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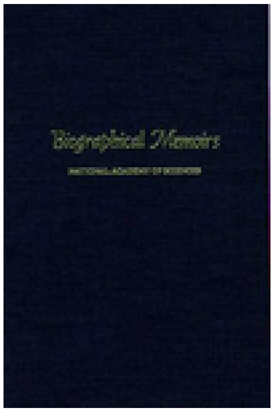

Biographical Memoirs V.62

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Advisers to the Nation on Science, Engineering, and Medicine

Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA

Biographical Memoirs

VOLUME 62

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PREFACE

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

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

PETER H. RAVEN
Home Secretary

Biographical Memoirs

VOLUME 62

George F. Barker.

GEORGE FREDERIC BARKER

July 14, 1835-May 24, 1910

BY EDGAR F. SMITH

WHEN THE WRITER of the following paragraphs began the study of chemistry in 1972, his textbook was *Elementary Chemistry,* then in its tenth edition, written by Professor George F. Barker. At that time the writer never dreamed that it would be his privilege to become a colleague of this distinguished scientist, nor that later he would be called upon to write in memory of this splendid teacher, profound student, and man of noblest character.

The face of Dr. Barker was familiar to men of science, both in this country and abroad, as he made it a point, whenever possible, to meet with his fellows in science. On such occasions by his affability and courtesy he made a wide circle of friends, who in recent years have keenly felt his absence from their meetings, and were indeed shocked when the message of his death was announced. The writer had the opportunity to meet Dr. Barker daily for many years, not so intimately at first, but later with the greatest freedom and in true companionship. The impression made by him, at all times, was that of an earnest student of science, thoroughly conversant with its most recent advances

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Reprinted from *The American Journal of Science,* September 1910.

and able to render subjects, which were dry and unattractive though important, so simple and so fascinating, that the ordinary layman could comprehend them with ease. His lectures to students were celebrated for their clarity of presentation as well as for their wide scope. He was painstaking in the presentation of his subject, and his constant endeavor was to make his students grasp the problems he placed before them. He spared no pains to make abstruse points clear, and if at times he seemed to demand almost too much and be a bit brusque, yet no earnest student was ever turned away; he was, in a word, the true teacher, whose sole object was the welfare of those whom he taught.

In the lecture room he had rare skill and facility as an experimenter, and one of his chief joys was to illustrate his lectures, as far as possible, with an abundance of attractive and striking experiments. He often presented the most intricate topics before large audiences, reaching sometimes into the thousands, and so uniformly brilliant was his success that he became noted throughout the country as one capable of popularizing science as few could do it. The writer recalls an occasion in his younger days, when the Academy of Music in Philadelphia was filled to its dome with an intensely interested and intelligent audience assembled to listen to his lecture on "Sound," and, while seated in the "sky parlor" of the immense auditorium, enjoying the discomforts peculiar to his position, so intensely absorbing was the lecture and its experiments that at its conclusion it was difficult for him to realize that he had actually sat there more than an hour.

Dr. Barker was much esteemed in the community where he lived by persons of all ranks. For a number of years he served on the board of education in the city of Philadelphia, and there exerted an influence which was entirely for the good, and never to be forgotten. His contributions to municipal interests included studies of the local water supply, of the quality of illuminating gas, and of the means for protecting public buildings from lightning. At various times he appeared as an expert in scientific matters, and in this field further demonstrated his spirit of careful investigation and absolute integrity and loyalty, as well as [an] ability to be just and fair to all. Many of the cases called for the highest scientific knowledge and accuracy, which were abundantly supplied by him.

By his colleagues on the teaching staff of the University of Pennsylvania he was most highly valued. His services were constantly engaged upon committees, and those who worked with him in such duties entertained but one impression, to wit, that he was capable of handling the most intricate and perplexing problems with fairness, calmness, and the best judgment. Indeed, it was a pleasure to be associated with him in work of this description; his hearty cooperation and his many helpful suggestions in the solution of university problems were appreciated by all his colleagues.

Dr. Barker's early academic training was received in the Boston Public Schools, and at the academy in South Berwick, Maine. He further served an apprenticeship with Joseph Weightman, a maker of scientific instruments, going thence to the Sheffield Scientific School of Yale University, where he received the degree of Bachelor of Philosophy in the year 1858. He was deeply attached to his Alma Mater, and invariably spoke in terms of the highest praise and affection of his early teachers. His serious student days had, however, a bit of the modern in them, for if the writer remembers correctly, he was a member of the varsity crew in the year of his graduation. Some years later, in the autumn of 1869, he became professor of physiological chemistry and toxicology in the Yale Medical School, a chair created for him. During this period he served as expert for the state in several poison cases, the most noted being the Lydia Sherman case in New Haven.

His active scientific career may be said to have commenced about this time. Thus this Journal in 1967 (2, xliii, 252) contains a brief article "Upon the Silvering of Glass," which is a modification of a suggestion of Böttger, consisting in adding to a boiling solution of Rochelle salt a solution of argentic nitrate, after which the boiling was continued for eight or ten minutes, the liquid allowed to cool, and then filtered. A second portion of the original silver solution was treated with ammonium hydroxide until the precipitate formed was almost redissolved, after which water was added and the liquid allowed to cool, and then filtered. A second portion of the original silver solution was treated with ammonium hydroxide until the precipitate formed was almost redissolved, after which water was added and the liquid filtered. To silver glass, equal portions of these two fluids, thoroughly mixed, were poured upon it. After a lapse of about ten minutes, a brilliant layer of metallic silver was deposited. By repeating the process the layer could be thickened to any desired extent. A somewhat earlier article is entitled "Account of the Casting of a Gigantic (Rodman) Gun at Fort Pitt Foundry" (ibid., xxxvii, 296); this contains some important practical suggestions.

An interesting contribution is made by Dr. Barker, in the same Journal (2, xliv, 263, 1867), in support of the view that formic acid is carbonous acid. He believed this to be true because of the ready formation of formic acid by the partial oxidation of carbon, and also because it resulted from the oxidation of carbonic acid. For these reasons he further concluded that formic acid was the acid of bivalent carbon.

In an extended communication "On Normal and De rived Acids" (ibid., xliv, 384), he arrived at the following conclusions:

> "1. That all the bonds of any simple radical may be saturated by the monad hydryl (OH). 2. That the compounds this formed, being evidently normal, are conveniently designated by the prefix *ortho.* 3. That the equivalence of negative radicals varies through several stages, while that of positive rarely changes, and hence, that there may be a series of ortho-acids from a given negative radical, but only a single base from a positive one. 4. That by the removal of the elements of water from a normal or ortho-acid, a derived acid is produced, which may be indicated by the prefix *meta. 5.* That when there are several such derivatives, the Greek numeral prefixes di, tri, tetra, etc., may be used to indicate the number of molecules of water removed from the ortho-acid to yield the meta-form. 6. That intermediate between the simple ortho- and the meta-acids are others containing more than a single atom of the negative radical; and that these acids may be designated by di, tri, tetra, etc. (according to the number of negative atoms) prefixed to the name of the acid, while the number of molecules of water removed from a multiple of the normal acid to form them is indicated by the same numerals prefixed to the meta. 7. That while the negative atoms in the compounds just mentioned are united by oxygen, there may be other compounds whose negative or positive atoms are united directly; thus producing a fourth class of acids and of bases.
>
> "By classifying thus the substances known as acids and bases—and, of course, the salts derived from them—it is hoped that their relations to each other may be made clearer. And by giving them systematic names, their position in the series may be fixed, and a step taken toward the establishment of a national nomenclature."

In 1870 appeared his textbook of *Elementary Chemistry, Theoretical and Inorganic,* which ran through many editions as well as translations into other languages. This was the first book in our language in which modern chemistry was presented systematically. The style of the book, as so many can testify from its study, is concise and clear. Wolcott Gibbs spoke of it as "a book wholly in the spirit of the most advanced thought in the science."

During his life at New Haven, he contributed a note "On the spectrum of an Aurora which appeared at New Haven, November 9, 1871." This point of particular interest in this observation was the fact that the line of wave-length, 502, was not laid down in any authority accessible to the observer, as having been noted in the spectrum of the aurora. He adds: "Indeed, no previous observer, so far as I know, has seen any auroral line between the Frauenhofer lines *b* and F" (This Journal 3, ii, 465, 1871). Sometime later, he presented a second contribution "On the Spectrum of an Aurora of October 24, 1872." This aurora, like that of 1871, was distinguished by its radiant crimson color, and by its form. Dr. Barker remarks that in the lines that appeared in the spectrum, none was new, though no previous observer had seen all of them at once. Vogel had seen five and four had been seen by Dr. Barker. Two of the lines nearly coincided with the solar lines F and G, but a considerable difference was observed in the spectrum of the aurora of 1871.

Dr. Barker was assistant to Dr. Bacon in the Harvard Medical School from 1859 to 1861; professor of chemistry in Wheaton College, Illinois, 1861; then in the Albany Medical College, where he received the degree of Doctor of Medicine (1862-63), making while there a chemical examination of the viscera of a dead body, the first time it had ever been done in this country; next, in the University of Pittsburgh (1863); he also delivered the lectures on chemistry at Williams College in the years 1868 and 1869; and, after service in his Alma Mater, to which reference has already been made, he became professor of physics in the University of Pennsylvania (1872), where the remainder of his life was spent. At this period he published a contribution of considerable length, with the aid of illustrations, on "A New Vertical Lantern Galvanometer," in which claim is made for the general principles of construction of the instrument, and the advantages possessed by it in the readiness with which it could be put into use, the brilliancy of the illuminated circle of light which it gave upon the screen, its great range of delicacy by which all experimental requirements might be answered, and, finally, the satisfactory character of its performance as a demonstration galvanometer (Proc. Am. Phil. Soc., xiv, 440). This was followed by a communication "On the Measurement of Electromotive Force (ibid., xx, 649), in which the author states: "Having had occasion to make measurements of electromotive force by the method of comparison, I have been led to devise a form of standard cell, which appears to have advantages over others heretofore used as to justify me in bringing it before the Society."

In 1880, before the American Association for the Advancement of Science, at its Boston meeting, Dr. Barker, as retiring president, delivered an address upon "Some Modern Aspects of the Life-Question," from which the following paragraphs are introduced:

> "As Preston has suggested, if we regard this ether as a gas, defined by the kinetic theory that its molecules move in straight lines, but with an enormous length of free path, it is obvious that this ether may be clearly conceived of as the source of all the motions of ordinary matter. It is an enormous storehouse of energy, which is continually passing to and from ordinary matter, precisely as we know it to do in the case of radiant transmission. When potential energy becomes kinetic, the ether loses and the matter gains motion. When kinetic energy becomes potential, the lost energy of the matter is the motion gained by the ether. Before so simple a connection as this, both potential energy and action at a distance are easily given up. All energy is kinetic energy, the energy of motion. Giving now to the ether its storehouse of tremendous power, and giving to it the ability to transfer this power to ordinary matter upon opportunity, and we have an environment compared with which the strongest steel is but the breath of the summer air. In presence of such tremendous power do we act. Is it

> a wonder that out of such a reservoir the power by which we live should irresistibly rush into the organism and develop the transmitted energy which we recognize in the phenomena of life? Truly, as Spinoza has put it, 'Those who fondly think they act with free will, dream with their eyes open.'
>
> "Such are now the facts and theories to be found in the science of today considering the phenomena of life. Physiologically considered, life has no mysterious passages, no sacred precincts into which the unhallowed foot of science may not enter. Research has steadily diminished day by day the phenomena supposed vital. Physiology is daily assuming more and more the character of an applied science. Every action performed by the living body is sooner or later, apparently, to be pronounced chemical or physical. And when the last vestige of the vital principle as an independent entity shall disappear from the terminology of science, the word 'Life,' if it remain at all, will remain only to signify, as a collective term, the sum of the phenomena exhibited by an active organized or organic being."

In following the career of our friend there is plainly seen a versatility on his part, as well as a keen interest for other branches of science than that one to which he gave the best years of his life. Thus, he is found a member of an expedition to Rawlings, Wyoming, for the purpose of reporting "On the Total Solar Eclipse of July 29, 1878"; his particular duty being to observe with an analyzing spectroscope the presence, either of light, or of dark (Fraunhofer), lines in the spectrum of the corona. (See Proc. Am. Phil. Soc., xviii, 1880.) Again, in connection with Professor Rowland, he reported "On the Efficiency of Edison's Electric Light." See this Journal 3, xix, 337.

Dr. Barker was the first person to exhibit radium in this country (1894) after its isolation by Madame Curie in Paris. Radioactivity appealed so strongly to him, that it is not surprising to find a paper of his on "Radio-activity of Thorium Minerals" in this Journal (4, xvi, 161, 1903). In this communication, the author introduces a number of original contributions. He repeated the experiments of Hofmann and Zerban, relative to the radioactivity of Brazilian monazite, which contains no uranium, and confirmed the results of these observers, to wit: that the thorium from this monazite is probably radioactive. From a series of experiments, he further concluded that thorium emanation rapidly decays, falling to one-half its value in one minute, while that of the radium emanation retains its active properties for several weeks. On the other hand, the excited radioactivity produced by the former emanation is much more prominent than [that] produced by the latter. Since excited radioactivity can be produced on bodies if the emanation be present, even in the absence of a radioactive substance, and since the amount of emanation, it follows, first, that the production of excited radioactivity is a property of the emanation, and, therefore, is also produced in bodies where the radioactive emanations from thorium and radium are present: and second, that uranium and polonium, which do not give forth any emanation, do not possess the power of exciting radioactivity. In the present view of science, therefore, it would not be probable that the radioactivity of thorium is a secondary or excited radioactivity due to the uranium associated with it in the minerals previously named.

A very instructive address upon "Radio-activity in Chemistry" was delivered by Dr. Barker before the Chemical Society of Columbia University; it appeared in full in the School of Mines Quarterly (xxiv, 267). It has historical value, and will prove helpful to all wishing to familiarize themselves with the subject. It is accomplished with bibliographies covering ninety titles by the most prominent investigators in this particular field of research. In 1889, Harper and Bros. issued a small volume of seventy-five octavo pages on "Röntgen Rays," in which are incorporated memoirs by Röntgen, Stokes, and J. J. Thomson, translated and edited by Dr. Barker.

On the 27th of May, 1893, the American Philosophical Society celebrated the 150th anniversary of its foundation, on which occasion Dr. Barker offered a paper on "Electrical Progress since 1743." This paper is a review of the advances in physics since that early date, emphasizing in particular the contributions to electrical science by such persons as the immortal founder of the Society, Benjamin Franklin, and by Kinnersley, Robert Hare, Joseph Henry, Joseph Saxton, David Rittenhouse, and Alexander D. Bache. "The labors of these men have mightily contributed to advance the development of scientific thought throughout the world, and so to bring about that exceptional evolution of electrical facts and theories which is the distinguishing feature of the science of the nineteenth century." This little brochure is indeed worthy of study by every student of the physical sciences.

Still other communications of Dr. Barker are "On the Henry Draper Memorial Photographs of Stellar Spectra" (Am. Phil. Soc., ssiv, p. 166), "On the Use of Carbon Bisulphide in Prisms" (this Journal 3, xxix, 269), in which communication there is presented to the public the observations of his friend, Dr. Henry Draper, taken from the notes of the latter after his death; and "The Microphone of Hughes" (ibid. 3, xvi, 60), in which Dr. Barker takes occasion to say that the results obtained by Hughes had been clearly anticipated by more than a year by those of Edison.

Biographical memoirs of Frederick Augustus Genth, of Henry Draper, of John William Draper, and of M. Carey Lea were written for the National Academy by Dr. Barker, and he also prepared for the Smithsonian Institution annual reports upon physics from the year 1881 to 1885, inclusive. These amount to 253 pages and represent the most recent advances in the science during these years.

From 1868 to 1900 he was associate editor of this Jour nal, and the abstracts of chemical and physical papers which he contributed regularly during this period are remarkable for their clearness and accuracy. In 1874-75 he was editor of the journal of the Franklin Institute.

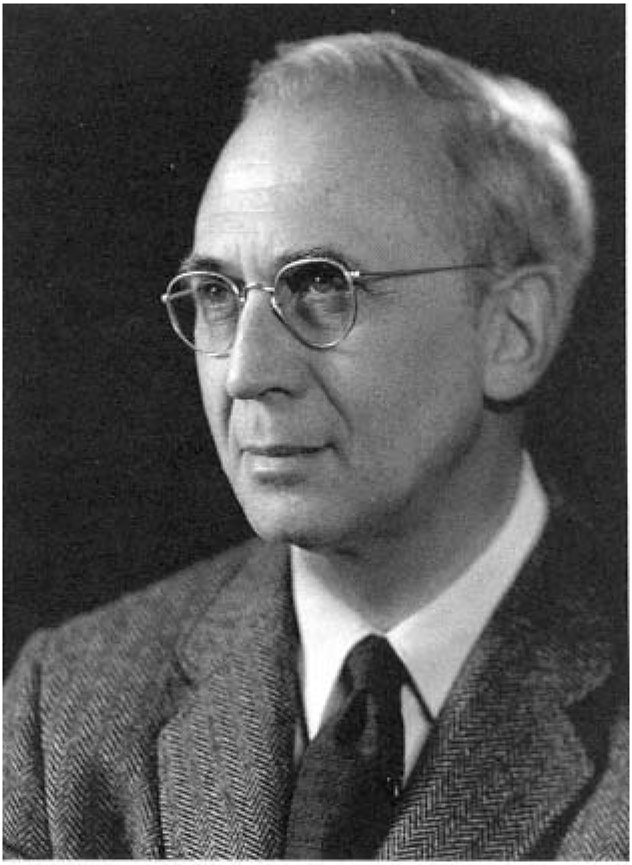
In 1892 appeared his "Physics, Advanced Course" from the press of Henry Holt & Company, which immediately met with a most hearty reception and became a standard among the textbooks on this important subject.

