam of x rays could be diffracted by scattering from a crystalline solid. Their experiment was fraught with many difficulties and tribulations. At that time the only way of getting the high voltage electrical power to operate a Röntgen ray tube was with a "spark coil" or "Ruhmkorff coil." Public electrical power (for lighting the university) was only of the constant voltage, direct current variety. The light sources were so-called "arc lamps" in which the light came from a direct current arc maintained between two graphite electrodes. Such a lamp has a nonlinear current-voltage characteristic which tends strongly to *amplify* any small accidental fluctuations in the supplied voltage. In order to operate the Ruhmkorff coil, one needed an *intermittent* electrical supply to it with an appropriate "interrupter"‡ and capacitor for generating high frequency oscillations. But the transient fluctuations of the general voltage supply induced by the interrupter were strongly amplified by all of the arc lights in the university,

* P. P. Koch, *Ann. Phys., 38,* 507 (1912).

† I am indebted for my dates and information on these early slit-diffraction experiments to the famous text of A. Sommerfeld, *Atomic Structure and Spectral Lines,* translation of H. L. Brose, E. P. Dutton and Co., New York, N.Y., 1923.

‡ A "Wehnelt interrupter," which interrupted the current about a thousand times per second, was used. I owe some of these details to a delightful account of the von Laue, Friedrich, and Knipping experiment at the University of Munich written by Max von Laue himself, which was printed and privately distributed by North American Philips, Inc., on the 50th anniversary of Röntgen's famous discovery.

which emitted deafening rattling noises every time the x-ray tube was in action. Knipping had constructed an automatic device which switched on the current from the university's electrical system for about five seconds and then switched it off again for twice that length of time. Fortunately the experiment started during vacation, but before the two scientists got any diffraction photographs, classes started and the nuisance of the "talking arc lamps" drowning out all the lectures can be readily imagined. Quoting from von Laue's account: "Due to some psychological law this primitive music was contagious to the students. They thought it a great joke to hum along with it. The merriment grew greater and greater until finally the whole lecture was ruined." Von Laue continues: "The rector of the university naturally ordered a strict investigation into the cause of the disturbance. All of the many committees which are part of a university were set in motion. But in vain. We physicists, who could have explained the whole thing, knew nothing about it!

"At the end of the first three weeks of the new semester the matter accidentally came to light. A mechanic, who had been ordered to look for the source of the disturbance, came into the cellar where the Wehnelt interrupter stood, listened, and at once reported it to the higher authorities. Then the waves of general indignation broke over all of our heads. All the various committees came and certainly did not show us the most agreeable side of their natures." They demanded an immediate remedy or else suspension of the experiments.

"Faced with this need, we turned to Röntgen to ask whether we might draw our current from his institute. We needed only to carry a conducting wire across the university court from the window of one institute to that of the other. And as soon as it was established that the university would thus no longer be disturbed, Röntgen gladly gave his consent. "Just as matters had reached this point, the building com mittee walked into Sommerfeld's institute. They were the most powerful of all the university committees and apparently the least popular with the professors. They, too, wanted to let us feel the force of their anger, but we did not give them a chance to speak. Instead, we at once told them of the arrangement that we had made with Röntgen. They were nevertheless suspicious. They went to Röntgen themselves to have this confirmed. They returned a few minutes later in a state of indignation. We had deceived them. Röntgen was absolutely opposed to supplying current from his institute. We must therefore discontinue our experiments at once.

"So the four of us sat there, Sommerfeld, Friedrich, Knipping, and I [von Laue], and did not know what we should do next. Luckily our quandary did not last long. The solution came a few minutes later in the person of a mechanic, a fat, affable Bavarian, from the Röntgen institute. In his deep bass and local dialect, which considerably increased the humor of the situation, he said, 'The Geheimrat (meaning Röntgen) told me to tell you that you can go ahead and put up the wire. He is keeping to his agreement. It is just that whenever the building commission people come to him, the Geheimrat always says NO to them!' "

It was thus that the experiments of von Laue, Friedrich, and Knipping were continued until the end. They had tried at first to study the radiation diffracted by a crystal at very large angles, i.e., in the backward direction to the incident beam. When at last they tried placing the photographic plate *on the far side* of the crystal (copper sulfate), they obtained on the plate a central spot, produced by the direct beam going through the crystal, and, forming a pattern around the central spot, a group of symmetrically arranged spots of lesser intensity whose arrangement and symmetry depended on how the crystal was oriented relative to the beam. Röntgen, who was deeply impressed, did not believe at first that the spots represented an interference phenomenon through x-ray diffraction by the crystal lattice. The complete explanation became evident only after further work by the British physicists W. W. Bragg, his son Lawrence, and H. G. J. Moseley at Cambridge as well as certain other work at Munich by E. Wagner and J. Brentano. The five scientists worked with two crystals which demonstrated the monochromatization of the rays in the first crystal.

Professor Epstein, after coming to Caltech, would recount to his students very dramatically the occasion of the first success of the von Laue, Friedrich, and Knipping experiment—indisputably one of the truly great "breakthroughs" of early Twentieth Century physics—much as I have given it here.

The group of physicists from the University of Munich had the pleasant custom of meeting for luncheon and coffee at the little round marble-topped tables out-of-doors in the garden of the Café Lutz just across the way. The custom was so well established and accepted that the waiters of the café would dutifully see to it that the particular table for this group, at which on previous days they may have been discussing mathematical physics while writing the equations in pencil on the marble top, would be saved from day to day without washing it off so the discussions could continue. On a certain beautiful warm spring day in the Easter holidays of 1912 von Laue arrived a few minutes late at the accustomed table. Paul Epstein, P. P. Koch, the mathematician Rosenthal, and the physicists E. Wagner and W. Lenz were already there. But an unusual atmosphere prevailed at the physicists' table. Instead of conversing as usual, each one silently read a newspaper. Von Laue sat down, ordered coffee, and took up a newspaper waiting for a conversation to begin. But none did. One of the company made a remark, shortly after another did the same, and so on around the table, all of which struck von Laue as incomprehensible and mystifying. Finally what must have happened, but which he had not yet heard about, dawned on him, and he said, "Well, gentlemen, I assume from your remarks that the inter ference experiment had a positive result and that each one of you has been told this confidentially. I knew nothing about it until now." And this was indeed what had occurred.

It was while walking home from the Café Lutz, von Laue related, that the idea came to him of the theory of three dimensional space-lattice interference with which his name will be associated as long as our physics and chemistry of the Twentieth Century are remembered.

I have told this story to illustrate how Paul Epstein's arrival at Caltech in 1921 brought to this campus all the intellectual excitement and drama of what had been taking place in the great scientific centers of Europe. It had been R. A. Millikan's avowed purpose to do exactly this. He had insisted on special funds for this purpose as a condition of leaving the University of Chicago and coming to Caltech, as he himself said in his autobiography, "to build the best physics department of which I am capable." Some of the great scientists who came here, each for a substantial period of lectures, were C. G. Darwin, H. A. Lorentz, Paul Ehrenfest, Max Born, and Arnold Sommerfeld. Sommerfeld stayed only for shorter periods, but made several visits over the years. But since Paul Epstein had accepted a permanent appointment at Caltech his influence on all of us, both graduate students and postdoctoral men, was enormous. He was here, save for a few sabbatical leaves of absence, almost continually for 32 years, and for a great part of this time he taught substantially *all* of the advanced courses in theoretical and mathematical physics.

For example, in the three terms of the academic year 1925-1926 at Caltech, our records show that Professor P. S. Epstein taught the following seven advanced physics courses of one term each: Thermodynamics, Statistical Mechanics, Röntgen Rays and Crystal Structure, Theory of Electricity and Magnetism, Heat Radiation and Quantum Theory, Physical Optics, and the Quantum Theory of Spectral Lines. In addition to these he taught simultaneously a three-term course, Partial Differential Equations of Mathematical Physics. Thus his teaching load averaged three of these heavy courses per term. While carrying a comparable teaching load for many years, he found no difficulty also in writing some seventy or more papers and contributions to encyclopedias and an important 400-page text on thermodynamics.

In addition to his heavy load of lecturing, Professor Epstein took responsibility for several important academic activities. Along with R. A. Millikan, he was in charge of the weekly seminars, held each Thursday, which were attended by the entire physics department and frequently by men of other disciplines. At these seminars one of the graduate students usually would be asked to report on recently published developments in physics. For example, I recall clearly being asked by "Eppie" to report on certain papers of Louis de Broglie, in which the future Nobel Laureate developed, in its original and most elementary form, his famous idea of the waves associated with the electron.* Clearly he had selected me to do this because of my familiarity with the French language and my interest in French science in general. I recall that my audience ridiculed de Broglie's epoch-making ideas as I reported them at that time. Two years later, however, C. J. Davisson and L. H. Germer at the Bell Telephone Laboratories, seeking an entirely different postulated phenomenon (tunneling of electrons through crystal lattices), actually stumbled upon the fact of electron diffraction in crystals and the complete quantitative verification of de Broglie's prediction of the associated phase waves.

Another of Eppie's academic chores was to supervise our Caltech physics library and the purchase of its books and

* Rayonnement Noir et Quanta de Lumiére, *J. Phys., 3,* 422-428 (1922); A Tentative Theory of Light Quanta, *Philos. Mag., 47,* 446-458 (1924); Recherches sur la Théorie des Quanta, *Ann. Phys., 3,* 22-128 (1925).

periodicals. In commemoration of this useful service over many years, a bronze bust of our dear old friend now stands on the seventh floor of the new Millikan Library, in the Physics and Mathematics section. An Epstein Memorial Fund to honor his memory has been established through donations from more than fifty of the many students whom he taught.

Besides these professional activities, Epstein was deeply interested in the ideas of Sigmund Freud about psychoanalysis. His interest had apparently been awakened during his two years in Zurich, where he had acquired an almost professional knowledge of the subject. In California, after he became Professor of Physics at Caltech, he joined a local informal group studying pyschoanalysis. The first Freudian psychoanalyst who settled in Los Angeles was Thomas Libbin (circa 1927), and Epstein immediately brought together jointly with Libbin a "Psychoanalytic Study Group" that operated for many years and was finally merged (in 1950) with the Los Angeles Institute for Psychoanalysis. In fact Professor Epstein was one of a number of members of the group who provided affidavits for prominent foreign psychoanalysts invited to immigrate in order to help build up a Psychoanalytic Institute in Los Angeles.

Epstein, though not an active Zionist, was deeply interested in the Jewish people. He knew well and was a friend of the famous mathematician Abraham Fraenkel (1891-1965), a longtime resident of Jerusalem, then Palestine, who may be said to have been the "grand old man" most responsible for the organization and planning of all secondary and advanced education when the State of Israel was established. Abraham Fraenkel was a prominent member of the faculty in the old campus of Jerusalem and remained for many years active in the new campus of the Hebrew University at Jerusalem. Within the American National Society of Friends of the Hebrew University he organized the Academic Council of Southern California and served for many years as its president.

After the Second World War Professor Epstein became concerned with the inroads that communism was making among some of the young intellectuals of America and was invited to join the American Committee of the Congress for Cultural Freedom. In 1951, he served as one of the three U.S. delegates to the seminar conducted by the Congress in Strasbourg, France.

The writer was fortunate to have been one of some half dozen or more graduate students in physics, the first group who attended Professor Epstein's three-term course, Partial Differential Equations of Mathematical Physics, given when he first arrived at Caltech. He was master of three languages, Russian, German, and French, but his English at that time was still halting and afflicted with a heavy foreign accent reminiscent of all of the three languages more familiar to him. He would write the equations on the blackboard, but his first attempts to explain them in English were hampered painfully by deficiencies in his vocabulary. However, we were all amazed by the rapidity with which he progressed in his facility with English. A little later I learned that he had worked diligently to build up his English vocabulary, as by reading the newspapers and working the crossword puzzles with an English-German dictionary close at hand. It has been rumored also that he loved to read the Encyclopaedia Britannica for relaxation, starting with the A's and going systematically straight through the alphabet. He was gifted with an amazingly retentive memory. Nothing which interested him ever seemed to escape him. One of my fellow graduate students once remarked aptly, "Eppie's memory is like sticky flypaper!" I once asked Eppie if he could advise others how to acquire so wonderful a memory as his. His answer: "Jesse, to have a good memory the first thing you must do is to *trust* your memory."

During my studies as a graduate student working for the doctor's degree at Caltech I attended essentially every course Professor Epstein ever gave, and I am sure that I have learned far more physics from him than from anyone else. During none of his lectures can I recall ever seeing him refer to notes, nor have I ever seen him consult a table of definite integrals! Nevertheless his lectures, though completely logical, were never prepared in advance. He once told me that he made it a principle to lecture in this way, extemporaneously, only planning in his mind what he would say as he walked from his home to Caltech, a distance of perhaps half a mile. The result was excellent, because it forced him to do his reasoning *viva voce* in front of his class and on the blackboard. Thus we could watch him think and reason—an excellent lesson in how to do the same ourselves. I can only recall two other lecturers who were his equal in this facility and freedom of presentation, H. A. Lorentz and Arnold Sommerfeld. Eppie frequently made mistakes, but seemed able to sense quite promptly that something was wrong and would hunt for the error, frequently with our participation in the search. Because of this his classes were never dull, but what also added greatly to their interest was his extensive knowledge of the history of the physical sciences and of the characters of the people, from charlatans to geniuses, who had discovered or created its concepts and facts. The story of the discovery of x-ray diffraction from crystals, which I have recounted here, is only one of dozens with which his lectures were seasoned.

When Professor Epstein arrived at Caltech to teach, the advent of so celebrated a scientist from the cultural centers of Europe received considerable newspaper publicity. A certain club of socially prominent and highly influential ladies in Los Angeles (the name of which I have forgotten) sought to lionize him socially, as though he were some famous artist or orchestra conductor. They invited Eppie to lecture to their group on Science. He decided to put an end to such requests once and for all by a very characteristic and forthright method. He accepted graciously, with the proviso that they furnish him with black board and chalk. On the appointed day he went to their clubhouse and with perfect equanimity delivered a lecture on Planck's quantum theory, complete with all the mathematics. He knew perfectly well, of course, that his audience understood not one sentence of his talk nor one symbol on the blackboard. Few people in this world could have managed to do this successfully, because most speakers are to some extent psychologically dependent on a display of understanding and approval from their audience. Eppie, being very nearsighted and a little hard of hearing, was sufficiently insulated from the ladies to be able to talk almost as naturally as though he were addressing a class at Caltech or the National Academy of Sciences. He was rewarded with the result he had hoped for—they never invited him again!

In 1930, nine years after his arrival at Caltech, Paul Epstein married Alice Emelie Ryckman. Their home at 1484 Oakdale Avenue in Pasadena was the scene of many a warm and hospitable festivity, treasured in the memory of his students and associates.

The Epsteins had one daughter, Sari, now Mrs. Frank Mittelbach. The deep affection Eppie felt for his wife is clear in the dedication of his text on thermodynamics to her.

With the growth of the Caltech faculty, many of the subjects that had so heavily overloaded Eppie's teaching schedule were gradually taken over by newly-appointed members of the physics department. To the end of his career, however, he maintained responsibility for thermodynamics and statistical mechanics. In addition to his excellent text on thermodynamics, he wrote two extended articles on Willard Gibbs and his scientific contributions which were published by the Yale University Press in 1936.

In 1927 and 1929 Paul Epstein served as exchange professor at the Institute of Technology at Aachen, Germany.

In his research career, after his arrival at Caltech, Epstein at first continued his work on Bohr's form of the quantum theory, culminating it in 1922 with three papers in the *Zeitschrift für Physik* and one in the *Physical Review.* Later Epstein took part in the development of quantum mechanics initiated by Heisenberg and Schroedinger. An important paper in the *Physical Review* (1926), "The Stark Effect from the Point of View of Schrödinger's Quantum Theory,"* should be mentioned in this connection.

In 1930, Epstein was elected to the National Academy of Sciences.

P. S. Epstein also devoted considerable attention to borderline problems related simultaneously to both physics and several cognate sciences. Examples are "Zur Theorie des Radiometers" (1929), "Reflection of Waves in an Inhomogeneous Absorbing Medium" [the Heaviside Layer] (1930), "On the Air Resistance of Projectiles" (1931). Other examples of borderline problems which Epstein studied were the settling of gases in the atmosphere, the theory of vibrations of shells and plates, and the absorption of sound in fogs and suspensions. Two of his articles in this category outside of physics are especially worthy of mention. Both appeared in a monthly literary and scientific magazine, *Reflex,* published in the 1930's in Los Angeles, California, and edited by Dr. S. M. Melamed. The first of these articles, "The Frontiers of Science," is a highly scholarly presentation of certain central problems of both philosophy and religion set forth in their relationship to recent concepts on the frontiers of physics and mathematics. His discussion of the old philosophical and religious problem of free will *vs.* the concept of "scientific determinism" and the "law of causality" is particularly noteworthy since, in one form or another, all of human

* See also in this connection "The New Quantum Theory and the Zeeman Effect" (1926); "The Magnetic Dipole in Undulatory Mechanics" (1927).

kind has struggled for centuries with these questions. Epstein invokes the "principle of indetermination" of Werner Heisenberg, enunciated in 1927 and points out that, built into the very structure of Nature herself, there is a basic principle which precludes mankind from making with indefinitely high accuracy the requisite physical measurements to predict the future from a knowledge of the present with the ideal certainty postulated by S. Laplace in the Seventeenth Century. This article is indeed a rewarding one to the reader.

Epstein's other article in *Reflex* is "Uses and Abuses of Nationalism." In it he reveals a deep and farsighted understanding of certain patterns in the history of the political development of nations. In this discussion Eppie's complete alignment on the side of liberalism becomes self-evident. He takes the history of France as the vehicle for his argument and perceives the Dreyfus affair in the Nineteenth Century as an important turning point, away from imperialism and militarism at home and toward friendly cooperation abroad. In the opinion of the writer this article of Epstein's revealed his deep prescience in world affairs. It was written long before de Gaulle made the wise decision to withdraw France from its military commitments, first in Southeast Asia and later in Algeria. Other nations could well "profit by this example."

It is a pity that these two articles, splendidly exemplifying Paul Epstein's remarkable scholarship, erudition, and prescience in humanistic matters well outside his own fields of specialization, should be lost from the far wider circulation they deserve. The writer wishes to suggest that they be republished.

After Paul Epstein's retirement as Emeritus Professor at Caltech in 1953 he served as a consultant for several large industrial companies. Prominent among the many reports submitted by him in such work was his "Theory of Wave Propagation in a Gyromagnetic Medium" (1956).

Paul Epstein died at his home in Pasadena on February 8, 1966, at the age of 83, after suffering with admirable stoicism a prolonged and painful illness (herpes zoster or shingles). He was beloved of many students and colleagues, and his long and useful life stands as a splendid tribute to his brilliant mind and his altruistic sharing of it with others.

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KEY TO ABBREVIATIONS

Am. J. Phys. = American Journal of Physics
Ann. Physik. = Annalen der Physik
Naturwiss. = Die Naturwissenschaften
Phys. Rev. = Physical Review
Physik. Blätt. = Physikalische Blätter
Physik. Z. = Physikalische Zeitschrift
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Rev. Mod. Phys. = Review of Modern Physics
Verhandl. Deut. Physik. Gesell. = Verhandlungen der Deutschen Physikalischen
Gesellschaft
Z. Physik. = Zeitschrift für Physik

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Herbert McLean Evans

September 23, 1882-March 6, 1971

by George W. Corner

Herbert McLean Evans, anatomist, endocrinologist, and bibliophile, was born in Modesto, California, September 23, 1882. His father, Clayburn Wayne Evans, a native of Alabama, was the leading physician and surgeon in the then-small town; he is said to have been the first in the upper San Joaquin Valley to do abdominal surgery. Herbert Evans's mother, née Bessie McLean, came of a Virginia family. Her father practiced medicine in Modesto, and her brother, Robert McLean, was professor of surgery and dean of the San Francisco medical faculty of the University of California. Dr. C. W. Evans was a man of vigorous rather than polished character; Bessie McLean Evans and her brother Robert were persons of refined manners and tastes. Herbert Evans thus began life in a strongly medical family and with a varied store of traits and temperaments.

He attributed much of his early interest in science, literature, and history to a cultivated high school principal in his home town and the excellent library at the school. This exposure to books in boyhood was fortunate, for with his family background and strong pressure from his father it was inevitable that the young man should enter the medical profession and that his college education (at Berkeley) would be directed to that end, at the expense of cultural interests. Indeed, his father seems to have been dubious about letting him have more than the two years of premedical preparation then required; and young Evans himself concentrated the rest of his college years on science. Among his teachers, one who especially won his admiration was the celebrated paleontologist John C. Merriam, who during Evans's senior year in college took him on a field trip to Idaho. Evans's first published contribution to science, about the time of his graduation from college, was the description of a fossil fish spine from the Triassic of Idaho, which Merriam encouraged him to study.

In college, Evans distinguished himself sufficiently to be, at the commencement of 1904, one of the two students then customarily chosen to deliver brief graduation addresses. He spoke on the importance of biological research for human welfare. In the autumn he enrolled in the medical school of the university, in whose Berkeley laboratories he pursued the courses of the first year of professional study. The professor of anatomy was Joseph Marshall Flint, a surgeon who had done some anatomical research at Johns Hopkins and a stimulating lecturer; his associates were Irving Hardesty, a productive histologist, and Robert Orton Moody, a competent teacher of gross human anatomy.

The next summer Evans boldly took two steps that were quite contrary to his parents' judgment. In the first place he decided not to continue the study of medicine at the University of California, but instead to go to the School of Medicine of Johns Hopkins University where, his father feared, his leaning toward science might turn him away from the practice of medicine. The other, equally bold step was to marry his college sweetheart, Anabel Tulloch, like him tall and handsome, and in her way as spirited and impetuous as he Since her family also strongly disapproved the match, the young people were married privately and departed for Baltimore.

At that time married medical students were unheard of at Johns Hopkins and most other medical schools. Fearing that he might not be admitted, Herbert concealed his marriage from the admissions office. He and Anabel set up housekeeping at some distance from the school, where in a poor city neighborhood the bride from California's broad and verdant spaces was cooped up and separated from her husband all day long, among unintellectual neighbors, seeing nothing of his associates and activities, feeling lonely and neglected. Years afterward each of them separately spoke to the writer of this memoir about that strange interlude, he with an expression of remorse, she with a trace of lingering resentment, but each realizing that their sacrifice had helped to put Evans on the way to professional achievement. After about a year, the birth of their daughter Marian in the Johns Hopkins Hospital ended the deception and Anabel's isolation, although as the wife of an impecunious and intensely busy student her lot was still far from easy.

During his medical year at Berkeley, Evans had taken the usual course in human anatomy with dissection. At Johns Hopkins, at that time, second-year students were given continuing instruction in that subject, but the professor—Franklin P. Mall, a shrewd and subtle judge of men—allowed Evans to spend his time in the laboratory more or less as he wished. Because Mall himself had engaged in research on the pattern of microscopic blood vessels in various organs and in embryos, Evans learned from him methods of injecting blood vessels with colored fluids to make them readily visible. Working with finer and finer glass cannulas, under the microscope, he became expert. His special status in the laboratory gave him much closer association with the departmental staff than he would otherwise have had. Mall, the most influential anatomist in the country, had gathered about him probably the strongest anatomical research group in the English-speaking world, including

Warren H. Lewis, Ross G. Harrison, and Florence R. Sabin—all of them destined, as was Mall, to become members of the National Academy of Sciences.

One day the school's professor of surgery, the austerely scientific William H. Halsted, came to Mall for help with a clinical problem. A patient of his, following an operation on the thyroid gland, had developed acute tetany. Knowing of other surgical mishaps of the same kind, Halsted rightly conjectured that he had inadvertently either removed the inconspicuous but indispensable parathyroid glands along with the thyroid, or had tied off their blood supply. He felt that with more exact knowledge of the small arteries supplying the parathyroids, operations could be planned to conserve the glandules. Mall arranged for Evans to take on the problem under Halsted's supervision. By injection and careful dissection of the branches of the thyroid arteries in a few cadavers, Evans solved the problem, and thus his name appeared with Halsted's at the head of an authoritative little article that appeared in *Annals of Surgery* in 1907 while the junior author was still a medical student. In the same year Evans published in the *American Journal of Anatomy* a very creditable article on the blood circulation in the walls of large lymphatic vessels, illustrated with his own drawings made in the style of the great Johns Hopkins medical artist Max Broedel. About the same time he had begun research on embryonic blood vessels. Before taking his medical degree he completed a study of the earliest vessels in the limb buds of chick embryos, which he published in full in 1909. Another significant accomplishment while a medical student was his demonstration of the growth of lymphatic vessels into a malignant tumor, published in 1908.

While Evans was thus exhibiting his remarkable talent for anatomical investigation, Franklin Mall missed no occasion to foster his training for a professional career. He not only encouraged Evans's research, but had him write book reviews for the *Anatomical Record,* then being published from Mall's laboratory, and finally gave him a quite extraordinary opportunity for so young a man. In 1907, Mall was organizing, jointly with Franz Keibel of Freiburg, Baden, Germany, their great *Manual of Human Embryology* (1910, Philadelphia and Leipzig), to be written by leading investigators of Germany and America. When a European embryologist who was to contribute a section on the blood-vascular system was unable to do so, Mall entrusted the task to Evans. Too ambitious merely to compile what was already known, Evans prepared himself for the assignment not only by studying the literature and then examining well-understood human embryos in Mall's collection but also by making original observations on the little-known earliest development of the aorta and the other great vessels. By extremely skillful injection of chick embryos, working under the microscope, he proved—against the supposition of Hochstetter and others—that these ultimately large channels begin, as do the peripheral arteries and veins, as a network of capillaries. This fundamental observation was the basis of Evans's authoritative contribution to the Keibel-Mall *Manual,* Chapter XVIII, Section III.

From the first, Evans felt little interest in the clinical courses at Johns Hopkins, particularly when they took his time from such exciting activities as injecting embryonic blood vessels. He cut classes without regard to consequences. A preposterous story got abroad that he was granted his medical diploma only on his promise that he would never practice medicine. He himself contributed to the persistence of such legends by his own, equally apocryphal statement* that he was expelled from the medical school at the end of his third year for incompetence in surgical bandaging, obstetrical manikin exercises, and prescription-writing and was only restored to academic status with the help and advice of Dr. William H. Howell, then dean of the school. No doubt he did neglect such practical routines, but in

* *Bulletin of the Johns Hopkins Hospital,* 68, 300 (1947).

sober fact the records of the medical school do not mention any disciplinary action ever taken with respect to H. M. Evans,* and he was graduated in medicine in 1908.

Immediately afterward he joined Mall's department as assistant in anatomy and by this step confirmed, as his father had been fearing for some years, that he would not return to Modesto to assist with and later take over the older man's practice. To the senior Dr. Evans this was a heavy blow, for he thought practice far more important than research. It was undoubtedly the feeling that his father undervalued his choice of career that instilled in Herbert Evans an urgent desire to impress his parents by success in his chosen work and in later life to win the highest academic honors.

While Evans remained at Johns Hopkins, Mall arranged several times for him to go to Germany in the summer vacations. During a stay at Freiburg he was fascinated by the novel experiments of the surgeon Erwin Goldman on intravitam staining of animal tissues by acid azo dyes (e.g., trypan blue). With a young Freiburg chemist, Werner Schulemann, he began experiments to find out whether the spectacular coloration, inside and out, of living rats, mice, and rabbits by injection of such dyes is a physical or chemical phenomenon. Continuing the work for some years, he found that these dyes do not truly dissolve in the body fluids, but are dispersed as extremely fine particles, forming a fluid suspension. When injected under a rat's skin, the dyestuff gets into the bloodstream as submicroscopic aggregates that the macrophage cells of connective tissues take up and then store in their cytoplasm. The recognition of this important class of cells and their role in the storage of particulate matter was much forwarded by Evans's work. Among the numerous dyes he and Schulemann studied, one now called Evans blue proved

* Information kindly supplied by Mary E. Foy, Registrar, School of Medicine, Johns Hopkins University.

useful in a method of measuring the blood volume of living animals and human surgical patients.

As a member of Mall's staff, Evans was busy with teaching, with his work on vital staining, and with studying the blood vessels of pig and chick embryos as well as of human embryos on the rare occasion when one was received in a sufficiently fresh state to be injected with india ink. In 1913, on obtaining funds from the Carnegie Institution of Washington for the support of his large collection of human embryos and to develop research in that field, Mall created the department of embryology of the Carnegie Institution. This was first housed in the Johns Hopkins anatomical laboratory and later in part of an adjacent new building. Evans, who was given a Carnegie appointment concurrently with his Johns Hopkins post, then devoted a good deal of his time to the sectioning of early human embryos and to reconstructing them in wax from the sections—"A wearisome thing to do," he said, "compared with making the living embryo pump india ink as though it were blood to show the multitudinous vascular channels."

This strong inclination to experimental rather than purely morphological research was a partial cause of Evans's dropping a major project that Mall had suggested, a descriptive study of the human embryo during the period of somite formation. Another reason was that when Evans finally left Baltimore, Mall was unwilling to let him take along, even for his temporary use, the rare and precious serially sectioned embryos necessary for the study—one or two of which, at least, Evans had himself collected and laboriously sectioned during Mall's summer absences. Evans was disappointed and hurt by what he regarded as his chief's ungenerosity. However, a few years later when Mall died in the prime of life with the rift between them still unhealed, Evans was deeply grieved. As a kind of penance for his part in the disagreement, he proposed to write a biography of Mall, hinting that this would be a profound analysis of a distinguished scientific mind (as indeed it might well have been), but in time the plan was forgotten. Evans's work on the embryos, however, was not lost. Several years later G. W. Bartelmez of the University of Chicago took up the study of the somite stage of human development, first studying the Carnegie embryos in Baltimore, then going to Berkeley to secure Evans's collaboration and the use of his notes and drawings. The result was an important monograph in the Carnegie *Contributions to Embryology* (1926) under their joint authorship.

In 1915, when Evans was in his thirty-third year, President Benjamin Ide Wheeler of the University of California offered him the chair of anatomy at Berkeley vacated by the departure of Flint in 1907 and of his acting successor, Irving Hardesty, in 1909. Since then, the direction of the department had reverted to the worthy pedagogue Robert O. Moody, under whom Evans a dozen years before had studied gross human anatomy. Research had practically ceased. Even routine teaching had suffered because of Moody's frail health and the illness of another member of the small staff. Philip E. Smith was the only man left in the department who was in good health and had sufficient experience to teach gross and microscopic anatomy. During the year before Evans's advent, Smith and his wife, who had done some postgraduate work in biology, had carried almost the entire teaching load, to the detriment of the research program in experimental embryology that Smith had brought with him from Cornell.

To build up the department, Evans took with him from Johns Hopkins two young people who had shown competence for anatomical research—Katherine J. Scott (now Katherine Scott Bishop), a medical graduate of 1915, and George W. Corner, who was just completing an internship in the women's clinic. "Gynecologists ought to know more about the female reproductive cycle," said Dr. Evans to Corner. "Come to Berkeley with me and do your gynecology for a while on rats and rabbits." At Berkeley, recognizing Philip Smith's great abilities, Evans arranged for him a much-lightened teaching schedule and in every possible way facilitated the research that ultimately won for Smith an international reputation and the chair of anatomy at Columbia University. It was during this early period that Smith perfected his operation of hypophysectomy in the rat, which became an invaluable procedure in research on the pituitary gland. Associate Professor Moody, perhaps a little surprised by the inrush of all this youthful enthusiasm, retained charge of gross anatomy, with Smith and one of the newcomers helping him.

With the staff thus fully manned, the reorganized department resumed its work in the autumn of 1919 in a small frame building, once the university's printing shop, that had been adapted for the teaching of human anatomy when the San Francisco earthquake and fire of 1906 forced the transfer of preclinical classes to Berkeley. Evans took teaching quite seriously, in his own way, which was tinctured with the pride of intellect he never concealed. His course in histology was radically new in its extensive use of fresh and experimentally prepared tissues along with the traditional fixed and routinely stained sections. In the class laboratory he was usually to be seen at the microscope beside one of the better students. With the general run he was tolerant; with the duller minds, barely so and occasionally sarcastic. He did not believe in lecturing on gross anatomy, a finished science, but his lectures on microscopic anatomy were superb from the standpoint of his staff, for whom they constituted a postgraduate course. As for the medical students, he was heard to say that he aimed his lectures at only the four or five best students in the class (of forty), tacitly implying that the assistant professors and instructors could take care of the rest.

For the best ten percent the instruction (or, it might be more correct to say, the freedom to learn) provided by this brilliant professor opened new vistas in medical science. Years later Elmer Belt, a member of the first class Evans taught at Berkeley and now California's most distinguished urologist, wrote of Evans and his young associates that "The effect of their scholarship and idealism upon the freshman class in medicine was electric. Each of us realized how great an opportunity it was to enter the study of medicine under their guidance and for us the study of medicine became an obsession. The routine work of gross dissection and histology was time-consuming but most of us, in addition, were stimulated to take up a separate problem in research. We were thus led to seek out and read recent contributions to the literature concerned with our special subjects. This pursuit inevitably led us to doubt didactic textbook statements unless verified by our own personal observations. This atmosphere of doubt and verification prevailed through the department and led to intense application. For most of us this was our first taste of scholarly research." *

Departmental administration was for Herbert Evans a duty reluctantly borne. His compulsive urge to work intensively at research led him to put off administrative routine, the writing of articles against deadlines, and other less congenial tasks until the last minute. Thus the course of departmental affairs was interrupted from time to time by minor or even major crises. One of these in the early Berkeley days, somewhat mysterious to Evans's associates, evidently caused him deep concern. He and his secretary for several days were intently busy, occupied with account books and the adding machine. Evans's brother, a businessman familiar with accounting, was called in; there were urgent messages to and from the university bursar's office. Probably the professor had overrun his budget. On another occasion he was overtaken by the deadline for an article long promised

* Elmer Belt in *There Was Light: Autobiography of a University, Berkeley, 1868-1969,* ed: Irving Stone, Doubleday & Co., Garden City, New York, 1970, p. 354.

to an eastern scientific journal. In despair he retired to his study at home, sent for the secretary and her typewriter, and dictated hectically for three or four days. Typed sheets were sent up to the laboratory to be proofread by an assistant professor; the manuscript was assembled at home as the last pages were being typed, while Mrs. Evans sat in the family car at the door, ready to dash to the Berkeley Southern Pacific station to get the parcel on the Overland Limited for Chicago and the East.

Despite harassing episodes such as these, Evans had a wife still willing to yield her impetuosities to his, junior colleagues anxious to be helpful, and a devoted secretary and thus was able to keep the Berkeley department of anatomy happily productive and himself moving from one achievement to another. Resuming work on the vital staining of connective tissue cells, he quickly produced in collaboration with Katherine Scott a monograph (1921), beautiful in its clarity and certainty, on the differential reactions of macrophages and fibroblasts to particulate matter such as the acid azo dyes.

While still in Baltimore Evans's attention had been directed to the ovary by the observation that in mice vitally stained with trypan blue the moribund cells of atretic follicles were heavily laden with dye particles. Following this up at Berkeley he sought information about the ovarian cycle of rodents from Joseph A. Long of the zoology department, a quietly persistent worker who for years had been studying the estrous cycles of albino mice and rats. The length of the cycle in these animals was still a puzzle because the outward signs of estrus in them are very inconspicuous, as compared with those of household and barnyard animals. Joseph Long, by an ingenious but (as it turned out) inadequate method, had set it tentatively at eleven days. About this time (1916) Charles R. Stockard and G. N. Papanicolaou published a study of the estrous cycle of the guinea pig. They had revived a forgotten observation that the lining of the vagina undergoes periodic changes produced (as we now know) by action of the ovarian estrogenic hormone. Such changes are revealed by microscopic examination of cells scraped from the vagina. With this simple and expeditious method Evans and Long, working together in the anatomical laboratory, soon found that the female rat has a quite regular cycle of about five days and that the occurrence of estrus and the time of ovulation can be determined by the vaginal test. This exciting discovery, obviously opening the way to a vast field of research in the physiology of reproduction, stimulated Evans to furious further activity in which the gentle and studious Long was caught up. Long's chief, the senior professor of zoology, was annoyed by Long's frequent absence from his own laboratory, and both Mrs. Long and Mrs. Evans were even more disturbed by their husbands' lateness at dinnertimes and absences at weekends. The writer of this memoir was the embarrassed witness, in the rat room late one afternoon, of a dramatic entrance of the two ladies with a loud peremptory demand that their respective husbands should quit work at once and go to their homes where they belonged at that time of day.

The outcome of Evans's collaboration with Long was a monograph, now a classic in its field, published in 1922, *The Oestrous Cycle in the Rat and Its Associated Phenomena.* Evans drafted it but, in deference to Long's prior work, put his colleague's name first on the title page. The book has had a very great influence, partly because of the attention it directed, in conjunction with Stockard's and Papanicolaou's study of the guinea pig, to the ovarian-uterine cycle as a general phenomenon. It made available for experimentation on the cycle the albino rat, a mammal that is inexpensive, hardy, and easy to house, feed, and handle. From this work stemmed directly the discovery, next to be discussed, by Evans, Scott-Bishop, and Burr, of vitamin E, and almost as directly the isolation of the ovarian estrogenic hormone by Edgar Allen and E. A. Doisy at St. Louis; and the work helped greatly to stimulate the whole advance of our knowledge of the steroid hormones and the general chemistry of steroids. Not only did the monograph attain international fame; so also did the animals whose activities it described, for the Long-Evans standardized strain of white rats with gray hoods is currently specified in many accounts of research on the physiology of the rat.