It follows naturally that to one so active in the scientific world there should have been awarded numerous honors. Thus, in 1881, Dr. Barker was United States Commissioner to the Paris Electrical Exhibit, a delegate to the Electrical Congress, and vice president of the Jury of Awards, receiving the decoration of Commander of the Legion of Honor in France; in 1884, he was U. S. Commissioner to the Electrical Exhibit in Philadelphia; and in 1893, a member of the Jury of Awards of the World's Columbian Exposition. He was an active member of the National Academy of Sciences, serving on many of its important committees, and also of the American Association for the Advancement of Science, of which he was vice president twice, delivering on one of these occasions an address on "The Molecule and the Atom," a most valuable contribution to theoretical chemistry, and president in 1879; his presidential address in 1880 has already been referred to. He was a corresponding member of the British Association. He was president of the American Chemical Society (1891), the subject of his presidential address being "The Borderland between Physics and Chemistry." He was secretary and later vice president of the American Philosophical Society from 1899 until 1909. He was a member of the Physical Society and of the Deutsche Chemische Gesellschaft. In 1899 he became an honorary member of the Royal Institute of Great Britain. He was the recipient of the following academic honors: Doctor of Science from the University of Pennsylvania in 1898; LL.D. from Allegheny College in 1898; and LL.D. from McGill University in 1900.

He became Emeritus Professor of Physics in the University of Pennsylvania in 1900. He was a member of the Century Club of New York and the University Club of Washington.

In 1861, Dr. Barker was married to Mary M. Treadway of New Haven, Connecticut, who with three daughters survive this devoted and loving husband and father.

Dr. Barker was born at Charlestown, Massachusetts, July 14, 1835, and died at Philadelphia on May 24, 1910. His was a beautiful life, so full of service to his fellow men and so rich in its achievements that it will ever remain a most precious memory to his many friends.



William Bloom

WILLIAM BLOOM

September 15, 1899-May 11, 1972

BY RONALD SINGER

WILLIAM ("BILL") BLOOM was born in Baltimore, Maryland, on September 15, 1899, the eldest of the four children of Mayer and Bertha Singer Bloom. Both parents had emigrated from Lithuania in the 1880s, settling in Baltimore—Mayer Bloom at the age of 17, Bertha Singer as a child of 8, accompanying her parents and brothers. Mayer established a wholesale business in women's clothing, and both he and his wife were interested in civic affairs and were active in the Jewish community of Baltimore. Typically, they had a high regard for learning and a great love of books, traits which they transmitted to all of their children. In fact, by the time Bill was fifteen, his father had given him the right to charge any book he wanted from any store in Baltimore. Bill's brother, Benson, received an M.D. degree from Johns Hopkins and became a practicing physician in the Southwest, while his other brother, Frank, was a lawyer for the National Labor Relations Board in Washington, D.C. Their sister, Sophia, obtained a Ph.D. from the School of Social Service Administration at the University of Chicago.

Bill attended the Friends School, Baltimore City College (a municipal high school), and the Johns Hopkins University. Following brief service in the U.S. Army (Students'

Army Training Corps) in World War I, he returned to Hopkins, where he received his A.B. degree in 1919 and M.D. in 1923. In his first year of college there were three books which, he said, greatly influenced his thinking, namely, Huxley's *Man's Place in Nature,* Haeckel's *The Riddle of the Universe* and Moore's *The Literature of the Old Testament,* the first two were largely responsible for his electing biology courses in college, while the third was his first acquaintance with a critical interpretation of the Bible and "finished the last tenuous vestiges of organized religion in me." However, in later life he never denied or tried to hide his religious ancestry. I recall a conversation with him in the early 1960s when we were discussing prejudice, particularly antisemitism in academia; Bill noted that he believed he was the first Jew to be appointed chairman in an American anatomy department.

Bill Bloom enjoyed his college years. His interest in and mastery of both French and English literature prompted the instructor in each to encourage him to major in that subject and continue to graduate school. However, it was the introductory course in biology in his second year, taught by Professor E. A. Andrews, that provided the determining factor for the course of Bill's life as far as his field of endeavor was concerned. This was the first instructor he encountered who made students think and question. In the comparative anatomy course the following year, Andrews showed the students material, provided some pertinent facts, and left them to draw their own conclusions. Andrews encouraged Bill to use the department's photomicrographic apparatus, and in 1918 he offered Bill a summer fellowship to work at Cold Spring Harbor, but he could not accept as he had to go to Plattsburg for the Reserve Officers' Training Corps. Before graduating, Bill told Professor Andrews that he wished to study for a Ph.D. in biology, but Andrews advised him (and others) that there were very few opportunities in research in biology but there were more in medical schools. In the fall of 1919 Bill entered the Johns Hopkins University Medical School.

In the first semester, Bloom was impressed by Professor Florence Sabin's histology course, which comprised laboratory work mainly and a total of eight lectures on scientists and their work. At least two of these were to play important roles in his later development. The first lecture was on connective tissue and Alexander A. Maximow, who, Sabin announced, was the greatest histologist in modern times, but "he had died in the Russian Revolution of 1917." One of Bloom's most treasured letters was that from Dr. Sabin in 1930 (then at the Rockefeller Institute for Medical Research) in which she thanked him for the copy of the first edition of *Textbook of Histology.* She recalled her first lecture of 1919 and stated that "it is clear no one can afford to be without [the textbook]" and "you are certainly to be congratulated tremendously on this very beautiful text of histology, which combines all of Maximow's splendid grasp of the subject … with your own experience which you have gained through association with such a master." The second scientist mentioned in the course was Robert R. Bensley, also of the University of Chicago. Sabin distributed some of Bensley's slides on mitochondria, stating that if he had done nothing else but work on the pancreas he would have been a great scientist. Later, he provided the "open sesame" to the Department of Anatomy at the University of Chicago, of which he was chairman and which was probably one of the greatest departments of anatomy during the 1930s and 1940s.

During that first year Bloom pondered on and discussed with various faculty members some research problems, but none came to fruition. That summer he injected rats with various drugs and trained them to run in Watson's maze for Dr. D. I. Macht, a lecturer in Professor Abel's Department of Pharmacology. Macht used the data in several papers and in a number of preliminary notes on work in which Bloom was mentioned as the junior author. However, what Bloom relished most that summer were the daily luncheons in the laboratory with Abel and his staff; discussions ranged over all sorts of topics from current events, to great and not-so-great books in world literature, to items of science. Abel wanted Bloom to take over the full-time work that Father Roca, who had to return to Spain, had started with Abel on histamine-like substances in the pituitary stalk and hypothalamus. But in those days medical students did not drop out of school for a while to do research, and so Abel asked Bloom to come back when he completed the M.D.

In his second year, Bloom began his first independent research to test his idea that histamine might be the cause of inflammation. He worked in the pathology laboratory of Dr. Arnold Rich, and he published his negative results in *the Johns Hopkins Hospital Bulletin* (34:165-88, 1922). In the summer of 1921 Bloom went to Europe on a cattleboat with Alan and Manfred Guttmacher. They visited a number of renowned professors in medical institutes and clinics in Germany and France. Bloom particularly enjoyed pursuing his interest in the history of science, and he purchased nearly 600 old scientific books and journals, such as by Semmelweiss, Laennec, Corvisart, Bidloo, Morgagni, and Hunter, the most valuable of which he later gave to libraries (e.g., Crerar Library, University of Chicago libraries, and Einstein Medical School in New York).

Although his third year in medical school consisted largely of lectures, clinics, and dispensaries every day, Bloom managed to do some research on experimental adhesions of peritoneum in dogs. That summer he worked as a substitute intern in medicine, obstetrics, and surgery at Michael Reese Hospital in Chicago. Little did he realize then that most of his life would be spent in that city.

At the beginning of Bloom's fourth year, Dr. Rich returned from Eppinger's laboratory in Vienna and busied himself with work on jaundice. He suggested that Bloom should repeat experiments on the old problem of the route of absorption of bile pigments from the liver in early obstructive jaundice, using the new Van den Bergh test for bilirubin. Bloom also added the use of Eppinger's stain of bile capillaries to see if they ruptured in this condition. He found an unequivocal increase in bilirubin in the lymphocytes long before it increased in the blood and also that the bile capillaries were not ruptured (*Johns Hopkins Hospital Bulletin* 34:316-20, 1923).

After graduation in 1923, Bloom went to Chicago as the first resident pathologist of the Michael Reese Hospital under Dr. O. T. Schultz, who Bloom considered excellent, especially in microscopic diagnosis. Bloom performed many autopsies, did much surgical pathology, and continued experiments on obstructive jaundice. While comparing bilirubin in the blood with blood levels of phenoltetrachlorphthalein (*Archives of Internal Medicine,* 1924), he found a new method for measuring the latter in blood serum containing much hemoglobin. During the next two years he worked out the early stages of the embryogenesis of human bile capillaries (*American Journal of Anatomy,* 1926) and completed a long paper on the histopathology of Gaucher's and Niemann's diseases *(American Journal of Pathology,* 1925) based on two cases of the former and three of the latter. Also, with Ms. Kern, he analyzed lipids extracted from the spleens of the latter and described these findings in some detail along with the histochemistry of the lipids *(Archives of Internal Medicine,* 1927).

Bloom showed his slides of embryonic bile capillaries to Dr. G. Bartelmez at the University of Chicago, who suggested that Maximow would undoubtedly like to see them. Bloom later described the meeting with Maximow as the most exciting encounter with a scientist he had ever had. Two weeks later Bloom phoned for an appointment to show him histological and cytological changes in the lipid storage diseases he had studied. Maximow spent four hours examining the sections (even forgoing his routine afternoon stroll), and, when he was leaving, Bloom asked if he could work with him. At first Maximow refused, but when Bloom offered to do a Ph.D. under him, he was advised to discuss it with Bensley. The latter indicated that Bloom did not need a Ph.D. if he had an M.D. from Hopkins, and he suggested that Bloom should register for one course in research in anatomy. He was also warned that Maximow was an unbelievably hard taskmaster.

Some months later Bloom was called home because his father was terminally ill. When he returned to Chicago two weeks later he had decided that he could not do effective research under the primitive conditions and equipment at Michael Reese. Maximow offered him the Douglas Smith Fellowship and provided space in his laboratory with a new microtome and a new microscope. Bloom resigned from Reese to the annoyance of Schultz, who had counted on Bloom being his successor.

Bloom's introduction to experimental hematology resulted in the publication of "Transformation of Small Lymphocytes into Myelocytes in Germinal Centers" (*Folia Haematologica* 33, 1926). Then he began tissue culture experiments with immune reactions *(Archives of Pathology* 3, 1927) and also work on lymphocytes of the thoracic duct, extensive stud ies on the origin of monocytes, and further papers on lipid tissues.

Until this time, the two greatest influences in Bloom's scientific development had been E. A. Andrews 'and J. J. Abel. Then came almost four years with Maximow (February 5, 1925, until his death on December 4, 1928), who taught him many rudiments of laboratory technology. This experience and his subsequent association with Robert R. Bensley and C. Judson Herrick in the Anatomy Department exposed Bloom to three of the foremost histologists of the time.

In 1929 Bloom was appointed assistant professor of anatomy, then associate professor in 1933, professor in 1941, and the Charles H. Swift Distinguished Service Professor in 1957. He served as chairman of the Department of Anatomy from 1941 to 1946. He was also a member of the Institute of Radiobiology, 1946-54, and a member of the Committee on Biophysics (later the Department of Biophysics, in the establishment of which he played a major role), 1954-69. He assumed emeritus status in 1965 and retired in 1969.

Dr. Bloom was also a member of the Executive Committee of the American Association of Anatomists, 1946-50, and served as vice-president, 1952-54. He was a member of the National Academy of Sciences, the American Society of Experimental Pathologists, and the American Society of Cell Biology and was a founding member of the International Society of Cell Biology. He was one of three Americans awarded honorary doctoral degrees at the 600th anniversary of the Jagellonian University, Cracow, Poland, in 1964.

When Bloom began to work with Maximow, the latter was the leading proponent of the unitarian theory of the origin of blood cells, which held that all types of blood cells derive from a common stem cell that he identified as the lymphocyte. The circulating small lymphocyte of the blood was considered to be a hemopoietic stem cell (hemocytoblast) in a resting condition. Under certain conditions after migration into the tissues, the small lymphocyte was believed to hypertrophy to form a large lymphocyte or hematocytoblast, which, in turn, was capable of giving rise to the precursors of erythrocytes, granular leukocytes, and megakaryocytes. At that time American hematology was dominated by dualistic or polyphyletic theories of hemopoiesis, which insisted that there is no common totipotential "stem cell" but that the erythrocytes and leukocytes came from distinct precursor or stem cells and that the lymphocyte was a fully differentiated cell with no potentiality for further development except into plasma cells. This school of thought was led by Florence Sabin and her co-workers at the Johns Hopkins School of Medicine. Despite Bloom's exposure to the latter during his student years, he rapidly gained a great admiration for Maximow's technical skills and his deep insight into the relationship of cells of the blood to those of the connective tissues. Bloom's first paper on the hemopoietic potency of the small lymphocyte was followed by a series of penetrating studies on the origin and nature of the monocyte and on the behavior of lymphoid and blood-forming organs in tissue culture. These studies strengthened his conviction as to the multiple developmental potentialities of the lymphocyte. With the untimely death of Maximow at the age of fifty-four, Bloom became the principal advocate of the unitarian theory, and he rapidly gained an international reputation in morphological hematology. His investigations promulgating and extending Maximow's views were presented in *Handbuch der Allgemeinen Hamatologie* in 1932 and Downey's *Handbook of Hematology* in 1938 (see Selected Bibliography). He had a profound influence upon clinical hematology through both his writings and his students.

Bloom was critical of the almost exclusive reliance of many investigators upon dry smears and insisted that no single technique was sufficient—and, indeed, that the sum of all the techniques then available was insufficient to answer the central questions of morphological histology. In his own work, he made skillful use of the light microscope and selective stains for observations on living and fixed cells. By experimental induction of inflammatory reactions and of extramedullary hemopoiesis, he took advantage of pathological conditions to shed light on the normal origins of cells and their potentialities for transformation to other cell types. His belief that lymphocytes can transform into macrophages and into fibroblasts has not been validated, but some of the central tenets of the unitarian concept have withstood the test of time. Now some fifty years later, the capacity of small lymphocytes when stimulated by lectin or antigen to undergo hypertrophy and proliferate has been clearly demonstrated, and its significance for the immune response has been established. The development from single stem cells of spleen colonies containing both erythro- and myelopoietic cells has firmly established the validity of the unitarian concept of hemopoiesis. Bloom's observations in experimental embryology and pathology contributed to an understanding of the origin, structure, and function of the blood-forming tissues, which he summated in *Physiological Reviews* in 1937.

In the 1930s, Bloom collaborated with William H. Taliaferro in studying inflammatory and cellular immune reactions in monkey and avian malaria. With Clay G. Huff he described the hemopoietic cells of canaries infected with a malarial parasite. With George W. Bartelmez he reported on the embryogenesis of the blood cells of the human yolk sac.

During this same decade William and Margaret Bloom collaborated with Franklin G. McLean, carrying out extensive studies on bone and its development, many in egg-laying birds. They were the first to demonstrate the importance of estrogens in laying down of bone minerals in bone metabolism. They recorded the transformation of osteoblasts into osteoclasts. Bloom also performed many experiments with Morris Kharasch, the most interesting of which turned out to be on iron and manganese catalysis and bacterial growth and the production of pigments in bacteria by varying the types of aldehydes and ketones in the growth media. For several years he worked with Percival Bailey and Roy Grinker; with the former he cultured brain tumors and with Grinker he studied experimental aseptic inflammation in the brain.

At the time of his death, Maximow was writing a textbook on histology. A few chapters had been completed, but many were in rough manuscript or still in Russian. His colleagues in the Department of Anatomy "felt very keenly the desirability of seeing the book completed," and, at the suggestion of Bensley, Bloom undertook the unenviable task. The details of how this was done are presented in the preface of the first edition of the now famous Maximow and Bloom's *Textbook of Histology,* as well as in the insightful paper by John L. Dusseau in *Perspectives in Biology and Medicine* (30:108-16, 1986). After seven English editions, Bloom was joined by Professor Don W. Fawcett, the distinguished anatomist and cytologist of Harvard University, for the eighth and ninth editions of this authoritative reference. It has been translated into many languages, including Spanish, Portuguese, and Korean. Subsequent editions have been rewritten in part and edited by Fawcett only.

During World War II, under the auspices of the Committee on Medical Research, Bloom collaborated in testing the effects of vesicant war gases on animals and men and the effectiveness of protective ointments and clothing. Commander J. Troxel acted as his liaison at the Glenview Naval Station. Later in the war, working on the Metallurgical Project under the Manhattan District, Bloom assembled a research team to assess the biological effects of irradiation. Their findings were published (with Bloom as editor) after the war (1948) as volume 22-I of the National Nuclear Energy Series, titled *Histopathology of Irradiation from External and Internal Sources.*

After the war, Bloom collaborated with Raymond E. Zirkle in examining the effects on dividing cells of irradiation with a proton microbeam. Changes in the cells were followed by means of phase-contrast microscopy and recorded in time-lapse motion pictures. This study of the mitotic process in newt fibroblasts in tissue culture and the effect of focal irradiation (using an ultraviolet microbeam) of parts of chromosomes or the spindle was subsequently extended to electron microscopic analysis of changes in individual cells that had previously been irradiated. Bloom's last publication (1970) described electron microscopic observations of unirradiated mesothelial cells throughout the mitotic cycle in vitro.

In his rough autobiographical notes, Bloom proudly states, "No account of my last 40 years' work would be complete without consideration of the contribution of my wife who has worked devotedly on many problems with me in the laboratory and has also helped me with the books and many of the papers which I have written." In addition to several publications of her own, Bloom's wife, Margaret, shared responsibility for carrying out and reporting a number of experiments.

Margaret Abt was born in Chicago in 1898, the daughter of Solomon Lincoln and Clara Abt (nee Hirsh). Margaret first met Bill in 1923 (shortly after his arrival in Chicago) in the home of a cousin who had been his classmate at Johns Hopkins. She was working toward her Ph.D. in home economics and had taken a histology course taught by Maximow, as well as a physiology course. She became interested in Bill's work and began assisting him in his laboratory. Then she dropped the idea of finishing her Ph.D. They were married in 1928.

During the 1930s, Bloom and the physiologist Ralph Gerard were instrumental in revising the medical curriculum at the University of Chicago, pioneering the provision of an elective quarter for freshmen to stimulate research interests. Bloom had great awareness of the significance of the physical sciences for biology. He was a consultant to the Argonne National Laboratory, and he played an important part in establishing biophysics as a discipline at the university. He was known as a master teacher, for his unexcelled knowledge of his subject and for his insistence on painstaking work in a lifetime of teaching at the university. Bloom placed great emphasis on laboratory teaching in his courses, and the student loan collection of slides covered most of the sources of the illustrations in his text. The students were personally taught to look for the source of the textbook statement and not to have it found for them in kodachromes.

Bloom directed the research of many young scientists on whom his meticulous techniques, incisive thinking, and broad scholarship had lasting effects. His graduate students included Roscoe McKinney, Clayton Loosli, Leo Clemente, Raymond Murray, Matthew Block, Eleanor Conway, and Minnie Heller. In addition, dozens of medical students worked laboriously in his laboratory, while many postdoctoral fellows and colleagues from many parts of the world carried out investigations under his guidance in his laboratory.

Bloom was able to rescue several anatomists and their families from Hitler's Europe by procuring funds or positions for them in the United States. Among them was Franz Weidenreich, gross anatomist, histologist, and physical anthropologist (famed for his discoveries of and research on the so-called Peking Man fossils), who Bloom stated was "one of a few great men of the older generation whom I had the privilege of knowing intimately." He brought Fritz Wasserman from Germany to teach gross anatomy and Peter De Bruyn from Holland to teach histology in the Department of Anatomy.

Bloom's scientific activities left him with little leisure time. He was a devoted listener of recorded music and collected classical records. He regretted that he had neither learned to play a musical instrument nor read music. An enthusiastic amateur painter in oils, he preferred copying old masters rather than painting original pictures. He read widely in general literature, biographies, history of modern times, and the history of science. He had a fine collection of old books on the history of biology and medicine, most of which he later donated to libraries.

Like so many of his friends, I am immeasurably richer for having known "perhaps the last great general histologist" (W. L. Doyle in *The Anatomical Record* 177:108-9, 1973) in the tradition of Giuseppe Levi and Weidenreich.

I AM INDEBTED TO Margaret Bloom, Don W. Fawcett, and William L. Doyle for numerous documents and verbal information from which I have extracted liberally. I had useful discussions with Humberto Fernandez-Moran, who is particularly grateful to William Bloom for bringing him to Chicago.

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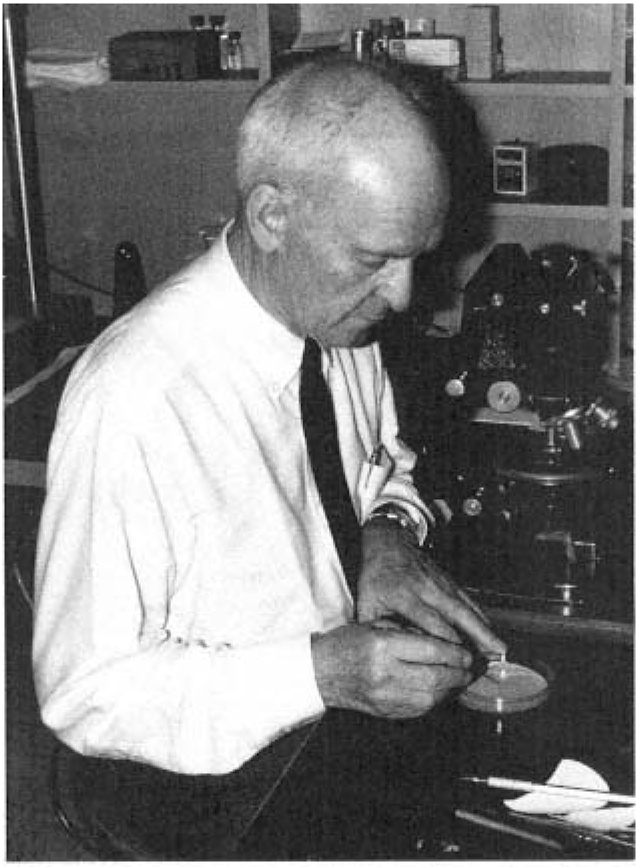

J. N. Couch

JOHN NATHANIEL COUCH

October 12, 1896-December 16, 1986

BY PAUL J. SZANISZLO

"HAVE YOU SEEN our latest book?" The question came from John Couch, my graduate professor and mentor from some twenty years earlier. This man was ninety years old, and he was signing and presenting me with a publication that was hot off the press! As we sat in his comfortable living room that pleasant afternoon in Chapel Hill, we discussed the book and his thoughts for the future. He asked about my work and students, and he questioned my daughter, who was entering the University of North Carolina as a freshman, to ascertain whether or not her father had given her an ample foundation in mycology!

I was amazed at his sharpness and his continuing interest, and I thought as he talked how extensive this man's influence has been in his lifetime. He taught his first students in 1919, and here he was, nearly seventy years later, ready to search anew for a spark of interest in a college freshman. Indeed, his time and accomplishments span an even greater distance.

From his birthplace in Prince Edward County, Virginia, in the fall of 1896 to his final resting place in the Old Chapel Hill Cemetery in the closing days of 1986, John Nathaniel Couch's life journey took him across the southern United States, through a Europe at war, to Long Island

and a young woman destined to share his life, to the steamy forests of the Caribbean and the cooler American Midwest, and back to his beloved South. In each of these destinations, another facet of John N. Couch—the gentle man, the curious scientist, the diligent teacher—would develop. A curiosity was sparked, and he followed it. The trail that he blazed during that life's journey leaves a plethora of knowledge about fungi from which the scientific community will continue to benefit and which Couch's students, and theirs, will carry through research and teaching into the next century. By then, the journey and influence of John Couch will have touched at least three centuries.

John Couch contributed to a broad spectrum of professional activities during his long and distinguished career: research, teaching, administration, and service to professional organizations. He believed that one area related to the other and that activities in one benefited all. However, he is probably best known for his research in mycology through his work with numerous diverse fungi. His earliest major contribution was born out of his Ph.D. research in which he described for the first time the existence of physiologically distinct and separate male and female strains in an oomycete. In subsequent major research, Couch described, with his mentor W. C. Coker, the Gasteromycetes of the eastern United States and Canada.

He then moved on to do extensive work with *Septobasidium,* a large genus of fungi which previously were thought harmless to the trees on which they grew and, in fact, beneficial because members destroyed infestations of scale insects on the trees. Couch found quite the opposite, that not only did *Septobasidium* and the scale insects have a mutually reliant relationship, but that together they destroyed the host tree. His treatise on *Septobasidium,* published in 1938, may remain to this day the most definitive contribution related to these fungi and their symbiosis and pathogenesis.

All types of fungi were of interest to John Couch, but particularly his interest in the aquatic fungi led to his inadvertent discovery of a new group of bacteria. He eventually established this group as a family, the Actinoplanaceae, which he included in the bacterial order Actinomycetales. This pioneering work at first appeared to establish a link between the "higher bacteria" and the "lower fungi," but Couch remained skeptical. Later research conducted in his laboratory proved that, although morphological similarities exist between Actinoplanaceae and some fungi, the similarities are superficial and only reflect the parallel evolutionary trends that created the sporangial bacteria and fungi.

In his later years, John's major research emphasis involved the potential role that fungi of the genus *Coelomomyces* might serve in biologically controlling mosquitoes by parasitizing and killing their larvae. The possibility of controlling malaria and other mosquito-borne diseases with fungi held exciting prospects for a devoted bench scientist. His nearly fifty-year fascination with these fungi, commencing with studies in the 1940s and his early recognition of their blastocladiaceous affinities, culminated in 1985 with his last major research contribution, the publication of the edited volume with Charles E. Bland, *The Genus Coelomomyces.*

Although deeply involved in research, Couch also found time to be a conscientious teacher and to serve his university, state, and nation. He first taught general biology at the secondary level and then botany and mycology at the university level for more than forty-five years. During this time he was recognized with numerous awards for his teaching abilities and dedication to students. Forty students received graduate degrees under his tutelage. During a major portion of this same time, Couch also served the University of

North Carolina as department chairman from 1944 to 1960. His success in this capacity, and the continuous support provided by his colleagues during such a long period of leadership, attests to the personal and professional traits of this gentleman and scholar.

In these very active years, John Couch still found time and energy to serve several professional organizations as an officer, chair, or editor. He served as president of the Elisha Mitchell Scientific Society; secretary-treasurer, vice-president, and president of the Mycological Society of America; president of the North Carolina Academy of Sciences; vice-president of the Botanical Society of America; and chairman of its southeastern section. He also served as associate editor of *Mycologia* and as editor of the *Journal of the Elisha Mitchell Scientific Society* and was on the editorial board of *Mycopathologia et Mycologia Applicata.* His scholarship and research activities led to a variety of other honors, including his election to membership in the National Academy of Sciences (U.S.A.) in 1943, being named Kenan Professor of Botany at the University of North Carolina at Chapel Hill in 1945, and being elected in 1955 an honorary foreign member of the National Academy of Sciences of India.

THE EARLY YEARS: FAMILY AND EDUCATION

John Nathaniel Couch was born in Prince Edward County, Virginia, on October 12, 1896, to John Henry and Sally Terry Couch. His father was a Baptist minister and his mother a teacher. One of seven children, John's early education was influenced at home by his mother, a disciplined and aggressive teacher, and at seven different public schools—his father following the calls of Baptist churches throughout several southern states. By John's high school years, the Couches resided in Chapel Hill, North Carolina, from where he traveled to Durham to attend high school.

Upon graduation in 1914, Trinity College in Durham (later to become Duke University) admitted John and, under a common practice at the time, provided a tuition-free education to him as the son of a minister. So, in the fall of 1914, as his parents moved on to another call, John the freshman moved in with an uncle in Durham and, with some financial help from home, began his higher education. For two years he studied mostly classical subjects—literature, history, language, and mathematics. He read widely and, like most college freshmen and sophomores, pondered where his interests and abilities lay.

After narrowing his choices to law and medicine, John, during his third year at the university, had his first major exposure to natural science while studying biology and chemistry. His curiosity was awakened in Professor J. J. Wolfe's botany class and by a subsequent invitation to join the Biology Journal Club. A precursor of what ultimately would be John Couch's passion came with his first report to that club, "Edible and Poisonous Fungi." His interest in botany had become so keen that he asked to work in Professor Wolfe's laboratory for the summer, where his time was spent collecting and identifying freshwater algae under Wolfe's direction.

His attraction to science now clear, John dropped thoughts of a career in law and transferred for his senior year to the University of North Carolina (UNC) in Chapel Hill. His goal was to prepare for admission to its medical school. However, at UNC his path crossed that of another botanist, this time the eminent botanist and mycologist W. C. Coker, whose work with fungi fascinated Couch. John's decision was made. Medicine, like law before, was no longer his choice. He would continue his education in graduate study with Professor Coker, and mycology would henceforth be forever enriched.

THE INTERMEDIATE YEARS: GRADUATE TRAINING

Battlefield, Laboratory, Classroom

Events an ocean away deterred Couch's immediate plans for graduate school. World War I and Uncle Sam called. Service as a private with Company B, 56 Pioneer Infantry, took him to Belgium, France, and Germany from August 5, 1918, to July 27, 1919. However, while waiting to be mustered out and sent home after the Armistice, he managed to spend four months studying botany at L'Université de Nancy in France.

Upon his return to Chapel Hill in 1919, John began his formal graduate work at UNC under Dr. Coker's supervision. In order to pay for and while continuing his studies, he also taught science at Chapel Hill High School and the following year at Alexander Graham High School in Charlotte. After finishing a master of arts degree in botany in 1922 with his thesis, "Spore Formation and Discharge in Some Genera of Water Molds," Couch became an instructor in the UNC Botany Department as he continued his Ph.D. studies. Thus began Couch's faculty association with the university and the department that was to continue for over half a century.

The doctoral degree was conferred two years later. Couch's dissertation, "Sexual Reproduction and Variability in the Genus *Dictyuchus,"* included the report of his discovery of the mode of sexual reproduction, called heterothallism, in the water mold *Dictyuchus.* Some believe that his dissertation may have been his most significant contribution to mycology. In it he described for the first time separate "male" and "female" strains in *Dictyuchus.* While studying the physiology of sex in some members of this genus, he observed that the male branches were attracted over relatively long distances by the female. This observation ultimately led to the discovery of sex hormones in Oomycota by John R. Raper, one of Couch's first graduate students, whom Couch introduced to *Achlya.* Later still, hormone A, stored for years after Raper's initial research, was characterized as the first steroid hormone of nonanimal origin by another of Couch's students, Alma Wiffin Barksdale, and by Trevor McMorris at the New York Botanical Garden.[1] The original work of Raper on *Achlya* contributed to his election to the National Academy of Sciences. The chain of events in this research exemplifies the multiplier effects of Couch's observations and the span of his influence.

Except for the summer of 1923, when Couch studied with Professors E. M. Gilbert and C. E. Allen at the University of Wisconsin, all of his graduate work was done with Dr. Coker. The culmination of their years of collaboration, *The Gasteromycetes of the Eastern United States and Canada,* was published in 1928, although it is clear that as an instructor with Coker, Couch was an important contributor to *The Saprolegniaceae,* published in 1923. It was probably this latter effort that most imbued Couch with a lifelong love of the aquatic fungi. His initial work with *Dictyuchus* was so revolutionary for oomycetes at that time that he was awarded a National Research Council fellowship for postdoctoral work under the direction of A. F. Blakeslee at the Carnegie Institution at Cold Spring Harbor, Long Island, New York, for one year and with B. M. Duggar at the Missouri Botanical Garden in St. Louis for an additional year. The stint with Blakeslee was particularly appropriate because it was Blakeslee and his colleagues who first showed just after the turn of the century that Mucorales were strictly homothallic or heterothallic. With Duggar he most assuredly was introduced to the intricacies of spore dormancy and germination phenomena.

In the summer of 1926, between his two postdoctoral appointments, Couch jumped at the chance to spend two months in Jamaica, British West Indies, with the Johns Hopkins Botanical Group directed by Duncan S. Johnson. Typical of his curiosity and love of observation, he wanted to go just to see what fungi he could find. It was there in the steamy forests of Jamaica that Couch became interested in *Septobasidium,* a group of fungi found in abundance growing on trees heavily infested with scale insects. Previous to Couch's intrigue with the characteristics of these fungi, the commonly accepted assumption was that species of *Septobasidium* killed scale insects, thereby preventing them from destroying the trees they infested, and that the phenomenon was unique to the tropics. Couch's research described, however, a mutually beneficial relationship between the fungus and the scale insects, which combined to destroy the host tree or shrub. He also demonstrated that this pathogenic and symbiotic existence was widespread beyond the tropics into temperate climates as well. His findings were published in 1929 and garnered considerable interest. Later, these subsequent and related findings were to result in a round of accolades, including highest honors bestowed by his university and by the United States scientific community. But we're getting ahead of his story …

THE PROFESSIONAL YEARS: GENTLEMAN AND SCHOLAR

It could be argued that John Couch was a professional scientist earlier than some magical date on which he received a degree or began a career as an academician. Clearly, his work on *Dictyuchus* and *Septobasidium,* and his work with Coker on the Saprolegniaceae and Gasteromycetes, was conducted with the curiosity, thoroughness, and integrity of a professional. His discoveries already uncovered in his preparative training could be envied by scientists twice his age. Formally, however, John Couch returned to the

University of North Carolina in Chapel Hill as an assistant professor in 1927.

John's return from his postdoctoral studies was with more than a keen eye for new fungi. While at Cold Spring Harbor, his eyes had fallen on something else with attractive appeal and intellectual stimulation, which would keep his interest for the rest of his life. This young woman, whom John met and married, was Else Dorothy Ruprecht, a recent Wellesley graduate who had started her first job working at the Carnegie Institution studying animal genetics—mapping genes in fruit flies—with Dr. Charles Metz. Thus, John Couch, the gentleman, brought his gentle lady with him to Chapel Hill where he was to become, over the next fifty years, the scholar as he is known today. The partnership that was established between John and Else was strengthened during these years by her complementary interests, allowing her to understand and appreciate his long hours in the laboratory, absence from home, and enthusiasm for scientific discovery. Mrs. Couch's skills in foreign language were periodically put to use in translating papers for Couch and many of his students. Her artistic skills are reflected in many of his publications that she helped illustrate.

With his formal training complete, and his home established, Professor Couch embarked on forty additional years of teaching and research, advancing to associate professor in 1929 and to full professor in 1932. The first students to receive graduate degrees under his direction, Andrew G. Lang, Ph.D., and John R. Raper, M.A., completed their work in 1936.

In 1937, Couch began a long history of service to the several professional organizations to which he belonged by serving as president of the Elisha Mitchell Scientific Society and as an associate editor of *Mycologia.* Also commenc ing in 1937 was what was to become a steady stream of awards, with receipt of the Jefferson Medal and Poteat Award from the North Carolina Academy of Sciences. In 1938, Couch received the Walker Prize of the Boston Society of Natural History for his contribution to clarifying and correcting the natural history of the fungi/scale insect/host relationship in his work on *Septobasidium.* This was a fitting tribute to Couch's efforts and correlated with the publication of his classic book, *The Genus Septobasidium,* which represented the culmination of over ten years of research that elucidated the fungus-host-plant relationships, redescribed about ninety known species, and described for the first time eighty-two new species. The semidiagrammatic transverse sectional view, depicted by Couch in 1931, of the mycelial mat of *S. burtu* parasitizing a scale insect that in turn is parasitizing the cambial tissue of a tree continues to this day to be a standard illustration in most mycology textbooks.