Once having developed a standard strain of experimental animals and a practical method of following their cycles, Evans proceeded with various colleagues to attack major problems in the physiology of reproduction. First of these was the question of dietary factors in the cycle. That fertility could be impaired or abolished by inadequate diet was well known. With Katherine Bishop he established a standard diet of known composition on which their rats maintained regular estrous cycles, then proceeded to experiment with various dietary deficiencies. In 1923 they announced the existence of a hitherto unrecognized dietary factor that is essential for reproduction; in its absence female rats, although they grew well and in maturity ran regular cycles, could not carry their young to birth because early in pregnancy they suffered breakdown of the placentas and resorbed their fetuses. Male rats, if their food lacked the same ingredient, became sterile by deterioration of the sperm-forming cells of the testes. This dietary factor, essential for reproduction, Evans and Bishop found is fat-soluble, is present in green leaves (lettuce), and occurs in especially high concentration in wheat germ. With a chemist, George O. Burr, Evans obtained a partially purified extract from wheat-germ oil. The active ingredient acquired the designation "vitamin E," as the next in a series after E. V. McCollum's antirachitic vitamin D. In the next few years biochemists elsewhere carried the purification further and identified the factor as an alcohol; in 1936 Evans, with the biochemists Oliver H. and Gladys A. Emerson, prepared a product pure enough to yield the empirical formula $C_{29}H_{50}0_2$, which he named "tocopherol." During the course of these experiments with varied diets Evans and his co-workers at the time (Burr, the Emersons, Samuel Lepkovski, and others) were also among the first to recognize the importance of fatty acids as dietary constituents.

Evans's attention was next drawn to the hypophysis (pituitary gland) and its remarkable endocrine effects. It had recently dawned upon the medical profession that a form of gigantism, acromegaly, results from pituitary overactivity, usually caused by a tumor of the gland. Evans's admired teacher in medical school, Harvey Cushing, had been experimentally removing the gland from dogs, with striking effects on growth. When Evans took over the Berkeley anatomy department Philip Smith, already there, was becoming expert in extirpating the rudiment of the anterior lobe of the hypophysis of frog tadpoles, causing remarkable repressive effects on growth, on pigmentation, and on the development of the thyroid and adrenal glands. In 1920 Evans began experiments with extracts of the mammalian anterior lobe, at first with J. A. Long, and in 1922 they announced that, by injecting an alkaline extract of ox pituitaries into young rats, they had produced such enhanced growth that some of the treated animals grew far heavier than the largest untreated rats in the colony. This was the first essential step toward recognition of the pituitary growth hormone.

The extracts also delayed the onset of sexual maturity and lengthened the estrous cycle or completely suppressed it. In the ovaries of treated female rats an excessive amount of luteal tissue was formed by luteinization of the ovarian follicles. This observation was the basis of the subsequent search for a specific luteotropic hormone of the anterior pituitary lobe.

Henceforth, the isolation and identification of the several hormones of the anterior pituitary was to be for forty years the main theme of Evans's research. Because, however, of his practice—in his maturity—of always working with one or more collaborators, he stepped aside not infrequently to attack, with one or another of them, problems not closely related to the pituitary endocrines. One of these ventures was with Olive Swezy, a research associate several years his senior, in the neighboring department of zoology. They jointly published reports on two important topics. One of these was the old question of whether or not in mammals the formation of egg cells (ova) from the germinal layer of the ovaries continues after the birth of the female infant and on through adult life. After much debate, it had been generally accepted that all the ova in the ovaries of a sexually mature woman or other mammalian female are formed during the fetal period. The opposite conclusion of Evans and Swezy (1929, 1931), based largely on study of the rhesus monkey, that ova are formed after birth and in sexual maturity, has not been confirmed by subsequent workers carefully examining similar material. Their other chief topic was the number of chromosomes in man. For this study Evans personally obtained exceptionally well-preserved material by attending, at San Quentin prison, executions of criminals whose bodies were not to be claimed by relatives. Swezy did most of the counting, from thin serial sections. The currently available method of displaying the chromosomes and examining them in bits of tissue selectively stained and then flattened by gentle pressure was not then known. The count of 48 chromosomes in each cell, published by Evans and Swezy, was unfortunately incorrect; it is now certain that 46 is the correct number. In 1969, to a gathering of scientific friends, Evans explained that semidetached portions of two chromosomes had been counted as separate units.

Another, more successful sideline was a descriptive account of the canine estrous cycle, published in 1930 as a joint monograph with H. H. Cole. For this research Evans was himself prepared by a study of the early embryology of the dog, done while still at Baltimore, where by greasing the palms of dog-pound attendants he was permitted to mate bitches in heat at stated times before they were put in the gas chamber. A few carefully preserved jewel-like blastocysts thus obtained remained for years in vials on his shelves at Berkeley; he never published the embryological findings. References to other joint investigations with various members of his group will be found in the bibliography appended to this memoir.

Evans's principal collaborator in the continuing research on the pituitary hormones was Miriam E. Simpson, who after an outstanding record in Evans's department as a medical student and in other preclinical departments at Berkeley transferred to Johns Hopkins for the latter half of the medical curriculum, took her M.D. there in 1923, and returned immediately to Berkeley as a member of the department of anatomy. For about a decade their efforts were centered largely on the relations between the pituitary gland and the ovaries. In his work with J. A. Long on the growth hormone, Evans as stated above had found in 1921 that their extracts of the anterior lobe strongly affected the ovaries of experimentally treated female rats, causing persistence of the corpora lutea and filling up immature follicles with lutein-type cells. The rhythm of the female reproductive cycle was, of course, seriously deranged by such alterations. This was the first direct experimental demonstration of an action of the anterior pituitary on the gonads. Here, then, was apparently a second hormone of that organ; its activity and its chemical structure must be distinguished by further research from those of the growth hormone.

Shortly afterward workers elsewhere found that pituitary extracts would stimulate growth and maturation of the ovarian follicles. For some years it remained uncertain whether the luteotropic hormone (LH) and the follicle-stimulating hormone (FSH) were one and the same. A further complication was the finding by others that the urine of pregnant women contains a substance that when injected into female animals, causes hyperemia of the ovaries, growth of the follicles, and, in some species, ovulation. This is the basis of the Ascheim-Zondek and Friedman tests for pregnancy. For some time the similarity of action of the urinary and the anterior pituitary hormones was quite confusing. To this problem Evans and Simpson devoted much attention during the earlier years of their collaboration, finally demonstrating that the urinary gonadotropin (since shown to be produced by the chorionic part of the placenta) and the pituitary FSH are different substances.

In 1929 the two collaborators published another significant discovery, that something in their pituitary extracts caused hypertrophy of the mammary glands in virgin rats. This gave a strong hint of still another hormone of the anterior lobe. The hint was confirmed in 1933 by Stricker and Grueter of Strasbourg, who by injecting Evans-type pituitary extract caused rabbits actually to secrete milk; by G. W. Corner at Rochester, New York, who proved that previous action of the corpora lutea is not required for this effect; by Oscar Riddle of the Carnegie Institution at Cold Spring Harbor, New York, who isolated and almost competely purified the lactation hormone (now called prolactin); and by Abraham White of Yale University, who completed the purification.

While Evans, with his collaborators, had thus been engaged since 1920 in the characterization and isolation of the pituitary gonadotropic (follicle-stimulating and luteinizing) hormones and in obtaining the earliest evidence for a mammatropic hormone, investigators elsewhere had recognized that two other organs are also targets for the action of anterior pituitary hormones. Philip Smith's skillful ablation of the pituitary rudiment of frog tadpoles, done in Evans's laboratory, as already mentioned, had revealed that in tadpoles so deprived the thyroid gland does not develop. When Smith succeeded in performing hypophysectomy in the rat, a similar finding reinforced the suspicion that a thyreotropic hormone of the anterior pituitary was waiting to be identified. Actual isolation of such an agent in crude form was accomplished in several American and European laboratories in 1929. About the same time several workers observed deficient development of the cortex of the adrenal gland in hypophysectomized animals. J. B. Collip of Toronto and his associates, in 1933, were the first to isolate an effective if only partially purified adrenotropic substance.

By the mid-1930's the six hormonal products of the anterior lobe of the pituitary gland now generally recognized as distinctive substances had been isolated in various degrees of purity, namely those for growth (somatotropic, STH), follicle stimulation (FSH), stimulation of lutein and interstitial cells (LH, ICSH), lactation (prolactin), stimulation of the thyroid gland (TSH), and stimulation of the adrenal cortex (ACTH). These hormones have been extremely difficult to separate, to purify, and to characterize chemically, for they are proteins and hence exceedingly complex in molecular structure. Furthermore, they are effective in very small dosage, and it is therefore difficult to know when a preparation of any one of them is free of contamination by another. As Evans and his associates attempted to identify them and distinguish their functional activity, their progress, like that of other workers, was slow and confusing. One well-known American endocrinologist, Oscar Riddle, insisted for a long time that Evans's growth hormone was identical with prolactin. As Evans himself has said, he and his colleagues published again and again statements that they had "purified" the follicle-stimulating hormone, as indicated by tests available at the time, only to find out themselves or learn from others that with different tests or different dosages their "pure" preparations still produced effects that must be ascribed to another of the anterior lobe endocrines. Even at the present writing, when some of the amino acid chains constituting these potent proteins have been identified, one of Evans's former co-workers, C. H. Li, attributes the growth effect and the mammatropic effect to the same group of amino acids, while another of the group, W. R. Lyons, disagrees.

Because of these perplexities, the biographer and historian find it difficult to chart the course of discovery in this area of research and to assign credit to any one man or group of coworkers for one or another item in an ever-changing pattern of knowledge. We shall do ample justice to Herbert Evans when we say that for thirty years he led his able associates in productive study of each and all of the anterior pituitary hormones. When he first began to experiment with pituitary extracts, nothing was known about the endocrine activity of the anterior lobe of the pituitary gland except that in some vague way it exerted control over bodily growth and was somehow essential to the reproductive function. When he retired from active research, worldwide investigation had recognized the six hormones, characterized their activities, and to a large extent revealed their chemical structure. Evans was involved in some way in almost every aspect of this great advance.

The names of many who worked at his side are recorded in the long list of joint publications appended to this memoir. One of them has written that because this was a period of group research in Evans's laboratory, the contributions of individual members might well be obscured or even forgotten, but none of those who took part in this large and bold enterprise would ever forget Evans's own contribution through wise choice of personnel and the provision of facilities for work, or his conferences in which divergent views were analyzed and conclusions reached.

The work of Evans and his group could not long be accommodated in the little gray building where he began at Berkeley in 1915. In 1930 the department of anatomy and with it his research laboratories were removed to the newly built Life Sciences Building, further west on the Berkeley campus. In the same year the University of California created an Institute of

Experimental Biology and in recognition of Evans's achievements in research appointed him its director, thus providing for him and those he had gathered around him far ampler space and superior equipment. With the new appointment went the specially endowed Herzstein Professorship of Biology.

The progress of research in the new laboratories was interrupted temporarily in 1932 when Simon Flexner, Director of the Rockefeller Institute (now Rockefeller University) in New York City, invited Evans to spend a year at the Institute as a guest investigator. Evans's rising reputation for endocrinological research perhaps sufficiently explains this rather unusual invitation, but he believed, as did many of the Institute's permanent staff, that it had been arranged to test his eligibility to succeed to the directorship. Flexner, sixty-nine years old, would soon have to step down. Evans at fifty was of suitable age and obviously possessed the scope and courage to deal with difficult scientific problems and the ability to lead group research. He had just finished a two-year term as president of the American Association of Anatomists. Before scientific audiences he spoke with impressive manner and style.

Evans took with him to New York Miriam E. Simpson and Richard Pencharz, then junior members of his Berkeley group. At the Rockefeller Institute he added a pharmacologist, E. L. Gustus, and a biochemist, Paul R. Austin. Gustus recalls still his admiration of Evans's broad knowledge of science in general, his charm, and his encouragement of the younger men. In a personal communication Austin describes the characteristic drive and intensity of research under Evans's direction: "Dr. Evans impressed everyone with his infectious energy and enthusiasm. He sent a technician to the Institute ahead of the rest of his group to get a rat colony started. . . . This colony was soon supplemented by purchase of rats from outside suppliers so that at our peak of activity we had 700 21-day-old rats available every

Monday morning for our assays. . . . As you will guess, the situation was pretty hectic at times and there were occasions when the rats drove the program rather than have the program limited by the rats. But we did get a lot of work done with a small group in the short period of eight or nine months."

The group, in fact, made significant progress in the purification of the follicle-stimulating hormone of the pituitary gland and in clarification of the synergism between this agent and the pituitary luteinizing hormone (which they were then calling "interstitial cell stimulating hormone").

The minutes of the Board of Scientific Directors of the Rockefeller Institute do not reveal anything about this visit, and none of the senior staff of that time survive to tell why nothing more came of it, if indeed it was a trial run for the brilliant Californian. In all probability Simon Flexner and his advisers doubted whether Evans could subordinate his own program of research to the broader duty of leading the distinguished and diversified Institute.

Few of Evans's fellow scientists knew that he was an ardent collector of rare books, especially the great classics of medical, biological, and physical science. His adoption of such an avocation, ordinarily far beyond the means of a professor, is easily understood in the light of his aspiration to cultural distinction, long hampered by his lack of classical education. As a young man in Baltimore he was already sufficiently interested in the history of his profession to publish one or two brief reviews of new books in that field. As he matured, even if he did not read the Greek of Galen, the Latin of Vesalius, or the antique French of Ambroise Paré, he came to relish the sight and feel of the volumes in which the works of these heroes of science were enshrined and steeped himself in historical and bibliographical lore until he could talk freely with bibliophiles and historians of science. His urge to collect books could not be indulged, how ever, for lack of the necessary money, until he was well established in his professorship and the family exchequer was strengthened by parental bequests.

His remarkable career as a bibliophile (bibliomane would hardly be too strong a term) has been described since his death by Jacob I. Zeitlin, the well-known Los Angeles dealer in rare books and manuscripts.* Evans began serious collecting about 1930. His earliest purchases were financed, it seems, by borrowings from his wife Anabel's patrimony. By 1934 his first collection was sufficiently important to be exhibited at the Berkeley Faculty Club by the History of Science Club of the University of California. A small catalog he prepared for the exhibit shows that he had interested himself especially in books embodying notable individual discoveries, the formulation of scientific laws, and announcements of important hypotheses. According to Zeitlin the catalog was a pioneer effort to compile a selected list of the most significant books in the history of science. It is still a valuable guide for advanced collectors and dealers. Zeitlin considers it largely responsible for the great increase in American demand for books in the history of science during the past thirty-five years and the consequent increase of prices.

Evans did not keep this first collection very long. Domestic difficulties requiring reimbursement of Mrs. Evans forced him to sell it. Almost at once he began another, of first editions in the sciences, accompanied by a collection of bibliographic reference books on the subject. This too was sold, to settle the estate of his wife, who died soon after their estrangement and divorce in 1932.

Somehow Evans found the means to continue collecting, periodically getting himself in debt and selling off the books. Each time he received payment for the latest collection, says Zeitlin (through whose hands most of them passed), "He would

* Jacob I. Zeitlin, "Herbert M. Evans, Pioneer Collector of Books in the History of Science," *Isis,* 62, 507 (1972).

plunge into another passionate campaign by letter, cable, telephone, and overnight drives or air flights to all parts of the world, to try to recapture the treasures he had parted with a few days before."

The history and present whereabouts of the seven successive scientific collections show how widely this obsessive urge has ultimately served the scholars of our country, for almost all these books, estimated to number more than 20,000, are now in the possession of universities or other scholarly owners.

Mr. Zeitlin tentatively lists the medical and scientific collections as follows:

1930 (?): Classics in the Medical Sciences. Purchased by Dr. and Mrs. James Waring, Finley L. McFarland, and Mrs. Dora Porter Mason. Presented to the Denver Medical Society, Denver, Colorado.

1950: First Editions in the Sciences, together with a Reference Collection on the History and Bibliography of Science. Purchased from the conservator of the estate of Mrs. Anabel Evans by Zeitlin & Ver Brugge and John Howell:Books, on behalf of Lessing J. Rosenwald, and presented by him to the Institute for Advanced Study, Princeton, New Jersey.

1953: First Editions in the Sciences, together with a Reference Library on the History and Bibliography of Science. Purchased by Zeitlin & Ver Brugge. Described in several catalogs. Many of the outstanding items were acquired by the Burndy Library and by E. L. DeGolyer for the DeGolyer Collection at the University of Oklahoma at Norman, Oklahoma.

1957: First Editions in the Sciences. Purchased by Bernard M. Rosenthal and John Fleming for Louis Silver of Chicago and presented to the University of Chicago. Some duplicates and out-of-scope works were sold off by John Fleming. (One of these, Semmelweis, *Die Aetiologie, der Begriff und die Prophylaxis des Kindbettfiebers,* 1861, Zeitlin purchased back for Dr. Evans, the second time this book had passed from his hands to Dr. Evans's.)

1961: First Editions in the Sciences. Sold by Zeitlin & Ver Brugge and John Howell:Books, as agents for Dr. Evans, to Samuel A. Barchas of Tucson, Arizona.

1962: First Editions in the Sciences, together with a Collection on the History and Bibliography of Science. Sold by Zeitlin & Ver Brugge and John Howell:Books, as agents for Dr. Evans, to Lew D. Feldman, acting for the University of Texas, Austin, Texas.

1967: First Editions in the History of Science and a Collection on the History and Bibliography of Science. Purchased by John Howell:Books, San Francisco, and Zeitlin & Ver Brugge, Los Angeles, and dispersed in a number of catalogs of both firms. The major part of the collection is now at the University of Utah, Salt Lake City, Utah.

In addition to the medical and scientific books, Evans formed and disposed of two large collections of western Americana, two collections of Japanese prints, one of the prints of Jacques Callot, and a general library of poetry, art, and the humanities.

Dr. Evans was tall and broad-shouldered. His mobile features expressed alert interest in people and things about him, mingled now and then with a quizzical glance or the subtle reflection of an *arrière pensée* that might have meant anything from mockery to disdain. In friendly conversation he was gay, sometimes extravagantly enthusiastic. He liked especially to talk with intelligent women, who responded warmly to his deferential manner. In more formal conversation and in talk about scientific matters or book-lore, though expressively courteous, he could not conceal an air of superiority of which he may not always have been conscious, an air evinced by allusions to facts or personalities beyond his hearers' acquaintance, introduced in such a way as to suggest that one really ought to know about them. The same urge to be recognized as a cultivated person caused him, in his writings, to employ a high and sometimes high-flown literary style. As would be expected of a man always expending his full energies at the highest level of individual talent, he would not busy himself with the organizational routines of the scientific profession. He frequently attended the annual meetings of the American Association of Anatomists, presenting the results of his research at the scientific sessions and heartily enjoying the sociability of these gatherings, but when he was president of the Association (1930-1932), he left the routine business to be handled by the vice president. Duty required him to take the chair at the executive session, but he did not inform himself of the agenda and was quite unabashed when he had to be coached by the secretary sitting beside him on the platform.

Dr. Evans's three marriages marked cardinal phases of his life. Anabel Tulloch, the bride of his student days, exhibited as much independence and impetuosity as he had when in disregard of family pressure, he boldly set out on a scientific career. But as shown by episodes narrated earlier in this memoir she did not willingly endure the stresses of marriage to a man so intensely dedicated to science. The daughter born of this marriage was greatly loved by her father and deeply mourned when she died in young womanhood.

Evans's second wife, Marjorie E. Sadler, was a highly competent research worker in his laboratory, fully qualified to tolerate the exigencies of his scientific activity. They were married in 1932 when his career in endocrinological research was at its crest. This union also ended in divorce. A daughter, Gail Evans (Mrs. Rolfe LaForge), no less beloved than was her half-sister, resides at Mill Valley, California.

The wife of Evans's later years, Dorothy F. Atkinson, was at the time of their marriage in 1945 acting director of the department of English at Mills College, Oakland, California. The third Mrs. Evans was prepared by her training and career to sympathize with her husband's interest in the history of science and his zest for collecting books and prints. Her humanistic studies gave her, indeed, a gentle superiority over his less-deeply rooted literary and artistic qualifications, and her mature years gave her confidence to smile at his exuberances. No doubt, also, he had mellowed domestically. They lived quietly and happily together in their Berkeley hillside home, amid old books and objects of art, until her death in January 1969.

In 1970 a severe stroke terminated Dr. Evans's intellectual activities and ended his hopes of amassing yet another distinguished collection of rare books. He lingered until his death on March 6, 1971.

Dr. Evans received and appreciated many honors from the learned world, more than ample to justify the choice of a career that he had made with filial concern in young manhood.* They include honorary degrees from universities on three continents, among them the Sorbonne, the University of Geneva, San Marcos de Lima, and his own *almae matres,* California and Johns Hopkins. He was elected a member of the National Academy of Sciences of the United States (1927) and was a foreign member of the Royal Society of London and other learned societies in Europe and South America. Appointments to numerous endowed lectureships in the United States, England, and Scotland testify to the admiration of fellow scientists and their desire to have their students see and hear this notable investigator and discoverer.

* The record in *Journal of Reproduction and Fertility,* 19, 1-49 (1969), of an interview with Dr. Evans arranged by Sir Alan Parkes of Cambridge a few days after Evans's eighty-fifth birthday contains, besides a very interesting and revealing conversation, a *curriculum vitae* and a full list of honors, as well as a selected list of Dr. Evans's publications on endocrinology and the physiology of reproduction.

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KEY TO ABBREVIATIONS

Am. J. Physiol. = American Journal of Physiology
Anat. Record = Anatomical Record
Cancer Res. = Cancer Research
Carnegie Inst. Wash. Contrib. Embryol. = Carnegie Institution of Washington,
Contributions to Embryology
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Biol. Chem. = Journal of Biological Chemistry
J. Nutr. = Journal of Nutrition
Mem. Univ. Calif. = Memoirs of the University of California
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society of Experimental Biology and
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Victor Kuhn LaMer

June 15, 1895-September 26, 1966

by Louis P. Hammett

Victor LaMer had a lifelong devotion to the cause of good science and good teaching of science. That he was a perfectionist shows throughout his scientific work. In everything he did the background in principle was thoroughly studied, the experimentation was of the highest possible precision and showed the most careful attention to detail and to completeness, and the publications that resulted were clear and effective. He had the ability and the willingness, which are all too rare, to recognize the limitations of a theory as well as its strengths. In many areas his work remains a key reference, sometimes even after several decades.

He taught a central course in the graduate curriculum in chemistry at Columbia University, and he taught it with a continuing interest that reached deeply into the history of the subject as well as into the logic of its organization. He expected much of his students: He could be emphatic in his disapproval of carelessness or incompetence, but he could be equally emphatic in his praise of ability and accomplishment.

He gave richly of his time and his energy to the doctoral candidates who worked with him. He schooled them well in his own principles of probity, precision, and thoroughness, and they looked on him with respect and affection.

LaMer's own doctoral work, carried out with Henry Sher man, was in the field of the chemistry of food and nutrition—a subject to which his thesis made important contributions, especially in the application of statistical methods. His interests soon turned, however, to physical chemistry. In 1922 and 1923 a fellowship took him to Europe, where he arrived in Brønsted's laboratory in Copenhagen at an exciting period. New vistas in the old field of electrolyte chemistry had suddenly been opened by the ideas of Brønsted and Debye, and LaMer became a leader in the exploitation and development of these ideas. His publications of the next decade on the activity coefficients of multiply charged ions and on the rates of reactions involving such ions still deserve the most careful study and attention from anyone concerned with the chemistry of electrolytes. He and his co-workers also did important theoretical work on electrolyte solutions.

When in 1931 the existence of deuterium was discovered at Columbia, LaMer did pioneering work on the properties of solutions in heavy water. His studies on acid-base equilibria in that solvent were especially significant. In 1933 he questioned the prevalent myth that activation energy is independent of temperature, and shortly thereafter he and his students demonstrated experimentally that it does depend sharply on temperature for reactions involving ions in solution. This lent valuable support to the then nascent transition state theory of reaction rate, a theory that has revolutionized the way in which chemists interpret the rates of reactions in solution. In the same year he reported a study of acid-base equilibria in the poorly ionizing solvent benzene. This was a ground-breaking investigation in a field that has since become one of major importance.

With the arrival of World War II, LaMer undertook as a patriotic service the investigation of smokes and other fine dispersions. He and Sinclair established principles and developed what is now a standard apparatus for the preparation of monodisperse aerosols. They further discovered a new optical effect—the higher order Tyndall spectra—that enables one to measure particle size rapidly and simply.

When the war ended LaMer was of an age when many scientists tend to slow down and to continue along well-trodden paths. For him, however, the post-war period was one of adventure into new fields and of highly original activity. His interest in gaseous dispersions expanded to liquid systems, he made contributions to the difficult problems involved in sedimentation and filtration, and he developed principles and made valuable new observations with respect to flocculation and dispersion—processes of potentially large technical importance as well as of purely scientific interest. Novel studies of the rate of evaporation through surface monolayers also combine scientific interest with potential applications of value for the conservation of water supplies.

Victor Kuhn LaMer was born in Leavenworth, Kansas, on June 15, 1895, the son of Joseph Secondule LaMer and Anna Pauline Kuhn. He obtained the A.B. degree at the University of Kansas in 1915. During the next two years he was a high school teacher, a student at the University of Chicago, and a research chemist at the Carnegie Institution of Washington. In 1917 he was commissioned 1st Lieutenant in the Sanitary Corps, U.S. Army. He entered graduate school at Columbia University in 1919 and obtained the Ph.D. degree there in 1921. Appointed instructor in general and inorganic chemistry at Columbia in 1920, he rose through various grades at that institution, attaining full professorship in 1935. Awarded a Cutting Fellowship, he worked at Cambridge University in 1922 and at the University of Copenhagen in 1923. He was a member of Division 10 of the Office of Scientific Research and Development, 1940-1945. He became Emeritus Professor of Chemistry in 1961, but continued his scientific activities. He held the position of Senior Researcher in Mineral Engineering at Columbia, and he continued until 1965 as editor of the *Journal of Colloid and Interface Science.* He had been the founding editor of that journal in 1956, and in March 1966 a *Festschrift* edition honored him on his retirement as editor and on his seventieth birthday. He was in England to present a paper at a meeting of the Faraday Society at the time of his sudden death in Nottingham on September 26, 1966.

LaMer was honored by the Presidential Certificate of Merit in 1945, by the Kendall Award in Colloid Chemistry in 1956, by the honorary D.Sc. degree of Clarkson College of Technology in 1962, and by election to the Royal Belgian Academy of Arts, Letters, and Sciences and to the Royal Danish Academy of Science. He was Honorary Professor of San Marcos University of Lima, Peru, in 1950, Fullbright Professor at Copenhagen in 1953, and Fullbright Lecturer in Australia in 1959.

He was elected to the National Academy of Sciences in 1948. He was also a member of the American Chemical Society, the American Physical Society, the Faraday Society, and Sigma Xi and Phi Lambda Upsilon; he was a Fellow of the New York Academy of Sciences, of which he had been President in 1949. He was a member of the Cosmos Club of Washington and of the Men's Faculty Club of Columbia University.

On July 31, 1918, he married Ethel Agatha McGreevy. They had three daughters: Luella Belle (Mrs. A. P. Slaner), Anna Pauline (Mrs. Alex Burgo), and Eugenia Angelique (who died in childhood). The LaMers lived in Leonia, New Jersey, which was the home of many others of the Columbia faculty, and were active in social and community affairs of that town.

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KEY TO ABBREVIATIONS

Am. J. Phys. = American Journal of Physics
Am. J. Physiol. = American Journal of Physiology
Ann. N.Y. Acad. Sci. = Annals of the New York Academy of Sciences
Chem. Rev. = Chemical Reviews
Ind. Eng. Chem. (Anal. Ed.) = Industrial and Engineering Chemistry (Analytical Edition)
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Biol. Chem. = Journal of Biological Chemistry
J. Chem. Educ. = Journal of Chemical Education
J. Chem. Phys. = Journal of Chemical Physics
J. Colloid Sci. = Journal of Colloid Science
J. Phys. Chem. = Journal of Physical Chemistry
Phys. Rev. = Physical Review
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Trans. Faraday Soc. = Transactions of the Faraday Society

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Irving Langmuir

January 31, 1881-August 16, 1957

by C. Guy Suits and Miles J. Martin

Few scientists, in either university or industry, have made as many, and as significant, contributions to scientific progress as did Dr. Irving Langmuir, the 1932 Nobel Prize winner in chemistry. It was on July 19, 1909, that Langmuir joined the General Electric Research Laboratory in which he was to become, first, assistant director and then associate director and which was to be the scene of his greatest achievements.

One of Langmuir's first achievements was in the field of lighting. After Dr. William D. Coolidge, also of the Research Laboratory, developed the drawn tungsten-filament incandescent lamp, it fell to Langmuir to further develop an improved lamp—a gas-filled one instead of the vacuum type—and thereby make a great gain in lighting efficiency.

The gas-filled lamp soon began driving arc lamps from the street lights, greatly increasing the use of electric lighting by increasing efficiency. With the lower cost of lighting came a large increase in the amount of light used, so that electric utility revenues from lighting were soon higher than ever before. They continued to increase steadily as efficiency improved. Further improvements in incandescent lamps were to be made in various laboratories, but Coolidge's tungsten filament and Langmuir's gas filling remain today two basic elements of incandescent lamps.

Irving Langmuir, in the forty-one active years he was a versatile researcher in the General Electric laboratory, was distinguished for his epoch-making discoveries in science and also for the many very important practical applications that were made of his work. His scientific productivity was prodigious. He published about five scientific papers a year for the full period of his research career, and the resulting group of more than 200 papers* included a great diversity of topics, for example:

The Laws of Convection and Conduction of Heat in Gases (1912).

The Effect of Space Charge and Residual Gases or Thermionic Currents in High Vacuum (1913).

The Constitution and Fundamental Properties of Solids and Liquids (1916).

The Condensation Pump: An Improved Form of High-Vacuum Pump (1916).

The Arrangement of Electrons in Atoms and Molecules (1919).

Chemical Reactions on Surfaces (1921).

The Electron Emission from Thoriated Tungsten Filaments (1921).

Atomic Hydrogen Arc Welding (1926).

The Theory of Collectors in Gaseous Discharges (1926).

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Oxygen Films on Tungsten (1931).

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Rates of Evaporation of Water through Compressed Monolayers on Water (1943).

Studies on the Effects Produced by Dry-Ice Seeding of Stratus Clouds (1948).

* See the twelve volumes entitled *The Collected Works of Irving Langmuir,* Pergamon Press, Elmsford, N.Y. (1962).

ELECTRONICS INDUSTRY

Langmuir's study of thermionic phenomena produced effects that later became the heart of the electronics industry. His research gave the world the first high-vacuum electron tubes and the first high-emission electron tube cathodes.

Not only was the study of heat transfer in gases the scientific source of Langmuir's basic invention of the gas-filled lamp and atomic hydrogen welding but it also provided the technology for hydrogen-cooled turbines.

Langmuir made basic contributions to the understanding of gaseous discharge phenomena—he invented the word *plasma*—and his work on surface films, later protein films on water, provided an important new technique in biochemistry. He received the Nobel Prize in 1932. Later he devoted his time more to "science out-of-doors."

SCIENTIFIC WORK

Irving Langmuir's scientific career covered fifty years, starting in 1904 with his doctoral dissertation at Göttingen, "Ueber partielle Wiedervereinigung dissociierter Gase im Verlauf einer Abkühlung," and ending in 1955 with an unpublished report on "Widespread Control of Weather by Silver Iodide Seeding." In order to convey a feeling for the diversity of his work, Langmuir's published scientific work has been grouped into seven categories below, this grouping following rather closely that used by Langmuir himself in the Introduction to *Phenomena, Atoms, and Molecules,* a reprint* of some twenty of his papers selected by him in 1950. The dates associated with each category indicate when most of the relevant work was published, although it will be clear from the span of some of these dates that Langmuir's productive interest in certain areas continued

* Philosophical Library, Inc., New York, N.Y. (1950).

throughout a large part of his active scientific life and even into retirement:

1906 to 1921	Chemical Reactions at High Temperatures and Low Pressures
1911 to 1936	Thermal Effects in Gases
1919 to 1921	Atomic Structure
1913 to 1937	Thermionic Emission and Surfaces in Vacuum
1916 to 1943	"Chemical Forces" in Solids, Liquids, and Surface Films
1923 to 1932	Electrical Discharges in Gases
1938 to 1955	Science Out-of-Doors

Chemical Reactions at High Temperatures and Low Pressures

When Langmuir commenced his doctoral work at Göttingen, Professor Walther Nernst suggested as a thesis subject the study of the formation of nitric oxide from air in the vicinity of a glowing Nernst filament. It was thought that the filament would act catalytically on the reaction between oxygen and nitrogen and that the final equilibrium would correspond to the temperature of the filament. This method of studying equilibria looked extremely attractive because of the simplicity of the apparatus involved, compared with the complexity of the equipment more generally used in such studies. This simple hypothesis proved not to be applicable to the interaction of nitrogen and oxygen in the vicinity of a glowing Nernst filament, and the thesis effort was shifted to studying other gaseous equilibria, such as the dissociation of carbon dioxide brought about by a glowing platinum filament, where the hypothesis was found to be valid.

This very early work is especially interesting as a foreshadowing of Langmuir's predilection for experiments requiring only simple apparatus but where understanding of the experimental results might involve new, bold concepts and extended theoretical analysis. In this case, the work led to an understanding of the unexpectedly much greater importance of thermal conduction, as compared with convection, in determining the heat loss from a filament through the first few tenths of a millimeter of gas surrounding it.

This thesis work was also important in orienting Langmuir's scientific interests in 1909 when, at Dr. Willis R. Whitney's invitation, he joined the Research Laboratory of the General Electric Company, which laboratory had been established only nine years before. Dr. Whitney suggested that Langmuir should spend a few days looking around to see what was going on, and the first entry in his laboratory notebook reads:

> July 19-July 21
>
> Spent these two days looking thru lab and seeing what work was being done.

Apparently "these two days" were sufficient to show Langmuir that the laboratory was intensely interested in problems connected with making good incandescent lamps out of the then-new ductile tungsten wire just introduced by Coolidge. The first experiments of his choice were concerned with preparing pure hydrogen and studying the effects of heating tungsten wire in it.

At that time, all incandescent lamps were vacuum lamps, and the general feeling was that, if the vacuum could be made better, the life of the lamp would be improved. Langmuir, on the other hand, had been impressed with how much better lamp-factory vacuum was than what had been available to him at the university, and, not knowing how to improve on this, he resolved to see what effects the opposite approach of adding various gases would have on the life of tungsten lamps. He was also impressed with the ready availability of tungsten wires capable of being heated electrically to very high temperatures. From this combination of good vacuum and high-temperature filaments grew his work on chemical reactions at high tempera tures and low pressures. These studies included the discovery and detailed investigation of the formation of atomic hydrogen by contact of molecular hydrogen with a hot tungsten filament, a careful analysis of the effects of water vapor in incandescent lamps, and a systematic investigation of the mechanism of "cleanup" of oxygen, nitrogen, and other gases at low pressure by hot tungsten and molybdenum filaments.

Thermal Effects in Gases

Langmuir had established that, apart from a special chain reaction with water vapor, the life of a tungsten vacuum lamp was insensitive to the residual gases usually present and was determined entirely by the evaporation of tungsten. This encouraged him to experiment with lamps containing much higher pressures of inert gases and to study heat losses from filaments under these conditions.

He found that the evaporation of tungsten in nitrogen at approximately atmospheric density is essentially a diffusion process and obeys laws similar to those of conduction or convection of heat from a wire; that is, for wires of small diameters, the actual amount of tungsten evaporated is almost independent of the size of the wire, an unfavorable result for the very small filaments used in most lamps. On the other hand, experiment showed that, for several reasons, life and efficiency were better for large filaments in nitrogen. This dilemma was resolved by coiling the small wire tightly into a helix of substantially larger diameter, a form of construction that led to widespread adoption of the gas-filled lamp.

The dissociation of hydrogen by a hot tungsten filament had been postulated by Langmuir to explain the sudden increase in heat loss from a filament in hydrogen at high temperatures. Estimates of the heat of dissociation were made, and some properties of atomic hydrogen were observed, such as its adsorption on a cold glass wall.

Several years later Langmuir's attention was attracted by R. W. Wood's[*] preparation of concentrated atomic hydrogen in an electric discharge tube and Wood's observations on the heating effects produced by the recombination of the atomic hydrogen on a variety of surfaces. This led Langmuir to the invention of the atomic hydrogen welding torch, in which large amounts of atomic hydrogen are produced by an arc between tungsten electrodes in hydrogen, and the atoms are allowed to recombine on the metal to be heated.