The ten-year span between 1935 and 1945 was packed with increasing activity, responsibility, and honors for Couch. Yet he continued to carry on in his modest way, eager to contribute directly and indirectly to mycology in whatever way he could. By 1940, Couch was serving as secretary-treasurer of the Mycological Society of America, a track that would take him through the vice-presidency to president in 1943. He also served that year as a special adviser to the chairman of the U.S. Office of Scientific Research and Development and was elected to membership in the National Academy of Sciences. In 1945 the University of North Carolina named him Kenan Professor of Botany. It was mainly during this period that Couch made a series of observations that ultimately led to a complete rethinking of the relationships existing among the diverse organisms known collectively at the time as the Phycomycetes or the lower fungi. Starting with an abstract in *Science* in 1938, and followed by his classical paper in 1941 in the *American Journal of Botany,* Couch first clearly demonstrated the presence of at least two kinds of flagella in fungi, the tinsel type and the whiplash type. He then proposed for the first time that the flagellation patterns exhibited by the zoospores and some gametes of certain fungi were reflective of phylogeny. This insight, in my opinion, remains to this day one of his most important legacies to mycology. His observations clearly represented the starting point and basis for the eventual replacement of the class Phycomycetes with a number of major taxa, which today are each given ranks as low as classes or as lofty as phyla.

Couch's work with graduate students during this same ten-year period continued as intensively as ever, with an additional twelve master's and three doctoral students (George A. Christenberry, James A. Doubles, Jr., and Alma J. Whiffen) completing their degrees. The breadth of their activities and of Couch's interests at the time is reflected in the titles of these students' theses and dissertations, which indicate efforts ranging from taxonomy to cytology and physiology, and with fungi and related organisms as diverse as *Octomyxa, Blastocladia, Pythium,* the *Mucorales,* some yeasts, *Aspergillus* and some myxomycetes.

By the end of these ten productive years, Couch turned fifty, an age when many scientists begin to reevaluate their career goals, often begin to slow their research efforts, and even begin to rest on past laurels. Certainly with his scientific record and stature secure, Couch could have done the same. However, the laurels continued, and so did his own research and that of his students. Colleagues in his adopted home state honored him between 1945 and 1950 with additional awards, such as an honorary doctor of science from Catawba College, electing him president of the North

Carolina Academy of Sciences, and appointing him editor of the *Journal of the Elisha Mitchell Scientific Society* (a post he held until 1961). He returned these honors by graduating another doctoral student (Arthur W. Ziegler) and seven more students with master's degrees. He also began to describe for the first time a number of sporangial bacteria that he had discovered. The first of these new organisms had been isolated from soil collected by Lane Barksdale, the husband of his former student Alma Wiffin, while he was in the Philippine Islands in 1945. Couch isolated the first species of these bacteria by the classical fungal procedure known as the sporangial push technique, recognized that it represented a filamentous bacterium of the bacterial order Actinomycetales, clarified its uniqueness by observing the production of vesicles at hyphal tips in which flagellated motile reproductive cells originated, and then described it in 1950 as *Actinoplanes philippininsis.* Ultimately, Couch described five genera and ten species and erected the family Actinoplanaceae, which today contains a number of additional genera and species.

The momentum increasing instead of decreasing, Couch passed through the 1950s at full tilt—continuing throughout the decade to teach; to practice bench science; to administer the Department of Botany; and to serve as an officer, committee chair, editor, mentor, husband, and father. Recognition of his contributions continued in the United States by his receipt of a Meritorious Teaching Award in 1955 from the Association of Southeastern Biologists and a Golden Jubilee Merit Citation in 1956 from the Botanical Society of America. The latter was awarded to him as one "whose studies of the small, the intricate, and the odd among fungi and their relatives have come to fructification in the vivid, the significant, and the delectable."[2] During this decade, Couch's influence was also recognized beyond the United States as well. Admiration of his work, presentations, and personal demeanor by colleagues in the international arena resulted in election as an honorary foreign member of India's National Academy of Sciences.

Remarkably, John Couch's guidance of advanced graduate students during this same period intensified instead of diminishing, with the number of his students receiving degrees increasing by nine, with five of those receiving doctorates (Elizabeth K. Goldie-Smith, Maeburn B. Huneycutt, William J. Koch, Charles E. Miller, and J. Thomas Mullins). He also significantly contributed to graduate education in India by serving on numerous Ph.D. committees of Indian graduate students. Not infrequently he would receive bound theses for review and hand-carved slide boxes containing preserved documenting evidence for the conclusions reached by aspiring Indian Ph.D. candidates. It is well known that he judiciously reviewed these items, and, consequently, no doubt significantly influenced the current cadre of Indian mycologists.

By the 1960s, Dr. Couch was in constant demand as a scientific speaker, consultant, or adviser, serving on the North Carolina Governor's Science Advisory Commission, as adviser for the U.S. Public Health Service's Communicable Disease Center, and on a review committee of the University Grants Commission of India. The State of North Carolina honored him with a Gold Medal Science Award in 1964, and the university where he began his undergraduate studies, Duke University, awarded him an honorary doctor of science degree in 1965. He continued his service to the many professional organizations to which he belonged as vice-president and chairman of the Botany Section of the American Association for the Advancement of Science, as vice-president of the Botanical Society of America, and as a member of the editoral board of *Mycopathologia et Mycologia Applicata.*

During the final decade before his retirement, numerous students continued to pursue degrees under Couch's direction and in his laboratory. Thus, at the venerable age of sixty-five-plus years, he graduated his final three master's students and five more doctoral degree students (Clyde J. Umphlett, Miriam K. Slifkin, Paul J. Szaniszlo, William A. Sherwood, and Charles E. Bland). These were his golden years—and also transition years for Couch and these students who elected at such a late date in his scientific life to learn from a master biologist, microbiologist, and mycologist who had five incredibly productive decades of experience. They were transition years for him and his students because both were keenly aware that biology was changing, whether one liked it or not (Couch never confided that particular opinion to me). The change was from a predominantly organismal orientation in such fields as botany, mycology, algology, and zoology toward the more detailed study of fewer and fewer model organisms at the most sophisticated physiological, biochemical, and genetic levels. Thus, Couch had to convey to his last students his consuming enthusiasm for organisms in general, and fungi in particular, without discouraging their interest in or need to study systems at more "modern" levels, a sometimes difficult, but in his case, not impossible task.

In my own experience, as Couch's third-to-last Ph.D. student and as one having interests not only in traditional mycology but also in the application of newer technologies to mycological questions, I found my mentor surprisingly receptive to forward thinking. As long as students with tangential scientific interests attempted to appreciate and master traditional mycological concepts based on morphology and cytology, Dr. Couch would enthusiastically embrace their attempts to use the then-new biology. In fact, although he did not require his last students to master biochemistry or genetics, he certainly encouraged such efforts by students who felt compelled to learn and conduct research along those lines. This attribute of his character was particularly important because it allowed some of his students to work at a leading edge of microbiological science, within the framework of his years of experiences with numerous fungi and bacteria. In fact, in my case he formally established that my Ph.D. would involve cosupervisors, himself and Harry Gooder of the Department of Microbiology and Immunology in the UNC Medical School. Couch's foresight regarding my scientific future and training ensured that my Ph.D. research would be microbiologically competitive, a gift for which I thank him to this day.

When I look back into my own past and his influence on my scientific career, I realize that John Couch provided me with a number of other gifts in the form of a venue of very important philosophical approaches to teaching and academic research. These can be identified in retrospect as guiding principles that at first unknowingly influenced my research activities and my interactions with graduate students and subsequently seemed to solidify somewhat into my own philosophy. Foremost among these is the need to encourage students to understand the nature of relevant whole organisms and their relationships prior to committing them to detailed study of their isolated parts and functions. For Couch this seemed to allow students to appreciate better their observations and discoveries and to put them into the context of broader issues. In my own case Couch encouraged me first to study a whole fungus at the research level before attempting to investigate cell-wall biochemical aspects of Actinoplanaceae, the bacteria he discovered and on which I originally went to his laboratory to work. I am very grateful for this particular guidance because it encouraged me early in my career to study in some detail one of those wonderful eucaryotic microbes he loved so well and at a time when one could still pay attention to their morphological and cytological beauty, in an environment of true organismal appreciation, without any trace of embarrassment. It was this experience, coupled with my love of microbiological approaches, that prompts me to this day to continue to return to the fungi as research resources after brief and frequent excursions into other systems.

Among the other approaches used by Dr. Couch, as unwritten guides in his research activities and interactions with students, I think I recognized at least three that are additional legacies from his philosophy of academic science. Most important of these may be to allow students beginning their research activities to find their own problems within the context of the major professor's interests and within the feasibility of the funds available. Couch always suggested that from his experience those students who defined, or thought they defined, their own M.A. or Ph.D. problems were the most committed to and interested in the outcome of their research. Next in importance may be to allow students to study their research questions using the methodologies most commensurate with their interests, talents, and background. Certainly it was obvious that he did not require students to be clones of himself, although as already mentioned he did require them to be mycologically literate and to do competitive work. Finally, he had one somewhat selfish approach that seems to have served him well and hopefully will do the same for those of us who continue in his footsteps. On a number of occasions he confided that he tried consciously to change his research emphasis about every ten years. Most likely he did this to learn new things, maintain enthusiasm, and avoid the trivialization of his own efforts. It is clear by any measure that this last philosophical approach, as reflected by his scientific accomplishments, was adhered to by Dr. Couch, either purposely or by accident, and was enormously successful.

THE LATER YEARS: RETIREMENT

Though John Couch officially retired from the University of North Carolina in 1968 at the age of seventy-two, "retired" was not the way most of us would have described him. He was, in fact, active until his death at the age of ninety. Except that he was weaker and unable to keep long hours, his mind remained active and creative. He was an editor, with Charles Bland, of his final publication in 1985, *The Genus Coelomomyces,* and even remarked to Chuck after its completion, "Now that we have gotten *Coelomomyces* out of the way, we need to get started on a revision of *The Genus Septobasidium.*[3] Like the latter, *The Genus Coelomomyces* represented a retrospective review of years of his own work and work by colleagues and students. In addition to his recognition of the blastocladiaceous affinities of *Coelomomyces,* Couch was responsible for establishing the family Coelomomycetaceae, describing numerous new species and varieties, possibly maintaining for the first time in the laboratory mosquitoes infected with *C. punctatus,* and making innumerable additional observations that helped other investigators clarify the life cycle of *Coelomomyces.*

Even after retirement the honors continued. The University of North Carolina at Chapel Hill (UNC-CH) awarded Couch an honorary doctor of science degree in 1973, and the Mycological Society of America recognized him with the Distinguished Mycologist Award in 1981. In 1979 the Department of Botany at UNC-CH named its library in honor of Dr. Couch who, over the years, had generously provided books, subscriptions, and other support for its collections. And he continued to return the favors in his retirement as he had throughout his career, culminating with the John N. Couch Professorship in Botany established in 1984 by Dr. and Mrs. Couch.

The Couches shared sixty years of marriage and enjoyed a son, a daughter, three grandchildren, and two great-grandchildren. His wife, Else, remains in Chapel Hill, where she has maintained her late husband's tradition of giving by establishing a memorial fund in the UNC-CH Department of Biology (botany and zoology merged into one department in 1982) to be used for a John N. Couch Undergraduate Award for Scholarship in the Plant Sciences. Their son, John Philip, is a professor of romance languages at UNC-Greensboro. Their daughter, Sally Couch Vilas, an artist and wife of UNC-CH professor of bacteriology Harry Gooder, lives in Chapel Hill.

As those who knew him will attest, John Couch was a modest man who took his many honors as results of contributions he could make, rather than as personal accomplishments. He believed in hard work, in frugality, and in leaving no possibility unexplored. He carried his personal and professional life in his modest way, with curiosity in the lead, with intensity of purpose and attention to detail close behind, and always with integrity and kindness. His life's journey has touched many, and its influence will continue into generations ahead.

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W. J. Koch, and work done by William R. Burk of the Couch Library at UNC-CH and Chuck Bland, Couch's last graduate student and collaborator, of the Department of Biology, East Carolina University, culminating in 'John Nathaniel Couch, 1896-1986," *Mycologia,* 81:181-89, 1989.

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NOTES

1. "John Robert Raper (1911-74)." In *Biographical Memoirs, National Academy of Sciences,* vol. 57, pp. 347-70. Washington, D.C.: National Academy Press.

2. "John Nathaniel Couch," *Journal of the Elisha Mitchell Scientific Society* (Spring, 1968):4.

3. William R. Burk and Charles E. Bland, "John Nathaniel Couch, 1896-1986," *Mycologia* 81(1989):185.

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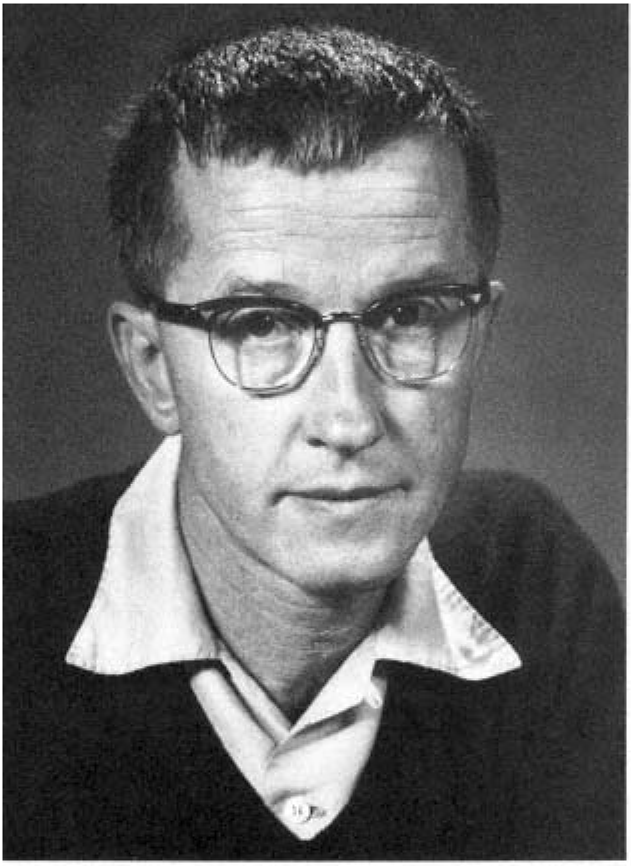

Courtesy of the Archives, California Institute of Technology

Max Delbrück

MAX LUDWIG HENNING DELBRÜCK

September 4, 1906-March 10, 1981

BY WILLIAM HAYES

MAX DELBRÜCK, or just Max as he was called by all his associates, was one of the outstanding natural scientists of our time. A man of rare intellectual ability, and clarity of thought and perception, he excelled in theoretical physics, biology, and philosophy, and possessed a deep knowledge and appreciation of the arts. His dedication to truth, and his intolerance of half-truths and intellectual pretension, were sometimes expressed with a disturbing frankness and abruptness of manner, often construed as arrogance by those who did not know him well. His disclaimer, "I don't believe a word of it," when told of some new experimental result or hypothesis, became famous among his colleagues. In fact, Max was very gregarious and had a rich vein of friendship and affection in his nature which he was always ready to share with others of all ages.

Above all, Max was a born leader whose Socratic influence on those who worked with him was enormous, whose rare praise was something to be coveted and remembered, and whose criticism was welcomed with respect; although

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he was often wrong in his scientific judgement, he was always the first to admit it. On a personal level he engendered in the minds of his friends and colleagues a deep respect and affection that they will not forget.

Max was the foremost pioneer of a new approach to an understanding of fundamental biological processes, now known as molecular biology. His most significant studies concerned the multiplication in their host cells of bacterial viruses, called bacteriophages or phages for short. These tiny particles are made up of about equal parts of two chemical components, protein and nucleic acid; infection of a bacterium by a single particle is followed, about 30 minutes later, by rupture of the cell and liberation of a hundred or more progeny particles. As long ago as 1922 the American geneticist H.J. Muller had suggested phage as the simplest possible model for studying the nature and behaviour of genes.

For their novel and important studies in this field, Max and his colleagues, Salvador Luria and Alfred Hershey, were awarded the Nobel Prize in Physiology or Medicine in 1969. However, no account of Max's published work can do justice to his overall influence as the leader of a formidable group of workers, many of them physicists like himself, who infused a new way of thinking, and a new life, into biological research. In addition, he was a direct source of encouragement and inspiration to young research workers of many nationalities and from many disciplines who came to work with him on bacteriophage at the California Institute of Technology, in Pasadena, California, or to attend his famous "Phage Course" at the Cold Spring Harbor Laboratory, Long Island, New York, and to whom his intellectual approach to biological problems became an inspiration for their own thinking.

FAMILY BACKGROUND

Max grew up in the Grunewald suburb of Berlin, the youngest of seven children (four girls and three boys) of an extremely prominent academic family. His father, Hans Delbrück, who was 58 years older than Max, was Professor of History at Berlin University, specializing in the history of the art of war, as well as sole editor for at least 30 years of a monthly journal, *Preussische Jahrbücher,* for which he wrote a column commenting on German politics. Three of his father's first cousins were, respectively, Professor of German Literature at Jena, Chief Justice of the Imperial Supreme Court, and Minister of State. His maternal great-grandfather was the famous Justus von Liebig, Professor of Chemistry at Giessen and Münich, Foreign Member of the Royal Society, and Copley Medalist.

His mother's brother-in-law, Adolf von Harnack, was Professor of Theology at Berlin University and a church historian; he was also Director of the Prussian State Library and, in 1910, became co-founder and President of the Kaiser-Wilhelm-Gesellschaft. The Harnacks, the Delbrück's nearest relatives, were also a large family and lived next door, while Karl Bonhoeffer, a Professor of Psychiatry, and his family were around the corner and the Max Planck family not far away. One of the Bonhoeffer sons, Klaus, married Max Delbrück's sister Emmie.