Atomic Structure

Some of Langmuir's most productive thinking was guided by consideration of the differences between what he called "physical forces" and "chemical forces." This thinking led to his concept of the adsorption process and also to his rather brief sortie into the field of atomic structure during 1919-1921.

The Bohr[†] theory was then well established by reason of its spectacular spectroscopic successes. Langmuir considered this to be a typical "physical force" theory based on forces acting according to simple laws between mathematical points separated by relatively large distances. The chemist, on the other hand, did not think of molecules as point centers of force, but rather as complex entities having structures which made the outward acting "chemical forces" at one part of the molecule quite different from those at another. Moreover, the "chemical forces" were usually of shorter range than the "physical forces." This thinking, together with G. N. Lewis's[‡] theory of the "cubical atom" and a keen feeling for the complex chemical phenomena to be explained, led Langmuir to his "octet theory" of atomic structure, in which Bohr's centrally orbiting electrons were replaced by electrons distributed in regions throughout the

* R. W. Wood, *Proc. Roy. Soc.,* 102, 1 (1922), and *Phil. Mag.,* 44, 538 (1922).
† N. Bohr, *Phil. Mag.,* 26, 1 (1913).
‡ G. N. Lewis, J. *Am. Chem. Soc.,* 38, 762 (1913).

atom, each electron being stationary in its region or describing a restricted orbit within the region.

With these concepts, and a limited number of postulates, Langmuir was able to correlate a tremendous variety of chemical phenomena. Further detailed calculations, however, led to the need for more assumptions, and it was not long until the advent of quantum-mechanical concepts of chemical bonds led him to transfer his efforts to other problems. Langmuir, while appreciating the great conceptual contributions made by quantum mechanics, was impressed by the tremendous mathematical difficulties of attempting to understand chemical properties in detail by this route. Because of this he apparently made a decision not to develop a working knowledge of these new tools for himself, but to continue his work where more classical methods were still fruitful.

Thermionic Emission and Surfaces in Vacuum

As a natural outgrowth of his earlier work on tungsten lamps, Langmuir entered the field of thermionic emission in 1913 to answer the specific question of why relatively large electron currents did not appear as shunt currents from the negative leg to the positive leg of a tungsten lamp with a hairpin filament. At that time the true origin of thermionic emission was still in doubt, and there were even suggestions that the thermionic electrons were by-products of a chemical reaction and, therefore, that the absence of the shunt current in lamps was due to the very high vacuum. Langmuir made experiments with lamps containing two separate hairpin filaments and soon arrived at the concept that the shunt currents were small because the charges on the electrons in the space between the legs of the filament shielded the negative leg from the acclerating field due to the positive leg.

This hypothesis was at once submitted to theory and calcu lation, resulting in the Child-Langmuir* space-charge equation, according to which the electron current between electrodes of any shape in vacuum is proportional to the 3/2 power of the potential difference between the electrodes. This celebrated law was followed through in great detail for various electrode configurations, and corrections for the initial thermal velocities of the electrons were introduced. The 3/2 power law became an important issue in a hard-fought patent suit concerning electron discharges in very high vacuum, a major result of which, perhaps, was to illustrate the difficulty of patenting something that came so close to being a law of nature.

Thorium oxide is added to tungsten lamp filaments to improve their mechanical behavior at high temperatures, and it had been observed sporadically that abnormally high thermionic emission was obtained from some lamp filaments. When Langmuir undertook a systematic study of this problem, he soon showed that the abnormally high emission was definitely associated with the presence of thoria in the filament. He worked out in great detail the temperature treatment needed to obtain thoriated emission and the magnitude of the emission under various conditions. His theoretical study of the phenomenon showed that the enhanced emission could be explained in terms of the formation by diffusion of a single, more or less complete layer of thorium atoms on the surface of the filament. These rather detailed and involved concepts were obtained by interpretation of experiments with the simplest of vacuum tubes and current measurements with a portable microammeter.

It is interesting to observe that the interpretations of such simple experiments, in the hands of so great a master, at times cause corrections in detail. Langmuir interpreted the transient

* C. D. Child, *Phys. Rev.,* 32, 492 (1911). Independent derivations of this equation were made by Langmuir for electrons and by Child for positive ions about two years earlier.

behavior of the surface film in formation as being due to a combination of the diffusion of the thorium atoms through the tungsten lattice plus a reasonable assumption of "induced evaporation" when a new thorium atom arrived under one already in the surface layer. It was not until considerably later that more complicated experiments by P. Clausing* showed that the thorium really diffused to the surface through the intercrystalline material and then spread over the surface from these lines of access in a two-dimensional diffusion. Yet, some thirty years later Clausing revealed that Langmuir's computations had been the correct ones after all.

Another extended series of thermionic studies, done in collaboration with K. H. Kingdon and J. B. Taylor, involved new phenomena observed when cesium is put into a vacuum tube containing a tungsten filament. At low filament temperatures, and particularly if the filament is first coated with a monatomic layer of oxygen, the cesium atoms are strongly adsorbed from the vapor onto the surface of the filament. Such a cesium-oxygen-tungsten surface is the most efficient thermionic emitter known, and high hopes were entertained at first for its application in radio tubes. However, the advent of conventional barium oxide cathodes heated from the alternating current supply replaced this possible application.

Another new phenomenon observed was that, at higher filament temperatures, cesium atoms (ionizing potential 3.9 volts) striking a tungsten filament are robbed of an electron by the filament (work function 4.5 volts) and come off as positive ions that may be collected at a negative electrode. Langmuir developed a theoretical interpretation of these phenomena in terms of his concepts of adsorbed films and the Saha equation. This equation gives the equilibrium concentrations of ions, electrons, and neutral atoms at a known temperature in a gas with known ionization potential and for this application must

* P. Clausing, *Physica,* 7, 193 (1927).

be modified to include the electron-emitting capability of the hot filament, since this shifts the temperature equilibrium.

Langmuir had great hopes for the use of this controlled source of ionization for neutralizing electron space charge in power tubes, but experiment and, later, theory showed that only modest effects could be obtained. The principle of ionization at a hot surface found early application as a molecular beam detector and at present (1965) is being investigated as a source of ion propulsion for space vehicles.

All kinds of studies and processes requiring evacuated enclosures were aided tremendously by Langmuir's invention of the condensation pump. That pump was simple to construct, had very high speed, and, with the aid of refrigerants, produced an extremely high vacuum. It came rapidly into widespread use.

"Chemical Forces" in Solids, Liquids, and Surface Films

"Chemical forces" in solids, liquids, and surface films is the area of science to which about one quarter of Langmuir's publications are devoted and for which he received the Nobel Prize in Chemistry in 1932. His ideas on the short-range character of "chemical forces" led him to a new concept of adsorption, in which every molecule striking a surface remained in intimate contact with the surface for a short or long time and then evaporated. This adsorption contact was so intimate that it might be thought of as a chemical bond, and thus the concept of a firmly held, single layer of adsorbed atoms replaced the existing idea of a relatively thick adsorbed sheath extending some distance out from the surface with concentration decreasing with distance.

Langmuir made an early application of his ideas on surface films to the study of films on water surfaces, an area of science to which his attention had been drawn by the beautifully simple experimental techniques developed over the years by Miss

Pockels, Lord Raleigh, Devaux, and Marcelin.* Langmuir made a tremendous extension of this technique by the introduction of a surface balance method for measuring the spreading force of the films and showed that these films were truly monomolecular. In collaboration with Katharine B. Blodgett and Vincent J. Schaefer, he developed a whole series of techniques for working with surface films and used them to study gaseous, liquid, and solid films, including films of such complicated molecules as proteins.

Langmuir's ideas on adsorbed films were also applied to films on solids, leading to the Langmuir adsorption isotherm, which gives an expression for the fraction of the surface covered by the adsorbed layer in terms of ambient pressure and a temperature-dependent variable characterizing rates of condensation and evaporation at the surface. A theory was developed for the catalytic effect of an adsorbing surface which considered the chemical reaction as actually occurring in the adsorbed film and elucidated many features of such reactions which hitherto had been obscure. This theory became the basic approach to surface kinetics.

Electric Discharge in Gases

In 1914, mercury arc rectifiers were in common use, and Langmuir gradually became interested in some of the scientific problems of electric discharges in gases. He was impressed with the opportunities for electron tubes capable of controlling high power and endeavored to apply the newly understood phenomena of vacuum electron amplifier tubes to the much larger currents of gas tubes. Ernst F. W. Alexanderson pointed out in conversation that in alternating-current power circuits the important thing was to be able to control the initiation of the current in any part of the circuit and that, if this were done,

* A. Marcelin, *Ann. Phys.,* 1, 19 (1914).

the circuit reversal of voltage could be used to extinguish the current at a later time in the cycle. Langmuir in 1914 came upon the idea of inserting a grid in a mercury arc device to control the starting of current to a main anode. The study of such devices was carried on in collaboration with A. W. Hull and others.

Langmuir's activity in the gas-discharge field developed rather slowly, but in 1923 he was analyzing the current-voltage characteristics of currents to probe electrodes placed in a mercury arc. He found that the current to a negatively charged plane collector was independent of voltage over a wide range and showed by gridding tests that the current was due to the arrival of positive ions rather than the emission of photoelectrons. The independence of voltage was explained in terms of a plane-parallel "positive ion space-charge sheath" in front of the plane electrode, whose thickness increased with voltage in accordance with the 3/2 power space-charge law, but whose outer surface was entered by an invariant stream of positive ions.

This initial work with collecting electrodes or probes led to a most fruitful series of experiments in collaboration with H. M. Mott-Smith, Jr., and Lewi Tonks. The current-voltage characteristics of probe electrodes of all sorts were studied experimentally and theoretically, the results being used to elucidate the mechanism and behavior of electric discharges in gases under a wide range of conditions. One of the most important concepts arising from this work was the interpretation of the volt-ampere characteristic of a probe collecting electrons in terms of a Maxwellian temperature distribution of the electrons. The temperatures found were very high, of the order of 15,000 K, and showed that the electrons were continually receiving energy from the drift gradient in the discharge and were far from being in temperature equilibrium with either the positive ions or the neutral gas. The ionization in the discharge could be explained quantitatively in terms of the ionization effectiveness of the electrons in the high energy Maxwellian tail.

Langmuir and Tonks also made a study of electrical oscillations in an ionized gas. Langmuir was so impressed with the implication of organization and structure in the ionized gas, evidenced by the capability of oscillation, that he adopted the word "plasma" to indicate the fundamental nature of a volume of ionized gas essentially free of space charge. At the same time he was much impressed with the instabilities and the energy-transfer capabilities of a plasma, properties which have come to the fore in recent studies of fusion plasmas.

Science Out-of-Doors

Langmuir had a keen and continuing interest in the scientific basis of outdoor phenomena. One of the earlier examples of this was his explanation of the streaks or windrows of seaweed and bubbles which form on the ocean parallel to the direction of a moderate wind. He had noticed this phenomenon especially during an Atlantic crossing in August 1927, and he studied windrows for several years at Lake George, New York, where he had a summer cottage. By simple but carefully planned experiments, using such apparatus as oriented umbrellas and lamp-bulb floats, Langmuir was able to establish that the windrows were caused by wind-induced circulation of the surface water, the water on the surface flowing toward the windrows, downward under them, and up again at a point halfway between them.

An extended series of experiments with V. J. Schaefer was devoted to the production of particles of various desired sizes in atmospheric air, their behavior in filters, and the development of apparatus for generation of oil smokes for military use. The resulting smoke generator was of the order of one hundred times more effective than existing generators and was adopted and used by the United States forces in World War II.

Another series of studies with Schaefer concerned the nucleation of ice crystals in supercooled clouds by seeding with particles of solid carbon dioxide; Schaefer showed that the low temperature is the important thing. This led to a great deal of outdoor experimentation related to possible modification of the weather by such processes. This was the last field of science in which Langmuir took an active part.

The diversity of Langmuir's scientific work shows his great breadth of interest and also indicates his characteristic approach—which was to seize on some unusual phenomenon or technique, exploit this until the returns showed signs of diminishing, and then pass on to something else.

Examples of this approach were his realization that the ready availability of tungsten filaments and good vacuum conditions in the General Electric Research Laboratory (in 1909) offered an excellent opportunity for the study of chemical reactions at high temperatures and low pressures; his attack on the random, anomalously high electron emission observed from tungsten filaments containing thoria; his appreciation of the possibilities of the surface tension trough for the study of surface film phenomena, coupled with the insight that his ideas on localized "chemical forces" gave him into the fundamentals of the problem; and his realization of the power of the probe-characteristic technique for studying the mechanism of electric discharge in gases.

Langmuir's brilliant insight into fruitful directions for applying his effort was coupled with the characteristic of being a tremendous worker. Although he was not by any means a slave to science, science was never far from his thoughts, whether in the laboratory, at home, traveling, or out-of-doors.

Perhaps the best demonstration of this work effort is given by Langmuir's notebooks. During his many active years in the laboratory, Langmuir filled fifty-four notebooks, of three hundred thirty pages each, with the details of his scientific work. These notebooks exhibit the vast foundation of data, theory, and numerical calculations on which the structure of Langmuir's published scientific work was erected. There was always a notebook with him, and he wrote in it at any time, even in a sleeping-car berth. His records were especially detailed regarding the genesis of ideas and must have been the delight (or the embarrassment?) of all patent lawyers who encountered them.

Langmuir combined his unusual scientific ability with a strong practical bent and was always on the lookout for applications of any idea or new piece of knowledge with which he came in contact. For example, he was keenly aware of the great opportunities for electron tubes capable of controlling power circuits, and his notebooks contain detailed schemes of using "trapped ions" and cesium thermions for neutralizing electron space charge. Both of these possibilities were later made obsolete by the development of controlled plasma tubes.

Langmuir made one hundred thirty-eight personal patent disclosures, of which sixty-three led to issued patents. Some of these were of great practical importance, such as the gas-filled incandescent lamp, high-vacuum electron tube principles, the condensation pump, the thoriated tungsten filament, atomic hydrogen welding, the grid-controlled arc, and the military smoke generator. Much of Langmuir's scientific work lay in areas in which there was little opportunity for patent protection, but whenever the opportunity existed, Langmuir was quick to recognize it.

Langmuir had well-developed abilities as an applied mathematician. His notebooks were filled with theoretical work related to his experimental investigations and with the results of extended computations made in testing the theories. Most of these computations were on the desk computer scale, since much of his work was done before the advent of large computers.

Most of Langmuir's scientific work was accomplished with the assistance of relatively few people. He never directed a large research team. In much of his earlier experimental work he was assisted by Samuel P. Sweetser, whose name appears in many publications. Sweetser prepared the experimental equipment and, with meticulous care, took volumes of characteristic curves for analysis by Langmuir. At one time, when the electron emission studies were very active, Langmuir's notebook gave a list of some eighty-two experiments that he was interested in having Sweetser do. With such a backlog of work it would seem that the direct experimental staff might have been expanded, but apparently Langmuir preferred not to do this.

This may have been partly because Langmuir was usually closely connected with experimental work being done by some of the younger scientists in the laboratory, many of whom, at any given time, could be found working on problems he had suggested. In addition to these activities, the notebooks are filled with suggestions for work made to many people inside and outside the laboratory who were not at all directly connected with Langmuir. In this way, without formal organization, his scientific influence touched a great many people.

Langmuir's personal characteristics may be described by such words as sincerity, intensity, vigor, intellectual integrity, breadth of interest, depth of approach. His speech was rapid, emphatic, and filled with the intensity of his interest in the ideas that he was trying to convey.

In spite of these hard-driving characteristics, Langmuir was always approachable and willing to give his best attention to any sensible problem brought to him. If he passed anyone in the hall without recognition, this was not a demonstration of aloofness, but rather of his complete absorption in the current scientific problem. If the problems of others demanded too much of his time, he could always retire to his study at home until his own current idea was satisfactorily advanced. Monday morning was frequently dedicated to an exposition of the scientific developments of the weekend to those who might be interested.

Irving Langmuir was born in Brooklyn, New York, on January 31, 1881, the son of Charles and Sadie Comings Langmuir. His mother was a descendant of the Lunt family, which came to this country on the *Mayflower.* His father, who was in the insurance business, came from a Scottish family.

After obtaining elementary education in public schools in Brooklyn, he traveled with his parents to Paris, where he studied for three years. He then returned to the United States, studied for a year at Chestnut Hill Academy, Philadelphia, then in Brooklyn at Pratt Institute, and at the School of Mines, Columbia University. In 1903 he was graduated from Columbia with a degree in metallurgical engineering.

"The metallurgical engineering course was strong in chemistry," he explained. "It had more physics than the chemical course, and more mathematics than the course in physics—and I wanted all three."

Again he visited Europe, this time to study at the University of Göttingen, in Germany, where he was awarded M.A. and Ph.D. degrees in 1906. From then until he joined the General Electric Company, in 1909, he taught chemistry at the Stevens Institute of Technology.

Dr. Langmuir and his wife, the former Marion Mersereau of South Orange, New Jersey, whom he married in 1912, made their home at 1176 Stratford Road, Schenectady, New York, with a son, Kenneth, and a daughter, Barbara.

He died August 16, 1957.

HONORS

Among scientific honors that were bestowed on Langmuir, the Nichols Medal, awarded by the New York Section of the American Chemical Society, was twice given to him, once in

1915 for his work on chemical reactions at low pressures and again in 1920 for his work on atomic structure.

In 1918 Langmuir was elected to the National Academy of Sciences and in the same year was awarded the Hughes Medal of the Royal Society of London for his researches in molecular physics.

In 1920, the American Academy of Arts and Sciences gave him the Rumford Medal for his thermionic researches and for his development of the gas-filled incandescent lamp.

In 1925, the Royal Academy of Lincei at Rome, Italy, bestowed on him the Cannizaro Prize. In 1928 he was the recipient of the Perkin Medal of the Society of Chemical Industry, and in 1930 he was awarded the Chandler Medal of Columbia University and the Willard Gibbs Medal of the Chicago Section of the American Chemical Society.

In 1932, Langmuir became the first American industrial chemist to be awarded the Nobel Prize, granted him for researches in surface chemistry. In the same year, *Popular Science Monthly* magazine awarded him its annual medal and honorarium of $10,000 for "an American who has done notable scientific work."

The Franklin Medal of the Franklin Institute and the Holly Medal of the American Society of Mechanical Engineers were given him in 1934, and the city of Philadelphia presented him the John Scott Award in 1937.

In 1940 he received a plaque as a "Modern Pioneer of Industry" from the National Association of Manufacturers, and in 1943 he became an honorary member of the British Institute of Metals.

In 1944, Langmuir became the fourth American to receive the coveted Faraday Medal of the British Institute of Electrical Engineers.

The Mascart Medal of the Société Française des Electriciens was presented to Langmuir in 1950.

He was a foreign member of the Royal Society of London and a Fellow of the American Physical Society. He had served as president of the American Chemical Society in 1929 and as president of the American Association for the Advancement of Science in 1941.

An honorary member of several societies, including the Chemical Society of London, Langmuir held honorary degrees from the following colleges and universities: Northwestern, Union, Edinburgh (Scotland), Columbia, Kenyon, Princeton, Lehigh, Harvard, Oxford, Johns Hopkins, Rutgers, Queens (Canada), and Stevens Institute of Technology.

Irving Langmuir spoke frequently of "the freedom that characterizes democracy and is necessary for making discoveries." His contributions to men everywhere should be measured both by the value of his scientific discoveries and by the extent of his efforts to maintain and enlarge the meaning of freedom. All of us who were honored to be his associates are sustained by the unparalleled legacy of knowledge and inspiration he has left to us and all the world.

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KEY TO ABBREVIATIONS

Chem. Rev. = Chemical Reviews
Gen. Elec. Rev. = General Electric Review
Ind. Eng. Chem. = Industrial and Engineering Chemistry
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Chem. Phys. = Journal of Chemical Physics
J. Franklin Inst. = Journal of the Franklin Institute
Phys. Rev. = Physical Review
Proc. Am. Inst. Elec. Eng. = Proceedings of the American Institute of Electrical Engineers
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Phys. Soc. (London) = Proceedings of the Physical Society of London
Proc. Roy. Soc. (London) = Proceedings of the Royal Society of London
Rev. Mod. Phys. = Reviews of Modern Physics
Trans. Am. Electrochem. Soc. = Transactions of the American Electrochemical Society
Trans. Faraday Soc. = Transactions of the Faraday Society
Z. Electrochem. = Zeitschrift für Electrochemie

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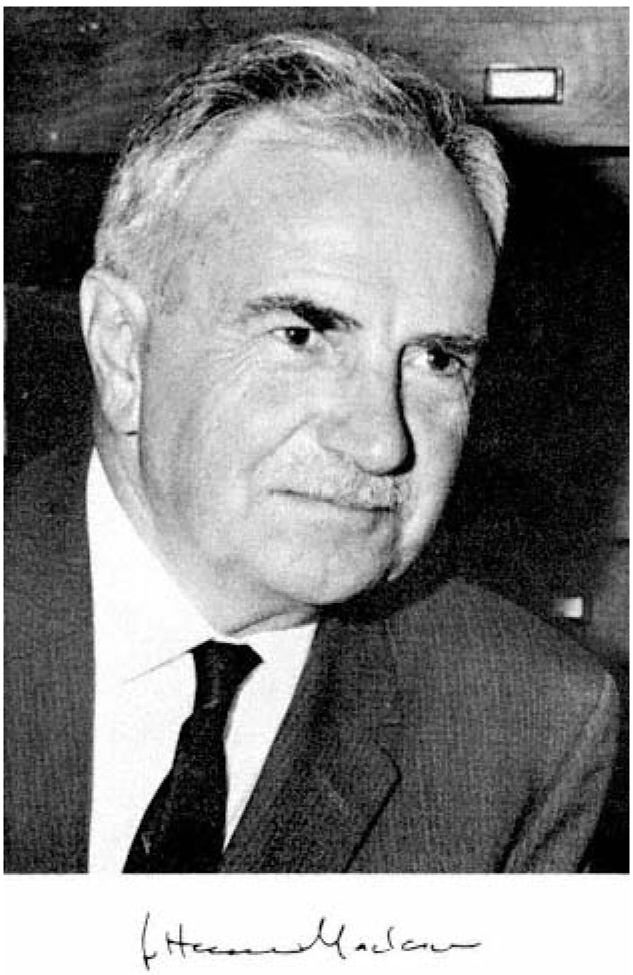

Joseph Hoover Mackin

November 16, 1905-August 12, 1968

by Harold L. James

Deep in the heart of Antarctica is a huge windswept plateau that bears the name Mackin Table. It was mapped and so designated by four of Hoover Mackin's former students-Dwight Schmidt, Paul Williams, Willis Nelson, and Arthur Ford-as a tribute to a great teacher of earth science. It is an impressive memorial to a man who was a dominant figure in American geology for more than three decades.

Joseph Hoover Mackin was born November 16, 1905, in Oswego, in upstate New York. He was the youngest of seven children of William David Mackin and Catherine Hoover Mackin. His father, who died when Hoover was only seven years old, was of Irish descent, his mother of German descent. Despite the early death of his father and despite the fact that he spent two years immobilized in a cast after being stricken with poliomyelitis at the age of four, Hoover's early years quite evidently were happy ones as the youngest child in a closely knit family. He outgrew the effects of his childhood illness and developed into a powerfully built youth of great energy. He played football—as a lineman—both at Oswego High School and Oswego Normal School, and between studies and sports he still found time to work at various jobs.

After graduation from Oswego High School in 1924 and two years at Oswego Normal School, the young Mackin left upper

New York State to enter New York University. Initially, he intended to become a journalist, but switched from journalism to geology after hearing lectures by Professor George I. Finley. He received the B.S. degree in geology from NYU in 1930 and then entered the graduate school of Columbia University. His professed major interest at that time was in petrology, but he soon came under the stimulating influence of Professors Douglas Johnson and William Morris Davis, leading American teachers of the science of landforms. From then on, Mackin's course was set: He would become a geomorphologist. He was granted the M.A. degree by Columbia in 1932 and the Ph.D. in 1936.

In 1929, at the start of the Great Depression, Mackin was married to Esther Fisk, daughter of longtime friends of the Mackin family in Oswego. In the years following, as a student, he worked at many odd jobs—in a telegraph office, as a painter, as a sandhog in subway construction, and as a tutor. In 1933, however, the young couple achieved a considerable level of affluence as the result of the award to Mackin of a $1,600 fellowship.

In 1934, after completing all requirements for the doctorate at Columbia other than a thesis, Mackin accepted an appointment as an instructor at the University of Washington, there to begin a distinguished career as a teacher that was to span thirty-four years—twenty-eight years at Washington and six years as Farish Professor of Geology at the University of Texas at Austin.

Mackin always considered himself a geomorphologist, though a glance at his bibliography reveals far greater scope to his actual research activities. His doctoral thesis dealt with the origin of surface features of the Big Horn Basin in Wyoming. In it he introduced the concept of lateral planation by a stream essentially at grade, producing gravel-mantled terraces as the stream gradually deepens its valley, as opposed to formation of terraces by stream dissection of earlier alluvial plains

Mackin's further analysis led to publication in 1948 of what is the most well-known of his contributions to geomorphology, the classic paper, "Concept of the Graded River." In this paper Mackin refers to "the almost telegraphic rapidity with which the first phases of reaction of a graded stream to a number of artificial changes are propagated upvalley and downvalley"—a warning of profound importance to stream engineers. In later pages of the same paper, Mackin makes the warning more explicit: " . . . the engineer who alters natural equilibrium relations by diversion or damming or channel-improvement measures will often find that he has a bull by the tail and is unable to let go . . . . " This statement illustrates both Mackin's gift for pithy expression and his willingness to extend the implications of scientific research into the realm of human affairs.

Throughout his career, Mackin chose to concern himself with the consequences of man's activities on the natural environment, and he often exerted a little-known but profound influence on proposed developments. He became something of a *bête noire* to engineers, but to their credit it must be pointed out that he was called upon often for advice. As a small but specific example of his influence, he pointed out that the engineering plan to "save" Ediz Hook, a scenic sandspit on the north side of the Olympic Peninsula, if put into effect, might well result in the complete destruction of Ediz Hook in the next major storm. His private report on the situation resulted in cancellation of the intended work. Clearly, Hoover Mackin was an environmentalist before that term was coined.

One of Mackin's earliest papers was written with E. B. Bailey and based on a brief field trip taken while Mackin was still a graduate student and the famous British geologist was in this country on a visit. The paper dealt with the complex folding in the Pennsylvania Piedmont and the use of b-lineation in structural analysis. This paper was perhaps the first attempt to apply the concepts of recumbent folding and nappe structure to geologic interpretation of the piedmont. Since then, these concepts have been shown to be widely applicable, even though the specific area of the Mackin-Bailey paper remains a subject of controversy, as indicated by Mackin's 1962 "Note" in the *Bulletin of the Geological Society of America.* A by-product of the initial study was Mackin's 1950 paper on the "down structure" method of viewing and interpreting geologic maps. The principle employed was not new, but nowhere had it been expressed so concisely; it remains required reading for the beginning student of structural geology.

In World War II, Mackin became affiliated with the U.S. Geological Survey, an organization with which he was to retain close ties for the remainder of his career. As part of the Survey's wartime emphasis on sources of strategic minerals, Mackin studied quicksilver deposits near Morton, Washington, and placer deposits containing radioactive minerals in Idaho. His major effort, however, which continued after the end of World War II, was on the iron deposits of the Iron Springs district of Utah. The results of this study comprise one of the finest contributions to the science of ore deposits of the past three decades. The first paper, written within the space of a few days as a guidebook for a Utah Geological Society field conference, almost surely is the most widely referenced informal publication in economic geology literature. In it and in later papers, Mackin demonstrated beyond reasonable doubt precisely *where* the iron that forms the major economic deposits of the district came from, *how* it was separated from the parent body of intrusive quartz monzonite, *why* it was deposited in adjacent limestone in the particular places now found, and *when* this process took place in the igneous and structural history of the area. In the course of the study, Mackin was to demonstrate that in certain types of magmatic flow, phenocrysts and inclusions become oriented normal, rather than parallel, to the direction of magma movement; a leading authority on granite tectonics ranks the brief guidebook discussion as the most incisive and definitive treatment of the subject of magmatic flow in the English-language literature.

That contributions of such significance to the understanding of ore deposits and of granite tectonics could be made by one who classed himself as a geomorphologist, and who disclaimed any competence as a student of economic geology and igneous petrology, must seem remarkable to those who did not know the man. But careful examination of Mackin's papers will reveal that these scientific advances were achieved by the same kind of thinking and analysis he had used in assessing the dynamics of surface processes. The mass of twenty million-year-old quartz monzonite, the source of the ore fluids, was not in Mackin's eyes a dead body: He had the imagination necessary to visualize it during its emplacement as a fluid silicate melt, the ability to analyze its probable behavior and to predict the likely consequences, and the observational capacity to locate the critical evidence. Aside from its contribution to granite tectonics, this study remains perhaps the only documented example in world literature of the exact genetic relation between an igneous intrusion and an associated hydrothermal ore deposit.

Mackin's work in the Iron Springs district led him by progressive stages, involving several of his students, into the broader problems of the volcanic and structural history of the Great Basin of Utah and Nevada. An area of some 3,000 square miles was mapped at a scale of 1:62,500 or larger, and an additional 7,000 square miles was mapped in reconnaissance. On the basis of this work, a regional stratigraphy of the ignimbrite sequences was established using a technique based on quantitative measurement of phenocryst content. Mackin concluded, in agreement with the very early work of Gilbert, that the characteristic block faulting of the area was due to "dominantly vertical displacements of comparatively rigid blocks," rather than to com pressive forces, and he suggested that these movements were a consequence of the withdrawal from depth and surface extrusion of the estimated 50,000 cubic miles of silicic volcanics. In the 1960 paper in which these views were presented is a promise of another paper, "to follow shortly," in which the concept was to be developed more fully; this paper, unfortunately, never appeared.

Mackin's basic philosophy in scientific endeavor is expressed in his 1963 paper, "Rational and Empirical Methods of Investigation in Geology." In it he reveals a basically traditional approach—the "rational" method, patterned after G. K. Gilbert, T. C. Chamberlin, and Douglas Johnson. This method of problem-solving involves a close interplay between observation and deductive reasoning, with emphasis on the use of logic to establish multiple working hypotheses and ultimate definition of the critical diagnostic data required for choice of conclusions. Mackin viewed with considerable skepticism what he referred to as the "empirical" (or "engineering") method, in which emphasis is laid on accumulation and subsequent treatment of large amounts of quantitative data. This often has been taken to mean opposition by Mackin to the quantitative approach in scientific problems, and perhaps the criticism was justified to some extent, particularly when applied to his attitude toward quantification in his own field of specialization, geomorphology. On occasion he was known to dismiss modern developments in geomorphologic research as the work of "numerologists." Yet any examination of Mackin's own research will reveal him to have been a diligent, careful observer. The geologic maps that support Mackin's analysis of ore deposition in the Iron Springs district, for example, are models of detail and accuracy; they are quantitative to a high degree. Mackin's point, however, is that geologic problems, like those of biology and other complex sciences, present an almost infinite number of elements susceptible of measurement, and that the "rational" method is necessary to select for observation those that are critical. In private conversation, Mackin often spoke disparagingly of the "shotgun" approach to problems, and in his 1963 paper he quotes with approval James Gilluly's statement that "most exposures provide answers only to questions that are put to them." Mackin was not basically "antiquantitative"; he simply demanded of himself and his students that the data-gathering be preceded and continually accompanied by intensive logical analysis of the phenomenon under investigation.

As a teacher of earth science, Mackin was almost without a peer during his lifetime. His lectures were models of clarity, and they were delivered with a completely infectious intensity and enthusiasm, whether given to the beginning freshman class or to a group of advanced graduate students. He was remarkably adept at blackboard illustration; with chalk in one hand and eraser in the other he could truly make geologic processes and geologic history come to life. He encouraged divergent views—provided they were based on good, logical thinking—and detested the mere parroting of textbook or classroom notes. In his famous—infamous, to some—course in map interpretation, he surprised students continually with an "A" grade for the wrong answer reached by careful analysis and reasoning, and a "C" or worse for the right answer based on an inadequate or improper approach. Logical thinking was paramount, and of necessity the survivor of "map interp" acquired both the ability to think clearly and a thick skin—valuable assets for his professional years ahead. For Mackin could be cutting in criticism, though always in a way that made it evident that his concern was for the development of the student.

Mackin was a gregarious man and typically was the center of discussion groups in the field or at meetings. His views always were expressed crisply, concisely, and with humor. He loved a good argument and he started many; in one of his papers he remarks that "it is more important that a working hypothesis be provocative than it be right." He was often on the attack—but he attacked ideas, not people, and his vigorous and sometimes earthy remarks never left a residue of ill will.

Stories of Mackin's personal idiosyncrasies would fill a book—everyone who knew him would have contributions. He was a model train enthusiast and at times had tracks throughout the living areas of his home. He was fearsome at the wheel of a car, generally driving with one hand and waving the other as he analyzed the geomorphology of the rapidly passing terrain to his terror-stricken passengers. He was the proverbial absentminded professor: He habitually lost or mislaid keys, forgot where he had parked his car, wore mismatched socks (and even shoes), and left personal items strewn from one end of the country to the other. But he was a delight to be with; to use the old cliché, there was never a dull moment when Hoover Mackin was around.

In addition to teaching and research, Mackin was extremely active in scientific affairs, even after he was afflicted with cardiac malfunction in the mid-1960's. He was chairman of the Earth Sciences Division of the National Research Council from 1963 to 1965; delegate of the National Academy of Sciences to the 1967 meetings of the International Association of Hydrologists in Istanbul and of the International Union of Geodesy and Geophysics in Zurich in the same year; and he was the keynote speaker at the Symposium on Pediments, held in Budapest early in 1968. He participated actively in the early planning and design of the lunar geology experiments as a member of the U.S. Geological Survey team sponsored by the National Aeronautics and Space Administration, and he initiated the idea of a sampling tube to be driven into the lunar soil. Throughout his career Mackin was a sought-after speaker; he was a guest lecturer at many universities, and he was the Distinguished Lecturer for the American Association of Petroleum Geologists in 1953 and National Lecturer for Sigma Xi in 1963. Mackin was a member of the National Academy of Sciences, the Geological Society of America (Council, 1950-1953; chairman of Cordilleran Section, 1950), the Society of Economic Geologists, the American Geophysical Union, the American Association of Petroleum Geologists, the American Association for the Advancement of Science, and Sigma Xi.

Mackin died on August 12, 1968, at the height of his career and while preparing to serve as delegate of the U.S. National Committee on Geology to the International Geological Congress to be held in Prague later in 1968. He is survived by his widow, Esther Fisk Mackin; a daughter, Barbara Catherine Barker, wife of Dr. Daniel Barker, of the geology department of the University of Texas; a son, Robert Fisk Mackin, a design engineer; and two granddaughters.

I wish to acknowledge and to express my gratitude to those who have aided me in the preparation of this memoir: Esther Fisk Mackin on family details; Arthur B. Ford, Peter B. Rowley, Paul L. Williams, W. W. Rubey, and John T. Hack on professional aspects; and Birdena Schroeder, departmental secretary at the University of Texas, for biographic and bibliographic material.

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KEY TO ABBREVIATIONS

Am. J. Sci. = American Journal of Science

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

J. Geol. = Journal of Geology

U.S. Geol. Surv. Bull. = United States Department of the Interior, Geological Survey, Bulletin

U.S. Geol. Surv. Profess. Paper = United States Department of the Interior, Geological Survey, Professional Paper

U.S. Geol. Surv. Trace Elem. Memo. Rept. = United States Department of the Interior, Geological Survey, Trace Elements Memorandum Reports

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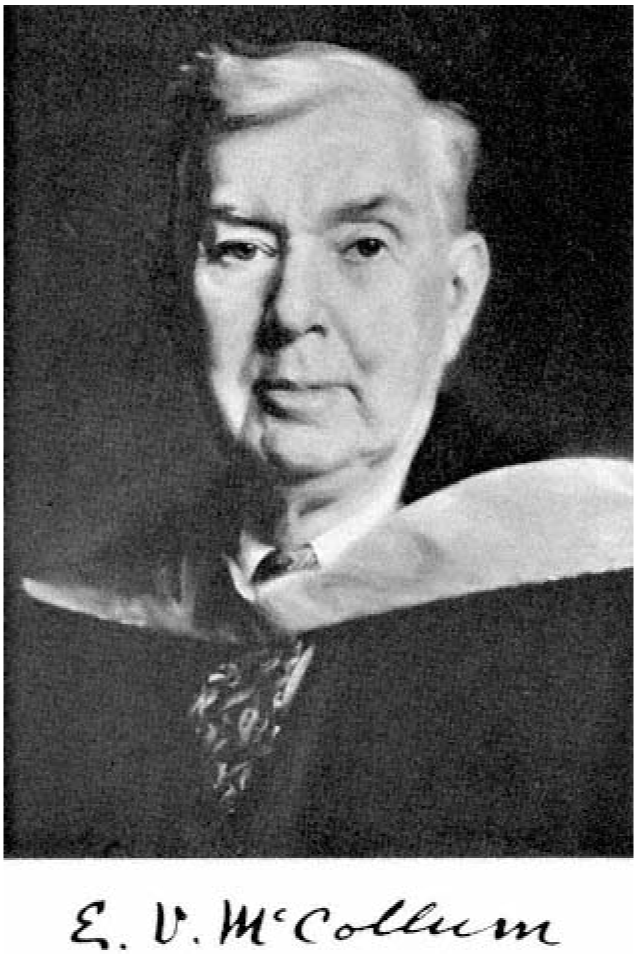
E. V. McCollum

Elmer Verner McCollum

March 3, 1879-November 15, 1967

by Harry G. Day

THE FORMATIVE YEARS

Elmer Verner McCollum was the child of a pioneer family. Pioneering was also the hallmark of his scientific achievements and humanitarian contributions. He was the first son and fourth child of Cornelius Armstrong McCollum and Martha Catherine Kidwell McCollum who sixteen years before had become homesteaders on one hundred sixty acres ten miles west and one mile north of Fort Scott, Kansas. His only brother, Burton, was sixteen months younger. The two were inseparable companions throughout their youth, and each influenced the other as long as they both lived. Each of the three sisters graduated from Lombard University, a Universalist Divinity School with a preparatory division, and each married a Universalist minister. Since the brother as well as Dr. McCollum graduated from the University of Kansas, all five of the McCollum children attained a much higher level of formal education than the parents.

Dr. McCollum developed in humble circumstances. But in spite of the stark realities of frontier life, with parents who had received scarcely any formal schooling, he showed unusual capacity to learn and reflect on much that he observed. The parents and the other children also had high regard for learning.

The mother had attended a backwoods school only two winters. She could scarcely read when the first child was born. But in spite of all the burdens of farm chores, raising and preserving food for the family, and innumerable household duties, she learned to read well, and she devoted time to the education of all the children. As Dr. McCollum wrote of her, "She valued education for its own sake and for its influence on human dignity and refinement, because it enabled people to escape drudgery, increased their earning power, and won the esteem of people who cared for culture." Her determination, high ethical values, and respect for culture and her thrifty management of the family and its meager income and expenditures evidently were continuously felt in the family circle.

The father read every moment that he had opportunity, but the supply of books and magazines was markedly limited and there were scarcely any well-informed and scholarly persons around him. As Dr. McCollum described him, "He was continually thinking inquiringly," but the unevenness of his knowledge and the driving necessity of devoting nearly all his time to manual work kept him imprisoned by some remarkably naive beliefs.

The parents were innovative and strongly motivated to attain economic security and were in fact leaders in their community. The mother frequently assisted the neighbors in sickness and bereavement. Her resourcefulness proved highly essential to the family at the beginning of her elder son's tenth year. The father became chronically ill, possibly due to tuberculosis of the bones. With the onset of this family crisis, it became necessary for the young McCollum to assume some of the responsibilities of an adult. In spite of the father's illness, it was economically necessary to continue operation of the farm until the year that the boy became seventeen. For the entire family these years were marked by never-ending toil and anxiety.

Elmer McCollum, with his younger brother, attended a one-room school which at best provided limited educational experiences. The pressure of duties on the farm caused him to miss school many days in autumn and spring. In his autobiography, produced sixty-eight years later, Dr. McCollum wrote of these early years: "While I lived on the farm I did not come into contact with a single individual who was both well informed and well endowed intellectually in any branch of learning, or who was motivated to inquire into the phenomena of nature. My environment was without stimulation of mental activity."

In 1893, when Elmer was fourteen years old, Mrs. McCollum took the two boys to the World's Columbian Exposition in Chicago. This experience broadened his horizons even though it also was devoid of much intellectual stimulation.

Probably one of the most significant actions for Dr. McCollum, and ultimately for advances in nutritional sciences, was the brave decision by the mother in 1896 to move the family to the vicinity of Lawrence, Kansas. The purpose was to provide opportunity for her two sons to attend high school and then the University of Kansas. In this decision she was supported by her invalid husband, but at that time neither son had a strong desire for much formal education.

The McCollum family's strategy was similar to that of many other rural families in that era in the Middle West. They secured as much cash as possible by selling nearly all the livestock and farm implements. The farm was rented as a source of income, and with the cash they purchased a fifteen-acre tract almost adjacent to the campus of the University of Kansas. This was converted to a fruit farm on which they hoped to produce enough income, along with the farm rental income, to meet the essential needs of the parents as well as the sons. Because the income was never enough, it became necessary for the boys to obtain employment in Lawrence.

When the family arrived at Lawrence, Elmer was already seventeen years old, and he had not yet received any formal education beyond that afforded by the district school. He had suffered the humiliation of failing the general certification examinations held that spring.

Because he lacked the certificate of graduation, the high school principal, Mr. Frank A. Olney, inquired concerning his preparation. On the strength that he had read many books and that he had memorized many poems, such as Lowell's "Vision of Sir Launfal," Mr. Olney admitted him provisionally. This was a great relief to the new student. In later years Dr. McCollum often recalled with appreciation this understanding action.

The intellectual competence and interests of the youth were quickly and unmistakably revealed in the high school record. His studies included Latin, history, mathematics, chemistry, and physics. He learned in such depth that when he entered the university, advanced credits were awarded in English composition, chemistry, and physics. Owing to the excessive hours of employment and his responsibilities on the family acreage, there was little time for social contacts. Nevertheless, he was elected class president in both his junior and senior years. In this role he delivered an oration at the graduation exercises. The subject was "The Puerto Rican Tariff," a lively topic at that time. Dr. McCollum's wide interests and knowledge were greatly influenced by his discovery and instant love of the *Encyclopaedia Britannica,* which was kept in the assembly room of the high school. He was so strongly impressed that he arranged for a book dealer in Lawrence to obtain for him a good used set. This cost him $25, an amount representing about two months of his earnings at that time. He retained the set and used it assiduously for about twenty-five years, when he purchased a new edition.

Throughout his adult life Dr. McCollum liked to recount some of his vivid memories of these high school years. They were largely concerned with his grinding preoccupation with physical labor through which he earned the money needed for his education and to supplement the meager resources of his family. His main job was lighting and extinguishing the gas lamps on the streets of one half of Lawrence. This job was continued throughout high school and his first two years in the university. Since this did not provide enough money to meet all his needs, he also obtained employment in the office of the Lawrence *Daily World.*

Each day a little before sunset he reported at the gasworks. If it was cloudy or if the moon was not shining brightly, he would start his lamplighting rounds; but if the manager decided that the moon would shine brightly, he did not work. For such nights he did not receive wages, since he was paid only when he tended the lamps. Moonshine was literally for him an occupational handicap. After finishing the lighting he slept in a hammock in an attic above the retorts until midnight, when he would begin his rounds extinguishing the lamps. This required about one and one-half hours. Following this he would walk about one-half mile to his home and, after eating, he would sleep the rest of the night.

The work at the newspaper office was largely in the afternoons following school. Frequently on Saturdays he collected for advertising by the merchants of Lawrence. Thus it is doubtful that any other boy in high school during that period had so little free time as the young McCollum.

On many occasions Dr. McCollum expressed his gratitude for the influence of several of the high school teachers on his development. For example, he wrote, "Listening to them talk, individually or in classes, and observing their ways of doing things opened my eyes to new and creditable standards of thought and conduct. Four of them greatly influenced my thinking and standards, although they were doubtless quite unaware of what they were doing for me."

About the time the young McCollum entered high school he joined the Unitarian church. Throughout his life he had a broad and active interest in the religions and philosophies of all ages, but he did not attend church services or participate in any kind of church programs.

After entering the University of Kansas, in September 1900, he continued to light lamps and work at the newspaper office until he received an appointment as a student instructor in his third year. The latter provided more income and an opportunity for adequate sleep.

Owing to his initial interest in preparing for medicine, during his first college year he devoted much time to the study of human anatomy. He also gave time to bacteriology and to courses in qualitative and quantitative chemical analysis. At the begining of the second year he studied organic chemistry under Dr. Edward Bartow. He became so fascinated with the subject that he abandoned all thought of becoming a physician. There were few limiting regulations of the university on the distribution of study time or in the choice of courses. Thus the young McCollum devoted nearly all his attention to courses in chemistry and to special work on the preparation of many compounds described in Gatterman's textbook, *Organic Chemistry.*

Toward the end of his second year he began the analysis of samples of petroleum sent to Dr. Bartow by crude oil producers in the Oklahoma Territory and in southern Kansas. Thus a new and valuable source of income was established.

Through academic work during the summers and the opportunity to receive credits through special examinations, McCollum earned his A.B. degree in three years.

He was immediately admitted for work leading to an M.S.

degree in chemistry. This included an appointment as teaching assistant in chemistry with a stipend of $300 for the year. His courses included a series of lectures in physiological chemistry. The master's thesis was based on a study of the composition of the gas in the hollow stems of the giant water lily, *Nelumbo lutea* (American lotus), when the plant was exposed to sunlight and during the hours of darkness. This thesis was accepted, but the work was never published. Dr. Arthur Harris, who was highly respected by the young McCollum, directed the work. A few years earlier Harris had served with him in lighting the gas lamps of Lawrence.

Other influential teachers respected by the young McCollum included Professor E. C. Franklin and Dr. H. P. Cady. The latter was especially helpful in guiding him toward a superior graduate school for further training in organic chemistry. Through his study of current chemical journals he decided that the work of Dr. H. L. Wheeler and Dr. T. B. Johnson at Yale University was the most promising for him.

In 1904 the young man applied to Yale for admission and a fellowship. He was promptly admitted, but he was given only a scholarship exempting him from paying tuition. Undaunted, he accepted the offer in the belief that some way would be found to earn the money needed for his maintenance.

On his way by train to New Haven, McCollum stopped in St. Louis for one day to visit the Lewis and Clark Exposition and to attend briefly the International Congress of Chemists that was in session. There he introduced himself to Professor Russell Chittenden, with whom he had been in correspondence concerning his admission to Yale. This began a lasting and fruitful friendship. Assured that every effort would be made to assist him, he resumed his journey to New Haven. He arrived there with $82 in his pocket and with no firm prospects of securing more.

The tall, extremely thin young man had come into a new world. With hard work, foresight, and desire to accommodate to that world, the two years he spent in earning the Ph.D. degree proved to be extraordinarily fruitful and pleasing. The system for the conduct of graduate study that he experienced and the patterns of work and self-development that he pursued were followed throughout his life.

When he arrived in New Haven he had rarely been away from home more than a few hours at a time. Most of the students he had known in high school and in undergraduate school were similar to him in background and experience. Scarcely any came from families of wealth and position. But at Yale he met every variety of person. He tried to learn from all, and he cultivated the qualities of their lives that seemed to be important in making the most of his own life.

During the first year he found it possible to live in the home of one of his former high school teachers, Arthur L. Corbin, who had studied law and was at the time a faculty member in the Yale law school. After this transition period in the Corbin family he was invited by Professor Chittenden to live in one of the two suites of living rooms on the top floor of the chemistry building. This was home for him during the remaining two years at Yale. His roommate was Phillip Mitchell, who was a student of Professor Lafayette B. Mendel and later became Professor of Physiology at Brown University.

Immediately after entering the Sheffield Scientific School at Yale, McCollum became associated with Professor Treat B. Johnson, who directed his doctoral research. The research was concerned with the preparation and study of pyrimidines. Students closely associated with him included Stanley R. Benedict, Stanley Bristol, Samuel H. Clapp, William B. Cramer, Samuel Dudley, George S. Jamieson, Eli M. K. Ryder, Carl O. Johns, and Johannes G. Statiropulous.

The young McCollum appreciated Dr. Johnson's method of providing guidance. To suggest steps that might be taken to prepare the desired compounds, Dr. Johnson used large sheets of newsprint, drew the formulas of known starting compounds, and indicated reagents and conditions to be used. Together they would discuss the procedures and the conditions that seemed to be most promising.

During his second year McCollum contracted pneumonia. The enforced absences from classes and his debilitated health for some time thereafter seriously interfered with his progress. For a time it appeared that this might delay the completion of his degree. The illness and his intense preoccupation with research in organic chemistry had caused him to get seriously behind in crystallography, his second minor. When the teacher, Professor Samuel L. Penfield, learned about the situation he decided that the young man needed to improve his health more than he needed to spend time drawing crystals. He promised that if the student would spend time canoeing on the river three times a week the rest of the semester, he would receive credit for the course! This unorthodox but extraordinary act of kindness was a significant influence throughout Dr. McCollum's long life with students. He never ceased to be grateful to Professor Penfield.

The intensive reading habits cultivated during his graduate years were followed throughout Dr. McCollum's life. On his first visit to the laboratory Professor Horace L. Wells gave him a key to the large library provided by Professor Wheeler. Thereafter young McCollum spent many evenings in the library. This significant pattern is aptly described in his autobiography:

"I took down in succession the volumes of a series of journals and turned every page, leisurely scanning them, until I came upon a title which interested me. Then I read carefully the introduction. . . . Next I examined the experimental observations and studied the conclusions which the author drew from them. Before proceeding further I reflected on what I might do in order to shed more light on the program."

Earning money to pay expenses was still essential. McCollum was soon employed to give instruction in elementary chemistry three evenings per week at the YMCA. This paid $10.00 per week, and it required only a small expenditure of time and labor. Another source of income was in tutoring students. After a few months, a number of students with money began to come to him for assistance. The tutors to whom students were attracted charged a minimum of $3.00 per hour. At such rates the young McCollum developed for the first time in his life a sense of financial security. Even while a full time and productive graduate student he began to earn enough money to accumulate savings.

Another financial and scholastic advantage was gained by the young Kansan when he competed with six other students in a comprehensive chemistry examination at the end of the first year and earned the coveted Loomis Prize of $400. Through the help of his closest friend, Bill Cramer, he obtained work one summer as a clerk at a hotel on Block Island, Rhode Island. This added more to his financial resources and to his experience. He always cherished the warm friendship and great help of this thoughtful man.

The doctoral research progressed so rapidly and well that two months before the degree was granted, in June 1906, Dr. McCollum began work in Dr. T. B. Osborne's laboratory. The work continued six months. During that time he used the Fischer ester method for the analysis of protein hydrolysates. Working with Samuel Clapp he learned about Osborne's purified proteins from different seed grains and the general status of protein investigations.

THE UNIVERSITY OF WISCONSIN: TEN PIONEERING YEARS

Because there was no desirable academic position available at the end of the summer of 1906, McCollum went to Professor

L. B. Mendel's laboratory to learn more about biochemistry. This proved to be an eventful decision. During the year he attended the lectures of Mendel, F. P. Underhill, and R. H. Chittenden, and he worked in the laboratory gaining familiarity with the analytical methods applicable to biochemistry. In the following spring, through Dr. Mendel, a promising position was obtained at the University of Wisconsin. On July 1, 1907, he began investigational work in Madison as an instructor in agricultural chemistry. The beginning annual salary was $1200. This undertaking proved to be a most significant event in the advancement of nutrition, even though at the time McCollum would have preferred an academic position in organic chemistry.

Having been appointed to a promising position, McCollum in 1907 married Constance Carruth, whom he had known at Lawrence, Kansas. The children were Donald C., who became a physician, and four daughters, Jean Westwick, Margaret Sprague, Kathleen Albright, and Elsbeth Fox. Many years later the marriage ended in divorce.

Under the direction of Professor E. B. Hart, McCollum became a part of the laboratory team assigned to the famous experiment with heifers restricted to rations derived from single-plant sources. These sources were wheat, corn, and oat plants. The experiment had been planned by Professor S. M. Babcock, who had preceded Dr. Hart as head of agricultural chemistry. The purpose was to determine whether the accepted methods of food analysis actually gave results on which nutritive values could be judged. Dr. Babcock was skeptical.

The rations were intended to include all parts of the plant except the roots. The parts were furnished in such proportions that the entire ration for each of the three experimental groups of animals had the same composition as shown by the official accepted methods of chemical analysis. A fourth group received a ration derived from all three plants. This was to determine whether variety in source of nutrients was of nutritional importance.

To the enthusiastic but inexperienced McCollum, the experiment was already yielding impressive if not meaningful results. By the time he first saw the heifers, after several months on the rations, the groups had differentiated greatly. The remarkable differences in appearance and physiological status stimulated him to find the correct explanation. He decided that "something fundamental remained to be discovered."

It is notable that the plan of study included a searching chemical examination of the rations and the determination of their digestibilities. Enormous numbers of analytical determinations were carried out on blood, urine, feces, and tissues of the cows. After four years of work it was finally concluded that satisfying explanations for the nutritional shortcomings of the single-plant rations could not be achieved through this kind of design and analysis.

Trying to gain a breakthrough in understanding the nutritional requirements of man and animals, McCollum read extensively in the literature. Of greatest interest to him was Maly's *Jahresbericht uber die Fortschritte der Tier-Chemie.* He became so impressed with the importance of many abstracts and articles in the yearbook that he purchased an entire set (thirty-seven volumes at that time) so that he could study at home. He learned about a number of investigations conducted between 1873 and 1906 in which small animals, mostly mice, had been restricted to diets composed of isolated and somewhat purified proteins, carbohydrates, fats, and inorganic salts. Concerning this, he wrote, "I was struck by the fact that in every instance in which small animals had been restricted to such 'purified' diets they promptly failed in health, rapidly deteriorated physically, and lived only a few weeks."

From these literature studies and his initial experiences with the heifers on restricted rations McCollum concluded that "the most important problem in nutrition was to discover what was lacking in such diets." He decided that it was essential to experiment with small animals with a short life span. He reasoned that if they were fed simplified diets composed of purified nutrients it should be possible to determine the specific chemical substances necessary in nutrition. This was the turning point in his career. He decided to begin experiments using rats, since they seemed to satisfy all the criteria for the initial studies.

This fertile concept was explained to his superiors at the university, but only Dr. Babcock understood its import. The Dean of the College of Agriculture refused to support any experiments with rats. Dr. Babcock said to Dr. McCollum, "I think the Dean is wrong in his pronouncement on your new project. I think we should go ahead and do it anyway." Being confident that Dr. Babcock, although retired, had sufficient standing to give him any needed backing, McCollum did go ahead without the official support or endorsement from his dean.

The first animals for his colony were wild rats he captured one Saturday afternoon in the old horse barn on the Station farm. He soon found that they were too wild to be satisfactory. They were replaced with twelve young albino rats he obtained from a dealer in Chicago. The wire screen used to make the cages cost $2.00, and the rats cost $6.00. These expenses were borne by McCollum, since the project was tolerated rather than approved, except by Dr. Babcock. Later, Professor Hart authorized the expenditure of $50 of university funds for the construction of two animal units, each with twelve compartments. The carpenter, being sympathetic, made three units for the money provided.

Dr. McCollum's rat colony was the first in the United States maintained for nutritional investigations. It was started in January 1908.

The first experiments were designed to test his idea that the nutritive failure already reported in mice on rations of purified components was due to lack of palatability. The experiments involved the determination of the effect on growth and apparent well-being when the source and nature of the seemingly purified components of the rations were varied. At first, McCollum believed he had demonstrated that if the "purified food mixtures" were made palatable, through variation in composition and flavor, the animals would eat enough to satisfy their nutritional requirements. Although his findings were inconclusive, it was not until 1911 that his hypothesis became seriously in doubt.

In that year Osborne and Mendel reported that the demonstration of nutritional differences between proteins required the use of a supplement of "protein-free milk." This enabled the rats to grow well on a diet of purified components, even though the diet was not supplemented with flavors.

In reporting some of their work, Osborne and Mendel implied that McCollum had been careless in his experiments. This embarrassing experience led him to discover that milk sugar used in his diets failed to promote growth after it had been subjected to greater purification. In addition to the correction of this error, he pointed out that the "protein-free milk" used by Osborne and Mendel in their rations supplied nine percent of the total nitrogen of the diets. Thus it might have furnished a variety of unknown substances. This was soon proved to be true. But the confrontation involved in this encounter had an even more far-reaching result because it stimulated the young scientist's development in nutritional experimentation and in the analysis of problems.

The new research based on the use of a rat colony absorbed much of his time, but he continued to perform his duties in the feeding experiment with the heifers until 1911. The care of the laboratory rats and the detailed management of many of the feeding experiments required much more time than he could provide in order to make the research progress at the desired rate. Fortunately, about eighteen months after the colony had been started, Marguerite Davis, a university graduate who was keeping house for her retired father in Madison, was referred to McCollum to study biochemistry. After learning about the objectives of the experiments with the small rat colony, Miss Davis volunteered to care for the animals. Except Saturdays and Sundays, she worked every day without pay for five years. Finally, Dr. McCollum managed to have her paid $600 for her sixth and last year.

The generous and competent collaboration of Miss Davis in these fertile years was an important factor in McCollum's discovery of the first known fat-soluble vitamin, later designated vitamin A. Also, she was involved in the development of what McCollum designated "the biological method for the analysis of a food." This resulted in the publication of many papers with Miss Davis, and later with others, concerning the supplementary relations among the common foodstuffs.

Pursuing the idea of observing the consequences of feeding diets of the simplest composition, McCollum and Davis restricted weanling rats to single kinds of seeds, such as maize, wheat, oats, rye, and beans. To McCollum's surprise, the animals grew only slightly, if at all, when limited to seed rations, whether alone or in combination with other seeds.

He considered that one or more of several factors might account for this low "biological value" of the seeds. Published analyses showed that seeds in general had low amounts of calcium. He believed it might be important that his recently discovered fat-soluble dietary essential was absent from oils obtained from seeds. Also, new information being published on the pronounced differences in the content of some amino acids in proteins suggested that the apparent nutritional inferiority of the seeds might be attributable to some inadequacy of their proteins.

Through experiments with various supplements, McCollum found that to obtain good growth, reproduction, and a long life span on a wheat ration, it was necessary to provide additional (supplemental) protein, a source of calcium, and the "growth-promoting fat" (source of vitamin A).

Further feeding experiments made it clear that all the common seeds tested had the same qualitative deficiencies as wheat. By 1915 McCollum and Davis had found that when water or alcohol extractions of wheat germ or rice polishings were added, polished rice was greatly improved in nutritional quality. These experiments constituted the basis for their discovery that the antiberiberi factor, necessary to relieve polyneuritis in pigeons, was necessary for rats and that there were apparently only two unidentified nutrients necessary for such animals.

Concerned with developing problems of nomenclature, in 1916, McCollum and C. Kennedy suggested the provisional use of alphabetical terms, using a prefix designating characteristic solubility. They proposed the term fat-soluble A and water-soluble B, respectively, to designate the two unidentified nutrients. These terms found widespread acceptance. The system was expanded as other essential factors were discovered.

The experiments with rats were extended to comparable studies with pigs kept on board floors. The results with rats and pigs confirmed the impressions gained by farmers that corn alone results in poor growth in swine. Further advances in the newer knowledge of nutrition were made when McCollum, with Nina Simmonds and W. Pitz, reported in 1917 that leaf material added to diets of one or more kinds of seeds greatly improved the growth rate and general well-being of the animals.

While McCollum was deeply immersed in these simple but impressive experiments during the first part of the germinal decade of 1907-1917, he continued his pattern of reading in the literature, but some highly relevant papers escaped his attention and lack of the information contained therein handicapped his progress. In particular, these included the notable observations and conclusions of C. Eijkman and of G. Grijns on chickens fed a diet of polished rice in Java. McCollum did not become acquainted with this work until 1913. However, early in his work at Madison he learned about the findings of N. Lunin in 1881, of E. G. Willcock and F. G. Hopkins in 1906, and of various other pioneers at the turn of the century. He was greatly impressed and stimulated by the books of E. B. Vedder in 1913 and C. Funk in 1914.

In retrospect, McCollum's inadvertent use of only partially purified milk sugar in some of the early feeding trials, and the earlier caging procedure that allowed the young rats access to their feces, probably were decisive in bringing about the discovery of vitamin A as early as 1912 by McCollum and Davis. The rats grew fairly well and seemed to be in good nutritional condition when butterfat or egg-yolk fat was included in the diet. They failed rapidly when lard or olive oil was the source of fat. This evidence that certain fats contain a hitherto unidentified nutrient was rigorously confirmed when McCollum and Davis saponified the butterfat, suspended the nonsaponifiable fraction in olive oil, and fed this to the rats. A prompt growth response made it clear that they were dealing with a new and essential nutrient. They soon found that an ether extract of leaves of plants contained a nutrient with the same effect.

Until this time the maintenance of the rat colony at Wisconsin was tolerated only because the elderly and respected Dr. Babcock insisted to the authorities that this kind of research was important. But in December 1912, when Dean H. L. Russell learned that the comparative study of fats was showing that butterfat was superior to olive oil and lard, he delightedly insisted that the findings should be published right away. Clearly, this would be good news to the farmers of Wisconsin and others with major agricultural interests. However, the first manuscript was not submitted for publication until April 1913. It was sent to Professor Mendel, since he was one of the four editors of the *Journal of Biological Chemistry,* where it was published. Five months following the publication, Osborne and Mendel reported findings in the same journal that confirmed the existence of the factor in butterfat now known to be vitamin A.

Although for a time there was a counterclaim concerning credit for the discovery of vitamin A, within a few years it became quite clear that the credit belonged to McCollum alone.

All of McCollum's published experimental work had the help of Davis from 1909 to 1916 and of Nina Simmonds from 1916 to 1929. The work of these three caused many scientists and members of the public to regard food, nutrition, and health in a new light and to look forward to further developments.

It is now apparent that the period from 1909 to 1916 was one of the most rewarding and productive in all the years of McCollum's life, at least as measured by the energy and originality of his effort and the influence of his contributions in shaping the thinking and practices of others in nutrition.

His achievements and promise were promptly recognized by his institution, even though it had required several years for the importance of his rat colony to become recognized. He was promoted in academic rank beyond his expectations, passing from instructor, to assistant professor, to associate professor, and to full professor in six years.

His first distinctly national recognition came in 1916 when he received an invitation to lecture before the Harvey Society in New York. Later he wrote: "I was overjoyed by the invitation. Here was evidence that my observations on nutrition in relation to foods had gained recognition by discriminating men. It had high spiritual value for me. I accepted and chose the title 'The Supplementary Relations among Our Common Foodstuffs.' "

The increasing recognition of McCollum for his originality and significant achievements in experimental nutrition stimulated him to reflect intensively on the quality of human and animal dietaries throughout the world. He considered practical ways through which inferior diets could be improved. His developing involvement in such significant matters, beginning in 1917, is aptly stated in his article, "My Early Experiences in the Study of Foods and Nutrition," published in the *Annual Review of Biochemistry* in 1953. He wrote:

"The new knowledge of the dietary properties of seed, leaf, milk (which we found to be an excellent supplement to seeds), and some observations of the dietary deficiencies of muscle meat, together with the new information about polished rice and the superiority of the germ as a source of nutrients, led me to make some important generalizations on human dietaries. I criticized the typical American's diet of that period as being of poor quality because it was derived too largely from white flour or cornmeal, muscle meats, potatoes, and sugar. Sugar, I asserted, when eaten to the extent of an average of more than 100 pounds per capita per annum, crowded out from the diet significant amounts of better constituted foods. The foods listed, I declared, were not constituted to supplement each other by making good their deficiencies. I recommended a diet containing more milk and leafy vegetables, and extolled the glandular organs of animals as superior to the muscle meats as sources of nutrients. Milk and leafy vegetables I distinguished as 'protective foods' because they were so constituted as to make good the deficiencies of whatever else we liked to eat."

THE MOVE TO JOHNS HOPKINS

The Harvey Lecture, in January 1917, brought with it a gratifying surprise. It was an invitation, received a few hours prior to the lecture, to visit Professor William H. Howell at The Johns Hopkins University before returning to the University of Wisconsin. When McCollum arrived in Baltimore, he learned that the Rockefeller Foundation had offered to finance and eventually to endow the establishment of a School of Hygiene and Public Health at Johns Hopkins and that Dr. William H. Welch was to be the director of the new school. Professor Howell was to be assistant director and professor of physiology. These two were to select the faculty. They invited Dr. McCollum to become professor and head of the department of chemical hygiene (later changed to biochemistry).

Dr. McCollum promptly accepted, but he was asked not to mention the decision until the trustees had confirmed it. He returned to Madison and resumed his research and teaching. Although week after week passed without further information about the appointment, he painfully refrained from informing any of the officials at Wisconsin concerning his new plans. Adding to his distress, early in April, just as the United States entered World War I, he received a letter from Mr. Herbert Hoover asking him to become a member of his Advisory Committee on Nutrition and to attend a meeting of this committee in Washington. Requesting authorization to attend the highly important meeting without divulging his plans to resign from his appointment was, indeed, embarrassing. After the two-day session in Washington, Mr. Hoover asked him to remain about two weeks to help prepare some bulletins for the guidance of homemakers on the use of substitute foods during the war. This he did, but with added reluctance, since he felt a special obligation to be in the laboratory at Madison. To his great relief upon his return he found the long-expected letter from President Goodnow informing him that the Board of Trustees of Johns Hopkins had endorsed his appointment. In referring to this significant event Dr. McCollum wrote in his autobiography:

" . . . I made a number of important discoveries [at the

University of Wisconsin] which had great influence on arousing interest in others in studies in nutrition. The breeding stock of the rat colony which I left behind was continued and used by Professors Hart and Steenbock in making contributions to nutritional science which brought fame to them. Collectively our studies with the colony inaugurated a new era in scientific work at the University of Wisconsin."

In the new position there was essentially unlimited freedom in research and opportunity to meet distinguished specialists in public health and the supporting sciences. McCollum quickly resumed nutritional investigations based largely on use of his transplanted rat colony. The new life was dominated by his driving desire to understand the chemical basis of nutrition and his goal to render a high level of public service in teaching and promoting the newer knowledge of nutrition. He knew that he was the first faculty appointment to the new school, but he could not know that his years of service there would extend beyond those of all the other original faculty members.

The pattern of his life for all of his years on the Hopkins faculty became singularly fixed in a routine that maximized efficiency. There was always some time allowed to be at home with his family. But virtually every working week included all day Saturday and frequently some time on Sundays when he worked in his office. For many years, to save time and to keep in good touch with the work of his department, he joined his staff daily at lunch, which was prepared and served in a room near his office. He read extensively and widely in the journals and other scientific literature both during the day and at home during the evenings. However, when he reached the age of fifty he gave up the reading of scientific literature at home. Thereafter he generally devoted evenings to nonscience, largely the classics, poetry, history, geography, and biography.

Owing to the grossly oversimplified and erroneous concepts of foods and nutrition about 1920, it was assumed by medical men and public health specialists that the important advances would be made in other areas of science and medicine. Thus most of McCollum's colleagues at Johns Hopkins had little real interest in what he was doing. But gradually they began to find stimulation and promise in the work and in the expanding newer knowledge of nutrition. Among those impressed were Dr. Florence Sabin, Dr. and Mrs. Warren Lewis, Dr. Walter Jones, Dr. and Mrs. John J. Abel, and Dr. L. F. Barker. The latter asked McCollum to contribute two chapters on nutrition to his comprehensive treatise on endocrinology.

MAKING KNOWN THE NEWER KNOWLEDGE OF NUTRITION

In the spring of 1918 McCollum accepted Dr. Milton J. Rosenau's invitation to give the three Cutter Lectures on Hygiene and Preventive Medicine at Harvard University. The lectures were received so favorably that Dr. Rosenau later referred to them as the high-water mark of all the Cutter Lectures. At his suggestion, that same year McCollum had the lectures published as a book, *The Newer Knowledge of Nutrition.* Within three years 14,000 copies were sold. This turned out to be his principal contribution in the area of educational writing. In 1922 the second edition appeared. The fifth and last edition appeared in 1939. Nina Simmonds was co-author of the fourth edition. E. Orent-Keiles and Harry G. Day were the co-authors of the last edition.

This book was unique in that it focused attention on current findings in animal and human nutrition, and it presented many examples of malnutrition owing to inadequate diets. It was primarily addressed to students of nutrition, physicians, public health workers, and others concerned with health. Clearly, the different editions showed that there were many unsolved problems in nutrition and that the experimental approach that had been pioneered by McCollum held promise for further invaluable advances.

The beginnings of this country's participation in World War I had much to do with McCollum's involvement in making known the newer knowledge of nutrition. From the beginning of the war he participated as a member of Mr. Herbert Hoover's Advisory Committee on Nutrition. Several members were frankly doubtful that his laboratory findings and views should be taken seriously. The committee, including McCollum, first prepared a few small bulletins designed to aid housewives in conserving wheat, fat, and sugar through use of "food substitutes."

In 1918 Dr. McCollum was asked to speak at a meeting on the conservation of foods that was attended by Mr. Hoover. The response was so favorable that Mr. Hoover promptly arranged for him to give talks on the same subject in many of the major cities. The talks discussed the poor quality of the typical diet in the United States, and they showed how menus could be constructed with combinations of foods that tended to correct the deficiencies of each. This was generally a revelation to the audiences that heard him. The teachers and students of dietetics and home economics began at once to show interest and to adopt the newer knowledge. On this extensive tour McCollum used the term "protective foods" in lecturing. He had used it in his book *The Newer Knowledge of Nutrition.*

He emphasized the effects of several kinds of diets on growth, fertility, success in rearing young, and life span of experimental animals. There was emphasis on planning the diet around a foundation of about a quart of milk and two servings of leafy vegetables per day. The virtues were extolled of frequent inclusion in the menus of eggs and the glandular organs, such as liver. He emphasized the value of cereal germs in contrast to the starchy portions of the food grains.

It is difficult to assess the degree to which McCollum's pronouncements on the newer knowledge accounted for the changes in the consumption of different foods in the several years that followed. Rapid changes occurred at about that time in food refrigeration, transportation, and marketing. However, his lecturing and writing must be reckoned a potent influence that keyed rapid and far-reaching changes in food patterns. For example, between 1919 and 1926 the national production of milk products increased by one third. "Milk Weeks" were endorsed by city officials, and boxing bouts were held for the benefit of milk funds as early as 1921. Some evidence shows that typically middle-class families in 1926-1927 spent about four times as much of their food money for milk, fruit, and vegetables as did middle-class families a century earlier. Surely, in the years of World War I and the decade that followed, McCollum contributed to these invaluable changes more than any other individual.

There were various other ways through which McCollum strongly influenced human dietary practices and animal feeding. In 1915 he was asked to write a series of articles for *Hoard's Dairyman.* The articles succinctly presented the newer knowledge concerning quality in foods and good and poor combinations. For almost twenty-five years, beginning in 1922, he regularly wrote articles on nutrition and foods for *McCall's Magazine.* Eventually these were prepared with the assistance of trained staff writers. A number of newspapers began to reprint excerpts from the articles. The great breadth of his public visibility created by this service, and the professional eminence he was attaining in science—he was elected to the National Academy of Sciences in 1920—led to other exposures of his views on the nature and importance of foods and nutrition in health. This included special articles based on interviews with McCollum that appeared in such publications as *The New York Times* and *The Saturday Evening Post.* Another avenue of major influence was through summer courses in nutrition that he gave for several years at universities in California, Colorado, Missouri, Ohio, and Utah. Home economists, medical and dental societies, and various other professional groups invited him to address their meetings. His personal acceptability was high, and his speaking was persuasive.

Another avenue through which McCollum moved many people to better understanding was the little book *Food, Nutrition, and Health,* which he wrote and published privately, in the first editions (1925-1933) with Nina Simmonds and in the following editions with Ernestine Becker. The latter was associated with him in research and teaching throughout nearly all his years at The Johns Hopkins University. She became his wife in 1945.

THE DISCOVERY OF VITAMIN D

The extraordinary variety of experimental diets systematically employed by McCollum in studying the nutritional inadequacies of plants led him to the chance observation in 1918 that young rats develop a ricketic condition when restricted to diets composed principally of cereal grains and providing disproportionate calcium-to-phosphorus ratios. The harmful effect of unfavorable ratios of calcium to phosphorus in the diet was largely alleviated by the provision of small amounts of cod liver oil.

Fortunately, the right combination of motivated specialists was at hand at Johns Hopkins to help exploit these basic observations for the good of humanity. They were Dr. John Howland and two members of his staff in pediatrics, Dr. Edwards A. Park and Dr. Paul G. Shipley. Participating with McCollum were Nina Simmonds, Ernestine Becker, and H. T. Parsons.

At the first meeting of Howland and McCollum, in 1918, it was decided by the two that the latter had most probably discovered the correct approach to the elucidation of the origin and treatment of rickets. Before they separated that day, they agreed to undertake a cooperative study of the abnormalities of bone growth produced by designed dietary defects.

During the following three years the McCollum group tested the effects of more than three hundred experimental rations. The Howland group made histological studies of bone sections taken from five or six rats of each experimental group. They laboriously sifted through a huge mass of data. It was soon recognized that the source and the amount of fat, regardless of whether it contained vitamin A and the ratios between calcium and phosphorus, were the major factors in the growth and soundness of bones. Because only a small amount of cod liver oil was as effective as a much greater amount of butterfat in the improvement of faulty bone structures, they assumed that the difference was due to variation in the content of a specific nutrient required by the rats. Of course, they knew that cod liver oil had been employed therapeutically for a long time, but the basis for its somewhat-questioned usefulness was entirely a mystery.