Max's family enjoyed "a modest degree of affluence and apparently the life until 1914 was pretty free and very hospitable. As war came and life became more and more of a nightmare in every respect, of course all this darkened …. I think three-quarters of the young men in the family [including his eldest brother] were killed. So that was all very sad, and in addition then there came these pretty severe food and coal shortages and then the total mess in 1918.

So this relatively affluent residential suburb after the war became almost a ghost town" (1).

World War II also brought tragedy to the Delbrück family. Two Bonhoeffer brothers, Klaus (Max's brother-in-law) and Pastor Dietrich, two Bonhoeffer sons-in-law, and two von Harnack cousins, Ernst and Arvid, together with the latter's American wife, were executed by the Nazis as leading members of the Resistance. Max's brother Justus was imprisoned by the Nazis, and liberated after the fall of Berlin but ten days later was arrested by the Russians and died in a diphtheria epidemic in a Russian camp. The husbands of two of Max's sisters also were killed by marauding soldiers in the last days of the war.

EARLY INTEREST IN SCIENCE

Of all the many children in the Delbrück, Harnack and Bonhoeffer families, Max was the youngest. Moreover, none of his intimates, save one, had any knowledge of, or interest in, science. The exception was Karl Friedrich Bonhoeffer, 8 years his senior, who became a distinguished physical chemist and Max's mentor and lifelong friend. Max's main boyhood interests were astronomy and mathematics. In retrospect, some 40 years later, he considered that he chose astronomy as a means of finding and establishing his own identity in an intimate society of so many able and strong personalities, all of them older than himself; but only he was an astronomer, and proclaimed himself one during his last 2-3 years at the Grunewald Gymnasium. He read popular books on the subject, was the enthusiastic possessor of a 2-inch telescope, and sometimes woke the whole household with the loudest of alarm clocks in the small hours of the morning when he had an appointment with the stars! (1) Despite Max's nuisance value, his parents proved tolerant and even helpful, while his knowledge of astronomy blossomed under the tutelage and friendship of Karl Friedrich Bonhoeffer.

It thus became Max's intention to study astronomy at the university. In 1924, at the age of 171/2, he went first to Tübingen where Hans Rosenberg offered an introduction to astrophysics which was then in its infancy; he also took courses in mathematics and physics, but chemistry failed to attract him and he never learned this subject as a student. He spent only one semester at Tübingen and then moved for a semester to Berlin where he had free tuition because of his father's professorship there, and thence to Bonn and back to Berlin again until, in the summer of 1926, he finally settled at Göttingen for 3 years until he obtained his degree.

Although Göttingen was at that time the center of excitement in theoretical physics, following Heisenberg's discovery of quantum mechanics in 1925, Max continued to be interested in astronomy and mathematics until his attempt to write a Ph.D. thesis on novae failed because, he admitted, the mathematics of astrophysical theory of the interior of stars was beyond him, while the relevant literature was in English which he did not know at the time. But in the effort he had had to learn a good deal of quantum mechanics which brought him into contact with some of the theoretical physicists, among them Max Born, Pasqual Jordan, Eugene Wigner and Walter Heitler.

At this time he wrote a short paper (1929) providing formal mathematical proofs for a theorem that Wigner had used in the application of group theory to theoretical physics. Born, who was Professor of Theoretical Physics, thereupon offered Max a Teaching Assistantship, and Heitler suggested that he extend to lithium the quantum mechanical theory of the homopolar bond that had just been developed for hydrogen by Heitler and London. His conclusion was that the bond energy in Li_2 is considerably smaller than in H_2, not because of the repulsion of the K shells but because the bond electrons were two s electrons (1930a). Max recently averred that this topic turned out to be a nightmare for him because of the complexity of the mathematics involved and that he had never dared to look at his thesis again (1); but nevertheless it won him his Ph.D. Degree in 1930.

EARLY CAREER IN PHYSICS (1929-32)

Bristol

John E. Lennard-Jones, Professor of Theoretical Physics at the new H. H. Wills Physics Laboratory, University of Bristol, England, spent some months at Göttingen in 1929 and was anxious to attract to Bristol two of Max Born's students for whom research grants had been provided. Gerhard Herzberg, then a postdoctoral Fellow, and Max Delbrüick were appointed. Max remained at Bristol for 18 months and became very friendly with Cecil F. Powell, with whom he roomed. Among other friends at that time were P. M. S. Blackett, later to become President of the Royal Society, P. A. M. Dirac and H. W. P. Skinner. Of these early associates four were later to win Nobel Prizes, three in physics (Dirac, Blackett and Powell) and one in chemistry (Herzberg).

An unpublished history of the Bristol Department, written by the late Professor A. M. Tyndall, related that "M. Delbrück, Prussian by birth but cosmopolitan by nature, a theoretical physicist recommended by M. Born, brought with him intellectual stimulus, critical judgement and social entertainment which gave help and pleasure to many and sundry." Another member of the department at the time, who remembers him quite well (J. Burrow, quoted by

N.T.)* describes him as a cheerful, outgoing person and one who rapidly established a reputation as a theoretician who was always ready to discuss problems of any kind with experimentalists who needed help and advice. Herzberg also recalled that he "fitted in very well with the group of younger physicists there because of his (then) gregarious ways and the ease with which he made friends." Max published two papers from Bristol in English (1930b; 1932), on topics related to the quantum mechanical theory of homopolar bonding on which he had written his thesis.

Copenhagen and Zürich

Following his Bristol experience, Max obtained a Rockefeller Fellowship (Physics) to study with Niels Bohr in Copenhagen where he spent the spring and summer of 1931, and then spent the last 6 months with another quantum physicist, Wolfgang Pauli, in Zürich. In Copenhagen he roomed, and collaborated on a nuclear physics project, with George Gamow (1931) with whom he established a lasting friendship. Also working with Bohr at that time was Victor Weisskopf, a very close friend since their student days together at Göttingen; they arrived in the United States almost simultaneously in 1937 *(see below)* and remained in personal contact until Max's death.

Anyone who might infer from all this that life in Copenhagen was a staid and serious business should read Max's lighthearted and facetious account of the gaiety and practical jokes of those days, in his contribution to a George Gamow Memorial Volume (1972c). Weisskopf (pers. comm.) has commented on his wonderful sense of humor: "There was a custom in Copenhagen, at each of the early conferences

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* The initials in the text that indicate the source of quotations are explained in the acknowlegements at the end of the memoir.

organized by Niels Bohr, to have what we called a session of 'comic physics.' It was always Max who was the most spirited leader in these activities with his humour and intellectual fantasy. You must have heard of his rewriting of Goethe's Faust to make fun of the physics of that time."

Max's short visit to Copenhagen became of greater importance to him than he could have imagined, for it marked the turning point in his life that changed not only his career but his philosophical outlook as well. The determining influence was Bohr's formulation of the complementarity concept as a generalized extension of Heisenberg's uncertainty principle. Thus, the propagation of light may be unambiguously defined, in a probabilistic way, either as a continuous motion of electromagnetic waves or as the exchange of individual quanta of energy related to the wavelength of the former by Planck's constant, but not by both at the same time; the two expressions of reality stand in a mutually exclusive but *complementary* relation to one another. According to Max,

> Bohr then very vigorously asked the question whether this new dialectic wouldn't be important also in other aspects of science. He talked about that a lot, especially in relation to biology, in discussing the relation between life on the one hand, and physics and chemistry on the other—whether there wasn't an experimental mutual exclusion, so that you could look at a living organism either as a living organism or as a jumble of molecules; . . . you could make observations that tell you where the molecules are, *or* you could make observations that tell you how the animal behaves, but there might well exist a mutually exclusive feature, analogous to the one found in atomic physics . . . in many respects Bohr wasn't sufficiently familiar with the status of the science (biology). So it was intriguing and annoying at the same time.
>
> It was sufficiently intriguing for me, though, to decide to look more deeply, specifically into the relation of atomic physics and biology—and that means learn some biology"(1).

Much has been written of Bohr's profound influence on Max. Thus, Gunther Stent writes, "I think it is fair to say that with Max, Bohr found his most influential philosophical disciple outside the domain of physics, in that through Max, Bohr provided one of the intellectual fountainheads for the development of 20th century biology" (3). Again, Horace Judson said of Max, "His mind and style had been formed by Niels Bohr, the physicist, philosopher, poet and incessant Socratic questioner who made Copenhagen one of the capital cities of science between the wars" (4, p. 50). But Max himself saw more than this in the so-called Copenhagen Spirit, as shown by his reply to a question about the Phage Group: "Well, the phage group wasn't much of a group. I mean it was a group only in the sense that we all communicated with each other. And that the spirit was—open. This was copied straight from Copenhagen, and the circle around Bohr, so far as I was concerned. In that the first principle had to be openness. That you tell each other what you are doing and thinking. And that you don't care who—has the priority" (4, p. 61).

It followed that, after a further 6 months with Lennard Jones at Bristol, Max decided to accept an appointment as assistant to Lise Meitner at the Kaiser Wilhelm Institute for Chemistry in Berlin in the autumn of 1932, because of its proximity to the Kaiser Wilhelm Institute for Biology. But before returning to Berlin he paid a short visit to Copenhagen to hear Bohr deliver his famous address, "Light and Life," to the opening meeting of the International Congress on Light Therapy in August, in which he explicitly stated his views on complementarity in biology (9). Odd though these views may seem to us now, in retrospect, this lecture confirmed Max's decision to turn to biology.

THE BERLIN YEARS (1932-37)

Max's appointment as assistant to Lise Meitner, who was collaborating with Otto Hahn on the results of irradiating uranium with neutrons, was, in effect, to be a consultant on theoretical physics. During this period he did write a few papers, one of which turned out to be an important contribution on the scattering of gamma rays by a Coulomb field due to polarization of the vacuum produced by that field (1933). His conclusion proved to be theoretically sound but inapplicable to the case in point, but 20 years later Hans Bethe confirmed the phenomenon and named it "Delbrück scattering." A second seminal paper with Gert Molière, which Max referred to retrospectively as "very *learned"* (1), attempted to apply quantum mechanics to resolve the paradox of irreversibility in statistical mechanics (1936c).

Not long after the beginning of Max's Berlin period, which coincided with Hitler's rise to power, he organized a private group of five or six theoretical physicists to join in fairly regular discussions among themselves, often at his mother's house. At his suggestion some biochemists and biologists also joined the group. Among these were K. G. Zimmer whose interest was the dose effect of ionizing radiation on biological systems, and, most significantly for Max's future, N. W. Timoféeff-Ressovsky, a Russian geneticist from the Kaiser Wilhelm Institute for Brain Research who had been collaborating with Zimmer on the genetic effects of radiation for some 2 years before contact with Max was established. Timofeeff-Ressovsky's experimental organism was *Drosophila,* the fruit fly, which was then, and still is, very popular with geneticists because of its short generation time and the large populations that can be raised in the laboratory.

Zimmer records that he remembers vividly the discussion that followed: "Two or three times a week we met, mostly in Timoféeff-Ressovsky's home in Berlin, where we talked for ten hours or more without a break, taking some food during the session. There is no way of judging who learned most by this exchange of ideas, knowledge and experience, but it is a fact that after some months Delbrück was so deeply interested in quantitative biology, and particularly in genetics, that he stayed in this field permanently" (2, p. 33).

The upshot of all these discussions was a paper by Timoféeff-Ressovsky, Zimmer, and Delbrück (1935b) on the nature of gene mutation and gene structure, in which Max was mainly responsible for the theoretical interpretation. He supposed that the molecules from which genes are made must have a very unusual atomic constitution, since they show such remarkable stability in a cellular environment otherwise subject to constant chemical change. This stability suggested that each atom of the gene molecule is fixed in its mean position and electronic state by being sunk in "energy wells," so that discontinuous changes in their state, expressed as mutations, could arise only by the acquisition of very high energies such as ionizing radiations would impose (18, p. 26).

It is difficult to say how much interest this paper aroused at the time. Max reported that it got "a funeral first class" (1) since it was published in a little-known Göttingen journal, but Timoféeff-Ressovsky must have sent reprints to many geneticists although it is unlikely that they would have known enough physics to understand it. It was not until ten years later that the paper became famous through the publication in 1945 of Erwin Schrödinger's little book, *What Is Life?,* in which he maintained that Delbrück's model of the gene was the only possible one, and went on to put forward the romantic and paradoxical idea, first proposed by Bohr, that "from Delbrück's picture of the hereditary substance it emerges that living matter, while not eluding the 'laws of physics' as established up to date, is likely to involve hitherto unknown 'other laws of physics' which, however, once they have been revealed, will form just as integral a part of this science as the former" (16). Max, of course, was already long embarked on his quest for this Holy Grail, but Schrödinger's book was influential in attracting into biology many physicists, curious to solve the paradox (see Stent, 2, p. 3).

Meanwhile, when Max was spending all this time immersed in biophysics, Hahn and Meitner's work on the irradiation of uranium with neutrons was revealing the emission of many characterizable transuranium products that were interpreted as elements, but their number then became so large that they were assumed to be isomers of transuraniums, and Max went along with this. As he admitted (1), ". . . this was really immensely stupid of me; I should have guessed what was really going on, namely fission, but I, like everybody else, lacked imagination to see that . . . it was something any experimental physicist could easily have figured out . . . all you needed to know was that there was excess energy there; the neutron enters and there is enough energy there to blow the nucleus to pieces. You needed to just be able to add and subtract . . . and it didn't occur to anybody until they were literally forced to this conclusion only the year after I left."

Max's decision to visit the U.S.A. was prompted by three circumstances. One was his now dominant interest in quantitative biology and especially in *Drosophila* genetics, which he wished to experience first hand. Then, a few years after becoming Lise Meitner's assistant, he had considered a future as a lecturer at the university, but, apart from aca demic criteria, this entailed certification of "political maturity" following participation in "free discussion" groups at a Nazi indoctrination camp. His failure to display sufficient "maturity" at two sessions, probably as a result of too much frankness, made it clear that a university career would not be open to him in the foreseeable future. Finally, in 1937 the Rockefeller Foundation offered him an unsolicited Fellowship (Biology) to travel abroad, so he took this opportunity to visit the California Institute of Technology in order to learn *Drosophila* genetics from Thomas Hunt Morgan and his world-famous group.

THE BACTERIOPHAGE EPOCH (1937-53)

Early Days at Caltech

Max's initial introduction to Caltech was frustrating and disappointing, despite the help of A. H. Sturtevant, and Calvin Bridges with whom he was especially friendly, he found the highly specialized *Drosophila* jargon too difficult and exacting to grasp, let alone master, in a reasonable time. One day he inadvertently failed to attend a seminar on bacteriophages by Emory Ellis, and went to him to find out what he had missed: "I had vaguely heard about viruses and bacteriophages, and I had read the paper by Wendell M. Stanely on the crystallization of tobacco mosaic virus before I had left Germany. I had sort of the vaguest notions that viruses might be an interesting experimental object for a study of reproduction at a basic level" (1). Ellis showed him the very rudimentary materials and the simple techniques needed for his experiments, and Max saw for the first time the small macroscopic areas of clearing, or plaques, on a lawn of bacterial growth on solid culture medium, each plaque representing the multiplication of a single virus particle. Ellis also demonstrated some step-growth curves revealing the kinetics of a cycle of phage multiplication in newly infected bacterial populations. According to Ellis, Max's first comment was, "I don't believe it" (2, p. 53); but Max's own recollection was, "This seemed to me just beyond my wildest dreams of doing simple experiments on something like atoms in biology [which perhaps means the same thing!], and I asked him whether I could join him in his work, and he was very kind and invited me to do so" (1).

So began what has been called "The Phage Renaissance." Before this, d'Hérelle's initial studies demonstrating the particulate and viral nature of phage had been followed by the highly original investigations of F. M. (later Sir Macfarlane) Burnet and Martin Schlesinger, which laid the foundation of modern phage research, but Schlesinger died prematurely in 1936 and Burnet changed his field shortly afterwards; neither left disciples to carry on their pioneering mission. Moreover, after working for a year with Max, Ellis returned to his original work on malignant tumors of mice. So Max was left alone, and alone was responsible for giving continuity to phage research by founding and guiding an expanding, if loosely knit, lineage of phage workers that sowed the seeds that finally blossomed into modern molecular biology (see 5).

One of Max's first contributions, in his early days with Ellis, was to bring his analytical approach and mathematical knowledge to bear on study of the phage life cycle. For example, formulae were devised to check the rate of adsorption of free phage to bacteria under various experimental conditions, while the then unknown proportion of free particles able to produce plaques (the plating efficiency) was assessed by the application of Poisson's statistics of random sampling. In their only paper together, Ellis and Delbrück (1939) invented and greatly refined the one-step growth curve and devised the single-burst experiment, anticipated in essence by Burnet, which permitted a comparison of phage multiplication in individual cells, both key methods for the future progress of phage research. Of this and two other papers by Max on the same topics (1940c,d). T. F. Anderson wrote 16 years later that, of the many scientific papers he must have read at the time, he could remember only these three: "The experiments were beautifully designed and reported in an elegant style that was new to me. The three papers carrying the Delbrück label formed a little green island of logic in the mud-flat of conflicting reports, groundless speculations, and heated but pointless polemics that surrounded the Twort-D'Hérelle phenomenon" (2, p. 63).

Max had no difficulty renewing his Fellowship for a future year, and when this extension expired the war had started so that a return to his old job in Berlin, which had been guaranteed by Hahn and Meitner, was virtually impossible even had he wanted it. On the advice of the Rockefeller Foundation he accepted a lowly academic position of Instructor of Physics at Vanderbilt University, Nashville, Tennessee, where he remained from 1940 until 1947, being finally promoted to Associate Professor. However, the Foundation, with generous foresight, agreed with the university to pay half his salary on the condition that half his time was free for biological research.