To determine whether or not the nutrient might be vitamin A, which could be readily destroyed by oxidation, in 1922 the McCollum group passed air through heated cod liver oil and butterfat. Although the treated materials had lost all vitamin A potency, each retained its antiricketic activity. Thus they concluded that the antiricketic substance was distinct from vitamin A, and that the experiments showed "the existence of a fourth vitamin whose specific property . . . is to regulate the metabolism of bones."

Among the many diets the McCollum workers had studied, the one designated 3143 caused acute and severe rickets in weanling rats. With major contributions from Park, as well as from McCollum, the Johns Hopkins group developed an assay method for vitamin D that was based on the use of this diet. The assay end-point became known as the "line test." The administration of test substances containing vitamin D resulted in the prompt formation of a line of calcification that could be clearly delineated upon examination of a section of the isolated bone. With subsequent refinement and suitable application, this biological test became one of the most dependable and widely used methods for the analysis of foods and other materials for vitamin D activity and the extension of knowledge concerning vitamin D.

With this test the McCollum group showed in 1921 that sunshine protects against rickets. In so doing they partially explained the basis for K. Huldschinsky's finding in 1919 that ultraviolet light exerts a curative effect on rickets. Other investigators in other laboratories gradually elucidated the relationships among ultraviolet light, precursors of vitamin D, and rickets.

Credit for the myriad developments involving vitamin D is owed to a host of investigators, but much of the groundwork was done by McCollum and his associates.

INVESTIGATIONS ON OTHER NUTRIENTS

In other studies concerning lipids and fat-soluble vitamins many of McCollum's students and other associates were involved. Among these, some recognition needs to be given to the exciting work with his student Cosmo Mackenzie on vitamin E and muscular dystrophy. They showed that the muscular dystrophy occurring in rats and rabbits on vitamin E-free diets can be completely cured by the provision of alpha-tocopherol, the first chemically defined substance with vitamin E activity. However, their hopes that muscular dystrophy occurring spontaneously in human beings might be successfully treated with this vitamin were soon dashed. It didn't work.

Various students and others worked on problems concerned with B vitamins and with several other nutritional subjects. One of the stimulating results came from the work of H. J.

Prebluda that, through the finding of a reagent with high specificity for the thiazole portion of thiamine, provided the essential basis for the development of a method for the quantitative determination of thiamine in biological materials.

McCollum's contributions to the understanding of inorganic elements in nutrition spanned almost his entire life as an experimental investigator. In 1909, in work with laying hens, he proved that the phosphorus requirement could be satisfied from orthophosphate in the feed. Twenty-two years later, with his student Elsa Orent, he discovered the spectacular effects of extreme magnesium deficiency in young rats. This was followed in the Johns Hopkins laboratory by a number of studies that led to a much better understanding of the essential role of magnesium in nutrition. About the same time, McCollum and Orent established the essentiality of manganese and showed that extreme deficiency results in loss of the "maternal instinct" in postparturient rats. At that time the newspapers referred to manganese as the nutrient necessary for "maternal instinct." Also, male rats suffered testicular degeneration that led to complete sterility.

This general area of research was significantly furthered by a long-term grant from the Rockefeller Foundation that became effective in 1936. With the contributions of several younger associates, studies were concentrated on the effects in the rat of dietary deficiencies and imbalances of many inorganic elements, including sodium, potassium, phosphorus, iron, zinc, magnesium, calcium and boron. Dr. Richard Follis joined the McCollum group in 1938. This resulted in the detailed histological study of many tissues from rats deficient in potassium, in phosphorus, in sodium, and in zinc.

THE PUBLIC GOOD: CONSULTING AND MEMBERSHIP ON PUBLIC COMMISSIONS AND BOARDS

No other nutritional scientist probably rendered greater service in influencing the dietary practices of the people and the thinking of scientific bodies and public officials in matters concerning human nutrition than did McCollum. Such influence began to be significant before he left the University of Wisconsin. It continued in many ways through his years on the faculty at The Johns Hopkins University and for all of his retirement years.

Probably his most cherished service was to the Merrill-Palmer Institute of Detroit. He consulted with its director at its founding, in 1919, and he continued as a regular consultant through twelve years. Shortly after his eightieth birthday he was called back to the Institute to receive a citation for his "outstanding contribution to science and education in the area where the Institute's objectives are directed."

Through the years 1928-1937 McCollum was a consultant to the Bureau of Animal Industry of the United States Department of Agriculture at the Beltsville research center. From 1932 to 1949 he was a member of the U.S. Pharmacopeial Revision Board. In this role he made various contributions concerning vitamins and other nutrients. From 1933 to 1937 he was a member of the National Advisory Health Council. In 1941 he chaired the Section on Research of the National Conference on Nutrition and Defense and the U.S. Advisory Committee of the Coordinator of Information on Food and Nutrition. Also, in that year he became a member of the Food and Nutrition Board of the National Research Council. In addition, he became a member of the Scientific Advisory Committee of the newly formed Nutrition Foundation, Inc., on which he continued to serve until 1953.

The great demands of World War II caused Dr. McCollum in 1942 to become a member of the subcommittee, Emergency Research Committee on Food and Nutrition of the National Research Council. The next year involved him as consultant to the U.S. Lend-Lease Administration. Also, in 1943 he was consultant to the Industrial Hygiene Section of the U.S. Army.

These responsibilities were taken seriously by McCollum.

They occasioned many times the thoughtful preparation of comprehensive memoranda in which the essential facts and the pros and cons of different problems were weighed. He always did his homework in consulting and serving on commissions and boards.

At different times he was on the editorial boards of the *Journal of Biological Chemistry, Journal of Nutrition,* and *Nutrition Reviews.* He was the president of the American Society of Biological Chemists in 1927 and 1928 and of the American Institute of Nutrition in 1938.

The first of many international and national responsibilities on public commissions and councils started for McCollum in 1931 when he became a member of the first International Conference on Vitamin Standards. It met in London both in 1931 and in 1934. In 1931 he was also the United States delegate to the International Dairy Congress at Copenhagen. His international contributions were extended in 1935 when he became a member of the Permanent Commission on Nutrition of the League of Nations. During 1936-1937 he served on the Mixed Committee on Nutrition of the Health Section of the League. Also, in 1937 he was a member of the Technical Experts Commission of the League. One year later he was the chairman of the Nutrition Section of the Tenth Pan American Sanitary Conference, held in Bogota, Colombia. In 1939 he was the chairman of the Nutrition Section of the Pan American Bureau.

It may be added that until these international responsibilities were assumed, Dr. McCollum had never owned a wardrobe containing anything more dressy than an ordinary tuxedo. In preparation for the duties abroad he arranged for a haberdasher near the Johns Hopkins medical school to provide all that he might need. The commission was carried out with such thoroughness that he could have served at a high level in the diplomatic service. In later years McCollum smilingly looked back on it as the big fashion splurge of his life. It represented, indeed, a giant step from the Kansas farm.

CONTRIBUTIONS TO THE FOOD INDUSTRY

Throughout his professional life McCollum's interests and time remained broadly focused on nutritional research and the promotion of sound nutritional practices. This naturally included some associations with various food industries. The closest was with the dairy industry. The degree to which his influence accounted for the extraordinary development of dairy products in nutrition cannot be assessed, but surely it was not exceeded by that of any other person.

The clearest beginning probably was in April 1918, when he suggested at a meeting of the Associated Dairy Associations in Chicago that consideration be given to what might be done to increase the consumption of dairy products in the interest of better health. This was in connection with his speaking tour of the country on behalf of the policies of the Hoover Food Administration. These words from that Chicago address made a profound impression on this key group:

"I have been traveling almost continuously for the past two months, telling the people of this country to patronize the dairy industry. I have formed certain conclusions as the result of ten years of experimental study of nutrition, which it will be to your profit to hear and I want your support and assistance in my attempt to spread information concerning the paramount importance of dairy products in the nutrition of man."

Through regular contributions from various branches of the industry, the newly formed National Dairy Council began an extensive program of responsible education of health leaders and the public. This has continued over the years. McCollum maintained an active interest in the Council throughout his life. He never carried an official title, but staff members and others connected with the industry sought his advice.

For many years he maintained some professional connection with the Certified Milk Association.

In the 1930's and 1940's he had much to do with the plan ning, staffing, and supervising of the research work of the National Dairy Products Corporation. During this period the research laboratories were in Baltimore. His general practice was to stop at the laboratories early in the morning for an hour or two on the way to his office at the university. To increase his contacts with the laboratory, he also held a weekly dinner meeting at his home with the key personnel. This enterprise became one of the world's great food industry laboratories. It was characteristic of McCollum that a considerable part of the income from this arrangement was invested in an insurance policy on his life, with The Johns Hopkins University as the beneficiary. As he wrote in a letter to the author in 1956, "The University has afforded me wonderful opportunities, and I wanted to return as much as possible of what was given me as salary."

THE BREAD ENRICHMENT DEBATE

Owing to the widely recognized nutritional deficiencies of white bread, which over the years McCollum demonstrated and publicized, it was inevitable that the developments in producing certain synthetic vitamins should lead to proposals for their use in programs for the fortification of bread and flour. Thus in 1941, with all the enthusiasm and urgency that war-borne causes and new converts can command, the national campaign to enrich bread and flour with thiamine, niacin, and iron was powerfully launched. The addition of riboflavin and calcium was not stressed as vigorously. The principal source of authority for the action was the Food and Nutrition Board of the National Research Council. In the same year McCollum became a member of the Board. But contrary to the action of the other members, he was strongly critical of this apparent means of improving the national health, since in his judgment such nutrients alone failed to make up all the losses incurred in milling wheat. This led to an extended period of controversy which included a change in his status from Board member to "panel member." Following this change he was not invited to attend any other meetings of the Board.

In retrospect, the bases for his apprehension and objection were logical, but the relatively simple actions needed to assure the controlled addition of selected vitamins and iron to bread and flour were more inviting to the industry and many of the authorities in nutrition than was the implementation of McCollum's plan. His enrichment proposals included the addition of nonfat milk solids, brewer's yeast, and wheat and corn germs to flour and bread. Experimental evidence showed that such supplements improved most diets more than the adopted vitamin-iron enrichment plan. Also, tests of the consumer acceptability suggested that the McCollum plan could be made to work.

During the next quarter of a century that he lived McCollum continued to study and think about the supplementation of bread and related foods. He always felt that his plan was superior to the program that was adopted. Notably, continuing developments in nutrition supported his position that the adopted program did nothing to meet the needs for better protein. His foresight and his unequivocal stand on scientific evidence as the basis for public policy remains a monument to his wisdom and determination.

NUTRITION AND DENTAL HEALTH

Early in his nutritional studies McCollum began to consider the possible relations of diet to dental caries and some other dental problems. About 1920, dental societies began to invite him to address their meetings. During approximately the next twenty years his involvement in this manner and as a consultant occupied a substantial proportion of his time and thought. His droll wit and the exceptional breadth of his knowledge and perceptive exploration of new developments made him widely appreciated. This is evidenced by his receiving the Newell Sill Jenkin Medal of the Connecticut Dental Society, the Callahan Medal of the Ohio Dental Society, and the designation of nonresident Fellow of the New York Academy of Dentistry and honorary member of the American Academy of Dental Medicine.

In his own laboratory, both at Madison and later at Baltimore, McCollum and his associates pioneered in producing and describing dental and skeletal defects in experimental animals given faulty diets. In 1925 they were the first to note that a large excess of fluoride in the diet is dramatically harmful to the incisors. Much attention was given to dietary calcium and phosphorus as possible factors in the occurrence of dental caries. Hypoplastic enamel was produced in young animals given certain faulty diets. In 1922 they were the first to describe the gross appearance of caries in experimental rats and to publish photographs of some of the lesions they observed. Their findings and interpretations greatly stimulated experimental developments and serious study of the etiology and prevention of dental caries.

One of the signal measures of the esteem in which Dr. McCollum was held is evidenced by his selection to moderate a major conference on "The Cause and Prevention of Dental Caries" sponsored by the Good Teeth Council for Children, Inc., held in early July 1938, at Chicago. Probably it was the most comprehensive conference of this kind ever held. Seemingly every serious hypothesis and every important collection of evidence up to that time were considered. Characteristically, in beginning the conference, he said: "I hope that we can melt down here the experimental work of recent years and come to an agreement, at least on some points, as to what is established. And in the case of subjects on which we are not in agreement, I hope we may be able to see where the trouble lies and determine what to do next in dental research."

By coincidence, almost at the very time this notable assemblage of scientists was groping for understanding and a basis for hope that caries might be prevented, *Time* magazine reported that on the basis of statistical studies of the U.S. Public Health Service, fluoride in the drinking water reduces the incidence of dental caries. If the conference had been held a few months later, this epochal discovery surely would have dominated much of the searching discussion.

From the beginning of recorded evidence that controlled fluoridation of drinking water is beneficial, McCollum was a believer in this means of promoting dental health. This was typical of his keen awareness of new developments and his wisdom in their assessment. In a letter to the writer ten months before his death he wrote, "Fluoridation has tremendous health importance, and has only merit, not as so many contributions of science to public health, substituting morbidity for mortality by prolonging life in decrepitude."

RETIREMENT

Dr. McCollum lived twenty-three years following his retirement from the faculty of The Johns Hopkins University. This long span surely was rich in his continuing contributions to nutritional science, particularly in the production of his outstanding book *A History of Nutrition,* his autobiography *From Kansas Farm Boy to Scientist,* and a large series of reflective articles, including some research papers and patents. Moreover, until the last few months of his life his wish was fulfilled, "that in my old age I want to keep my mind in a state of continual adventure."

Retirement included much general and special reading, a practice he had cultivated from youth. In an inventory of the personal library in his home at the time of his death there were more than 1400 volumes, all of which he had used. They covered virtually every area of man's higher concerns, including:

Agriculture 33
Art and anthropology 33
Biochemistry and biology 71
Biography 245
Cartoons and humor 13
Chemistry 91
Dictionaries and encyclopedias 53
Economics 19
Education 14
Fiction 33
Geography and travel 33
History 179
Literature and languages 201
Medicine 32
Nature 21
Nutrition 113
Philosophy 42
Physics 12
Poetry 46
Religion 16
Royal Society 29
Science, general 37
Miscellaneous 40

The inventory does not include the many hundreds of scientific books and periodicals he gave to the department of biochemistry at Hopkins' School of Hygiene and Public Health at the time of his retirement. They are in the McCollum Reading Room in that department. Also, he had contributed several hundred books to the Welch Medical Library at Hopkins. There is no complete record of the hundreds of volumes he gave to a host of friends, family members, and former students. Surely great books were his constant companions, and he delighted in knowing and understanding their content.

Even in retirement, Dr. McCollum's never-ceasing concern for man's welfare many generations ahead was manifested in several ways. He gave much thought and effort to the arousal of interest in finding ways to minimize the loss of essential mineral nutrients, especially potassium and phosphorus, in our sewage disposal systems. He personally wrote to many scores of his friends and others concerned with public policy. The letters asked for action and suggestions on what should be done. In commenting on his concern he stated, "It is my belief that scientific investigations on how best to prevent this gigantic waste of our natural resources should be given high priority, and that at whatever cost, plant nutrients now wasted should be recovered before disposal of sewage effluent in the interest of the future of mankind."

During the years at Yale, McCollum gave some time to the use of the Fischer ester method for the analysis of two of Osborne's proteins. His interest in proteins and amino acids never waned. Ten years before he retired, with Olaf S. Rask, he began to search for chemical methods for separating specific amino acids from protein hydrolysates. In his retirement this became his exclusive laboratory interest. A special but modest laboratory was set up for him on the Homewood Campus of the university. Mrs. Agatha Ann Rider, who had worked with him at the School of Hygiene and Public Health, became his principal assistant. Six papers and three patents resulted from this effort. The last experimental contribution was a patent issued March 15, 1960 on the purification of glutamine. At this time Dr. McCollum was eighty-one years old. Since his first contribution was in 1903, his total span of scientific productivity was fifty-seven years.

There were many remembrances and glad periods of reunion with friends, students, and colleagues of earlier years. McCollum had an extraordinary gift for making devoted friends. Simply to visit with him was an elevating experience. Innumerable persons came to his office at "Homewood" or his spacious and comfortable home on Talbot Road simply to pay their respects, to enjoy his gracious hospitality, or to seek his counsel or assistance with a project. And there were a number of delightful periods when special recognitions were bestowed on him or worthy undertakings were established in his name.

In 1947, three years after McCollum's retirement, John Lee Pratt gave $500,000 to The Johns Hopkins University to support a research program on the biological significance of trace inorganic elements. This led immediately to the establishment of the McCollum-Pratt Institute. After its successful beginning, Pratt made another gift that was two times the original. Mc

Collum's participation in the Institute was somewhat limited, but he did propose the first director, Dr. William McElroy, and he took an active interest in several of its programs. Occasionally he gave lectures, and he attended many of the seminars. As he later wrote, "Association with the Institute has been a constant delight as well as of great educational value to me."

Seven years after his retirement, in 1951, the university honored him through a two-day symposium on "The Physiological Role of Certain Vitamins and Trace Elements." Fifteen distinguished scientists presented papers. This was followed by an impressive banquet. Many of his former students and associates returned to Hopkins for this appropriate tribute.

More than two hundred admirers of Dr. McCollum contributed several thousand dollars for an oil portrait. Most of the contributions were accompanied by warm tributes of affection and appreciation for his personal qualities and pioneering contributions. More than one hundred persons were present for the unveiling at the great Welch Medical Library in 1955. Characteristically, he wrote to the author, "It was a great party, and made me somewhat emotional, but I am recovering."

In Dr. McCollum's reflections on the presentation of the portrait, his emotion-laden words portrayed more succinctly than he must have realized the depth of his feeling in being a pioneer in nutritional science. He stated:

" . . . that much of whatever credit has been given me for investigations which have contributed to better understanding of the relation of food to health, must be shared with those who worked with me, and with those who provided for us almost unparalleled opportunity in the form of housing, equipment, salaries, and all other financial resources. Very few have ever been so fortunate as I, in being able, over so long an uninterrupted period, to do what he wanted to do, and with so few interfering obligations."

In 1965, twenty-one years after his retirement, probably the greatest honor of all was paid to Dr. McCollum when the University of Kansas dedicated McCollum Hall in his honor and that of his late brother Burton. (The brother had distinguished himself and became wealthy through his inventions and business operations in the discovery of oil deposits.) The magnificent ten-story dormitory houses more than 1000 students. A portrait of Dr. McCollum and one of his brother hang side by side in the entrance foyer. In writing to the author concerning the dedication, he stated, "For me it was an occasion for deep emotion—having my name associated with my distinguished brother in the naming of so fine a monument to the two of us. Many fine words were said."

Such recognition of this loyal son of the University of Kansas was surely merited. One little-known fact is that over the years Dr. McCollum always contributed his honoraria for public lectures to a student loan fund at the University of Kansas. This amounted to more than $40,000.

Also in 1965, three other sentimental events gave much satisfaction to Mr. and Mrs. McCollum. As he expressed it in a letter, "This has been a great year for us." It began with his attendance in Chicago as an honored guest at the fiftieth anniversary celebration of the founding of the National Dairy Council, organized at his suggestion in 1915.

Three months later he was escorted to Atlantic City to attend a dinner given by the American Society for Clinical Nutrition, where he was privileged to witness the bestowal of the first annual McCollum Award, which is administered by that Society. The Award is sponsored by the National Dairy Council.

One month later, again with Mrs. McCollum, he was in California as an honored speaker at the fiftieth anniversary of the appointment of his good friend, Agnes Fay Morgan, to the faculty of the University of California at Berkeley.

This was his crowning year.

The next year, twenty-two years after McCollum's retirement, the glow of his sunset was still bright. His eighty-seventh birthday brought greeting cards, letters, and telegrams from friends and well wishers from all over the world. He wrote, "I do not like to realize how many yesterdays and how few tomorrows there are, but scarcely anyone I know has been more fortunate in life than have I. . . . I never felt better in my life." Three months later, with Mrs. McCollum, he was invited to be an honored guest at the one-hundredth anniversary of the founding of his beloved University of Kansas. Physical disability at the time prevented him from attending.

The next birthday, in 1967, and his last, was equally pleasant for him, and he wrote, "I have had an exceptionally pleasant life, and am thankful." He continued to read widely and, as he wrote, "to keep in touch with the best that has been thought and said." Five months after that eighty-eighth birthday, his health failed precipitously, and after three months his physical life ended.

Sixteen years before, in commenting on his life, *Time* magazine recognized the measure of McCollum's lasting contributions by stating: "He has done more than any other man to put vitamins back in the nation's bread and milk, to put fruit on American breakfast tables, fresh vegetables and salad greens in the daily diet." And as concluded by his long-time friend and distinguished colleague, Dr. Edwards Park, three years before McCollum's death:

"McCollum's vision at the very start of his career of the necessity for an entirely new kind of attack and the revolutionary method to be employed in studies of nutrition was a scintillation of genius. It is no exaggeration to say that it started a new era in nutritional research."

He was a gifted scientist and effective humanitarian who, in his own words, had " . . . participated in a great drama of human endeavor which has demonstrated the new truth that the pro vision of a specific nutrient lacking in the diet of people in great numbers in many parts of the world will do more than argument, law, and sermons to create comfort, courage, optimism, and purpose."

And so Dr. McCollum is remembered.

I wish to express my appreciation to many persons for information and suggestions. I am especially indebted to Mrs. McCollum, to each of Dr. McCollum's children, and to George V. Mann, Olaf Mickelsen, Harry J. Prebluda, Agatha A. Rider, and Samuel Weisberg; but none is responsible for any omissions or errors in judgment that may have occurred in preparing the memoir. For any shortcomings I alone am responsible.

CHRONOLOGY

1879

Born March 3 near Fort Scott, Kansas

1896

Family moved to Lawrence, Kansas, for educational advantages

1900

As class president, delivered an oration at high school graduation exercises

Enrolled at University of Kansas with advanced credits in chemistry, physics, and English composition

1903

Membership in Sigma Xi, Kansas Chapter, although this was generally reserved for graduate students

A.B. in chemistry, University of Kansas

1904

M.A. in chemistry, University of Kansas

Began graduate work in organic chemistry, Yale University

1906

Ph.D. in organic chemistry, Yale University

1906-1907

Postdoctoral student under L. B. Mendel, Yale University

1907

Married Constance Carruth, Lawrence, Kansas

As instructor, became part of a team to conduct the famous nutritional experiments with heifers, University of Wisconsin

1908

Started his first experiments using rats "to study the nutritional requirements of animals," University of Wisconsin

1909

Proved that animals (chickens) utilize inorganic phosphorus for the phosphorylation of proteins, fats, and nucleic acids and that they synthesize purines and pyrimidines

1913

Published first proof for the existence of a fat-soluble nutrient (vitamin A)

1915

Wrote a series of articles for *Hoard's Dairyman* to acquaint the public with new viewpoints in animal feeding

1917

Harvey Society lecturer on "The Supplementary Relations among Our Common Foodstuffs"

Used for the first time the term "protective foods," which soon gained common usage in practical nutrition

Moved to The Johns Hopkins University as head of Department of Chemical Hygiene (later Biochemistry) in the School of Hygiene and Public Health

1917-1919

Member, Advisory Committee of the U.S. Food Administration

1918

Delivered the Cutter Lectures on Hygiene and Preventive Medicine at Harvard

The first edition of *The Newer Knowledge of Nutrition* was published

National lecturer for the U.S. Food Administration on improvements of diets and making better use of available foods

1919

Honorary member, The American Dietetic Association

1919-1931

Consultant, the Merrill-Palmer Institute, Detroit, Michigan

1920

Member, National Academy of Sciences Honorary Sc.D. degree, University of Cincinnati

1921

Howard N. Potts Gold Medal of the Franklin Institute "for Distinguished Scientific Work"

1922

The second edition of *The New Knowledge of Nutrition* was published

1923

Became a regular feature writer on nutrition for *McCall's Magazine*

Began successfully to encourage the use of skim milk in breadmaking

1924

The John Scott Medal and cash award, City of Philadelphia

1925

Foreign member, Deutsche Akademie der Naturforscher Leopoldina

Foreign member, The Royal Academy of Medicine of Belgium

1927-1929

President, American Society of Biological Chemists

1927

Newell Sill Jenkin Medal, Connecticut Dental Society

1928-1937

Consultant, Bureau of Animal Industry, U.S. Department of Agriculture

1929

Founded the central research laboratories of the National Dairy Products Corporation

1931

Member, First International Conference on Vitamin

Standards (London) of the League of Nations

U.S. delegate to the Eighth International Dairy Congress, at Copenhagen, Denmark

1932

Nonresident Fellow, the New York Academy of Dentistry

1932-1949

Member, U.S. Pharmacopeial Vitamin Advisory Council

1933-1937

Member, U.S. National Advisory Health Council

1934

Gold Medal, American Institute of New York

1935

The Callahan Medal, Ohio State Dental Association

Member, Permanent Commission on Nutrition of the League of Nations

Honorary LL.D. degree, University of Manitoba, Canada

1936-1937

Member, Mixed Committee on Nutrition, Health Section, League of Nations (Geneva)

1937

Collaborator, Bureau of Animal Industry, U.S. Department of Agriculture

Citation by the University of Kansas for conferring honor on the state and its university

Member, Conference of Technical Experts, League of Nations (London)

1938

Consultant, National Institute of Health

Chairman, Nutrition Section of the Tenth Pan American Sanitary Conference (Bogota)

Award, Associated Grocery Manufacturers of America

President, American Institute of Nutrition

1939

Chairman, Nutrition Section of the Pan American Sanitary Bureau

1940

The Mead Johnson Award, American Institute of Nutrition

Modern Pioneers Award, National Association of Manufacturers

1941

Chairman, Section on Research of the National Conference on Nutrition in Defense

Chairman, U.S. Advisory Committee of the Coordinator of Information on Food and Nutrition

Member, Food and Nutrition Board of the National Research Council

1941-1953

Member, Scientific Advisory Committee, Nutrition Foundation, Inc.

1941

Sculptured portrait of Dr. McCollum included in a frieze around the Hall of Fame, Museum of the Rochester Academy of Medicine. Frieze presents portraits of the twenty-four persons who in North America were judged to have made the most important discoveries in medical sciences in the previous one hundred-fifty years

1942

Member, Subcommittee on Emergency Research of the Committee on Foods and Nutrition of the National Research Council

1943

Consultant, Industrial Hygiene Section of the U.S. Army

Consultant, U.S. Lend-Lease Administration

Foreign member, Royal Swedish Academy of Sciences

1944

The Borden Award, American Institute of Nutrition

1944-1946

Retired from the faculty of The Johns Hopkins University and became emeritus professor, but continued to serve on a half-time basis until his successor was appointed in 1946

1945

Married J. Ernestine Becker, Baltimore, Maryland

Member, American Philosophical Society

1947

Chairman, Advisory Committee of the Robert Gould Foundation

1948

Beginning of the McCollum-Pratt Institute for research on "trace elements" at The Johns Hopkins University Member, Advisory Committee of the McCollum-Pratt Institute

1951

Honorary LL.D. degree, The Johns Hopkins University

Honoring Dr. McCollum, Symposium of the Robert Gould Research Foundation on "The Physiological Role of Certain Vitamins and Trace Elements"

Foreign member, The Royal Society of Arts (London)

1952

Honorary member, The British Nutrition Society

Honorary member, International Association for Dental Research

Samuel J. Crumbine Award, Kansas Public Health Association

1953

Honorary member, American Academy of Dental Medicine

1955

The Osborne and Mendel Award, American Institute of Nutrition

Presentation of commissioned portrait of Dr. McCollum to The Johns Hopkins University, by many friends and former students

1957

The Borden Centennial Award for Pre-eminent and Pioneering Achievement in Nutrition, The Borden Company

1958

The Charles F. Spencer Award, American Chemical Society

Citation by the Merrill-Palmer Institute for services as consultant at the founding and development of the Institute

1959

Honorary Doctor of Humane Letters degree, Brandeis University

Honorary President, International Union of Nutritional Sciences

1960

Honorary President, International Congress on Nutrition, Washington, D.C.

Modern Medicine Award for Distinguished Achievement in Furthering the Progress of Medicine through Scientific Research

1961

Medal and citation of the New York Academy of Medicine Foreign member, Royal Society of London

1965

McCollum Hall, dedicated in honor of E. V. McCollum and his brother, Burton McCollum, at the University of Kansas

McCollum Annual Award in Nutrition established by the National Dairy Council and to be administered by the American Society for Clinical Nutrition

1966

Citation by the Maryland Section of the University of Wisconsin Alumni Association for the initiation of research based on the use of laboratory rats to determine the chemical components of diet needed in nutrition

1971

Died, November 15, at Baltimore, Maryland

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1971

Elmer Verner McCollum Chair in Biochemistry, established in the School of Hygiene and Public Health at The Johns Hopkins University

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KEY TO ABBREVIATIONS

Am. Chem. J. = American Chemical Journal
Am. Food J. = American Food Journal
Am. J. Diseases Children = American Journal of Diseases of Children
Am. J. Hyg. = American Journal of Hygiene
Am. J. Physiol. = American Journal of Physiology
Am. J. Public Health = American Journal of Public Health
Ann. Am. Acad. Political Social Sci. = Annals of the American Academy of Political and
Social Science
Ann. Rev. Biochem. = Annual Review of Biochemistry
Arch. Biochem. Biophys. = Archives of Biochemistry and Biophysics
Arch. Therap. = Archives of Therapeutics
Bol. Ofic. Sanit. Panam. = Boletin de la Oficina Sanitaria Panamericana
Bull. Johns Hopkins Hosp. = Bulletin of the Johns Hopkins Hospital
Dent. Cosmos = Dental Cosmos
Exp. Sta. Rec. = Experiment Station Record
J. Am. Dent. Assoc. = Journal of the American Dental Association
J. Am. Dietet. Assoc. = Journal of the American Dietetic Association
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Biol. Chem. = Journal of Biological Chemistry
J. Chem. Soc. = Journal of the Chemical Society
J. Dental Res. = Journal of Dental Research
J. Home Econ. = Journal of Home Economics
J. Md. Acad. Sci. = Journal of the Maryland Academy of Science
J. Nutr. = Journal of Nutrition
J. Roy. Inst. Public Health Hyg. = Journal of the Royal Institute of Public Health and
Hygiene
Nutr. Rev. = Nutrition Reviews
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Public Health Rept. = Public Health Reports
U.S. Dept. Comm. Bur. Fish. Invest. Rept. = U.S. Department of Commerce Bureau of Fisheries, Investigational Report
Wis. Agr. Exp. Sta. Res. Bull. = Wisconsin Agricultural Experiment Station Research Bulletin

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George Richards Minot

December 2, 1885-February 25, 1950

by W. B. Castle

George Minot was born in Boston, Massachusetts, on December 2, 1885, the eldest of three sons of Dr. James Jackson and Elizabeth Frances (Whitney) Minot. His ancestors had been successful in business and professional careers in Boston. His father was a private practitioner and for many years a clinical teacher of medicine as a member of the staff of the Massachusetts General Hospital. In the second half of the nineteenth century his great uncle, Francis Minot, became the third Hersey Professor of the Theory and Practice of Physic at Harvard; and his cousin, Charles Sedgwick Minot, a distinguished anatomist, was Professor of Histology there in the early years of the twentieth century. George Minot's grandmother was the daughter of Dr. James Jackson, the second Hersey Professor and a cofounder with John Collins Warren of the Massachusetts General Hospital, which opened its doors in 1821. Thus his forebears, like those of other Boston medical families, were influential participants in the activities of the Harvard Medical School and its affiliated teaching hospital.

George was regarded by his physician-father as a delicate child who required physical protection and nourishing food. Brief vacation visits to Florida provided escape, thought desirable for him, from the rigors of Boston winters. Most of a winter spent with his parents in southern California gave further opportunity for outdoor life and amateur studies of butterflies. This led to the publication in 1902 of his first scientific paper, an early expression of his lifelong interest in natural history. George Minot's early education was at private schools in Boston in the "Back Bay" near the home in which he grew up. As a matter of course he went on to Harvard College, from which he was graduated in the spring of 1908. After a summer in Europe, despite anxiety about his physical capacity for the busy life of a doctor, he enrolled in the Harvard Medical School armed merely with the documentation that his college courses had included physics and chemistry.

During the summers of his second and third years in medical school Minot worked in an outpatient clinic operated by the faculty at the medical school for the benefit of employees and neighborhood residents. During his third year he achieved modest renown by demonstrating to the satisfaction of the medical staff of the Massachusetts General Hospital that a patient considered to have pernicious anemia was in reality suffering from a congenital hemolytic anemia. Among his teachers at the medical school were Walter B. Cannon in physiology, Otto Folin and Lawrence J. Henderson in biochemistry, Theobald Smith in comparative pathology, Richard C. Cabot and Henry A. Christian in medicine, and Maurice B. Richardson in surgery. Christian's systematic lecture presentations of medical topics were admirably balanced by Cabot's novel "case teaching" exercises, in which students in their clinical years participated actively. This educational technique, employed earlier in the Harvard Law School, had been proposed by Cannon when he was a senior medical student at Harvard. It was during Minot's final year that he first showed a serious interest in hematology by enrolling in an elective course in clinical pathology given at the medical school by Dr. J. Homer Wright, inventor of the well-known polychrome stain for blood films.

A few months after graduation young Dr. Minot began a coveted appointment as medical "House Pupil," as the interns at the Massachusetts General Hospital were then called. There, while working on the East Medical Service under Dr. David Edsall, the recently appointed Jackson Professor of Clinical Medicine at Harvard, he displayed increasing interest in diseases of the blood. For some reason, perhaps the familial insistence on the importance to health of good food, he began taking meticulous histories of the dietary habits of his anemic patients. He also exhibited an active interest in the laboratory findings, especially in the microscopic examination of stained films of their blood. Sixteen months later, at the end of November 1913, Minot completed his tour of duty as "Senior" and was borne in traditional fashion to the front door of the hospital in a wheelchair propelled by his "Junior." Among recent graduates of the medical services he was in good intellectual company with such future distinguished physicians as James Howard Means and Paul Dudley White. Like them he was advised by Dr. Edsall to go elsewhere for further training before returning to the Massachusetts General Hospital with the prospect of becoming a junior member of its staff.

It was soon arranged that Minot should go to Johns Hopkins and serve as Resident Physician under Dr. William S. Thayer, to whom Minot's special interest in patients with hematological problems soon became apparent. Consequently, after a few months Minot transferred his activities to the laboratory of William H. Howell, Professor of Physiology, whose principal research interest was in the coagulation of the blood. With another young physician from Boston, George Denny, Minot published in 1915 an article demonstrating that circulatory stasis during perfusion of the liver of the dog produced an increase in the antithrombin content of the blood in the hepatic vein. This insight, together with studies by others of his pupils, led three years later to Howell's successful preparation from liver of the valuable anticoagulant drug, heparin. Minot's other work, on the prothrombin and antithrombin factors involved in the abnormal clotting of the blood of various patients, would be interpreted somewhat differently today. However, oxalated blood samples from jaundiced patients with bleeding tendencies were observed to show abnormal delay in coagulation after recalcification. This Minot and Denny correctly attributed to a diminished level of prothrombin as then defined.

When Minot returned to Boston, in January 1915, he received appointments in the Harvard Medical School and in the Massachusetts General Hospital as Assistant in Medicine with a small stipend as a Dalton Scholar. In June he married Marian Linzee Weld in the Unitarian church in Milton, Massachusetts. At the hospital, Edsall's influence had brought to Boston the new era of scientific investigation of disease already begun in Baltimore and New York. In addition to participation in the care of patients Minot, as well as James H. Means and Paul D. White, who had returned from their studies abroad, and shortly Walter W. Palmer and James L. Gamble were attempting to apply scientific methodology to bedside medicine with the help of various types of simple equipment in improvised laboratories. This atmosphere of young inquiry was highly appropriate for Minot, who had an inherent faith in causality that made him optimistic that scientific understanding would lead to a bright future for clinical medicine. He found a bench with north lighting suitable for microscopic work in a small room next to the inner sanctum of the hospital's irascible pathologist, Dr. Wright. This was the man who had discovered that blood platelets were formed by large specialized cells in the bone marrow.

At that time the study of the blood of patients with anemia, leukemia, low platelet levels, and other abnormalities depended largely on the enumeration of the corpuscles and the microscopic examination of peripheral blood films stained by the use of the aniline dyes introduced by Ehrlich. This permitted recognition under the oil immersion lens of morphological abnormalities of the blood's three formed elements: red cells, white cells, and platelets. In hematology the only useful drug was iron, but a recent therapeutic advance was the transfusion of fresh blood. This, owing to the work of Landsteiner, Moss, and others in enabling the identification of suitable donors, had become a relatively safe, though technically demanding procedure.