Vanderbilt University and the Phage Group

Max had no students of biology at Vanderbilt and his only recruit there was A. H. Doermann who had just obtained his doctorate in Neurospora genetics and later became a prominent phage worker. At the end of 1940, Max met Salvador Luria, a recent Italian refugee from Europe, who was working on phage at the College of Physicians and Surgeons in New York. As Luria remarked 25 years later, "We were probably the only two people interested in phage from the point of view of molecular biology." They arranged to collaborate in experiments with mixed infections by phages T1 and T2 in the summer of 1941 at Cold Spring Harbor where Max was to read a paper at the annual symposium. In August that year Max married Mary (Manny) Adeline Bruce whom he had met during his fellowship at Caltech. The marriage took place in Pasadena and Manny has related that "Max took a whole week off from his experiments to get married. He couldn't wait to get back to Cold Spring Harbor" (7) where they spent their honeymoon.

For the mixed infection experiments Luria had isolated bacterial indicator strains, separately resistant to each of the two phages, and when he visited Max in Nashville a year later they began to discuss the problem of whether resistance arose by the adaptation of a constant small proportion of bacteria, induced by contact with the phage, or by spontaneous mutation. The obstacle to direct experimentation was that the only way to demonstrate resistance was by exposing the culture to the phage. It was Luria who first conceived the idea of comparing the numbers of resistant bacteria arising in otherwise identical *independent* cultures, initially seeded with only a few sensitive cells, with the numbers from equivalent samples from a single culture. If resistance was induced by contact with the phage, then variation in the numbers of resistant cells would, in either case, be within the limits expected by random sampling. In contrast, the occurrence of resistant *mutants,* which might arise spontaneously and begin to multiply at any time during the growth of each independent culture, would lead to a much wider variation. By this reasoning, a fluctuation greater than the sampling error, in the numbers of resistant bacteria from independent cultures, means that these variants arose as clones in the cultures before they were exposed to the phage and, therefore, were mutants.

Luria wrote to Max about his idea and two weeks later Max provided the manuscript of a fully worked-out mathematical theory as a basis for experiments. The experiments showed unambiguously that bacteria acquire resistance to phage by mutation, a finding which has subsequently been established by virtually all other bacterial variations.

The paper by Luria and Delbrück (1943a) reporting their findings and conclusions is a landmark in the history of molecular biology, for it provided the first real evidence that bacterial inheritance, like that of the cells of higher organisms, is mediated by genes and not by some Lamarckian mechanism of adaptation as was widely held at the time. Thus, it signaled the birth of bacterial genetics which became a basic tool for exploring the molecular basis of life. Indeed publication of this paper has been compared in importance to that of Mendel in 1865, ushering in the science of genetics itself (4, p. 56). At about the same time, in Nashville, Salvador Luria initiated his studies of host-range mutations in phage, but these were not completed until later and were published in 1945.

Max and Luria had become interested in some papers on phage by Alfred H. Hershey, a microbiologist at the Medical School of Washington University, in St. Louis, and at the beginning of 1943 Max invited him to Nashville for a few days and wrote to Luria about him. Then, at the end of the year, Luria gave a seminar at St. Louis which "had the good fortune of impressing Hershey with the remarkable possibilities of phage genetics" (2, p. 173). These three formed the nucleus of the Phage Group consisting, as Max quipped, of two enemy aliens "and another misfit in society" because of Hershey's liking for independence and solitude (4, p. 53).

In addition, an important collaboration was established in the early 1940s with the electron microscopist Thomas F. Anderson. The basic aim of the Group was to understand the mechanism of phage replication—how infection by a single particle resulted in the liberation of some 200 particles half an hour later—and, of course, the nature of the gene.

It is not my intention in this memoir to recount the many ideas and experiments which followed their zigzag course toward the solution of these problems, which may be culled from the titles in Max's bibliography, but rather to show Max's overall involvement and influence on this enterprise. However, one important technical decision should be mentioned. Until 1944 most workers used phage strains and bacterial hosts which they themselves had isolated, so that it was almost impossible to build up a body of comparable knowledge. Max therefore negotiated a "phage treaty" under which it was agreed that research be concentrated on a set of seven phages (T1-T7), all of which infected the same host, *Escherichia coli* strain B.

Cold Spring Harbor and the Phage and Phycomyces Courses

After their first visit in 1941, Max and Manny returned to the Cold Spring Harbor Laboratory for the summer months nearly every year. They were often joined by Salvador Luria, A. H. Doermann, A. D. Hershey, Mark Adams and many others over the years who became interested in phage, not only for research but, more importantly, for intellectual interaction and stimulus. In 1950 Hershey became a member of the Department of Genetics of the Carnegie Institution of Washington, which was also located at Cold Spring Harbor.

In 1945 Max organized the first of 26 successive annual Phage Courses at Cold Spring Harbor, and was the princi pal instructor in the first three of them. This was made possible through the vision and enterprise of Milislav Demerec, director of the Laboratory from 1941 to 1960. Demerec was a classical geneticist who foresaw the potential of bacteria and their phages as genetic tools, abandoned *Drosophila* to work with them, and helped others to do the same. The course was devised not only for biologists but also for biochemists and physicists, and the students ranged from young postdoctorals to eminent physicists such as Leo Szilard who took the course in 1947. The importance of a quantitative and statistical approach to the new biology was stressed by the fact that a prerequisite for the first course (checked by an admission test!) was "facility in the processes of multiplication and division of large numbers; elements of calculus; properties of exponential functions."

The recruitment value to the phage field of these courses, probably first suggested by Luria (1), may be guessed from the fact that the total number of students over the years was well over 400, including many from abroad. Moreover, of some 130 students who attended the first ten courses, not less than 30 became recognized phage workers or bacterial geneticists so their initial interest must at least have been confirmed.

In addition to these courses, Max also organized a series of Phage Meetings, the first three of which were held at Nashville. The first meeting, in 1947, attracted only eight people, including Anderson, Doermann and Hershey. The fourth meeting, also organized by Max, was at Cold Spring Harbor in 1950 and thereafter the meetings continued there annually, without interruption, through 1981, attended by hundreds of participants.

In the early 1950s Max became interested in sensory perception and transduction and chose, particularly, to study the phototropic response of the large aerial sporangiophores of the fungus *Phycomyces.* As in the case of phage, he became the leader of a *Phycomyces* Group, interested in various aspects of tropic behavior in this organism. From 1965 onwards Max organized the first of a series of eight *Phycomyces* Workshops, held at Cold Spring Harbor over the next twelve years. Each lasted about 2 months, they attracted, all told, more than 100 people, and Max led or participated in all of them.

The Cold Spring Harbor Laboratory therefore became a Mecca to which Max's followers in these two fields made their annual summer Hadj, not only to attend the more formal courses or workshops but also to continue their research in an exciting and stimulating environment. As James Watson, the present Director of the Laboratory, who became a PhD student of Luria in 1947, has reflected, "My approach to science as well as to people became indelibly fixed the following summer (1948) when we all came together at Cold Spring Harbor—the Delbrücks, the Lurias, Gunther Stent, Seymour Benzer and I—in an atmosphere that I can never remember as less than perfect. Now I realize that all the personality of Cold Spring Harbor, which I so loved then and still do, was given to it by Max" (3). It is therefore most fitting that a recently completed major extension of the Davenport Laboratory, the site of so much of Max's research as well as of the Phage and *Phycomyces* courses at Cold Spring Harbor, was dedicated as the Max Delbrück Laboratory in August 1981.

Return to Caltech

The war over, Max's preeminent role in the Phage Renaissance, and his novel and distinguished background as a theoretical physicist turned successful biologist, prompted offers, in 1946, of senior appointments at the Cold Spring Harbor Laboratory, the California Institute of Technology, and the Universities of Illinois and Manchester, England. Vanderbilt University responded by promising him everything he wanted. He was especially interested in the Chair of Biophysics at Manchester, negotiated about May 1946 by P. M. S. Blackett who was then Professor of Physics, and Max visited there to discuss the appointment; he and his wife Manny were tempted to move to England because of the many attachments he had formed there in his early postgraduate years, while Manny had grown up in a British environment on the island of Cyprus. Max was also willing to listen to the Vanderbilt enticements. However, when the offer of a Chair of Biology at Caltech arrived on 11 December 1946 it proved irresistible, and was accepted on 27 December. This was the first faculty appointment in biology made by George W. Beadle who had recently succeeded T. H. Morgan as Chairman of the Biology Division.

If Cold Spring Harbor had become the Phage Mecca, visited by the converted for their intellectual refreshment, Max's laboratory at Caltech "now became the Phage Group's Vatican, where most of the disciples of what was later to be called the 'informational school' of molecular biology took their orders" (6). Recruitment followed fast from both the physical and biological sciences and "it is likely that the sense of excitement which often permeates a developing cluster must be generated by someone with Delbrück's charismatic force of personality" (5, p. 79). It is perhaps of interest that, during what Stent (17) has called the "Romantic Period" of molecular biology (up to 1953), about the same proportion of recruits to the phage field came from the physical sciences as from the biological sciences (5, p. 66). It is likely that an appreciable proportion of the former was motivated by Schrödinger's imaginative prediction about the nature of the gene in his book *What Is Life?*. Indeed, one of Max's colleagues at Caltech at this time was Neville Symonds who came from postdoctoral studies on wave mechanics with Schrödinger, then working in Dublin as a former refugee from Nazi Germany. James Watson, on the other hand, whose interests and undergraduate background were in biology, admits that his main incentive was the "legendary figure" of Max evoked by Schrödinger's book.

Among the many phage devotees engaged in active research at Caltech during the early years of Max's leadership was Elie Wollman of the Pasteur Institute, Paris. André Lwoff, who was head of the Service de Physiologie Microbienne at the Pasteur Institute, had attended the 1946 Cold Spring Harbor Symposium and had there encountered Max and the Phage Group. He found the atmosphere stimulating and "swallowed everything with enthusiasm," but his interests at that time lay elsewhere; he did not attend the Phage Course nor start work on lysogeny until about 1949 (2, p. 88). Wollman was his first ambassador to Caltech, and thereafter many members of the American Phage Group worked for a time at the Pasteur Institute which became the European Vatican.

In 1949 the Delbrücks did not go to Cold Spring Harbor since Manny was expecting a child, so many of the Phage Group, including James Watson, came to Pasadena where "several times each week, there occurred seminars dominated by Delbrück's insistence that the results logically fit into some form of pretty hypothesis" (2, p. 239). Two new visitors to Caltech at this time were Ole Maaloe from Copenhagen University and Jean Weigle who was head of the Physics Institute in the University of Geneva; these two constituted a very small "Class of 49" that graduated under Max's supervision. (2, p. 265).

Weigle's account of his Caltech experiences, on his return to Geneva, decided to the electron microscopist, Ed ward Kellenberger, to apply his instrument to the study of phage (2, p. 116), while Weigle himself arranged with Max to spend his winters working at Caltech, and subsequently resigned his Geneva professorship for a wholetime Caltech research appointment. Maaloe also embarked on phage research in Copenhagen, and Watson worked with him there during the first year of his Fellowship in Europe in 1950, as well as with the Danish biochemist Herman Kalckar, who had attended the first Phage Course in 1945.

Thus, the gospel spread and its proselytes increased in number to discover that they had become members of an integrated, friendly and hospitable international family related by social as well as by intellectual bonds, with Max as their father figure.

DNA as Genetic Material

Max's early work on the one-step growth curve had shown that, following phage infection of bacterial cells, a latent period of about 20 minutes elapses before the cells begin to burst and liberate a hundred or more progeny particles. Mutation had also been revealed by Salvador Luria as the cause of variation in phage, as well as in bacteria as has already been recounted. Then Delbrück and W. T. Bailey (1946c) and A. D. Hershey independently (13) demonstrated genetic recombination when bacteria were doubly infected with phages that differed in two characters. This was the finding that led, about ten years later, to the ultimate genetic analysis of gene structure by Seymour Benzer (8). However, nothing whatsoever was known about the number or nature of the presumptive precursors inside the infected bacteria during the latent period. As Max remarked in a Harvey Lecture that he was invited to give in 1946, some 30 years after the first description of phage as a bacterial virus, "it should be our first aim to develop a method of determining the number of virus particles which are present in a bacterial cell at any one moment. Here I, and those who have been associated with me in this work, have to make the first admission of failure" (1946b).

Such a method was first developed between 1949 and 1952 by A. H. Doermann (12) who disrupted cells at intervals after infection but failed to find any plaque-forming entities during about the first 12 minutes; thereafter infective intracellular particles began to appear and increased *linearly. T*his "eclipse period" clearly showed that the phage changes its state immediately after infection, while the subsequent linear rather than exponential increase in phage numbers implied that this increase is not due to successive replications of its complete organism but is more compatible with an assembly of its component parts (see 2, p. 79).

At this stage it is interesting to note that Niels Bohr's influence and Schrödinger's prediction still retained a firm hold on Max's imagination. In an address entitled "A physicist looks at biology," delivered at the thousandth meeting of the Connecticut Academy of Arts and Sciences in 1949, he says,

> It may turn out that certain features of the living cell, including perhaps even replication, stand in a mutually exclusive relationship to the strict application of quantum mechanics, and that a new conceptual language has to be developed to embrace this situation. The limitation in the applicability of present day physics may then prove to be, not the dead end of our search, but the open door to the admission of fresh views of the matter. Just as we find features of the atom, its stability, for instance, which are not reducible to mechanics, we may find features of the living cell which are not reducible to atomic physics but whose appearance stands in a complementary relationship to those of atomic physics (1949b; also 2, p. 9).

In 1952 A. D. Hershey and M. Chase (14) published their famous experiment in which they infected cells with phage in which the DNA and protein were differentially labelled with radioactive phosphorus and sulphur respectively; they found that the DNA entered the cells but that most of the protein, in the form of empty heads, remained outside. The eclipse was therefore the period during which the phage DNA was replicating and directing the synthesis of nascent phage protein. Thus, it turned out that the genetic material was DNA and that the genetic material alone entered the cell to initiate a new viral generation.

As early as 1944, Oswald Avery and his colleagues at the Rockefeller Institute, in New York, had published good biochemical evidence that the "transforming principle" of pneumococci, which transfers the hereditary ability to synthesize a polysaccharide characteristic of one type to bacteria of other types, is highly polymerized DNA. Why, then, did the Phage Group seemingly ignore this obvious clue to the chemical nature of the gene until a member of the group itself came to the same conclusion by a less rigorous experiment? In fact, both Max and Salvador Luria were very interested in Avery's work a considerable time before its publication, visited him at the Rockefeller Institute, and admired him as a person. In mid-1943 Avery wrote a long letter to his brother Roy, who was a microbiologist at Vanderbilt University and knew Max and showed him the letter which explained the results of Oswald's research and suggested, very cautiously, that DNA might be the genetic material (2, p. 180).

Although pneumococcal transformation was certainly seen as a very interesting phenomenon by Delbrück and Luria, there were then understandable reasons for failing to recognize its *genetic* importance. The phenomenon appeared to be uniquely restricted to polysaccharide production by a single bacterial species and seemed remote from the problems that beset phage workers. Moreover, at that time, bacterial genetics did not exist, while DNA was generally regarded as a "stupid" molecule consisting simply of repeating tetrads of the same nucleotides which could hardly carry complex information; it was not until much later that contamination of transforming preparations with small amounts of protein, then favoured as the most likely genetic material, could be excluded.

However, the most cogent reason for failure to appreciate the importance of DNA in transformation was probably that it appeared as a biochemical problem, revealed by biochemical techniques. As Luria has said, "People like Delbrück and myself, not only were we not thinking biochemically, but we were somehow—and probably partly unconsciously—reacting negatively to biochemistry .... I don't think we attached great importance to whether the gene was protein or nucleic acid. The important thing for us was that the gene had the characteristics that it *had* to have" (4, p. 62).

But others had sensed the importance of DNA, confirmed by the Hershey-Chase experiment, and were working to elucidate its structure—an enterprise that culminated in the Watson-Crick double helix in 1953, a molecule that embodied all the genetic properties required by the gene (20). As soon as the model structure had been built and seemed right, James Watson revealed it first in a letter to Max (19), who was fascinated and thought it obviously right. Max then wrote to Bohr about the model, saying that he thought it equaled Rutherford's discovery of the nucleus of the atom (1).

Thus, as Gunther Stent has commented (18, p. 29), in one respect the Phage Group failed in its mission, for it did not discover the new laws of physics that Bohr and Schrödinger had prophesized. There turned out to be no paradox; only the hydrogen bond lay at the heart of the mystery. The really important achievement of the Group during this romantic phase of the growth of molecular biology was "the introduction into microbial genetics of previously unknown standards of experimental design, deductive logic, and data evaluation. These procedures had led to final and definitive settlement of matters that had been under dispute for ten or more years" (17).

THE PHYCOMYCES PERIOD (1953-81)

Max was basically a theoretician who lived to search for neat models and hypotheses to explain complex phenomena. About 1950, after discovery of the phage eclipse phase but before the Hershey-Chase experiment, he became interested in sensory perception and its transduction into physiological activity—a phenomenon more relevant to the complex behaviour of higher creatures. He also thought that, by then, phage research was "in good hands." His first choice of a simple model organism was the purple bacterium, *Rhodospirillum,* which is not only photosynthetic but also phototactic, swimming towards a light source. Max was co-author of a general article on *Rhodospirillum* (1951b) in which the responses of this organism to light were compared with those of nerve fibers to electrical stimuli. However, after some early experiments he forsook this organism in favour of a simple fungus, *Phycomyces.*

Phycomyces has a non-septate mycelium which sprouts large aerial stalks called sporangiophores, each crowned by a spherical sporangium containing many thousand spores. The attractiveness of this organism as a model for studying perception and response lay in the reactions of the rapidly growing sporangiophores to many stimuli. For example they grow towards the light (phototropism), against gravity (geotropism), into the wind (anemotropism), and away from nearby objects (avoidance response). On the other hand, *Phycomyces* does not naturally form heterokaryons and produces multinucleate asexual spores, while the sexual cycle, involving two mating types that initially were far from isogenic, takes several months to yield recombinant progeny. Thus the organism lacks the ease and refinement of genetic analysis that made some other microbial systems, such as *Escherichia coli,* ideal tools in molecular biology.

Early studies of phototropism were initiated at Cold Spring Harbor in 1953 and the next year Max persuaded Werner Reichardt, then studying insect optomotor responses at Tübingen, to join him in his *Phycomyces* project. This partnership resulted in a classic paper (1956b) proposing a kinetic model of adaptation to light that proved influential for other sensory systems, although it has recently been shown to be adequate only for dark adaptation in the normal intensity range in the case of *Phycomyces* (E. Lipson, pers. comm.).

Thereafter a *Phycomyces* Group grew slowly, recruitment being mainly from physicists with no defectors from the Phage Group apart from Max himself. The Cold Spring Harbor workshops, each lasting about two months and beginning in 1964, attracted many participants from abroad who spread the gospel. Some regularly visited Cold Spring Harbor or Caltech for periods of collaborative discussions or research, especially from France, Germany, Japan, and Spain.

In 1969 the *Phycomyces* cause was further publicized by a comprehensive review of the whole field, to which 12 members of the group made specialized contributions. In his introduction Max stated, "This review, then, is addressed to those who aim to push sensory physiology to the limits of molecular biology. We believe that what can be learned from *Phycomyces* is relevant to this next phase of our quest for a mechanistic understanding of life." While agreeing that *Phycomyces* does not permit analysis of electrical sig nals, "which sensory physiologists have come to consider the *sine qua non* of their trade," nevertheless he believed that there is "much room for similarities in earlier stages of the transducer chain . . . and the receptor potentials of animal sensory cells, and it is to these as yet obscure stages that we think *Phycomyces* work can make a contribution of general relevance" (1969).

The adaptation range of *Phycomyces* to light is about ten orders of magnitude, equivalent to that of the human eye, and sensitivity is specific for blue light (1960). Max's main interest in recent years was the nature of the photoreceptor, the most likely candidate being ß carotene or a flavin. With Katzir and Presti (1976a) he greatly extended the action spectrum and found absorption in the region of 600 nm which they interpreted as evidence for a flavin chromophore. Subsequently ß carotene was excluded by the use of mutants in which its synthesis was undetectable (1977a, 1978b). Finally, in his last published paper, Max and his colleagues (1981) found that the substitution of an analogue of riboflavin (roseoflavin, with a distinctive absorption spectrum) in a riboflavin auxotroph produces an equivalent shift in the action spectrum. It thus seems likely that the sporangiophore blue light receptor of *Phycomyces* is a flavin and not a carotene, although the precise nature of the compound remains unknown (see 15).

In the years that have elapsed since the 1969 review, much interesting work and some important technical advances have been made, especially in the field of behavioural genetics. For example, the introduction of a microsurgical technique for making heterokaryons and the development of isogenic mating types have revolutionized genetic analysis. A large number of behavioural mutants have now been isolated, involving photoresponses to sporangiophore development and carotene synthesis as well as various tro pisms. In addition, other mutants affecting the pathway of carotene biosynthesis have been obtained. Classification of these mutants according to their functional and sequential relationships is clarifying the organization of their underlying sensory pathways (review 15).

Although it is true that no major breakthrough has been made in understanding the basic mechanism of sensory transduction, this is also the case for other systems. It has been suggested that progress might have been quicker if more effort had been directed toward developing the basic genetics and biochemistry of *Phycomyces* during the early period. Only the physiological aspects were then energetically pursued, resulting in a lot of models unsupported by strong experimental evidence (A.P.E.).

Although Max remained dedicated to *Phycomyces* from 1953 onwards, he did not lose touch with phage research. Thus, with N. Visconti he developed a mathematical model of phage recombination based on multiple rounds of mating during the eclipse period (1953) while, a little later, he became interested in theoretical problems of DNA replication (1945b, 1957) and the genetic code (1958b).

The Cologne Interlude (1961-63)

After the war Max returned to Germany on several occasions, first in 1947, and then in 1954 when he visited Göttingen for 3 months. In 1956 he was invited to spend 3 months at Cologne by Josef Straub, who was Professor of Botany at the University and wanted Max to bring molecular genetics to his new institute which was among the first being built at that time. Max gave a phage course in the unfinished new building, still without electric light or concrete floors, "which was quite a tour de force" (1). It was during this course, at which Peter Starlinger (now Director of the Institute) came from Hamburg to give a seminar, that the idea took root of a Genetics Institute embracing several independent, integrated groups headed by professors, but having many facilities in common and an emphasis on research. This was a very novel concept for Germany and it was hoped that Max would agree to become the first director so that his reputation could be used in negotiations with the Government; but Max agreed for 2 years only, on leave of absence from Caltech, in the unlikely event of the project materializing.

A first step was the appointment of Carsten Bresch to a Chair of Microbiology in the Botany building, where he was joined by Rudi Haussmann, Peter Starlinger, Thomas Trautner, and A. H. Doermann who spent a sabbatical 1957 with them (P.S.). Then in 1959, thanks to the extraordinary negotiating ability of Josef Straub, the Institute was finally approved. During the developmental stages, Max visited Cologne about once a year to discuss plans for the future, and succeeded in obtaining funds from a semiprivate organization for two additional senior staff appointments which the university could not afford.

The Institute of Genetics building was eventually completed, and the staff moved during the summer of 1961. The Institute was formally dedicated in June 1962, with Niels Bohr as the principal speaker. His lecture, entitled "Light and life—revisited," commented on the original one of 1933, which had been the starting point of Max's interest in biology. It was to be Bohr's last formal lecture. He died before completing preparation of the manuscript of this lecture for publication (but see Delbrück 1976; also 10).

Max organized four groups of workers, under Carsten Bresch, Walter Harm (radiobiology), Peter Starlinger and Hans Zachau. In addition, he formed a group of his own which, surprisingly, he devoted to the study of the photochemical effects of ultraviolet light on DNA which had in terested him since the then recent discovery of thymine dimers (e.g., 1962b; 1963b).

Max also found time to talk to and encourage younger workers, and he established internal seminars which the whole Institute was supposed to attend in order to foster interactions. In addition, phage courses on the Cold Spring Harbor model were run every year from 1962 onwards, and in 1963, at Max's persuasion, a course on bacterial genetics was added (P.S.).

When Max left in 1963 he agreed to maintain connections with the Institute and was appointed as Honorary Professor. For some years thereafter he returned to Cologne every year or so to give a series of lectures, or just a seminar, often on a topic outside the normal curriculum. In Starlinger's opinion, Max's Cologne period was beneficial to German biology as a whole, not only on account of the courses he instituted, but also because of his extensive travelling and lecturing.

Later he was persuaded to serve as an adviser in natural science on the Founding Committee of the new University of Constance. "This led to a natural sciences faculty that was essentially all molecular biology—even the chemistry and physical chemistry were all molecular biology" (1). An agreement was reached with the university that he would spend one semester there in every six, but he did this only once, in the summer of 1969, when he indulged his more physical and mathematical interests (e.g., 2-D diffusion) with friends there. That was his and Manny's last long visit to Germany.

PERSONALITY

How can I begin to describe what Max was like to those who did not know him, for he was all things to those who did? His profound intelligence and scholarship in so many fields of science, philosophy and the arts, all of which he regarded as a cultural unity; his blend of critical and quietspoken aloofness with outgoing gregariousness, affection, and sense of fun; his basic seriousness and childish love of practical joking; all could be seen as the essence of paradox, or as the embodiment of "natural man." This, perhaps, has been best expressed by a close colleague of Max (D.R.S.) who "always thought of Max as a human archetype." Perhaps the best way to convey an impression of Max's individuality is through a kaleodoscope of reminiscences and impressions by various friends who knew him well.

> I remember vividly the discussions that we had and also the discussion among the circle of friends about physics, philosophy and human problems. Max was able to attract the best and most interesting people because of his wonderful personality and his direct approach to questions of interest. Many people know him as rather acid and critical, and sometimes even arrogant. It is true that he did not well tolerate half-truths and superficial remarks, but he was a warm friend to those whom he valued, and he was always ready to help, to discuss the problems and, last but not least, to have fun with his friends (V.F.W.).
>
> He abhorred the petty and in searching for the deepest of theories insisted that we work together in a collective generous fashion. The selfish and the avaricious were not tolerated, and those unfortunate souls who could only so survive, were not for Max. . . . He also had no use for stuffiness or protocol and was never Professor or Dr. Delbrück but Max to all who would learn with him (J.D.W.; 3).
>
> He was a compassionate man, very honest, with a slow but strong and deep intelligence (Germany style); he was half philosopher, half physicist, with a scale of values very different to the common scientific man. He enjoyed life every minute. He loved to talk with people and it is remarkable how he concentrated his mind to listen to them (A.P.E.).
>
> His playfulness translated quite literally into plays, the marionette shows he put on with his children, in which in a marvelous conceit, he often took the role of Uncle Max, the fusty professor with a thick German accent.

> Max was Max and sometimes he played Max. He also proposed to play Samuel Beckett, threatening to give the latter's Nobel acceptance speech for him when he failed to go to Stockholm (in 1969). He particularly admired the work of Beckett because, almost as a scientist, Beckett had reduced the complexities of human intercourse to their elements, a series of games turning in an eternal round (D.R.S.).
>
> . . .Delbrück's had been a kind of Gandhi of biology who, without possessing any temporal power at all, was an ever-present and sometimes irksome spiritual force. "What will Max think of it?" had become the central question of the molecular biology psyche (G.S.S.).

Among the most memorable features of life with the Delbrück group at Caltech was the extraordinary and informal hospitality of Max and Manny in their home in Pasadena which was "open house" to all and sundry; and the famous weekend camping trips to the desert, organized by Manny, that might include undergraduates, graduates, post-docs, staff, visitors, children and dogs, with long treks up and down the hills and canyons, on which Max might unexpectedly block the path by stopping abruptly to ponder a sudden thought. After returning to camp and a welcome siesta, Manny would prepare dinner over the camp fire. "Evening brought a big fire and wild stories until each wandered into the dark to find his own bag and pile of clothes under the sky freckled with stars. Yes *stars!* One would occasionally wake up to see a naked Max balancing his binoculars against the car. He was charting the movement of the planets and rediscovering for himself these movements as the ancients had done it" (N.D.). In recent years Max continued to enjoy desert trips and often he and Manny would take small groups of friends for mid-week picnic walks and talks over rough country closer to Pasadena.

Another more disciplinary aspect of Max's style is recalled by Seymour Benzer.

> The urge to do experiments was always so strong that we could not get ourselves to sit down and write up the results. Delbrück had a solution for this. He assembled all who had papers to write and whisked us off to Caltech's Marine Biology Station at Corona del Mar. There, we were locked up for three days and ordered to write. Delbrück's wife, Manny, typed as rapidly as we could spew the stuff out; we mercilessly criticized each other's drafts, and in three days everyone had completed a paper (2, pp. 157, 340).

Sense of Humour

Max's wit and humour were very much a part of his image because they accentuated the depth and seriousness of his personality in such a striking way. His wit was light and amusing, as when he told Jean Weigle that he supposed the *Festschrift* in honor of his (Max's) 60th birthday would be an opportunity for everyone to publish papers that had been rejected repeatedly by many journals. Again, he propounded his "Principle of limited sloppiness" to account for the emergence of important ideas from experiments that had not been rigorously controlled.

Another example of his wit, as well as of the playfulness mentioned above, is his introduction to the Commencement Address he delivered at Caltech in 1978, entitled "The arrow of time" (11). It appeared that a committee had suggested Max as speaker, while the students had again suggested the comedian, Woody Allen. "So," said Max, "what happened? Well, it's up to you to decide. Is it Max Delbrück as advertised, talking to you, or is it Woody Allen, impersonating a Senior Academic Citizen, scurrilously named Max Delbrück, or is it Max Delbrück, scurrilously pretending to be Woody Allen impersonating Max Delbrück? Having been trained in critical thinking for so long at Caltech I am sure you will enjoy pondering these alternatives while I, whoever I may be, go on with my talk"; and on he went to discuss very seriously the paradoxes of the nature of subjective and objective time, and of truth. Incidentally, I see in the margin of a copy of this address that he sent me the annotation, "Letter follows—but when?"!

Max's more farcical sense of comedy must be mentioned since it is an aspect of his personality that his friends remember so well, and which proved rather infectious within the Phage Group. For example, a British physicist (C.F.) who knew him in Berlin in 1937 remembers a summer party at his home to which "he invited half the guests in evening dress and the other half in casual tennis clothes, and he himself wore his grandfather's tail coat over old flannel bags."

In much the same vein, a visitor to Caltech in 1953 (E.S.A.) was invited to accompany the Delbrücks to a perfectly sober end of term students' play. To his astonishment, Max insisted on dressing up as a pregnant woman and Manny as "her" English husband, complete with moustache, bowler hat and furled umbrella, while he (E.S.A.) went as a friend attired in weird clothing. They arrived late at the play and "you can imagine the sensation we produced as we marched solemnly down the aisle to our seats near the front." Max and his party left before the end of the play, and it then transpired that the cast and many students and friends had been invited to the Delbrück home after the performance. Max now insisted that he and his guest exchange roles on the grounds that the prank would not otherwise be complete—a dénouement "which resulted in the utmost confusion when the guests arrived."

When Max was at Cologne he introduced a lifestyle that was quite atypical for Germany, such as organizing a treasure hunt through the whole of Lindenthal, while at parties in the Institute "there would be rather skillful cartoons exhibited, and sketches would be performed which would make fun of the Institute and mainly of the senior people" (P.S.). Of course, Max was sometimes "hoist with his own petard." For instance, it was his habit at Cologne to attend all the lectures of a course, reading a newspaper during the morning session, and then giving the last lecture himself. On one such occasion he was confronted, at *his* lecture, by the whole class who "pretended to be busy with their newspapers too. Max was a little startled at first, but then took it with good humor" (P.S.).

Finally, to show how intimidating Max might at first appear to those who didn't know him well, it was not uncommon for him, with a rather serious expression, to say to a lecturer after his performance, "Well, that was the worst seminar I have ever heard!"; but I should conclude this theme by saying that at least one victim of this comment of Max, George Streisinger, also recorded "the very great love and admiration that so many of us feel towards him" (2, p. 335).

The Intellectual Man

In the winter of 1972 Max gave an extensive course of 20 lectures at Caltech on "Evolutionary epistemology." He later condensed these into a long but elegant essay entitled "Mind from matter??" presented as a single lecture to the XIIIth Nobel Conference in 1977 (1978d). The essay ranges from cosmology and the beginning of life, through the evolution of prokaryotes and perception, higher organisms and behaviour, the nervous system, consciousness, language and culture, to cognitive ability. He then goes on to ask, if mind evolved and was selected merely for its survival value, "to let us get along in the cave, how can it that (it) permit(s) us to obtain deep insights into cosmology, elementary particles, molecular genetics, number theory? To this question I have no answer. . . . The feeling of absurdity that attaches to the notion "Mind from Matter" is perhaps of a similar nature to the feeling of absurdity we have learned to cope with when we permit relativity to reorganize time and space and quantum theory to reconcile waves and corpuscles. If so, then there may yet be hope for developing a formal approach permitting a Grand Synthesis." The essay begins with a brief recapitulation of Schrödinger's book, *What Is Life?,* and outlines Bohr's subtle complementarity argument. Thus Max's thinking continued to be swayed by Bohr's ideas, but in a new dimension, after 45 years.

However, my main object in mentioning this essay, and the series of lectures that beget it, is to emphasize the cosmic scope of Max's conceptions, and the breadth and quality of his educational influence at Caltech. Max taught regularly at Caltech and his "method of learning was to teach, and every year . . . he would assign himself the task of teaching a course in some new subject that *he* wanted to learn. This ranged all the way from statistical mechanics to epistemology. So Max became an expert in every one of those subjects. As recently as a year and a half ago, long after he had been officially retired, he volunteered to teach freshman physics here at Caltech as a sort of refresher course for himself" (S.B. 3). In fact he never lost his interest or skill in theoretical physics and mathematics and, as late as 1980, published a paper on Bose-Einstein statistics (1980a). Papers on *Phycomyces* phototropism continued to appear up to the year of his death.

However, his interest in formulations of objective reality was by no means limited to the modern era, but went back to Aristotle for whose biological observations and speculations he had great respect although his physics, as might be expected, rated "pretty much of a catastrophe." In his article, "How Aristotle discovered DNA" (1976c; see also 1971), Max explains, with many quotations, Aristotle's hypothesis that the male semen carries the *form principle* or plan of inheritance which does not become part of the embryo, unlike Hippocrates's theory that the semen consists of extracts or miniatures of each part of the body as in the later homunculus model. Indeed, two of Aristotle's arguments from observation, that the semen may determine either male or female, and that inheritance of skin colour can skip a generation, could well have served as a basis for Mendel's laws! (W.H.). Max goes on to recount the history of Aristotle's manuscripts (initially published in the *Scientific Athenian!),* and the final, accidental appropriation by theologians "of the most secondary and misguided aspects of Aristotle's speculations. It is due to this bizarre twist that we are encumbered today with a total barrier of understanding between the scientists and the theologians, from St. Thomas Aquinas to today."

David Smith, Associate Professor of Literature at Caltech, writes of Max, "His interest in the humanities was profound, of long duration, and increasing intensity. And it was a matter of day to day practical observance, as most things profound are. He often attended humanities seminars. He even sponsored one. . . . He was the most active supporter of the art gallery on the campus." Indeed the Berlin artist, Jeanne Mammen, was supported and encouraged for decades by Max but received recognition only after his death.

Poetry was of particular interest to Max and he was invited, in 1980, to lecture at the Poetry Center in New York, in the wake of such predecessors as T. S. Eliot and Dylan Thomas. He intended to talk about Rilke who interested him as the most intuitive of major German poets. Unfortunately, this fell through because of illness, but he had hopes for 1981 and had completed nine pages of his lecture at the time of his death, in which "he pursues Rilke's imagery, its sources, the shock value of image and syntax. He compares and criticizes translations, translates himself. He comments on the use of symbol, vocabulary, rhythm. He was intensely and poetically interested in words" (D.R.S.).