In the fall of 1915 Minot began work with Dr. Roger I. Lee, Clinical Professor of Medicine at Harvard and Chief of the West Medical Service at the Massachusetts General, in an attempt to learn more about the function of blood platelets—insignificant particles, but already thought to be intimately involved in hemostasis and blood coagulation. After meticulous washing in physiological saline, a suspension of normal platelets derived from blood samples rendered incoagulable by addition of oxalate was found by Lee and Minot to be seventy-five times as efficacious as a similarly prepared suspension of hemophilic platelets in shortening the prolonged clotting time of fresh hemophilic blood plasma. This last was derived by centrifugation of fresh blood in chilled, paraffin-coated tubes. Thus encouraged, the two physicians gave a transfusion of normal blood to a hemophilic patient and found that it caused a prompt reduction of the patient's prolonged blood clotting time. The effect lasted for three days, an interval then considered to correspond to the life-span of the platelet. From these observations they concluded, understandably but erroneously, that the platelets in hemophilia were defective. This interpretation, because of the impossibility of completely freeing platelets from a subtle plasma factor, was only corrected thirty years later when work in Minot's laboratory at the Boston City Hospital showed that platelet-free, citrated, normal blood plasma could shorten the coagulation time of hemophilic blood owing to the presence of a specific globulin.

From the Massachusetts General Hospital Minot published papers describing and classifying patients with anemia and markedly reduced numbers of blood platelets. He pointed out that a faltering bone marrow, in addition to producing too few new red cells, was often unable to sustain a normal number of circulating granular leukocytes and platelets. In clinical instances of low levels of platelets associated with normal production of red cells and leukocytes, he suggested that excessive destruction of platelets was responsible. He speculated that the great increase in platelets sometimes following splenectomy was due to enhanced production by an uninhibited bone marrow. Although today the role of the spleen as combined pool and filter in causing low platelet levels is better understood, the mechanism of the sometimes prolonged increase in numbers of platelets after surgical removal of the spleen is not. In an extensive study in 1916 of a young girl with idiopathic purpura hemorrhagica, who eventually bled to death because of her low level of blood platelets, Minot sought without success for evidence of platelet-agglutinating or -lysing properties in her serum. Even today these are detected only with great difficulty and only in some of such patients. In other work, with Dr. Wright, Minot studied the plasma factors involved in the so-called viscous metamorphosis of platelets, a step preliminary to their participation in normal blood-clot formation.

While in Baltimore in 1914 Minot had studied the effect of splenectomy in a patient with pernicious anemia, then almost invariably a fatal disease although for some months subject to apparently spontaneous remissions and discouraging unexplained relapses. The result of removal of the spleen in the patient had been temporarily favorable. Three years later in Boston Minot joined Lee and Beth Vincent, a surgical colleague, in studies of fifteen such patients skillfully subjected to splenectomy as a last resort. In some of these patients the results were beneficial for a few weeks or months, but they failed to be of a permanent value in any. However, Minot's careful ob servations of the peripheral blood were of great significance to his future work on the successful dietary treatment of pernicious anemia. Wright had shown Minot a method of supravital staining that the latter applied to the demonstration of reticulocytes in films of the peripheral blood of these anemic patients. That reticulocytes were newly formed red cells released by the bone marrow, rather than degenerating forms as originally supposed by Ehrlich, had first been demonstrated by Theobald Smith in 1891 in bleeding experiments with Texas cattle. Vogel and McCurdy in 1913 had proposed that anemias could be classified as being due either to increased blood destruction or blood loss with an active marrow response (increased reticulocytes) or being due to bone marrow inadequacies of various sorts (decreased reticulocytes). Now, Minot stated that in pernicious anemia "curves plotted from frequent observations [of the number of reticulated red cells] are reliable indicators of bone marrow activity and [increases] are the forerunners of increased red cell counts and clinical improvement."

When during World War I Base Hospital No. 6, organized by the Massachusetts General Hospital, sailed from New York for overseas duty, the work of the hospital staff of doctors and nurses left behind was greatly increased. Minot was directly involved with this extra burden of patient care as well as with his laboratory research and private practice. In addition, he was employed for a short time as Contract Surgeon by the army in examining recruits. He soon found much more interesting contributions to make to the war effort. At the suggestion of Dr. Alice Hamilton, then the only woman on the Harvard Medical Faculty and already an authority in industrial medicine, the army asked Minot to investigate the anemia of New Jersey ammunition workers engaged in filling shells with trinitrotoluene. He discovered the coexistence of methemoglobin and signs of a red cell-destroying process. A similar investigation concerned with manufacture of smokeless powder disclosed little but the anesthetic effects of various degrees of exposure to the ethyl ether used as a solvent. In the fall of 1918 Minot was involved in a desperate effort to prevent the spread of influenza among the students of Harvard College, many of whom were candidates for the Student Army Training Corps. Then in another month the epidemic waned, and the Armistice was signed.

After the war Minot continued to study patients with blood diseases at the Massachusetts General Hospital, but with increasing involvement in work at the Collis P. Huntington Memorial Hospital, where he had been appointed Assistant Consulting Physician in 1917 and Consulting Physician in 1919. Meanwhile, another essay in industrial medicine disclosed interesting changes in the blood of workers in an artificial silk factory. This led to a report in 1921 that an increase in the large mononuclear cells of the blood was a clear signal of liver damage to come, if exposure to tetrachlorethane—the volatile solvent inhaled—was not discontinued. The results of an attempt with Dr. Chester M. Jones to establish as a clinical entity the sporadic cases of infectious jaundice that appeared subsequent to the epidemics of World War I was published in 1923. Here, too, immature and abnormal lymphocytes and mononuclear cells appeared in the blood and resembled those seen in cases of so-called glandular fever or infectious mononucleosis. Indeed, at the time no serological test was available to discriminate glandular fever from infectious jaundice with certainty.

From the time of his first appointment at the Huntington Hospital Minot found himself becoming evermore interested in the special purposes of this "cancer hospital." Supervised by the Cancer Commission of Harvard University, composed of six distinguished physicians with Ernest E. Tyzzer as Director, the hospital had opened in 1912 for the purpose of providing what would be called today a "clinical research facility" where, in addition to the latest diagnostic and therapeutic modalities for the care of patients, basic research in the nature and cause of cancer and neoplastic blood disease could be conducted. Located close by the Harvard Medical School and the new Peter Bent Brigham Hospital, the Huntington Hospital was in no sense an institution merely for custodial care. Minot saw in this hospital setting a rare opportunity to extend his interest into new areas of blood disease with possible valuable repercussions on what he already knew. Moreover, in accepting the original appointment he was attracted by the delightful personality, broad clinical and research experience, and scholarly wisdom of the hospital's Consulting Physician, Dr. Francis W. Peabody. At the Huntington, Minot's clinical and research contributions were highly valued by his colleagues and by the Cancer Commission. When Peabody became Director of the new Thorndike Memorial Laboratory at the Boston City Hospital, in 1923, Minot was appointed to succeed him as Chief of the Medical Service at the Huntington.

At the cancer hospital Minot became impressed with a relationship between polycythemia vera, a disease producing too many red blood corpuscles, and myelogenous leukemia, a form of cancer of the blood in which too many abnormal white cells are formed. With his associates, Drs. Thomas E. Buckman and Raphael Isaacs, he published careful descriptions of the blood findings, clinical course, and results of x-ray treatment in myelogenous and other varieties of chronic leukemia. He concluded—as is still true today, with modern x-ray and chemotherapy—that the benefits of treatment were chiefly to extend the period of reasonably good health, rather than to prolong the life of the patient. In another classical paper, in 1924, Minot and Dr. Roy G. Spurling described the different durations of the effects of x-ray treatment on the formed elements of the blood of patients with localized cancer, including the value of the number of circulating white corpuscles as an indication of the amount of such radiation that could safely be applied. It was shortly before this time that Dr. Minot began to urge his private patients with pernicious anemia to improve their diets.

Meanwhile, a serious change in Minot's health and personal life had taken place. In October 1921, after noting for some days feelings of fatigue, weakness, and thirst, he tested his urine and found sugar. The next day Dr. Elliott P. Joslin confirmed the fact that at the age of thirty-six Minot had developed severe diabetes, for which the treatment was currently a form of semi-starvation. In those dark days Minot's cousin and medical school classmate, Dr. Francis M. Rackemann, and his wife gave cheerful support and wise counsel to the Minot family. The discovery of insulin by Banting and Best, announced in 1922, came in the nick of time to save Minot's life. After a year of dietary restriction and weight loss, during which Minot managed to struggle each day to the hospital, Dr. Joslin was able to secure for his patient small amounts of insulin. For the rest of his life Minot ate no food at home that was not weighed or measured and recorded. When dining out, advance knowledge of the menu aided him in estimating calories and carbohydrates. His wife, a charming and intelligent woman, was indispensible in sustaining the strict balance of dietary intake and insulin injections prescribed by Dr. Joslin.

Under these circumstances, it was a distinct advantage to Minot to be able to continue his private practice as a member of the small group of physicians that he had joined at the invitation of Dr. Edwin A. Locke in September 1921, shortly before the onset of his diabetes. This arrangement provided office space at 311 Beacon Street, with shared secretarial services and a laboratory with a technician trained to perform the usual simple studies of patients' blood and urine. Included in the group was a succession of younger physicians who in this way were entering the practice of medicine in Boston. Among them was Dr. William P. Murphy, who later was asked by Minot to be his col laborator in an effort to treat pernicious anemia by dietary means.

This disease was first briefly reported to colleagues in London in 1849 by Dr. Thomas Addison of Guy's Hospital. In 1855 Addison published, in the same monograph with his classic account of destructive disease of the suprarenal capsules, a description of an "idiopathic anaemia." His description, however, included none of the triad of clinical features that would have identified pernicious anemia, then as now, without benefit of laboratory studies: sore tongue, jaundice, and, especially, neurological manifestations, such as tingling and numbness of fingers and toes progressing in some patients to instability of gait and paralysis. Nevertheless, Addison's contemporaries seem to have recognized similar cases, perhaps at first retrospectively because the lack of explanatory postmortem findings, such as hemorrhage, tumor, or gross disease of any organ, served to separate Addison's anemia from other varieties.

By the beginning of this century it was recognized that the peripheral blood picture was characterized by the presence of large red cells well filled with hemoglobin. This feature of the blood was well known to Minot, as was the paucity of young red cells (reticulocytes) and frequently also of white corpuscles and platelets. Thus the findings in the peripheral blood, which were suggestive of diminished production of red cells, presented a puzzling contrast to the jaundice, increased iron deposits in liver and bone marrow, and large fecal excretion of bile pigments that were regarded as characteristic of increased red cell destruction. Indeed, in 1916 Minot and Sellards had demonstrated that, in pernicious anemia, an endogenous hemolytic process presumably interfered with the similar catabolism of experimentally injected hemoglobin. In 1922 Dr. George H. Whipple had suggested that the excessive bile pigment excretion might be a result of the breakdown of hemoglobin from other than the diminished number of circulating red cells in the anemic patient. Nevertheless, only with the modern advent of isotopic biological labels for hemoglobin during its formation has it been possible to demonstrate that the disturbed production of red cells in the bone marrow also involves excessive destruction of red cell precursors, including reticulocytes, in the marrow. Today all of these abnormalities are quickly corrected by injection of vitamin B_{12}: The jaundice and anemia promptly diminish, the sore tongue heals within days, and progression of the spinal cord lesion is arrested.

This conversion of an almost inevitably fatal disease into one that is now perhaps the easiest of all to manage, and with the fewest complications, was initiated as a result of combined insights derived from animal experimentation and careful clinical observation. The story of this therapeutic triumph begins in 1918 with the work of Dr. George H. Whipple, who was experimenting at the Hooper Foundation in San Francisco with the effect of various foods on hemoglobin production in anemic dogs. Later, as Professor of Pathology and Dean of the new University of Rochester School of Medicine and Dentistry, Whipple and his principal associate, Dr. Frieda Robscheit-Robbins, had evolved a reliable protocol for experiments with dogs whose hemoglobin concentration in the circulating blood was kept at about half the normal value by bleeding at regular and frequent intervals. By 1923 they had demonstrated that this chronic anemia provided a strong stimulus to hemoglobin regeneration by the animal's bone marrow to which, however, the marrow was unable to respond significantly unless supplements such as liver, pork muscle, or spinach were added to the basal diet. Of these, liver was the most potent. Fortunately, it was not until a decade later that it was realized that the available iron present in the dietary items tested had had the most significant influence on hemoglobin formation. This fact had been obscured by the variable assimilability of the food iron. Consequently, Whipple's early experiments left open the possibility that liver contained something novel that might benefit other types of anemia in man. In Minot and Murphy's hands this prospect was amply confirmed in 1926. It was not until 1932 that workers in Minot's laboratory showed in patients with iron deficiency anemia, often due to chronic blood loss as in Whipple's dogs, that when soluble iron was given parenterally it was quantitatively utilized in the production of new hemoglobin. Whipple confirmed this finding with intravenous injections in his experimental dogs, and thereafter he and his associates wrote yet another new chapter in experimental hematology by introducing the use of radioactive iron to the study of anemia.

Whipple's experimental observations were the immediate stimulus for attempts in the early twenties in more than one American clinic to treat pernicious anemia by dietary means. However, physicians familiar with pernicious anemia considered the modest improvement that sometimes followed to be no greater than what occurred in the so-called "spontaneous remissions" of the disease. Minot, however, had the advantage of a prior clinical suspicion that "something in food might be of advantage to patients with pernicious anemia." Ever since he had been a house officer at the Massachusetts General Hospital it had been his custom to cross-question such patients as to the details of their dietary history. In this way he came to believe that pernicious anemia patients often had lived on unbalanced diets for many years and sometimes developed a distaste for meat shortly before the onset of their fatal illness. To him the high fat content of the diet of some of these patients was consistent with the then-current notion of a blood-destroying effect, possibly the result of intestinal putrefaction. The idea of a relationship between good food and good health was a family tradition and instinctive with him; and the then-recent evidence from the work of Gibson and Howard that diets rich in protein and in iron could restore positive nitrogen and iron balances in pernicious anemia was at least consistent with his clinical impression of a prior nutritional defect. Finally, the quantitative dietary regulation required of him in the management of his own illness exemplified the rigorous program essential for a thorough therapeutic trial of dietary improvement in pernicious anemia.

At any rate, Minot's response to the suggestion provided by Whipple's work was to begin, during 1922, efforts to improve the diets of a few private patients with pernicious anemia. In some of these patients Minot thought that he detected clinical signs of improvement. This led to more detailed study of hospitalized patients, in which he and Dr. Murphy employed a regimen, "rich in iron and purine derivatives," containing 100-240 grams of liver, 120 grams of "muscle meat," leafy vegetables, "especially lettuce and spinach," fruit, and egg and milk. This was a large order, especially for patients with the characteristically poor appetite of the established disease, and it was by no means successfully achieved in their early efforts. Moreover, the dietary regimen was regarded skeptically by the few other physicians who knew about the work, and its application strained the patience of hard-working hospital dietitians and nurses. Yet by 1925, with the stout assistance of Dr. Murphy, the dose of lightly cooked liver was increased in some patients to an "optimal," though sometimes nauseating daily amount. The clinical improvement in these patients became regular and impressive, especially in one of them who really enjoyed eating liver and did so with enthusiasm.

Eventually, on May 4, 1926, Minot and Murphy reported their observations on forty-five patients to the Association of American Physicians, noting that the condition of all "became much better rather rapidly, soon after commencing the diet." Although admitting that these consistent improvements in health in their own series of cases "may not last longer than those of others," they felt it wise to urge pernicious anemia patients to take a diet of the sort described. The subsequent discussion, as recorded, suggests only limited appreciation of the momentous advance clearly defined by the description of the regular and striking improvement in this large group of patients. Indeed, the fact that Minot and Murphy made no report until this number had accumulated seems remarkably conservative. Perhaps this delay was in order to make the initial presentation of the successful control of a hitherto fatal malady to the most distinguished audience in American medicine.

The close correspondence between the initiation by their patients of the special diet and the beginning of the clinical improvement a few days later was clearly apparent to Minot and Murphy. In many of the later patients a more objective criterion of the prompt antianemic efficacy of the diet was provided by the daily enumeration of the newly formed reticulocytes that appeared within a few days in the patients' blood. That the significance of an orderly augmentation of these young forms as an index of enhanced red cell production by the bone marrow was well known to Minot as early as 1916 has already been mentioned. Moreover, in 1923 Minot and Sampson had refuted the claim that germanium dioxide was a useful remedy for anemia by showing that it lacked ability to cause reticulocyte responses in experimental animals or in man. In 1927 the significance of the reticulocyte reaction as a therapeutic index in the treatment of pernicious anemia was fully discussed by Minot in a presentation concerning the hematopoietic effects of the first liver extract.

Referred to by Minot and Murphy in their initial reports in 1926 merely as a "special diet" for the treatment of pernicious anemia, by 1927, as shown by the titles of other articles, liver had been recognized by them as the essential ingredient of the diet. In that year appeared the first of a series of articles based on collaborative work with Edwin J. Cohn and his associates under the title "The Nature of the Material in Liver Effective in Pernicious Anemia." This development had arisen from Minot's desire to provide a more convenient means of administering the active ingredient of liver in treatment and, if possible, to learn its exact nature. He discussed these objectives with Dr. Cohn, who was Professor of Physical Chemistry at the Harvard Medical School. It was agreed that Cohn would undertake chemical analysis of liver with respect to its therapeutic activity in pernicious anemia, guided by clinical responses to successive experimental fractions to be supplied by him for tests of efficacy by Minot in patients with untreated pernicious anemia. Fortunately, the promptness of the reticulocyte response made it possible to determine within ten days whether or not an experimental fraction of liver was therapeutically active. Indeed, the reticulocyte method sometimes permitted the study of a single anemic patient to yield information about the comparative activity of two or more liver preparations. This technique required daily counts of reticulocytes during successive and contiguous periods of the uniform daily administration of one after another of the fractions to be tested. Through arrangements with colleagues, patients were so studied at several of the hospitals affiliated with the Harvard Medical School. The discomforts of whole-liver feeding were soon eliminated when it was shown that the active principle was water-soluble and that the bulk of the liver proteins were inert. Thereafter the biochemical methods first employed for further liver fractionation resembled those used by others for concentrating what was then called "water-soluble vitamin B."

So began the protracted effort to identify the nature of the principle in liver that was active in pernicious anemia, an effort that engaged the attention of clinicians and scientists on both sides of the Atlantic for many years. Progress was slow because of the lack of any biological assay other than patients with untreated pernicious anemia. It accelerated only with the tardy discovery of a microbiological assay and culminated with the ap plication of partition and adsorption chromatography to water-soluble liver fractions. In 1948 the active principle, vitamin B_{12}, was isolated as cyanocobalamin at Merck & Co. in the United States by Karl Folkers and his associates and almost immediately thereafter by E. Lester Smith at Glaxo Laboratories in England.

Happily for the treatment of patients, Cohn and his associates as early as 1928 had reduced the daily requirement from 300 grams of liver to about 12.5 grams of a yellow powder, the so-called "fraction G," that possessed consistent activity in the treatment of pernicious anemia. In order that this experimental liver extract could be produced in quantity by a commercial process and submitted to clinical trial, a Committee on Pernicious Anemia of the Harvard Medical School was established. This committee, of which Cohn and Minot were members and Walter B. Cannon, Professor of Physiology, was chairman, undertook the responsibility of establishing the potency of successive lots of the commercial product before authorizing their release by the manufacturer to the medical profession. Through the scientific collaboration of its Research Director, Dr. G. H. A. Clowes, Eli Lilly and Company, which a few years before had successfully produced the first commercial preparation of insulin, was asked to undertake the manufacture of liver extract. Under the supervision of the committee, successive lots of this preparation were tested in Boston hospitals and supplied to fourteen medical centers in this country and one in Europe for therapeutic trials in pernicious anemia. The patent for the manufacturing process was dedicated to the public by the Lilly company.

Although Cohn, Minot, and their associates shortly produced, through further chemical fractionation, experimental extracts for intravenous trial, in which the activity of the original liver was increased more than three thousand times, the losses of activity in preparation were great. It remained for Gänsslen of Tubingen in 1930 to achieve a crude, nearly protein-free extract for parenteral use of which the amount derived from only 5 grams of fresh liver constituted the necessary daily dose for the treatment of pernicious anemia.

By 1936 the pharmaceutical industry was marketing relatively purified or concentrated extracts of liver for intramuscular injection. However, by that time it had become apparent that, owing to variations in the efficiency of manufacture, the therapeutic activity of these preparations did not necessarily correspond to the original amount of liver from which they were derived. Thus in 1936 the United States Pharmacopeia authorized the establishment of an "Anti-anemia Preparations Advisory Board," of which Minot was a member. The Board, with the voluntary cooperation of the pharmaceutical manufacturers, evaluated the results of the clinical assay procedures it prescribed. Each manufacturer arranged for appropriate clinical tests of his product and submitted the hematological data to the Board for evaluation. The amount of material that would produce specified reticulocyte rises and rates of red-cell increase, when given in uniform daily amounts to untreated patients with pernicious anemia, was defined by the Board as a USP Unit (oral or injectable) according to the route of administration of the product. This amount was stated on the label of each manufacturer's product. In this way the benefit of Minot and Murphy's discovery became reliably available to the medical profession long before the isolation of the active principle of liver, vitamin B_{12}, in 1948.

As a result of the latter accomplishment, it was soon discovered that the successful management of a patient with pernicious anemia required less than one millionth of a gram of vitamin B_{12} a day, conveniently supplied in practice by a monthly injection of 30 to 100 micrograms of cyanocobalamin. It is now understood that the need for parenteral injection of the vitamin arises from a primary failure of a specific gastric secretion essential for the assimilation of the small amounts of vitamin B_{12} in the normal diet. The vitamin is produced by microbial fermentation in the stomachs of ruminants whose forage contains cobalt and is absorbed and stored in the liver. The feeding of half a pound of beef liver a day was successful in treating pernicious anemia because the concentration of the vitamin in that animal organ was great enough to allow passive assimilation by patients lacking the special mechanism normally involved in the absorption of the low concentration of vitamin B_{12} in the usual diet.

In 1928, following the tragic early death of his friend and colleague, Francis Peabody, Minot, already internationally famous, was appointed Professor of Medicine at Harvard and succeeded Peabody as the second Director of the Thorndike Memorial Laboratory of the Boston City Hospital and Chief of the Fourth (Harvard) Medical Service there. The Thorndike Laboratory is better described as a metabolic ward for patients, together with upper floors for laboratories and animal quarters as well as offices for a small full-time staff. It was thus similar in its more general scientific purposes to the categorical objective of the Huntington Hospital. The Thorndike Laboratory, the first of its kind in a municipal hospital in this country, had been opened in 1923 and was a joint undertaking of the City of Boston and Harvard University that functioned brilliantly until 1973 when the hospital trustees dissolved the fifty-year affiliation. The Thorndike was part of the Harvard Medical Unit that provided medical care to hospital and clinic patients and offered research and educational opportunity for Harvard's undergraduate and postgraduate students as well as for the academic staff. In those days of limited opportunity for careful clinical investigation, this building, despite its relatively restricted bench space and equipment-laden corridors, was attractive to young physicians seriously interested in scientific research. Among Minot's younger associates in the early years were Herrman Blumgart, W. B. Castle, Maxwell Finland, Chester S. Keefer, Robert Nye, Joseph T. Wearn, and Soma Weiss.

With the production of experimental liver fractions well under way in Cohn's laboratory and their clinical evaluation in patients proceeding in the Harvard-affiliated Boston hospitals, Minot and his young pupils turned their attention to the study of the patients with so-called hypochromic anemia. In these patients, in contrast to those with pernicious anemia, the red corpuscles in the blood are pale and deficient in hemoglobin. Although it had been known since the Middle Ages that iron was therapeutically useful in an anemia of this sort affecting young women (known as "chlorosis"), the fact had become obscured during the nineteenth century by theoretical considerations concerning iron absorption. However, in 1932 Minot and Dr. Clark W. Heath showed that the administration of iron caused reticulocyte responses and improvement of hypochromic anemia in elderly female patients subsisting on limited diets. In many of these patients with "chronic chlorosis" the normal secretion of hydrochloric acid by the stomach was also greatly reduced or absent. This suggested a possible basis for the apparent failure of these patients to assimilate food iron, because iron salts were well-known to be insoluble in neutral or alkaline solutions. Consequently, Dr. Stacy R. Mettier and Minot, using serial reticulocyte responses as an index, showed that iron salts given by mouth in an acid-buffered medium produced a greater reticulocyte response than when given to the same patient in a neutral-buffered medium. Still, the variable effects on hemoglobin production when iron was given by mouth left uncertainty about its mode of therapeutic action. However in 1932 Heath and his associates showed that soluble iron, when injected in small daily amounts into patients with hypochromic anemia, reappeared almost quantitatively incorporated in the resulting increased amounts of hemoglobin in their blood.

Minot was interested in other clinical manifestations that he suspected to be the result of faulty diet as well as in nutritional deficiency anemias. In 1928 Dr. George C. Shattuck, a member of the Department of Tropical Medicine at the Harvard Medical School, had suggested that defective diet might be a factor common to both oriental beri-beri and to various forms of so-called "toxic" or "alcoholic" polyneuritis. Shattuck's clinical resident, Dr. Maurice B. Strauss, working in Minot's laboratory, had demonstrated the beneficial effect of improved nutrition in the so-called "toxic" neuritis of pregnancy. Minot recalled that his great grandfather, Dr. James Jackson, had written a classic description of alcoholic polyneuritis under the title, "On a Peculiar Disease Resulting from the Use of Ardent Spirits." Minot, Strauss, and Stanley Cobb, noting the defective diet and the digestive disturbances of patients with chronic alcoholism, together with the generally beneficial effect of better food, concluded that dietary deficiency, possibly of vitamin B_1, played an important role in the production of these patients' neurological disturbances. Two years later, in the Thorndike Ward, Strauss confirmed this supposition under controlled nutritional conditions by showing that a well-balanced diet, supplemented with components of the vitamin B complex, resulted in improvement of the neuritis of patients "allowed to continue their customary daily intake of spirituous liquor." This was certainly clinical investigation with the informed and happy consent of the patients.

As already mentioned, Minot as a young man had studied blood coagulation under Professor Howell in the Physiological Laboratory of the Johns Hopkins University School of Medicine. At the Thorndike he continued his interest in clinical disorders of platelets with the publication, in 1936, of an article describing varieties of purpura with low levels of platelets in the blood associated with lymphocytosis or occurring intermittently with menstruation. More significant was his interest in hemophilia, regenerated through the work of some of his young colleagues. In 1936, encouraged by Minot, Drs. Arthur J. Patek, Jr., and Richard P. Stetson undertook to analyze the effect of transfusion of normal citrated blood in promptly shortening the coagulation time of the blood in hemophilia. It was recalled that substantial *in vitro* evidence for regarding this phenomenon as a response to the replacement of defective plasma rather than of defective platelets had been presented by Thomas Addis in 1911. In the course of a series of studies in which F. H. L. Taylor, the biochemist of the Thorndike, played a prominent part, Minot and his associates prepared a so-called "globulin substance" from citrated cell-free normal plasma and showed it to have the effect of the parent platelet-free plasma in shortening the coagulation time of hemophilic blood both *in vitro* and when given intravenously. Globulin fractions of normal plasma devoid of prothrombin and of fibrinogen were equally effective. The corresponding fractions of hemophilic blood had little or no activity. Much of this work was carried out with the willing and intelligent cooperation of a single hemophilic patient, Russell White, who was in residence for many years on the Thorndike Ward. Thus was discovered and initially defined the component of normal plasma known today as antihemophilic globulin or factor VIII.

The development of plasma fractionation during World War II by Cohn and his associates provided opportunity for testing the fractions' antihemophilic activity in Minot's laboratory. Cohn's fraction I was found to contain most of that activity and to be suitable for clinical use by intravenous injection in the hemophilic patient. Unfortunately, at the time the potential therapeutic value of these fractions was found to be limited in practice by the expense of commercial preparation and because some patients developed circulating antibodies against the active principle. More recently the development of cryopre cipitates and other concentrates of factor VIII has become of lifesaving value in the management of hemophilic bleeding.

In 1930 the Second Medical Service of the City Hospital had come under Minot's direction to complete the formation of the Harvard Medical Unit. He soon found this added responsibility to be too taxing a burden, and in 1932 he relinquished the conduct of the two clinical services to his trusted and brilliant younger colleague, Dr. Soma Weiss. Nevertheless, as the years passed Minot became ever more deeply involved in the administrative duties of the Thorndike Laboratory and in the responsibilities of a senior professor in the medical school. He was often busy on the home telephone for long periods in the evenings. However, somehow he found time for stimulation and encouragement of his pupils, often implemented by handwritten notes on scraps of paper referring to recent articles of special relevance to their research work. Or in discussion with them he would cross each of four fingers of his left hand over the adjacent digit as a reminder of individual points to be made as each was subsequently released.

In these and other ways Minot fostered the development of a healthy degree of autonomy in members of his department who were carrying out investigations in various clinical fields. He also carried on an extensive correspondence with other professors and with physicians seeking advice about patients. Blood films on glass slides often accompanied such inquiries. The characteristic care with which he composed his replies added to the burden of these "paper consultations." Alone, or with his associates, Minot contributed chapters on blood diseases to leading textbooks of medicine through several revisions. In 1936, with W. B. Castle, he published his only book, a pioneering description of the pathophysiology of the anemias reprinted from a chapter in *The Oxford Medicine.* Minot also maintained a small consultation practice that involved seeing patients one or two afternoons a week at the office of the Beacon Street group. Some of these patients, as well as many of those on the medical wards of the City Hospital, had chronic conditions to which anxiety, fatigue, economic deprivation, and undesirable living conditions, as well as dietary abnormalities, at least contributed. He saw that correction of these defects could be beneficial and, in teaching and published articles, advocated proper attention by physicians to what he called "social medicine."

Despite the best of medical care, Minot developed in his middle fifties some of the vascular and neurological complications of diabetes. Informal conferences with close associates in his hospital office often took place while he was changing his socks and warming his feet dangerously close to an electric sun bowl near his desk. In 1942 he experienced transient numbness and tingling of his left arm and leg, and in April of 1947 he had a stroke that paralyzed his left side and resulted in a wheelchair existence for the rest of his life. His devoted and competent wife now became more than ever a source of encouragement as well as a perceptive judge of his needs and capacities.

With the physical assistance of the family chauffeur, Minot made occasional trips to the hospital or to visit friends. In 1948, he resigned from the directorship of the Thorndike Laboratory, but continued his kindly interest in the activities of its staff and in the progress of their research. As always, although he tired readily, he was ready to discuss medical matters with old friends and associates in the quiet of the book-lined study of his Brookline home. With the help of his young son he was able to renew stamp collecting, one of the hobbies of his early years. Despite an inexorable decline in health during the last weeks of his life, he carried on bravely to the limits of his ability and died quietly on February 25, 1950, at the age of sixty-five.

Honors and awards began early to come to Minot because of his great contribution to medicine and human welfare. In

1928, only sixteen years after graduation from the Harvard Medical School, he was given the honorary degree of S.D. by his alma mater. In the same year the University of Toronto awarded him the Charles Mickle Fellowship, and the Association of American Physicians, of which he was later elected president, gave him its Kober Medal. In 1937 he was elected to the National Academy of Sciences.

Three years earlier Minot had shared the Nobel Prize in Physiology or Medicine with Whipple and Murphy. This well-deserved joint award appeared at the time to recognize judiciously the experimental demonstration of a novel biological principle involved in normal blood formation and its dramatic application to a fatal human disease. In retrospect the work of these men, though, as discussed above, differently interpreted today, seems even more noteworthy. The journey to Stockholm, with lecture appearances en route in England, Holland, and Denmark, no less than the impressive ceremonies and banquets attendant upon the Nobel award, taxed the control of Minot's diabetes and his physical endurance. However, owing to his characteristic attention to detail, the advance preparations for the journey were complete and all went well, despite a great Atlantic storm on the return voyage. In addition the reassuring presence of medical expertise, if needed, was provided by Dr. Richard Stetson, a young colleague and friend, who with his wife accompanied the Minot's and their two adolescent daughters.

Among foreign honors that came to Minot were honorary fellowships of the Royal College of Physicians, Edinburgh; the Royal College of Physicians, London; the Royal Society of Medicine, London; and a corresponding membership in the Royal Academy of Medicine, Belgium. He was awarded the Cameron Prize of the University of Edinburgh and the Moxon Medal of the Royal College of Physicians of London. He also received various awards in the United States. The admiration and affection of his Harvard colleagues was expressed at a sixtieth birthday dinner in his honor held in Boston at which he was officially presented with the Distinguished Service Medal of the American Medical Association. Some of his associates conceived the idea of a *Festschrift.* A worldwide committee was formed, and eighty-four distinguished contributors to hematology wrote articles that were published together in 1949 as the *George R. Minot Symposium on Hematology.*

Minot's scientific contributions to medicine were inspired by a persistent curiosity about many things, important or even trivial, in the world about him. He was in essence a naturalist whose interests included flowers, insects, and every organic aspect of his patients as well as their emotional and social problems. His scientific work was implemented by his genius for taking infinite pains, and its relevance to clinical medicine changed the study of diseases of the blood from a largely descriptive to a dynamic subject that ever since has attracted productive basic and clinical investigation. In the area of nutritional anemias alone, the work of Minot and his pupils provided insight into matters not previously suspected to exist. Studies inspired by their work led eventually to the isolation of two new vitamins—folic acid and vitamin B_{12}—of fundamental importance to cellular proliferation and metabolism. Knowledge of the structure of folic acid (a B vitamin) permitted chemists to synthesize growth antagonists and inhibitors of the leukemic process. The discovery of cobalt in the vitamin B_{12} molecule explained its importance as a trace element in animal nutrition. Analysis of the relation of the secretory failure of the stomach in pernicious anemia to vitamin B_{12} deficiency has disclosed the specialized enteric mechanisms normally involved in the assimilation of the vitamin. The primary gastritis bids fair to be explained as an instance of a delayed cellular immune response of lymphocytes and macrophages occurring in genetically susceptible persons.

Although Minot is known to the world as the discoverer of the liver treatment of pernicious anemia and to clinical investigators for other discoveries as well, perhaps in the long run his greatest contribution to American medicine was through his personal influence over two decades as Director of the Thorndike Laboratory. During this time, guided by Minot and his small group of younger physician-scientists, there was a turnover of about a dozen young doctors a year, recruited as research fellows from all parts of the United States. Many of them had just completed training as residents on the clinical services of the Harvard Medical Unit. A few, usually more mature, came from across the Atlantic. All hoped to learn at first hand something of the scientific study of disease in patients and in the process, as often happened, to make original contributions themselves. After one, two, or three years with few exceptions they left the Thorndike in many instances to take up positions on other academic medical ladders. In 1948, at the time of Minot's resignation, more than four hundred young doctors had served in the Harvard Medical Unit, either as resident physicians on its medical services or as research fellows in the Thorndike. In 1956 almost fifty of these held professorships in medicine, pediatrics, preventive medicine, or in a preclinical department of medical schools in the United States. In addition, sixteen foreign physicians who had worked at the Thorndike occupied distinguished posts abroad. Thus was George Minot's great influence on academic medicine extended and multiplied through the men and women whose early medical careers he had moulded.

This account of George Minot owes much to the book about his life and times written by his cousin and medical classmate, Dr. Francis M. Rackemann.* Following Minot's death the minute of the Harvard Faculty of Medicine described him as:

* *The Inquisitive Physician,* Harvard University Press, Cambridge, Massachusetts, 1956, 288 pp.

"Generically . . . a Yankee of the Yankees, specifically . . . a proper Bostonian, . . . culturally an aristocrat, he was in behavior a democrat." His obituary in the *Transactions of the Association of American Physicians* for 1950 reads in part as follows: "Doctor Minot was a character in the best New England sense of that expression. His personality, like his hat, bore the impress of one who goes his accustomed way without concern that the way is different. Despite his achievements he was inherently modest and entirely democratic toward those who were sincere. He was an indefatigable correspondent and author of notes and memoranda to friends and colleagues. He loved to talk and a scheduled five-minute conference with him could easily extend to an hour of interesting conversational excursions. He had hobbies in which he took serious delight in every detail. Indoors he was an avid stamp collector. Outdoors he paid careful attention to the habits of birds and animals; and was proud of his flower garden in which he grew irises of prizewinning quality. Like many New Englanders, the sea was in his blood and as a summer sailor he was familiar with much of the rocky coast of Maine as well as with the warm waters of Buzzards Bay.

"George Minot's life was closely interwoven with his family and with his home. Family friends and foreign visitors alike found his a hospitable household. For serious discussions there was the study with its cheerful wood fire in winter and glimpses of the garden through the door in summer. The miracle of insulin saved his life, but without the understanding and constant devotion of his charming wife, Marian Weld Minot, his great work could never have been accomplished. She and their children Marian, Elizabeth, and Charles survive him."

DEGREES, APPOINTMENTS, AND HONORS

Degrees

1908 A.B., *cum laude,* Harvard University
1912 M.D., *cum laude,* Harvard University
1928 S.D., honorary, Harvard University

Hospital and University Appointments

1912-1913 Medical House Pupil, Massachusetts General Hospital
1913-1914 Assistant Resident Physician, Johns Hopkins Hospital
1914-1915 Assistant in Medicine and Research Fellow, Physiology Laboratory, Johns Hopkins Medical School
1915-1916 Assistant in Chemistry, Harvard University
1915-1918 Assistant in Medicine, Massachusetts General Hospital
Assistant in Medicine, Harvard Medical School
1916-1918 Visiting Physician, St. Luke's Convalescent Home
1917-1919 Assistant Consulting Physician, Collis P. Huntington Memorial Hospital
1918-1923 Associate in Medicine, Massachusetts General Hospital
1918-1927 Assistant Professor of Medicine, Harvard Medical School
1919-1923 Physician, Collis P. Huntington Memorial Hospital
1922-1924 Consulting Physician, Massachusetts Charitable Eye and Ear Infirmary
1923-1928 Chief of Medical Service, Collis P. Huntington Memorial Hospital
Physician to Special Clinic, Massachusetts General Hospital
1925-1927 Special Consultant in Diseases of the Blood, Massachusetts General Hospital
1925-1928 Associate in Medicine, Peter Bent Brigham Hospital
1927-1928 Clinical Professor of Medicine, Harvard Medical School
1927-1950 Member Board of Consultation, Massachusetts General Hospital
1928-1930 Chief, Fourth Medical Service, Boston City Hospital
1928-1948 Director, Thorndike Memorial Laboratory, Boston City Hospital
Visiting Physician, Boston City Hospital
1928-1950 Professor of Medicine, Harvard Medical School
Consulting Physician, Peter Bent Brigham Hospital

1929-1950 Consulting Physician, Beth Israel Hospital

1930-1932 Director, Second and Fourth Medical Services, Boston City Hospital

1943-1950 Consultant in Hematology, Palmer Memorial Hospital, N.E. Deaconess Hospital

1947 President, Senior Staff, Boston City Hospital

Member

1911

Alpha Omega Alpha

1917 American Society for Clinical Investigation

1921 Association of American Physicians

Council (1931)

President (1938)

1923 American Clinical and Climatological Association President (1933)

1927 American Academy of Arts and Sciences 1937 National Academy of Sciences

1946 Advisory Council of Physicians Forum

Honorary Member

1927-1928

The Harvey Society

1929 Phi Beta Kappa

1931-1939 Corresponding Member, Royal Academy of Medicine (Belgium)

1935 Kaiserlich Leopoldin-Carolinische Deutsche Akademie der Naturforscher (Halle)

1936 Society of Biological Chemists (India)

1938 Finnish Society of Internal Medicine (Helsingfors)

1939 Honorary Member, Royal Academy of Medicine (Belgium)

1945 Academy of Medicine of France

Fellow

1912 American Medical Association

1928 American College of Physicians

1935 American Philosophical Society *Honorary Fellow*

1931 Royal College of Physicians, Edinburgh

1932 Royal Society of Medicine, London

1933 New York Academy of Medicine Institute of Medicine of Chicago

1938

Royal College of Physicians, London Vice President étranger, Société Française d'Hematologie

1945 Medical Association of Finland

1947 College of Physicians of Philadelphia

Awards

1928 Charles Mickle Fellowship, University of Toronto

1929 Kober Medal, Association of American Physicians

1930

Cameron Prize, University of Edinburgh (with William P. Murphy) Gold Medal, National Institute of Social Sciences Gold Medal and Award, Popular Science Monthly (with George H. Whipple)

1933 Moxon Medal, Royal College of Physicians, London John Scott Medal of City of Philadelphia

1934 Nobel Prize in Physiology or Medicine, jointly with William Parry Murphy and George Hoyt Whipple, for "discoveries respecting liver therapy in anaemias"

1935 Gold Medal of Humane Society of Massachusetts

1936 Scroll Award of Associated Grocery Manufacturers of America

1939 Gordon Wilson Lecturer and Medalist, American Clinical and Climatological Association

1945 Distinguished Service Medal, American Medical Association

1949 *George R. Minot Symposium on Hematology*

BIBLIOGRAPHY

KEY TO ABBREVIATIONS

Am. J. Med. Sci. = American Journal of the Medical Sciences
Am. J. Physiol. = American Journal of Physiology
Ann. Internal Med. = Annals of Internal Medicine
Arch. Internal Med. = Archives of Internal Medicine
Boston Med. Surg. J. = Boston Medical and Surgical Journal
Brit. Med. J. = British Medical Journal
Bull. Am. Assoc. Med. Social Workers = Bulletin of the American Association of Medical Social Works
Entomol. News = Entomological News
Harvard Med. Alumni Bull. = Harvard Medical Alumni Bulletin
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Biol. Chem. = Journal of Biological Chemistry
J. Clin. Invest. = Journal of Clinical Investigation
J. Exp. Med. = Journal of Experimental Medicine
J. Ind. Hyg. = Journal of Industrial Hygiene
J. Med. Res. = Journal of Medical Research
Med. Clin. N. Am. = Medical Clinics of North America
New Engl. J. Med. = New England Journal of Medicine
Trans. Am. Clin. Climat. Assoc. = Transactions of the American Clinical and Climatological Association
Trans. Assoc. Am. Physicians = Transactions of the Association of American Physicians

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Carl Richard Moore

December 5, 1892-October 16, 1955

by Dorothy Price

When the distinguished endocrinologist Carl R. Moore died, a document of great importance to a biographer lay in his desk. It was headed "Biographical data prepared for files of National Academy of Sciences," and it was dated August 28, 1948, seven years before his death. Oddly enough, I remember that day and that occasion. I had gone to his office at the end of a particularly hot, muggy, Chicago day and had found him pecking away at an old typewriter in his inimitable fashion with one finger of each hand. When he said that he was writing biographical data for the National Academy of Sciences, I may have been somewhat surprised because he had been a member of the Academy since 1944. In any case, my memory caught and recorded the incident. I was to see the contents of those pages after his death and to recall him vividly as I knew him, the professor under whom I wrote my doctoral thesis in the Department of Zoology of the University of Chicago, the scientist with whom I served as a close collaborator for many years in studies on the physiology of reproduction, the chairman of a department in which I later became his colleague.

What he wrote in 1948 is remarkable in that he painted, quite unknowingly, an extraordinarily revealing picture of his life and scientific career as he saw them. Viewed through his eyes, his career was the proverbial one of the farm boy of limited circumstances who makes good mainly by dint of his own hard work and ambition. It is a typically American story, and in an ingenuous and engaging way he showed his satisfaction and pride that this was *his* story. He had indeed "made good" and could look with understandable pride at his position and accomplishments, his honors and awards.

A biographer who had never known Carl Moore might have been puzzled to find that more than one third of the six pages in his biographical notes of 1948 were devoted to a nostalgic recounting of his experiences on the farm in the Ozark region of Missouri where he was born and to details of his life and early schooling in Springfield. To me, this came as no great surprise. He had often talked at length about his belovèd Ozark country background, and the subject was always close to the surface of his mind. But when he wrote these biographical data his thoughts were certainly resting on a past that was illuminated for him by a rosy light. He had recently been granted an honorary degree from Drury College in Springfield, where he had received his B.S. and M.S. degrees. A visit to the scenes of his early years and a reunion with his family had undoubtedly revived old memories. But an additional reason for his preoccupation with his early years seems probable. He had sometimes spoken to me about returning to his Ozark country when he retired, perhaps to a farm near Springfield. In August of 1948, at the age of fifty-five years, he mentioned in his biographical notes that he had nine years before official retirement. He was apparently looking forward as well as backward, and a plan to retire to an ideal country spot near Springfield and its Drury College may well have crystallized in his mind. His life might then have ended where it began, but death anticipated him, and he did not live to retirement.

Carl Moore was born on December 5, 1892, on a farm in Green County, Missouri, twelve miles from the city of Springfield. His father, whose family was originally of Scottish an cestry, had been taken from Tennessee to southwest Missouri at the age of ten in a covered wagon drawn by oxen; Moore's mother, whose distant ancestors were English, was born in Missouri just before the Civil War. The farm on which Carl was born was cleared land cut from surrounding forest. Here life was simple, frugal, religious, and relatively primitive, and the boy learned to work hard and to do all the usual farm chores. He also learned to hunt and fish, and fishing remained a favorite outdoor sport and means of relaxation all his life. When he began school, he went to a one-room country school in which the teacher was one of his older sisters.

A new period began for him when he reached the age of nine years and his family removed to Springfield, a town of 20,000 people at that time. In this much less restricted environment, he went through elementary school and high school and entered Drury College. His family had discussed whether he should be "a preacher or a doctor," but the matter must have been settled by the time he registered in college as a premedical student. His tuition was paid largely from money he earned by doing all sorts of odd jobs, such as janitor service, window-washing, and delivering papers. But it was not all work. He still had time and abundant energy for tennis, horseback riding, hunting, and fishing.

At Drury College, Carl Moore found the teacher who unquestionably shaped the pattern of his future life in scientific research and teaching. Moore should speak for himself on this point just as he did in his biographical data:

"In college, biology in addition to being a rather natural interest from earlier farm experiences, became a favorite subject largely because of the commanding personality of the teacher, Charles Haddon Spurgeon; he was a self-made, jolly, fat man, of large physical stature who inspired youngsters by providing opportunity for work outside the regular curriculum. Being considerable of a critic, his encouragement and com mendations were vitalizing. Preparation of slides for histology, serial sectioning of embryos, and many additional activities in the laboratory occupied many all night sessions as well as those on Saturdays and Sundays."

When Moore spoke of Spurgeon's "power over youngsters" he knew whereof he spoke. Spurgeon's example was firmly imprinted on him as a young, eager student. He, too, became just such an inspiring and enthusiastic teacher, and he, too, gave undergraduate students an opportunity to work on projects outside the regular curriculum (often in his own research, with joint publication as an additional bonus). As for dedication to night and weekend work in the laboratory, Moore was a convert, and so were those of us who worked with him. Indeed, it was often necessary for our experiments, but it became a habit and, ultimately, almost a compulsion. It stayed with him in the last years of his life during his failing health, and I would find him in his office on Sunday mornings when he had hardly strength enough to lift his packed briefcase.

At Drury College, extracurricular laboratory work was a joy to the young Carl Moore. It held opportunities for adventure and exploration and a chance to solve the problems he met by some method of his own devising, be it orthodox or unorthodox. I can supplement his own biographical notes with a significant anecdote. When he had difficulty in obtaining good serial sections of embryos because the paraffin blocks crushed, he met his problem in a direct fashion by opening all the windows to let in cold air, putting on overcoat and muffler, and cranking the microtome around to cut again (with better success). There were much simpler and less heroic ways to obtain perfect serial sections, but he did not know them. It is doubtful that Spurgeon knew much about the matter either, but he seems to have given his students free rein to use their own initiative with whatever simple equipment was available and come up with the best results they could.

Moore learned these lessons well. In his own direction of the research of his graduate students, he allowed them free rein for months at a time to let them "find their own feet" (unless they came to him for help and advice). Some floundered hopelessly and dropped out; some completed their research project in a pedestrian way on well-worn paths; but some learned the priceless benefit of being free to develop independence, initiative, and imagination as Moore had learned earlier. They learned, too, to use relatively simple equipment with only the minimum of refined instruments. Money was not wasted on new and showy gadgets—good research did not depend upon such things. Moore was wont to conduct our distinguished guests through our research laboratories, operating rooms, and animal quarters and report to me afterward that he liked to show them what research could come out of a "setup" like ours. And he was right in large measure. An ever-increasing volume of good research *was* done under relatively primitive conditions; he did not ask for more. In later years, of course, he recognized the obvious advantages of modernization and air conditioning.

We may be sure that there were no special refinements in the Biology Department of Drury College when Moore was studying biology under Spurgeon, learning embryology from Lillie's *Development of the Chick,* and enthusiastically carrying on extracurricular projects. He obtained his B.S. degree in the spring of 1913. His family had no money to send him to medical school, but another opportunity opened. He was offered a position at Drury as an assistant in biology. The position carried a munificent salary of $100 for the year, and he could work under Spurgeon for an M.S. degree (incidentally, one of the very few ever granted by that institution, as Moore states in 1948). Moore snapped at the chance. But before he began his fifth and final year at Drury, he came to the University of Chicago and registered for summer quarter courses in the

Department of Zoology. Lillie's textbook had caught his interest and piqued his curiosity about the department where the famous embryologist was chairman. Of course, Lillie was not there—he spent every summer at the Marine Biological Laboratory at Woods Hole—but there was much to see and much to learn at the University of Chicago, and it was Moore's first trip away from home.

To obtain his M.S. degree at Drury, which he did in June of 1914, Moore assisted in courses and made what he later termed "an attempt at research" on the origin of the vena cava in bat embryos. He prepared slides of serial sections of embryos, projected the sections on melted beeswax, and by cutting out the projected sections and stacking them he produced models. He had never seen a wax model, but he made some in his own way (undoubtedly with the window open). This research problem might not seem the most interesting one for a young student. For him, it was an exciting foretaste of biological research, and he was thenceforth lost to medicine.

He had applied to some universities for support for graduate study and received offers of fellowships from three. The one he accepted was from the University of Chicago, and in choosing this fellowship he made one of the most important decisions of his life. He would have made a name for himself wherever he had gone, but it is open to doubt whether he would have advanced as rapidly and his name have loomed so large if he had gone elsewhere. Moore came to work with the right man—Frank R. Lillie—at just the right time in the development of Lillie's research program. The Department of Zoology was an ideal environment in which Carl Moore could mature; when he received his Ph.D. degree he was to step into that department as a member just before the beginning of the 1920's, a decade of great and brilliant advances in endocrinology. And Moore was to be in the middle of it all.

However, Moore could not gaze into the crystal ball. When he went to Woods Hole for the summer session in 1914 and met Frank R. Lillie for the first time, the young man fresh from the Ozark hills was in a state of "uncertainty and trepidation" as he described it. Others, too, had found the dignified, reticent, soft-spoken embryologist overawing. But this first meeting for Carl Moore was a successful one, and he was assigned a doctoral problem in Lillie's large program of studies on the fertilization of the eggs of marine invertebrates.

Lillie became his mentor, and Moore began to learn eagerly the elements of sophisticated scientific research and criticism from one of the best possible teachers. Moore always remained, in a sense, Lillie's protégé, and Moore repaid him with profound admiration, respect, and affection. Fortunately, the young student did not try to emulate too closely the middle-aged man of great distinction who patiently directed him. That would have been disastrous; their backgrounds and personalities were very different. But when Moore later gave the seminars for which Lillie had been famous, "Biology of Sex" and "Physiology of Reproduction," he followed almost exactly not only the lucid organization but much of the subject matter of Lillie's brilliant introductions.

Moore completed his doctoral thesis on fertilization and parthenogenesis in the eggs of a sea urchin in record time and received his Ph.D. degree from the University of Chicago in 1916. He was immediately appointed an associate in the Department of Zoology for the period from 1916 to 1918. He spent half the time in teaching an embryology course designed primarily for premedical students and the remainder in research. In 1918 he became instructor, and in the ensuing years he advanced rapidly, reaching a full professorship in 1928 and the chairmanship of the department in 1934.

In the period from 1919 to 1920 an event of great importance in Carl Moore's life and in his scientific career occurred. A student named Edith Naomi Abernethy caught his attention in a laboratory section he was teaching. Soon his interest was more than academic, and they were married in July of 1920. He acquired not only an attractive and charming wife, but a hostess who presided with grace and competence on the many occasions when they entertained students and scientific visitors from many places in America (in the broad sense) and abroad. She understood his need to consider his laboratory also a "home," and she shared his love of nature and the outdoors. Their summer home in Michigan was a haven for him and a belovèd spot for her. Their honeymoon was spent on a float trip on a river in the Ozarks (where else could it have been?). Two of their three children survived—Harris Mason and Ellen Abernethy.

After publishing his thesis, Moore completed a second paper on the sea urchin, *Arbacia,* and then turned abruptly to an entirely different line of research in which he was occupied for the rest of his scientific career. The reason was clear, but it requires explanation. Lillie published in 1916 the first of his classic papers on the freemartin, a bovine intersex that resulted from cases of heterosexual twinning when there were anastomoses of blood vessels in the fused fetal membranes. The type of intersexuality and the sterility usually found in freemartins posed intriguing problems. Lillie's observations and his masterful analysis resulted in a theory to explain freemartinism on the basis of masculinization of the female by male hormone produced in the testes of the male co-twin and crossed to the female through the vascular anastomoses. The ovaries were inhibited (antagonized), and the duct systems and glands were masculinized (stimulated). He then proposed that normal sex differentiation in the mammalian fetus might be controlled by bloodborne substances, hormones, secreted by fetal testes and ovaries. However, he cautioned that the theory was only tentative. Nothing was known about the possible effects of female hormone on the male fetus. Fetal gonadectomy—all-important for an under standing of normal sex differentiation—had never been accomplished and should be done as a critical test of the theory.

Lillie made "a mild suggestion" (in Moore's words) that Moore try to produce freemartins by experimental means. A mild suggestion was all that was required, and Moore plunged into the problem, or rather into the problems, for his research did not follow any straight path. Thirty-eight years and some one hundred publications later, he still had not produced freemartinism experimentally (nor, indeed, had anyone else). But by then he had developed a theory of his own to explain normal sex differentiation. This proposed that sex hormones from fetal gonads were not controlling normal sex differentiation. The evidence that he presented negated, in his opinion, Lillie's hormonal theory for freemartinism and its extension to normal sex differentiation. However, before Moore was led to this conclusion he made many outstanding contributions in the physiology of reproduction even as he digressed from his original purpose.

Moore began his attempts to subject fetuses to male hormone and produce experimental freemartinism by transplanting testicular tissue onto the membranes of rat and guinea pig fetuses. This failed dismally. Then he transplanted testes into young females in the hope of obtaining freemartins in the litters when these females were ultimately bred. This, too, failed, but he was far from discouraged. He had succeeded in obtaining well-developed testicular grafts in young females. The field of gonad transplantation with its postulates of sex gland antagonism lay open before him.

The Viennese scientist E. Steinach had first reported in 1910 that young spayed female rats and guinea pigs were masculinized when given grafts of testes, and young castrated males were feminized by grafts of ovaries but the hosts *must* be gonadectomized prior to receiving grafts. He therefore pro posed and strongly defended, then and later, a concept of sex gland antagonism involving a direct antagonism between testis and ovary.

The question of whether sex gland antagonism really existed was an important one. The validity of such a concept might have a direct bearing on Lillie's theory of freemartinism. Moore promptly sought confirmation or refutation of the concept and soon completely disproved it. He was successful in maintaining testis grafts in young females possessing ovaries and ovarian grafts in young males with testes. In these early experiments of Moore's there was a fortuitous circumstance that undoubtedly favored unusually successful "takes" of the grafts. He routinely exchanged one gonad each between young males and females at the time of grafting, apparently for efficient utilization of animals. Thus the grafts were placed in hosts that had just been hemispayed or hemicastrated. About ten years later, he and I were to propose a theory of balanced control between gonadal hormones and pituitary gonadotropins. The experimental design that he used in 1919 had a sound rationale of which he never dreamed at the time. The grafts that he obtained differed so materially from those described by Steinach that he was led into his next research problems.

Moore's grafts of testes had far better-developed tubules and fewer interstitial cells between them than those of Steinach. The two investigators agreed that spermatozoa were not present in the grafts. Contrary to Steinach, Moore found no evidence for increased hormone secretion and no basis for a contention that testis grafts might effect rejuvenation in senile animals and men.

This *fata morgana* of warding off all changes of aging in man, or rejuvenating the senile, had appeared again. Testis-secreted hormone was the miraculous cure-all. In France, the Russian-French surgeon S. Voronoff was rejuvenating senes cent rams by testis grafts. Soon, he reported fantastic results of all kinds in rejuvenation of senescent men with grafts of young human testes and those from monkeys. Voronoff's claims were astounding—and fallacious. Other doctors transplanted testes from sheep, goats, or chimpanzees into aging men, also with reputed success. The craze spread from Europe to the United States. Steinach proposed a new and simpler method in 1920, when he reported rejuvenation of "senile" male rats by vasectomy. He claimed that tying off the excurrent ducts of the testes or removing a segment caused (as in testis grafts) degeneration of tubules and germinal epithelium, compensatory hypertrophy of interstitial cells, and increased male hormone with its rejuvenating powers. A wave of "Steinach operations" spread. Hundreds were done in the United States alone.

In this frenetic period, now all but forgotten, Moore entered the field. He first studied the relationship between degeneration of the germinal epithelium of the testis and the condition of the interstitial cells. Cryptorchidism, the failure of normal descent of the testes into the scrotal sac, was known to be associated with degenerate testis tubules and sterility in man and in other mammals that possess a scrotum. Interstitial cells were known to be increased. Moore and his students produced experimental cryptorchidism in rats and guinea pigs and studied the changes that took place in testes in the abdominal environment and the repair that occurred when such testes were returned to the scrotum.

In Moore's experiments, testicular interstitial tissue was apparently increased, but male hormone production was not increased. More importantly, the germinal epithelium could not remain active nor complete spermatogenesis except in the scrotal environment. Moore's research answered at last the question of the function of the scrotum. He proved conclusively that it acts as a thermoregulator that maintains the testis at a temperature several degrees lower than that of the abdomen. Only at the cooler temperature can the temperature-sensitive germinal epithelium produce spermatozoa.

This research did far more than answer the academic question of scrotal function; it suggested a method of treatment for the problem of cryptorchidism in man. The medical world listened and remembered. In 1950, the American Urological Association presented him with an award for research on the human male reproductive tract. The citation read, in part: "Dr. Moore is best known to urologists for his researches which elucidated the thermoregulatory function of the scrotum. . . . His experiments were elegant, imaginative and conclusive; they provided a rational basis for the performance of orchidopexy."

While Moore's experiments on cryptorchidism were still in progress, he began studies on vasoligation. With the assistance of his students, he put Steinach's reported effects of vasectomy to critical tests. Again, he disproved Steinach's contentions. Vasectomy, as performed by Moore and his students on five species of laboratory mammals, did not cause general degeneration of germinal epithelium nor increase in interstitial cells (nor, indeed, did it increase male hormone production). Any claims of rejuvenation by means of vasectomy were, therefore, baseless, and the famous "Steinach operation" was worse than useless.

At that time, Moore's scientific career spanned only ten years, but he had made several major contributions. He had disproved the concept of sex gland antagonism and the validity of the "Steinach operation" as a means of rejuvenation; he had discredited the notion of rejuvenation by testis grafts; last but not least, he had proved that the scrotum is a thermoregulator for the testis. These were no mean accomplishments. Moore had already established himself as an authority on several aspects of the biology of the testis and scrotum. His basic findings were of great importance in an understanding of the physiology of reproduction. In addition, his findings had practical application to medical problems. He was already receiving invitations to address medical meetings and to submit reviews to medical journals and books. This continued all his life; his last publication was in a textbook on urology. He could say what medical practitioners needed to hear in a way they could understand, appreciate, and even enjoy. He was a forceful writer and lecturer and a good showman in the best sense of the word.

In 1927, an extraordinarily fortunate circumstance gave Moore the opportunity to advance his research by new methods. L. C. McGee, a graduate student in F. C. Koch's Department of Physiological Chemistry and Pharmacology in the University of Chicago had just succeeded in obtaining lipid extracts of bull testes which were definitely effective in growth-stimulation of the capon comb and thus contained male hormone. This was a breakthrough, the first major step in the study of testis hormones.

Purification of these extracts depended upon the development of sensitive bioindicator tests for male hormone, and mammalian indicators were particularly desirable. Lillie and Koch arranged an interdepartmental project, and an active research program began. Moore and his team of students and assistants rapidly worked out a number of useful bioindicator methods for testing the male hormone potency of the extracts. Chief among these was the prevention of retrogressive changes in accessory reproductive glands of the rat. Many of the changes that occur in mammalian accessory glands following castration had been known for a long time. However, the findings of Moore and his collaborators gave the first detailed description of the histological, cytological, and secretory changes that resulted from male hormone deprivation and the precise process and rate of restoration of normal structure and function following the administration of testis extract. The accessory glands proved highly sensitive bioindicators. These studies became classics in the physiology of reproduction and prompted a wide range of other studies. They contributed one of the main bases for an operational definition of an "androgen" and made further important contributions to the recognition that androgens are produced normally not only by testes but also by ovaries and the adrenal cortex.

While experiments on bioindicator tests were still in progress, Moore turned to an old problem. He had earlier disproved the concept of sex gland antagonism (implying sex hormone antagonism). Now he attacked the problem directly by using male hormone extracts and female hormone that was already available. He administered the sex hormones separately or simultaneously to intact and gonadectomized rats. The findings were very difficult to interpret. Some of them suggested sex hormone antagonism, as when the female hormone estrin inhibited the development and function of the testes of young rats. But male hormone *also* inhibited such testes. Sex hormone antagonism was not the answer; there must be some other explanation. There was! A reciprocal relationship, a negative feedback between the hormones of the gonads and the anterior pituitary gland not only could explain the observations but would fit the results of many other workers into a comprehensive pattern. We quickly tested the idea by using anterior pituitary implants, or extracts, coincidentally with sex hormones. The hypothesis was found valid.

This theory, now known as the Moore-Price negative feedback concept, or Moore-Price "law," or push-pull theory, had wide application in reproductive physiology. It was applicable not only to the relationship between the pituitary and the gonads but also between the pituitary and other endocrine glands. We did not take the next step and link the brain and hypothalamus into the system, nor did we anticipate at that time that our hypothesis would provide the basic principle for "the pill," a contraceptive method preventing ovulation by the administration of female hormones. However, the late Carl Hartman learned of the theory, recognized just this possibility, and tried unsuccessfully in 1933 to interest clinicians. It was not until 1940 that medical practitioners reported preventing ovulation in women by estrogen, but it was in quite another context. Many more years elapsed before the famous pill emerged.

Moore had finally disproved sex hormone antagonism and substituted the far-reaching concept of a feedback system that controlled gonadal and anterior pituitary function. Lillie was highly enthusiastic. But the freemartin problem was unsolved, and it always remained in the back of Carl Moore's mind as he turned to other research. In the late 1930's this problem came to the fore again, and he tried to cause freemartinism by injecting the male hormone testosterone into pregnant rats. This failed, as had the attempts of others who used the same method. Administration to the rodent of testosterone, a pure steroidal male hormone, did not cause the postulated sex-differentiating effects of fetal testicular hormone.

As confusing and conflicting results accumulated in the literature, Moore sought a different approach and chose the opossum, a marsupial, as an ideal subject for research. In marsupials the young are born in an undeveloped state and maintained in an external pouch where they could be treated with hormones directly (no placental complication here). More importantly, "fetal" gonadectomy could be performed, although it proved to be impossible at early stages and difficult later. However, Moore accomplished it at a stage that he considered young enough to give a critical test of the significance of gonadal hormones in sex differentiation.

The results of his research on opossums convinced Moore that fetal sex hormones play no critical role in sex differentiation. For Lillie's theory he substituted a new one that proposed that the "inherent genetic constitution," male or female, might control male and female sex differentiation; that any or all cells of the male or female body might contribute substances to the bloodstream and, depending on sex constitution, effect male or female sex differentiation. The masculinization of the freemartin could then be explained by humoral substances coming from the whole body of the male co-twin, not just from the testes.

In advancing this theory, Moore undoubtedly believed that he had made a happy compromise. Lillie's humoral theory was retained, but in modified form so that no fetal sex hormones from gonads were involved. Moore's theory did not win general acceptance. His conclusions and interpretations were questioned. However, the results of further research on opossums and on grafts of reproductive tracts of fetal rats confirmed his belief that his theory must be valid, and he published a monograph in 1947. The last important lecture that he gave was in Paris in 1950 at a colloquium on sex differentiation of vertebrates. Here, again, he presented many arguments against the idea that secretions of fetal gonads were important in sex differentiation. He professed himself not convinced that the "proper explanation for the freemartin has yet been suggested." In this he was right; no clear explanation for all the aspects of freemartinism had been presented then nor has it now. However, the year before Moore's monograph was published, three very important short reports had appeared. L. J. Wells (a former student of Moore's) and A. Jost and A. Raynaud (both in Paris) had succeeded in gonadectomizing fetal rats, rabbits, and mice, respectively. Lillie's critical experiment had at last been done. Fetal testicular hormone was indeed playing a key role in sex differentiation, and much of Lillie's hypothesis was confirmed. Moore found this evidence, and later evidence including results of experiments in my own research project, very difficult to accept. In 1950 he was still unconvinced.

Only a few of all those who heard Moore's lecture in Paris in 1950 realized how ill he was. Even in 1948 there had been small, ominous warnings that his health was beginning to fail. By 1950 he was barely able to travel to Paris. His health rapidly deteriorated. Between protracted periods of hospitalization he attempted to continue his teaching, research, and administrative duties. He had enormous courage and an unshakable determination to carry on in illness as he had in health. He gave up regretfully some of his committee obligations, but he was unwilling to relinquish any other duties. He struggled to carry what he could no longer carry effectively by summoning sheer courage, determination, and willpower. But Carl Moore would have known no other way. From his Ozark country boyhood through his scientific career he had always solved problems and overcome obstacles as best he could. He bore the grim reality of relentless illness and impending death by negation in the face of hopelessness. The Medal and Certificate of Award of the Endocrine Society was conferred upon him in the spring of 1955, just before the final phase of his illness.

But it is the vigorous Carl Moore of his earlier years who is best remembered—the dedicated and imaginative scientist, the enthusiastic and inspiring teacher, the man of warm and sympathetic character. In his career he valued highly the qualities of fairness, honesty, and just criticism without pettiness. One of his highest compliments was to call a man "a straight shooter." One of his favorite maxims was that "a man must win his spurs" to merit praise and recognition. This was usually applied to a high level of research accomplishment. His students were trained to respect his values. In all, fifteen students obtained the master's degree with him, and thirty-three, the doctor of philosophy. Many of these men and women have "won their spurs" and carried on in his tradition.

Moore was a member of many scientific societies and served as vice president, American Society of Zoologists, 1926; vice president of Section F, American Association for the Advance ment of Science, 1943; and president, American Association for the Study of Internal Secretions, 1944. He was active in the National Research Council as a member of the Committee for Research in Problems of Sex, the Committee on Growth, and the Committee on Human Reproduction. He took his share of editorial duties as a member of the editorial boards of the *Biological Bulletin* and of *Physiological Zoology.* He was a trustee of Drury College, Springfield, Missouri, and was awarded an honorary Sc.D. by that school in 1948. Among his distinctions were the Francis Amory Award of the National Academy of Arts and Sciences, 1941; the Award of the American Urological Association, 1950; and the Medal and Certificate of Award, the Endocrine Society, 1955.

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KEY TO ABBREVIATIONS

Am. J. Anat. = American Journal of Anatomy
Am. J. Obstet. Gynecol. = American Journal of Obstetrics and Gynecology
Am. J. Physiol. = American Journal of Physiology
Am. Naturalist = American Naturalist
Anat. Record = Anatomical Record
Biol. Bull. = Biological Bulletin
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Clin. Endocrinol. = Journal of Clinical Endocrinology
J. Exp. Zool. = Journal of Experimental Zoology
J. Urol. = Journal of Urology
Physiol. Zool. = Physiological Zoology
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society of Experimental Biology and Medicine

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James Flack Norris*

January 20, 1871-August 4, 1940

by John D. Roberts

James Flack Norris was born in Baltimore, Maryland, January 20, 1871. He was the fifth of nine children, having two brothers and two sisters older than himself. He therefore had early opportunity to learn the art of getting along with people. His father was a Methodist minister, a popular revivalist, and a forceful orator in the pulpit. The future chemist used to accompany his father to camp meetings, where it was his custom during the sermon to rest, perhaps to doze, on a bench behind the pulpit, out of sight of the audience, and then later to stand beside his father and lead the singing. He received his elementary education in the schools of Baltimore and Washington, D.C. As a boy he collected stamps, but the pastime grew wearisome and he exchanged the collection for a printing press. This supplied means for the exercise of a more synthetic ingenuity, and for two years he published a monthly literary newspaper for which he wrote the articles and made the woodcuts himself.

Norris received the A.B. degree from Johns Hopkins in 1892, graduating Phi Beta Kappa, and remained at the university for graduate work. He was Fellow in Chemistry, 1894-1895,

* The bulk of this memoir was compiled from an article by Professor Tenney L. Davis, a colleague of James Flack Norris at the Massachusetts Institute of Technology, which was published in *Industrial and Engineering Chemistry,* 14:325-26 (1936). The present author has edited and extended this material to bring it more into the usual format of the Memoirs.

and received the Ph.D. degree in 1895. At Hopkins he was a member of the "Tramp Club," whose members were initiated by being taken for a walk of twenty-five miles, and of the "House of Commons," a debating society in which his classmate, Newton D. Baker, was especially active and articulate. Norris was strongly attracted by Professor Ira Remsen and for three years' running attended his lectures on organic chemistry and on the history of chemistry. For his doctor's thesis, Remsen set him to work on complex compounds of selenium and tellurium. The lure of organic chemistry was strong, however, and the investigation evolved into a study of the double salts of selenium dichloride and tetrachloride with the aliphatic amines and thence into a study of the perbromides and periodides of aliphatic amines, especially tertiary amines, in connection with which the interesting observation was made that the amine hydrobromides form perbromides by taking on a single atom of bromine.

At the opening of the academic year in the fall of 1895 the new Ph.D. joined the staff of the chemistry department of the Massachusetts Institute of Technology. With him came Henry Fay, also a Hopkins 1895 Ph.D. The two were close friends; they went to the opera together and to parties at the homes of President Walker and of various professors. Their appearance and manners caused them to be envied by the younger members of the staff as models of what the man-about-town ought to be. The debonair Norris soon became known widely as "Sunny Jim."

Norris remained at MIT until 1904, when he took on the duties of the first professor of chemistry at Simmons College, a newly organized college for women that was at the time just opening in Boston. Here he outfitted the laboratories, organized courses in chemistry, and had general supervision of all instruction in science. He stayed at Simmons until 1915 except for a year of sabbatical leave. In 1915-1916 he was at Vanderbilt University in Tennessee, then in the war, and back again to

MIT, where he became professor of organic chemistry and director of the Research Laboratory of Organic Chemistry after its organization in 1926. He also gave courses at Harvard, Radcliffe, Clark, and Bowdoin. From Bowdoin he received the honorary Sc.D. in 1929. During his first period at MIT he gave, among other courses, one in the history of chemistry, having caught the contagion of that subject from Remsen. He also taught advanced organic and inorganic chemistry, qualitative analysis, physical chemistry, food analysis—in fact, chemistry of all sorts except quantitative analysis.

Norris's sabbatical leave from Simmons was during the academic year 1910-1911. Feeling the need for more physical chemistry, he went to Karlsruhe, in Germany, where he worked in the laboratory of Haber. Working there at that time were a number of chemists who later became well known, including Allemand, Robinson, Carter, and Askenasy. Norris wrote his textbooks in North Bridgton on Long Lake, Maine, during the summers of his period at Simmons. They were written without the assistance of reference books, except tables of physical constants, for the author believed that nothing ought to be included in the general texts that a chemist does not remember because he finds it useful.

At North Bridgton, Professor Norris worked in a cabin among the trees, apart from the dwelling house, where he had his study, a carpenter shop, a darkroom, and a laboratory. Here, with no reagents except those he could buy at the country grocery store and with no apparatus except a thermometer, a graduate, and a horn-pan balance, he contrived a number of experiments that are included in his inorganic laboratory manual. He also experimented with photography and devised a means of simultaneously developing and fixing the negative in a single bath. Others had the same idea, and chemicals for this purpose were put on the market shortly afterward by Lumière in France.

Professor Norris was an associate member of the Naval Con suiting Board in 1916. In 1917-1918 he was in charge of chemical research on agents of offense and war gas investigations of the U.S. Bureau of Mines. He entered the Army as Lieutenant Colonel in the Chemical Warfare Service. In 1918 he was in charge of the U.S. Chemical Warfare Service in England and in 1919 of the investigation of the manufacture of war gases in German chemical plants. After the war he served ten years as vice chairman and chairman (1924-1925) of the Division of Chemistry and Chemical Technology of the National Research Council and as a member of the Executive Board of the Council.

He was president of the American Chemical Society for two years, 1925 and 1926, during which time he did much to improve and clarify the finances of the Society. He was vice president of the International Union of Pure and Applied Chemistry from 1925 to 1928. He was made an honorary member of the Rumanian Chemical Society, had lunch with Queen Marie, and brought home a box of cigarettes, marked with the royal monogram, from each of which he secured enjoyment and satisfaction. He was also an honorary member of the Royal Institute of Chemistry of Great Britain.

The American Institute of Chemists gave him its gold medal in 1937 for "outstanding service as a teacher and as an investigator." Norris was elected to membership in the National Academy of Sciences in 1934. He was chairman of Section C (chemistry) of the American Association for the Advancement of Science in 1930.

A fitting tribute to his memory has been the James Flack Norris Award of the Northeastern Section of the American Chemical Society. The award originally recognized outstanding teaching in chemistry, but more recently has been for research in physical organic chemistry.

Although Norris was serious when occasion demanded it, profoundly interested in his work, and dignified to a degree, he was "Jimmie" to a host of friends who found him a gay com panion when there was no work at hand and the cheerful member of many an informal group. He was married in Washington, D.C., February 4, 1902, to Anne Bent, daughter of Lowell Augustus Chamberlin, Captain, U.S. Army; they had no children. James Flack Norris died in Cambridge, Massachusetts, on August 4, 1940.

The scientific work of James Flack Norris, insofar as may be judged by his seventy-odd scientific papers, was broadly interesting and important, but hardly had the impact of the work of some of his contemporaries in America, such as Gomberg, Stieglitz, and Nef, because he sometimes reached the wrong conclusions. Thus, at almost the beginning of his MIT career, Norris became engaged in, and lost, rather a vitriolic argument with Gomberg about the nature of triphenylmethyl. Norris held that Gomberg's analytical data were incorrect and that the "unsaturated hydrocarbon" formed from triphenylchloromethane and zinc in benzene was formed with loss of hydrogen chloride. He believed the correct structure to be $(C_6H_5)_2C = C_6H_4$, but did not specify just how the "phenylene" part of the molecule was arranged, although he clearly recognized that it was likely to react easily with oxygen.

After continuation of some work on selenium and tellurium, begun with Remsen, Norris became rather generally concerned with some of the preparations and reactions of relatively simple compounds such as those that made up the backbone of the synthetic aliphatic chemistry of the time. Thus, he and his students investigated the conversions of alcohols to halides and the reactions of these halides with hydrocarbons by Friedel-Crafts catalysts to build up more complex substances.

As a result of these studies, in which it must have become clear to him that there were large differences in reactivity associated with rather similar substances in the same preparative reaction, he published in 1925 the first of what was to be a twenty-paper series on "the reactivity of atoms and groups in organic compounds." Much, but not all, of this work was concerned with the rates of the reaction of alcohols with acyl halides. These studies began before the mechanisms of any of the reactions involved were known and, necessarily, wound up providing only empirical correlations. The correlations were useful nonetheless for planning synthetic work and for pointing to striking differences in behavior that would ultimately require explanation. An example of the latter was the discovery of a 2,800-fold greater reactivity of 4,4'-dimethyldiphenylchloromethane relative to 4,4'-dichlorodiphenylchloromethane toward ethyl alcohol in a reaction that is rather well, but still not perfectly, understood forty-five years later. This program involved two of the best-known Ph.D. students Norris had—A. A. Ashdown (who later became the storied master of MIT's graduate student house) and, three years subsequently, A. A. Morton (discoverer of the alfin polymerization catalyst and many interesting metalation reactions). The research on reactivity was extended gradually to include thermal decompositions of malonic acids, and certain parallels were noted between the effect of R as an a substituent in influencing the carboxylation of malonic acid and the effect of R in ROH in changing reactivity toward *p*-nitrobenzoyl chloride.

Norris was rather less successful in his work on reactivity than was his younger counterpart at Harvard, James B. Conant, who displayed an almost unerring instinct for choosing reactions for study of greater simplicity and involving wider ranges of reactivity, along with an excellent feel for the basic physical chemistry involved. Nonetheless, Norris had the prescience to be at the forefront of the still-developing area of making comparisons of organic reactivity under controlled conditions, and some of the reactions he was first to study are among the most important in preparative chemistry.

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KEY TO ABBREVIATIONS

Am. Chem. J. = American Chemical Journal
Ind. Eng. Chem. = Industrial and Engineering Chemistry
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Ind. Eng. Chem. = Journal of Industrial and Engineering Chemistry
Org. Syn. = Organic Syntheses

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Oscar Riddle

September 27, 1877-November 29, 1968

by George W. Corner

Oscar Riddle, zoologist and proponent of the freedom of science teaching, was born September 27, 1877, in Greene County, Indiana. His birthplace was a log house near a village called Cincinnati, twenty miles from the university town of Bloomington. His boyhood in this countryside of heavily wooded hills and narrow valleys is well described in autobiographical notes Riddle prepared for the files of the National Academy of Sciences. The first part of the present account of his life, scientific career, and writings largely follows his own narrative.

Oscar Riddle's father, Jonathan Riddle, came from a North-of-England family that had settled first in Virginia. On his Indiana land he made a comfortable living by farming and breeding livestock, though always with the narrow economic margin characteristic of pioneer life. He kept a racehorse and was an enthusiastic hunter of deer, wild turkey, and the bears that were then to be found in the hills of Indiana and neighboring states. It is of interest, in connection with his son's attitude toward dogmatic religion, that Jonathan Riddle was never active in any religious sect.

Oscar's mother, Amanda Emeline Carmichael, was born at Cincinnati, Indiana, of Scottish and northern Irish ancestry. Her father, relatively prosperous among the villagers, kept a general store and a flour mill. Something of a philosopher, Mr. Carmichael wrote a number of unpublished essays, well worded (according to his grandson) and showing deep interest in the question of free will and similar religiophilosophical topics. Sometimes, in the absence of the local Baptist preacher, he took the pulpit and preached sermons appreciated by the congregation.

Jonathan Riddle died in 1882 at the age of fifty-five, leaving nine children of whom the youngest was six months old. His wife, then forty-five years of age, raised all of them to maturity and lived on to the age of eighty-nine. Although during her husband's lifetime she professed no religious faith, after he died she joined the local Baptists, often remarking, however, that she did not believe in eternal punishment nor did she think it sinful not to be a professed Christian. Her husband's death left the family in straitened circumstances, and all the older children had to help with the farm work. In his written reminiscenses Oscar gives a graphic account of his early boyhood and schooling.

"In our home, and on our farm, there was much work for even the smallest hands to do. Drinking water had to be carried up a steep hill from a cold, fast-flowing spring 60 or 70 yards away; and in summer, to and from the milk-house at this spring all the milk, some fruits, and vegetables were carried. Each winter and spring some acres had to be cleared of forest; later a variety of crops had to be planted and this rough and stubborn terrain had to be cultivated and harvested.

"In order to obtain some money, it was necessary for the sons of our family to obtain work on nearby farms or in stores. Thus during all of my ninth and tenth years, except for the short term of school, I supported myself by work on a farm two miles from my home."

Oscar Riddle's first school, a one-room cabin, was a mile from the Riddle farm by way of a narrow path through woods and across fields. Like other boys of the neighborhood, Oscar walked barefoot, even in frosty weather, wearing boots only when snow lay on the ground. The school term was brief, about seventy days in each year. After two years at the country school, Oscar attended school in the village, with somewhat longer terms, as much as one hundred days. To attend school and such events in the village as spelling bees, debates, and church suppers, the Riddle children walked two miles each way.

When twelve years old, Oscar helped in a store and delivered newspapers; at thirteen he trapped furbearing animals in wintertime; and for two years he swept the schoolroom floor and built the fire, for ten cents a day. From his fourteenth year he not only supported himself year-round, but like his older brothers was able to turn over a little money to his mother.

Through hunting and trapping Oscar developed his lifelong interest in the habits of birds and mammals. As early as the age of eight his curiosity had been awakened by fossil shells and imprints he had noticed in the banks and gullies around his hillside home. These shapes in sandstone and limestone, he was told, represented animals of kinds that lived only in the sea.

"This seemed to indicate, and led me to suspect, that our earth must be very old. Yet all the preachers I had heard insisted, and cited the Biblical record in support, that the earth was created about 6,000 years ago, and that there had been one—and only one—big and short-lived flood. How could this flood have brought animals to our high hill from a sea that is almost a thousand miles away? Even more disconcerting to me were the dicta of these preachers, again supported by a Heaven-born Bible, that a hot Hell exists, and that after death all unbelievers go there and burn everlastingly. And I had to regard myself as such an unbeliever!"

This conflict between dogma and observed fact caused the boy great distress of a kind not uncommon in those days in youngsters whose inquiring minds were breaking away from the rigid beliefs of their elders. From the age of ten until he was thirteen, Oscar tells us, the threat of hellfire often wrung a prayer from him and brought frightened tears to his pillow before he slept at night.

These fears were suddenly brushed away one night when the boy attended a lecture at the village church, the very place where he had so often heard the threat of damnation. A college mate of his elder brother, named Francis Price, was studying zoology at Indiana University under a twenty-seven-year-old professor, Carl H. Eigenmann, who later became a member of the National Academy of Sciences. Price had arranged to give a talk at the church on the evolution of living things and to illustrate it borrowed from Eigenmann a collection of fishes preserved in alcohol, chosen to illustrate the principles of adaptation and natural selection. Either Price was very bold for the time or the current pastor was more liberal than those Oscar had heard earlier. At any rate the lad was so thrilled by the talk that he had Price invited to the Riddle house for the night. Thus enabled to examine the wonderful specimens for himself, with Price's kindly guidance, he understood the relics of ancient life in the hillside strata that had worked so powerfully upon his youthful mind. "I never prayed or wept upon my pillow again," he wrote in old age. "Nothing in a long life has equaled the release, thrill, and resolution obtained from this message, so simply delivered by a young man from a neighboring farm."

After completing grade school in the village of Cincinnati, Oscar Riddle attended high school in Bloomfield, the county seat of Greene County, and entered Indiana University in the spring of 1896. He began at once the formal study of biology and spent two summers at the university's biological field station, then at Turkey Lake, Indiana. In the summer of 1899 his good work on a survey of Winona Lake led Professor

Eigenmann to recommend him to the U.S. Commissioner of Fisheries for assignment to collect tide-pool and freshwater fishes of Puerto Rico, which had just become a possession of the United States. Taking a hasty course in Spanish, Riddle interrupted his college work and left for Puerto Rico in the autumn of 1899. The island's Commissioner of Education promptly asked him to teach biology to students of pharmacy and of education in the newly established Model and Training School at San Juan. Early in 1900 he also took over a beginning class in chemistry.

That summer he was one of five men chosen to conduct teachers' institutes in the ten largest cities of Puerto Rico. By this time he was able to lecture in Spanish. Traveling by railway, horse-drawn carriage, ox cart, and steamer, he covered much of the island and the neighboring smaller isle of Vieques. During a second year at San Juan Riddle taught classes in biology in the high school, some of them in Spanish, and followed up his course in chemistry for pharmacy students by teaching them zoology and physiology. Several of his class of fourteen, he learned years later, became physicians, one a lawyer, one a banker, another a legislator, and one a professor of Spanish in the new University of Puerto Rico. All this teaching had left but little time for zoological collecting, but in 1901 Riddle, at his own expense, made a summer's scientific expedition to the delta of the Orinoco River, south of Trinidad.

Returning home in the autumn of 1901, he registered at Indiana University for the final year required for his bachelor's degree. In January and February 1902 he accompanied Carl Eigenmann on a six-week trip to collect blindfishes (a special interest of Eigenmann's) in the caves and underground streams of western Cuba. During that year also he prepared an article on the fishes he had himself collected in Venezuela and Trini dad, but the manuscript that would have yielded his first publication in zoology was stolen from him. He sold his collection to the Field Museum in Chicago.

After graduation from college Riddle declined a generous offer from a family friend of a mercantile position in Indianapolis. He also declined a teaching post at the University of the Philippines and instead enrolled as a graduate student in the University of Chicago. There he was under the general leadership of Charles O. Whitman, but also followed (as he had hoped) the lectures of Jacques Loeb. His plan was to prepare himself for teaching and research, aiming for a career on the preclinical side of a medical school. In his first term he took Loeb's radically planned course in physiology, or rather general physiology as we would term it today. Although Riddle does not say so, it is obvious in retrospect that Loeb's departure that winter for the University of California was an intellectual loss to the young man, who could have benefited much if he had gone on to research under Loeb, from the latter's rigorous analytical thinking, of a kind that the still largely descriptive methods of zoology did not demand.

Riddle's postgraduate training was interrupted by his appointment in the spring of 1903 to teach physiology in Central High School of St. Louis, Missouri. Loeb's department had been asked to recommend a man capable of introducing laboratory work into their didactic course. The project interested Riddle, who moreover needed money to help a sister go to college in St. Louis. He spent altogether five half-year periods there (1903-1906) interspersed with other activities, including participation in the summer course in physiology at Woods Hole in 1903, a summer assistantship in zoology and biology at Indiana in 1904, and a similar post at Indiana for eight months in 1905 while on leave of absence from St. Louis. At St. Louis he was also principal of one of the city's evening schools and filled in what was left of his working hours by studying French and German at the local Berlitz school.

In February 1906, Riddle resigned his St. Louis post and returned to Chicago to complete his postgraduate studies while resuming his assistantship in zoology. He found Loeb's successor in the chair of physiology, G. N. Stewart, less sympathetic to the kind of training he wished to obtain than was Professor Whitman and therefore decided to make zoology, under Whitman's tutelage, his major subject for the doctorate. Even though he had accumulated sufficient credits for a minor in physiology, with Whitman's approval he chose biochemistry under Albert P. Matthews as his designated minor subject.

Whitman put him to work for his doctoral dissertation on a problem of considerable theoretical importance, the cause of the alternation of light and dark bars seen on the feathers of many kinds of birds, notably fowl and pigeons. Whitman's own long studies of the evolution of birds, and especially of their color patterns, had brought him face to face with this question, which, as he perceived, called for both genetic and biochemical studies. Thus was the course of Riddle's career as an investigator set by the time he took his Ph.D. in zoology, in June 1907. The guidance and companionship of Whitman, he says in his autobiographical statement, provided one of the most profitable and delightful epochs of his life: "Whitman became nearer to being a father to me than anyone I have known."

After taking his doctorate, Riddle remained at the University of Chicago as an associate (a rank between assistant and instructor) in zoology and embryology and also as an assistant in experimental therapeutics (a research post). The next year he was promoted to instructor in zoology and embryology, and in the following two years he twice gave the course in embryology for medical students and organized new courses in vertebrate zoology and general biology and a graduate course in the physiology of development, a quite novel topic. From his laboratory he published several papers on color formation in feathers, the development of yolk in hens' eggs, and the rate of digestion in cold-blooded animals. In July 1910, he obtained leave of absence for a year of travel and study in Europe. Whitman had assured him that upon his return he would be made assistant professor of biology and given charge of two of the three terms of the introductory course in zoology.

Riddle began serious work abroad by settling for a few weeks in Berlin, where in the university library he wrote a paper on melanin formation in feathers, which he presented at the Eighth International Zoological Congress, at Graz. After the Congress he visited various European countries as a tourist. In Frankfurt he called on Paul Ehrlich, who advised him about intravitam stains for studying oxidation and reduction in animal tissues, a topic he intended to investigate in the autumn at the Naples Zoological Station.

Riddle had not been long at Naples when he received the distressing news of Whitman's death on December 6, 1910. It can do no harm now to the memory of the distinguished personages upon whom Riddle's career depended at that critical time to say that Whitman's death was very unfortunate for him. Frank R. Lillie of the Chicago department of zoology, who had regarded himself as Whitman's heir apparent and in fact succeeded to the senior chair, was planning a radical redisposition of the staff. Lillie wrote to Riddle in January 1911 that there was internal opposition to him (as indeed there had been to Whitman) and that he would not be reappointed. At about the same time Whitman's friends in the university wrote of their fears that the late professor's extensive unpublished researches on the evolution of pigeons would never be published under the new regime. Riddle, therefore, with self-sacrificing loyalty to his late chief and mentor, left Naples and went home to see what could be done to salvage Whitman's lifework. The struggle to take on this task, he records, and the labor of completing it were more formidable than any other efforts of his lifetime.

Albert P. Matthews, Professor of Biochemistry, managed to get him a six-month appointment on the payroll of the Laboratory of Experimental Therapeutics, a research unit of Matthews's department. The Sprague Institute gave him $300 toward the expenses of maintaining Whitman's large breeding colony of pigeons, which was still kept at the late professor's home.

In 1912 came a great step forward in Riddle's career when the Carnegie Institution of Washington made him a salaried research associate, with funds to continue the pigeon colony, and undertook to pay for publishing the Whitman papers whenever they might be ready for the press. Late in 1913 Riddle moved, with the birds and the manuscripts, to the Carnegie Institution's Station for Experimental Evolution, at Cold Spring Harbor, Long Island. This appointment must have been initiated by Charles B. Davenport, founder in 1904 and director of the station. Yet Riddle states in his autobiographical notes that he had a constant struggle to obtain adequate quarters for his birds and efficient laboratory space for himself and indeed received little encouragement for his research until, after many years, Albert F. Blakeslee and later Milislav Demerec succeeded to the directorship.

Davenport's coolness toward Riddle arose, no doubt, not only from differences of temperament, but also from Riddle's devotion to the memory of Whitman, whose scientific ideas as revealed in the documents that his disciple was editing were deeply at variance with those of Davenport. The research program at Cold Spring Harbor was based on the Mendelian principles that, since their rediscovery in 1900, had revolutionized genetics. Whitman, on the other hand, had remained unresponsive to much of the new genetics. He had begun to study evolution in birds in 1892, at the age of fifty, under the influence of an older school of biological thought. To him recapitulation was the central fact of heredity; and he had, moreover, chosen as the hereditable factors to be studied in his hybrid birds three that did not lend themselves easily to Mendelian analysis: color patterns, which are exceedingly complex in birds, and sex determination and fertility, which are complex phenomena in all animals. He had never accepted the Mendelian ideas of unit characters and genetic dominance; he doubted the importance of mutations for evolution and declared that he had found evidence for evolution by orthogenesis. The presence in Davenport's laboratory of an outspoken, enthusiastic pupil of an anti-Mendelian must have irked the sensitive spirit of its director.

At any rate, Riddle, while organizing, against what he felt to be his chief's indifference, a laboratory that never quite matched his own standards and getting under way a broad program of research, toiled on and on with Whitman's voluminous and, to a large extent, ill-sorted papers. The task was varied and immense, requiring rearrangement and assemblage of misplaced portions of chapters, analysis of numerous tables, and placement of numerous illustrations. In this task also he did not get all the help he needed, for Mrs. Whitman had for reasons of her own at times limited his use of the materials. At last, in 1914, the Carnegie Institution published the Whitman papers in three large and handsomely illustrated volumes. The first two, edited solely by Riddle, present a clear statement of Whitman's studies on natural and hybrid pigeons and doves, their growth, and particularly their inheritance of feather patterns. In the third volume Riddle gathered together Whitman's intensive observations of sex behavior and reproductive activities. Feeling himself not competent to assess this material, he turned the detailed editing over to Harvey A. Carr, Associate Professor of Psychology at the University of Chicago. This third volume, largely free of the conjectural and controversial bias of the first two, is of more permanent value. It gave Riddle the physiological background of much of his experimental research.

Oscar remained at Cold Spring Harbor through the whole of his active scientific career, as a member of the Carnegie Institution's Station for Experimental Evolution, later called the Department of Genetics. It seems a pity that he was not in a teaching institution, for with his love of nature, his cordial outgoing manner, and his enthusiasm for the study of grand problems—inheritance, metabolism, and sex determination—he would have been an admirable college professor of biology. As it was, he worked away in relative intellectual isolation in his own laboratory, with an assistant or two, on a research program not intimately related to the studies on chromosomal and statistical genetics that interested his chief, C. B. Davenport, and the rest of the staff. He was largely out of contact with young people whose minds he might have awakened, as indeed he had done during his brief stay at the University of Puerto Rico and in St. Louis.

Life in the semirural ambience of Cold Spring Harbor well suited this country-bred man, who came to love the wooded slopes of Long Island's North Shore, where gentle streams glide through ponds and marshes down to tidal harbors and the broad salt waters of the Sound, and where the songs of forest birds and the mewing of seagulls mingled with the cooing of his caged pigeons. Long remaining a bachelor, Oscar had a room in the plain old country house that Davenport had adapted as a dormitory for the unmarried assistants. Most of the year his daily circuit took him to his nearby laboratory and back in the evening, with only an occasional visit to New York City, thirty miles away. From June to August life was somewhat more varied. Davenport had kept up, along with his Carnegie department, the summer courses of the Long Island Biological Association, from which the whole station had grown. Students and summertime investigators of marine life came to live on the grounds, and with them Oscar enjoyed an after-supper game of softball. Now and then on Saturday afternoons he could be seen with his stocky form and merry round face driving off with a carfull of the girl assistants—there was safety in numbers—for sea-bathing at Jones Beach on the South Shore. Occasionally, he invited one of the visiting biologists to a seafood dinner at a shore hotel, followed by a good talk on scientific matters and a moonlight swim. The writer of this memoir recalls one such evening when the water of the Sound was silver-smooth at slack tide under the August moon. Oscar, who was a chain smoker, lit a fresh cigarette before launching himself upon the salt water and then, swimming on his back in order not to douse his smoke, drifted out toward the open Sound, where all that could be seen of him in the moonlight was a bobbing shadow and the faint gleam of his cigarette.

At the outbreak of World War I, a company of the Home Guard of New York State was formed at Cold Spring Harbor. Oscar joined up as a private, but soon rose to the rank of sergeant and finally arrived at a first lieutenancy. In 1918 he achieved more serious military status when he was commissioned captain in the Sanitary Corps of the Army and served as nutrition officer and on related assignments at Fort Oglethorpe, Camp Meade, and overseas at Dijon and Bordeaux.

Back at Cold Spring Harbor after the war, Oscar's researches kept him busy, as before, through the summer months; therefore, he took short winter vacations, usually Caribbean cruises. One of these holiday voyages altered his way of life, for in 1937 at the age of sixty he astonished his friends by marrying a lady whom he had met on the previous winter's cruise. Leona Lewis, like himself a Midwesterner but some years younger than he, had been a school and college teacher of music in Iowa and Oregon, and during the depression years of the early 1930's was a supervisor on the Governor of Indiana's Relief Commission. As Oscar wrote of her after many years of marriage, she was "trained, industrious, beautiful, and able." Their friends would add that she was a very agreeable and hospitable hostess.

With his wife, Oscar settled down to contented domestic life in a commodious apartment occupying the first floor of a large old mansion, near the laboratory, that had been bequeathed to the Carnegie Institution by a wealthy New Yorker. There, in their spacious living room, Oscar and Leona loved to have dinner guests join them in a favorite game, reminiscent of their shipboard pleasures. The carpet, rolled back, revealed a shuffleboard diagram that Oscar had painted on the hardwood floor. Cues and disks were brought from a closet, and professional concerns were set aside as the guests, however dignified and eminent in the academic world, joined in an evening of simple party fun.

In Riddle's scientific career one achievement in particular stands out. This was the preliminary isolation, naming, and study of prolactin, the hormone of the pituitary gland that stimulates the mammary gland to produce milk. This work will be discussed later. As to the long and laborious studies to which he devoted most of his life, on the formation and inheritance of feather patterns, and on the nature, the inheritance, and the reversibility of sex, it must be said that, although he contributed numerous original observations, his general conclusions have largely been superseded.

A prefatory explanation is here urgently called for. Riddle boldly attacked broad general problems of animal life, of long concern to naturalists and zoologists, at a time when physiology and biochemistry were rapidly coming forward to supplement or supplant the older methods of morphology. It was tempting and all too easy to formulate biochemical and metabolic hypotheses to explain such mysteries as sex differentiation and the inheritance of color patterns. Riddle himself perceived in later years that some of his conclusions had been premature. "Fate or fortune," he wrote, "led me into two fields which were both of them in a stage best defined as quite exciting, rapidly changing, and beset with vast uncertainties. During this early period I made some lasting contributions to these two subjects; but my inexperience led me to an overconfidence and dogmatism which more mature years could only regret."

In an early paper on a supposed metabolic cause of sex differences in pigeons, based on some of Whitman's conclusions, Riddle relied too much, he later said, on impressions obtained from conversations with Whitman. Two self-admitted defects, dogmatism and overreliance on uncertain authority—oral and written—did not altogether disappear from his thinking even in more mature years. There would be no point in reviewing here in detail Riddle's voluminous research publications. To place his work, as far as can now be seen, in context with the biology of his time, we need only summarize his findings, according to the major topics that he himself listed in the autobiographical sketch referred to earlier.

Riddle's early effort to explain a prevalent special form of "barring" (i.e., alternating stripes of light and dark color) in the feathers of highly pigmented birds led to the conclusion that this pattern is produced by daily variation in the rate of formation of dark pigment (melanin), caused by a diurnal metabolic rhythm. Later studies of B. H. Willier have shown that the rhythm of pigment deposition is by no means as regular or as rapid as Riddle thought, and that its fluctuation is brought about by special cells known as melanocytes, the bar gene of which acts upon the biochemical mechanism of melanin formation in an "on and off" sequence.

Riddle devoted years of detailed physiological and biochemical research to the question of sex determination, concluding that maleness and femaleness depend directly upon a difference in the rate of basal metabolism, beginning in the ovum. This hypothesis was promptly questioned by geneticists, and today it is clear that sex differentiation is brought about by the balance of male- and female-determining genes. A difference between the basal metabolism of the two sexes is simply one of many effects of differing sex-chromosome control.

Carrying forward a study of sex reversal and hermaphroditism in hybrid pigeons, begun by C. O. Whitman, Riddle explained these aberrant phenomena as special cases of metabolic imbalance; subsequent investigators have shown that they result, in a complex manner, from genetic and endocrine causes. A remarkable side-product of Riddle's study of hermaphroditism, his establishment by selective breeding of a race of hermaphrodite-producing pigeons, is in fact difficult to reconcile with a metabolic theory of sex.

In Riddle's middle years, when endocrinology was barely getting under way, the reproductive cycle of higher animals was a highly mysterious phenomenon. Whether the cycles of ovulation, estrus, and menstruation (in primates) are regulated by the nervous system or the endocrine glands was a central problem. Riddle's work with birds led him to side, correctly, with those who spoke for the endocrine system. Experiments of his own, on which he laid great stress, purporting to demonstrate the existence of a hormone of the thymus gland ("thymovidin") responsible for the cyclic formation of the egg albumin, the activity of the shell gland, and deposition of the shell, have not been confirmed.

In the course of these studies of sex differences, reproductive cycles, and metabolism in pigeons, Riddle with extraordinary energy and persistence had tried every means he knew and could find in the literature to test his hypotheses by measuring the physiological and biochemical states of his birds. By 1939, when he had largely shifted his attack to other problems, he with his associates had published scores, even hundreds of journal articles and had compiled no less than 800 tables of quantitative experimental results—so many, in fact, that he could never get them published as a whole. In the late 1920's and early 1930's

Riddle used his great experience in the handling of pigeons in joint studies, with F. G. Benedict of the Carnegie Institution's Nutrition Laboratory, of the normal basal metabolism of pigeons and ring-doves and its variation under changing conditions of temperature, season, and endocrine balance.

For many years, in pursuance of his interest in evolution, Riddle carried on continuous breeding experiments aimed at demonstrating the production of distinctive races of ring-doves by selection followed by inbreeding of birds descended from a heterogeneous popoulation. In a monograph published in 1947, he reported having established several races characterized variously by large or small thyroid glands, long or short intestinal tracts, early or late sexual maturity of females, production of large or small eggs, and similar traits. Such artificial evolution differs from that carried on for millenia by practical breeders of domestic animals and plants only in that it was done with a scientific aim and advanced knowledge of genetic processes. It thus adds some little additional support for the concept of natural selection as a major factor in the production of races and species. Other biologists, for example, W. E. Castle, E. Carleton McDowell, and Fernandus Payne, had accomplished similar genetic selection of morphological characters; the novelty in Riddle's work was that most of the characters he succeeded in establishing were physiological.

Riddle's reputation as a scientist will in the long run no doubt rest chiefly on his pioneering chemical and physiological study of the pituitary mammatropic hormone, which he was the first to isolate in a form approaching purity and to which he gave the name "prolactin." Various experimenters from about 1905 on, seeking to find the cause of lactation, variously and unconvincingly ascribed it to the placenta, the corpus luteum, and the estrogenic hormone of the ovary. In 1928, however, P. Stricker and F. Grueter of Strasbourg announced that they had induced lactation in rabbits by injection of aqueous extracts of the anterior lobe of the pituitary gland. These investigators erroneously believed, however, that previous preparation of the mammary gland by the hormone of the corpus luteum was necessary for milk production. The writer of this memoir, working at the same time as the Alsatian scientists, but unaware of their studies, reported in 1930 experiments that convincingly showed that alkaline aqueous extracts of the anterior pituitary will cause active secretion of milk in spayed rabbits that had never ovulated. My attempts to purify the lactation hormone, however, completely failed even with the collaboration of a biochemist. We were unable to proceed beyond the preparation of a crude aqueous extract.

Riddle apparently did not know of Stricker's and Grueter's work on pituitary-induced lactation, but he read the article of Corner (1930) and was stimulated to investigate the subject himself. He was already an experienced endocrinologist, having been the first to use insulin, thyroxin, adrenalin, and pituitary gland extracts in metabolic studies on pigeons. With two associates, R. W. Bates and S. W. Dykshorn, he produced a highly (though not completely) purified lactogenic substance that he named prolactin and separated it from other hormonal substances sufficiently to indicate its independent status. This work was published in 1932. Riddle and his associates, Bates and E. L. Lahr, soon identified the substance as a protein. In the course of these biochemical studies, he made the remarkable discovery that the secretion of crop milk in the pigeon is induced by a pituitary hormone which he identified with prolactin as soon as he had the latter in relatively pure form. The growth of the pigeon's crop thus produced may readily be used, as Riddle pointed out, as a convenient, rapid, and relatively economical method of testing the lactogenic action of pituitary extracts. The crop gland test of Riddle was very soon applied to further purification of prolactin by W. R. Lyons at Berkeley and finally to its preparation in crystalline form by Abraham White and associates at New Haven.

Although Riddle spent all but the first years of his scientific career in an advanced research institute, he never lost interest in the elementary teaching of biology. His first published paper (1906) dealt with that subject and appeared in a journal of secondary education. For the next thirty years he pondered the subject in the light of his own boyhood experience of intellectual liberation through biological study and came more and more to blame the weakness of biology in the high schools on the antievolutionary pressure of dogmatic religious groups.

His feeling finally boiled over in an address at St. Louis on January 1, 1936, as a vice president of the American Association for the Advancement of Science and chairman of its section on zoology. Under the title, "The Confusion of Tongues," he eloquently contrasted the recent rapid development of the life sciences in American universities and colleges with the concurrent virtual suppression (as he saw it) of the essential evolutionary contents of these sciences in the high schools by religious dogmatism. Widening the scope of his address, he went on to a critical examination of supernaturalism in general. To speak as he did was, under the circumstances, quite bold and indeed indiscreet had not his position in an independent nonteaching institution protected him from the attacks of dismayed clerics and parents. The address naturally attracted nationwide attention. *The New York Times* printed much of it on the day after its delivery, and within the month *Science* published it in full in two installments. Riddle was inundated by several hundred letters of approval and condemnation.

The then-president of the Union of American Biological Societies, E. V. Cowdry, was in Riddle's audience at St. Louis. Impressed by the address, he asked Riddle to organize and preside over a committee to examine the current state of biology teaching. After some years' study of the high schools the committee published in 1942 a report largely confirming Riddle's charge of religious restraints upon the teaching of evolutionary theory.

Feeling that his own ideas and the committee's report deserved a wider audience, Riddle went to work in 1947 on a book, *The Unleashing of Evolutionary Thought,* and after four years had the manuscript ready. Ten prominent publishers, including a university press, one after another rejected it. Turning to a publisher of the "vanity press" type, who put out books subsidized by their authors, Riddle finally got his work in print in 1954. He blamed the repeated rejections on the timidity of publishers with regard to his antireligious position and ascribed the book's small sale and the scanty attention given to it, when published, by general and scientific reviewers, to inadequate advertising by the publisher. Actually, the volume did not effectively represent his wide information and humane ideas. Its outward appearance was unimpressive, its contents heavy and discursive. The first one hundred fifty pages, however, present a thorough and clear review of current thought about human nature and evolution. That part of the book, if separately published, lightened by a bit of the humor Riddle could exhibit in conversation, somewhat less belligerently antireligious, and attractively illustrated, might have been accepted by thoughtful general readers as a valuable statement of contemporary scientific humanism.

Two excerpts will show something of Riddle's personal thought on great problems of human duty and fate. The first can be recommended to both sides in the current discussion of "women's liberation."

"A unique thing about the abstract quality called sex is that it is observed and known only in terms of differences between two unlike types. In an all-male or an all-female world, our concept of sex could never arise. But once the idea and fact of sex difference was recognized by man, it became a dividing line between the two chief groups into which all humans are classified by themselves and by others. And here, indeed, there is no doubt of human biological inequality. To this contrast of bodily form and function, however, nature herself seems content to assign mainly unlike and unequal shares in reproduction and in a few diseases; meanwhile, and in addition, humans themselves have assigned to sex almost everything on earth and in heaven. In the numerous cultures of our own day—barbaric and civilized—there is scarcely a category of performance that is not apportioned on a basis of sex. To round out this absurdity, the apportionments of one culture contradict those of another.

"Just as the idea of, and the wish for, progress is either unknown or quite new among men, a declared and purposeful search for sound and unexplored ways of developing and utilizing the special qualities of each of the two sexes—and especially those of the female sex—is yet to be attempted in any culture anywhere. It is true, of course, that bars and taboos are being lifted in many countries; but tradition, masculine vanity, law, custom and religion all compete for rule within this exceedingly broad area. The divisive aspect of sex usually still has right of way over any concerted effort to exploit the rich possibilities of what can be made of two rather similar genetic endowments that are so flavored as to yield two (often) superb and complementary personalities."

Another passage presents Riddle's answer, as a biologist, to the question of individual immortality. After discrediting orthodox views and deploring a stagnation of literature that he thought resulted from them he writes,

"The boundaries of reality are indeed quite different from those of most bygone human imagination—of stretched and misty hope. Nevertheless, firm truth from biology and sociology now assures that two forms of near-immortality for man hover or hide on natural creation's crest: the same biological thread that saved for man the gains of a dim past ties also his own genes to a lineage that may end only with highest life on earth. And personal social worth puts its benign leverage upon the unborn tomorrows. Clearly, the countless blessings of our times point backward to uncounted personalities. A Lincoln or a Jefferson is gripped lightly by a grave, but firmly by a durably spreading society."

In the second part of *The Unleashing of Evolutionary Thought* Riddle attacks directly the opposition of dogmatic religion to the teaching of evolutionary theory, upon which he based such hopes for mankind as those suggested in the foregoing excerpts. The section is a vigorous polemic that may profitably be reread by anyone investigating the socioreligious state of our country in the 1940's. The book ends with a collection of comments, favorable and unfavorable, on Riddle's St. Louis address of 1936 and a final chapter predicting a continuing but ultimately victorious struggle, in the western world, of naturalism against supernaturalism.

Distressed but not discouraged by the slight impact of this heartfelt and laborious book, Riddle continued to publish his opinions on science and religion in a way he could handle more effectively, in articles in such journals as *The Humanist, The Rationalist, The Realist,* and *The Age of Reason.* Almost every year until 1967 this indefatigable fighter for rational thought and education brought out one or more papers on evolution and the need for unhampered teaching of biology.

In 1945, after Riddle reached the Carnegie Institution's stated age of retirement, he and his wife bought a house and five acres of land near Plant City, Florida, where he contentedly carried on his study and writing in the fields that had long interested him. After his book of 1954 was off the press he made several trips on invitation to lecture and lead seminars, at colleges all over the country, on endocrinology and the biology of reproduction. In 1959-1960 he was president of the American Rationalist Federation. In 1961, stimulated by cer tain criticisms of his published work on the role of prolactin in reproductive behavior of birds, he devoted much time to writing articles reviewing and defending his conclusions on that subject.

Riddle's lifelong good health began to break down in 1964, when at the age of 87 he suffered a mild coronary occlusion. Other illnesses followed, and in 1966 he was found to be suffering from an inoperable carcinoma of the prostate gland, which ended his life in 1968. His wife Leona died a few weeks later; there are no surviving close relatives.

Oscar Riddle's outgoing disposition, his enthusiasm for research, his broad view of biological processes, and his valiant fight for humane consideration of mankind's problems of life and destiny, on the basis of scientific understanding, won him wide contemporary recognition. He was elected a member of the three leading learned societies—the American Philosophical Society in 1926, the American Academy of Arts and Sciences in 1934, and the National Academy of Sciences in 1939. A dozen European and South American societies awarded him foreign membership. His alma mater, Indiana University, gave him the honorary degree of Doctor of Laws in 1933, and the Catholic University of Chile made him a Doctor *honoris causa* in 1946.

Dr. Riddle in his last years presented to the American Philosophical Society, in Philadelphia, a collection of his correspondence and other memorabilia, including a typescript copy of the unpublished autobiographical notes upon which this memoir is partly based and a complete list of his publications. His scientific work from 1904 to 1947 may be followed in detail in the successive Year Books of the Carnegie Institution of Washington.

The author is indebted to the late Dr. Benjamin H. Willier for expert comment on much of Riddle's work, especially that on feather patterns.

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KEY TO ABBREVIATIONS

Am. Biol. Teacher = American Biology Teacher
Am. J. Anat. = American Journal of Anatomy
Am. J. Physiol. = American Journal of Physiology
Am. Naturalist = American Naturalist
Anat. Record = Anatomical Record
Biol. Bull. = Biological Bulletin
Cold Spring Harbor Symp. Quant. Biol. = Cold Spring Harbor Symposia on Quantitative Biology
J. Am. Med. Assoc. = Journal of the American Medical Association
J. Biol. Chem. = Journal of Biological Chemistry
J. Exp. Zool. = Journal of Experimental Zoology
J. Nutrition = Journal of Nutrition
J. Pharmacol. Exp. Therap. = Journal of Pharmacology and Experimental Therapeutics
Physiol. Zool. = Physiological Zoology
Proc. Am. Phil. Soc. = Proceedings of the American Philosophical Society
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Sci. Monthly = Scientific Monthly

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