Max's musical tastes were classical, with a special liking for J. S. Bach. He played the piano poorly but with some enthusiasm, and taught himself the alto recorder well enough to play chamber music in home ensembles.

LAST DAYS

When Max reached the normal age of retirement in 1977, the Caltech trustees appointed him to the special position of Board of Trustees Professor of Biology, Emeritus, so that he could continue the research of his *Phycomyces* Group at the Institute. Early in 1978 he learnt that he was suffering from multiple myeloma, a cancer of the plasma cells of the bone marrow. This responded well to chemotherapy, apart from occasional remissions and the need for blood transfusions, so that he was able to travel to Paris with his daughter Nicola in the spring of 1979 to be inducted as a Foreign Member of the French Académie des Sciences.

He retained the interest of a scientist towards his disease from its beginning, never complained, and, from first to last, retained the upper hand. A few months before his death he suffered a mild stroke which impaired his vision on one side; he found this more interesting than disturbing, and smilingly said, "The students need me as a guinea pig; they are setting up some tests they cannot do with the monkeys" (B.C.).

When Max first learnt about his illness he started a diary which he called "Heimreise" ("Journey Home") to record his thoughts about its progress. Here are two entries:

> "Wohin gehen wir denn?" ("Where are we going?")
> "Immer nach Hause" ("Always toward home")

This quotation was written on 24 September 1978, and his thoughts on this theme were: "The journey of life which seems to be going outward, in the end turns out to have been going inward most of the time." On 5 March 1979 he wrote, "Im leichten Wellenschlag der Wochen treib ich dahin. Ein steuerloses Blatt bald zu verschwinden." ("I drift with the gentle undulation of the weeks. A rudderless leaf soon to disappear.")

During the last few weeks of his life, Max announced one day that he had decided to live for two more years in order to complete his autobiography which he had recently started to write. Only 3 days before his death he began to dictate the chapter "Light and life" (B.C.).

I WISH TO RECORD my most grateful thanks to Max's wife, Manny Delbrück, for her invaluable help in compiling this Memoir and commenting on the draft manuscript, and also to their daughter Nicola (N.D.) for her impressions of family life. Dr. Patricia Burke kindly provided me with a full bibliography, compiled with Manny's assistance, and Professor L. Hood provided an up-to-date list of Max's honours and other data. I am also indebted to many people who offered me impressions and reminiscences of Max. Personal and scientific recollections of his early career in theoretical physics were sent to me by Professor Sir Charles Frank, O.B.E., F.R.S. (C.F.) who also put me in touch with the University of Bristol, Dr. G. Herzberg, F.R.S., Professor N. Thompson (N.T.), and Professor V. F. Weisskopf (V.F.W.). Professor A. P. Eslava (A.P.E.) and Dr. E. D. Lipson provided assessments of the work of the *Phycomyces* Group, and Professor P. Starlinger (P.S.) enlightened me on the conception and birth of the Cologne Institute. Dr. P. M. Gresshoff, Professor G. S. Stent, and Professor M. J. D. White, F.R.S. kindly suggested appropriate amendments to the draft manuscript. Finally, I must also thank the following for permitting me to quote from their personal communications, contributions to Max's Memorial service, and other unpublished sources: Professor E. S. Anderson, F.R.S. (E.S.A.), Professor S. Benzer (S.B.), Fraulein Beate Carrière (B.C.) who recorded the last few weeks of Max's life (translated for me from the

German by Dr. P. M. Gresshoff), Dr. D. R. Smith (D.R.S.), Professor G. S. Stent (G.S.S.), and Dr. J. D. Watson (J.D.W.).

(The initials in brackets indicate the sources of quotations in the text.)

FAMILY

Married: 2 August 1941, Pasadena, California, to Mary Adeline Bruce (born 1917 in Butte, Montana, U.S.A.), daughter of James Latimer Bruce, mining engineer, and Leah Hills Bruce.

Children: son, Jonathan, born 1947 in Nashville, Tennessee;
daughter, Nicola, born 1949 in Pasadena, California;
son, Tobias, born 1960 in Pasadena, California;
daughter, Ludina, born 1962 in Cologne, W. Germany.

HONORS

Election to
U.S. National Academy of Sciences—1949
American Academy of Arts and Sciences—1959
Royal Danish Academy—1960
Deutsche Akademie der Naturforscher Leopoldina—1963
Royal Society of London, Foreign Member—1967
Académie des Sciences, Paris, Associé Étranger—1979

Honorary Degrees
Copenhagen University—1965: Doctor of Philosophy
University of Chicago—1967: Doctor of Science
Heidelberg University—1968
Harvard University—1971: Doctor of Science
Gustavus Adolphus College, St. Peter, Minnesota, U.S.A.—1977: Doctor of Science
University of Southern California—1981: Doctor of Science Göttingen University—1981: Doctor of Philosophy (to commemorate 50th anniversary of first degree)

Awards
Kimber Medal for Genetics (U.S. Academy of Science)—1964
Gregor Mendel Medal (Deutsche Akademie der Naturforscher-Leopoldina)—1967
Gross-Horwitz Prize (Columbia University)—1969
Nobel Prize for Physiology or Medicine—1969

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Michael Doudoroff

MICHAEL DOUDOROFF

November 14, 1911-April 4, 1975

BY H. A. BARKER

MICHAEL DOUDOROFF WAS A general microbiologist who made major contributions to knowledge of carbohydrate metabolism in bacteria. His early studies of sucrose utilization by *Pseudomonas saccharophila,* a bacterium he isolated and made famous, established the importance of glucosyl transfer reactions in metabolism and provided the first substantial evidence that an enzyme may function as a glucosyl carrier. His investigations of glucose oxidation by extracts of *P. saccharophila* resulted in the discovery of a major pathway of glucose degradation in bacteria, the Entner-Doudoroff pathway. Other sugars were shown to be metabolized by similar, but divergent, pathways. His studies of assimilatory processes in aerobic and photosynthetic bacteria demonstrated that poly-ß-hydroxybutyric acid is a major storage product formed from substrates metabolized via acetate or butyrate and is utilized by means of both intracellular and extracellular enzymes. In the latter part of his career Doudoroff and his associates extensively clarified taxonomic and phylogenetic relationships in the genus *Pseudomonas* and certain other aerobic bacteria.

Doudoroff was born in Petrograd (St. Petersburg), Russia, the son of a naval officer. In 1917 his father became a member of the short-lived Kerensky government and sub sequently was appointed naval attaché to the Russian embassy in Japan. The family left Russia shortly before the October revolution and lived in Tokyo for six years before moving to San Francisco in 1923. They moved to Palo Alto in 1930.

In Tokyo young Michael started his formal education in the third grade of an English-language school after having previously been tutored privately in English, French, and probably other subjects. In San Francisco he attended Lowell High School, the best college preparatory school in the city. Like many other children, Michael first developed an interest in biology by observation of curious or beautiful insects. He began collecting beetles and later butterflies in Japan and greatly enlarged his collection in California. One of the butterflies he collected turned out to be a new species and was given the specific name *doudoroffii.* On entering Stanford University in 1929, he planned to major in biology and specialize in entomology. However, as his exposure to science broadened, he was attracted to bacteriology and protozoology. As an undergraduate he carried out two short studies on aspects of bacterial variation under the guidance of Professor W. H. Manwaring. His master's thesis, done under the supervision of Dr. A. C. Giese, demonstrated that the survival of *Paramecium* at elevated temperatures is strongly influenced by its nutritional status. For his Ph.D. thesis research (1934-39), Doudoroff moved to the laboratory of Professor C. B. van Niel at the Hopkins Marine Station, where he investigated a topic of his own choosing, the adaptation of *E. coli* to elevated salt concentrations. He demonstrated that this involves both an acclimatization, independent of reproduction, and a selection of cells with an increased salt tolerance. While at the Marine Station he twice served as van Niel's assistant in the soon-to-become-famous course in general microbiol ogy and was introduced to the extraordinary physiological and biochemical diversity of the microbial world. This led him to undertake some studies of luminous bacteria and H_2-oxidizing bacteria. His main contribution to knowledge of bacterial luminescence resulted from the discovery that certain poorly luminescent strains are unable to synthesize riboflavin. Addition of a small amount of this vitamin to deficient media caused an increase in growth without affecting luminescence, whereas a larger addition increased luminescence without any further stimulation of growth or respiration. These observations provided the first evidence that riboflavin is directly involved in bacterial luminescence.

The H_2-oxidizing bacteria that Doudoroff isolated included a new species, *Pseudomonas saccharophila,* which can also oxidize a number of mono-, di-, and polysaccharides. Since most bacteria only oxidize di- and polysaccharides after first hydrolyzing them to monosaccharides, Doudoroff was surprised to find that cells of *P. saccharophila,* grown upon sucrose, oxidize this sugar much more rapidly than its constituent monosaccharides, glucose and fructose. His efforts to elucidate this anomaly—subsequently shown to be caused by the absence of permeases for the monosaccharides—led him in time to undertake a series of brilliant investigations on the enzymatic mechanisms of the degradation of sucrose and other sugars by bacteria.

In 1940 Doudoroff joined the faculty of the Bacteriology Department, University of California, as an instructor. His first major research contribution at Berkeley, made in collaboration with N. O. Kaplan and W. Z. Hassid, was the discovery that extracts of *P. saccharophila* catalyze a reaction between sucrose and inorganic phosphate to form glucose 1-phosphate and fructose. Since the reaction proved to be readily reversible, it was used to synthesize sucrose, a sugar not previously synthesized by either chemical or enzymatic methods. The enzyme catalyzing this reaction was subsequently partially purified and shown not to degrade or synthesize any common disaccharide other than sucrose. However, Doudoroff and his associates found that in the reverse (synthetic) reaction fructose can be replaced by certain analogs, D-ketoxylose and L-sorbose, resulting in the formation of novel analogs of sucrose.

Insight into the mode of action of sucrose phosphorylase was obtained by studying the incorporation of radioactive inorganic phosphate into glucose 1-phosphate. Initially, Doudoroff and associates thought the enzyme incorporated inorganic phosphate into glucose 1-phosphate only in the presence of fructose or sucrose, which permitted the reversal of the overall reaction. However, they found that only glucose 1-phosphate, orthophosphate, and enzyme are needed to effect a rapid exchange of phosphate between the two substrates. This led to the concept that sucrose phosphorylase functions as a transglucosidase, an enzyme that transfers the glucosyl residue from a suitable donor such as sucrose or glucose 1-phosphate to an appropriate glucosyl acceptor such as fructose or orthophosphate. Supporting evidence was provided by showing that the enzyme catalyzes transfer of the glucosyl moiety of sucrose to sorbose to form glucose 1-sorboside and fructose in the absence of orthophosphate. This and other similar experiments provided some of the first evidence for the formation of a substrate-enzyme complex as an intermediate in an enzymatic reaction.

Attempts by Weimberg and Doudoroff to demonstrate directly the formation of a glycosyl-enzyme complex in the sucrose phosphorylase reaction were unsuccessful because of insufficient purification of the enzyme by the methods then available and because of an intrinsic hydrolytic activity of the enzyme. After better purification techniques were developed, R. H. Abeles and his associates, in 1967, purified the enzyme to homogeneity and demonstrated that it does indeed bind transferable glucose, as had been postulated many years earlier.

The previously mentioned synthesis of the novel nonreducing sucrose analog, glucose 1-sorboside, by sucrose phosphorylase was followed by the synthesis of three other analogs, glucosido-D-ketoxyloside, glucosido-L.ketoarabinoside, and glucosido-rhamnoside (Doudoroff and Hassid, 1948,4). These compounds all contain 1-5 linkages between the monosaccharide units. Unexpectedly, the same enzyme was also found to catalyze the synthesis of glucosido-L-arabinose, a reducing sugar containing a 1-3 linkage. The role of sucrose phosphorylase in this synthesis appears to have been firmly established, but the mechanistic basis for the formation of this structurally distinct product could not be established.

Doudoroff, Hassid, and Barker (1947,1-4) found that arsenate can substitute for phosphate in the sucrose phosphorylase reaction as it does in the oxidation of 3-phosphoglyceraldehyde. The presumed product, glucose 1-arsenate, is unstable and is hydrolyzed to glucose and arsenate. The net result is an "arsenolytic" conversion of sucrose to glucose and fructose. In the presence of arsenate, glucose 1-phosphate undergoes a similar enzymic cleavage. Later Doudoroff, Katz, and Hassid (1948,1) showed that potato phosphorylase catalyzes an arsenolytic conversion of amylose and amylopectin to glucose.

A second type of phosphorolytic enzyme, maltose phosphorylase, was found in *Neisseria meningitidis* by Doudoroff and Fitting (1952,2). Extracts of this organism had been shown previously to catalyze a reaction between the disaccharide maltose and orthophosphate to form glucose and a phosphate ester with properties similar to those of glu cose 1-phosphate when the isolated ester was incubated with glucose and the *Neisseria* enzyme maltose was formed. However, the phosphate ester derived from maltose did not serve as a glucosyl donor in the sucrose phosphorylase reaction, and synthetic α-D-glucose 1-phosphate could not serve as a cosubstrate for the *Neisseria* enzyme. These observations led to the conclusion and subsequent demonstration that the phosphate ester product of maltose phosphorylase has the β rather than the α configuration. The mechanism of action of maltose phosphorylase was shown to differ from that of sucrose phosphorylase. The former, unlike the latter, is unable to catalyze a direct exchange between β-D-glucose 1-phosphate and orthophosphate or arsenate and is unable to cause an exchange between maltose and glucose in the absence of the phosphate ester. On the basis of these results, Doudoroff and Fitting proposed that the mechanism of the maltose phosphorylase reaction involves a maltose-enzyme-phosphate complex as a probable intermediate.

The discovery of sucrose and maltose phosphorylases led Doudoroff to investigate the mechanisms of synthesis or degradation of other polymeric carbohydrates by *P. saccharophila* and other bacteria. Raffinose—a trisaccharide of galactose, glucose, and fructose, and an analog of sucrose—was found not to undergo a phosphorolytic cleavage but to be hydrolyzed by the enzyme melibiase to galactose and sucrose (Doudoroff, 1945,2). Trehalose, a nonreducing glucose disaccharide, is also cleaved hydrolytically by the enzyme trehalase. Maltose was found to be neither hydrolyzed nor phosphorolyzed in *E. coli* but is converted by the enzyme amylomaltose, a transglucosidase, to glucose and a glucose polymer (Doudoroff et al., 1949,2). The latter then undergoes phosphorolysis to form glucose 1-phosphate, which is further metabolized via glucose 6-

phosphate. Doudoroff and O'Neal (1945,3) investigated the reversibility of the long-known bacterial conversion of sucrose to levulan, a fructose polymer, and glucose by an enzyme from *Bacillus subtilis.* By using invertase to detect small amounts of sucrose form in the enzymic reaction between levulan and glucose, they obtained evidence for the reversibility of levulan synthesis. Although the enzymatic mechanisms of oligosaccharide degradation elucidated by Doudoroff and his associates were of great interest, they did not account for the preferential ability to metabolize, for example, sucrose more rapidly than its constituent monosaccharides. Doudoroff was acutely aware of this lack of understanding and in two reviews (1945,2; 1951,2) proposed and critically evaluated various possible explanations. He reached the conclusion, later found by others to be correct, that the utilization of sugars is controlled by permeability mechanisms involving sugar-specific carrier proteins. His thoughtful analysis of this problem undoubtedly stimulated the development of this important area of research.

About 1950 Doudoroff and his associates began a series of investigations of the oxidative degradation of various sugars by *P. saccharophila* that revealed several new pathways of carbohydrate metabolism. Doudoroff and Entner (1952,1) studied the enzymatic oxidation of glucose and identified glucose 6-phosphate, 6-phosphogluconate, D-glyceraldehyde 3-phosphate, 3-phosphoglycerate, and pyruvate as intermediate products. The novel feature of this widely used pathway is the conversion of 6-phosphogluconate to pyruvate and glyceraldehyde 3-phosphate. 2-Keto-3-deoxy-6-phosphogluconate was postulated to be an intermediate in this reaction and was subsequently shown to fulfill this role by Doudoroff and MacGee (1954,1), who isolated and characterized the compound. The enzyme catalyzing cleavage of the keto acid, ketodeoxyphosphogluconate aldolase, was later purified and crystallized by Doudoroff and Shuster (1967).

Several other sugars were shown to be metabolized by *P. saccharophila* by similar but partially divergent pathways. L-and D-arabinose and L-galactose are all metabolized via the corresponding nonphosphorylated aldonic gamma-lactones and aldonic acids. Then the pathways diverge. L-Arabinose is oxidized via an unidentified, unstable intermediate to a-ketoglutarate in such a way that the carboxyl carbon adjacent to the carboxyl group is derived from the carboxyl carbon of arabonic acid (Doudoroff and Weimberg, 1955). Reactions of the tricarboxylic acid cycle were shown not to be involved in this novel reaction. The α-ketoglutarate is further oxidized to pyruvate. D-Arabonic acid is dehydrated to 2-keto-3-deoxy-arabonic acid, which is cleaved and oxidized to pyruvate, derived from carbon atoms 1 to 3, and glycolate, derived from carbon atoms 4 and 5 (Doudoroff et al., 1956,1,2; Doudoroff and Palleroni, 1956,3,4). D-Galacturonic acid is dehydrated to form 2-keto-3-deoxygalactonic acid, which, after phosphorylation, is cleaved by a specific aldolase to pyruvate and glyceraldehyde 3-phosphate (Doudoroff and DeLey, 1957,2; Doudoroffet al., 1957,3,4; Doudoroff and Wilkinson, 1964; Doudoroff and Shuster, 1967). Fructose, which is well utilized by certain mutants of *P. saccharophila,* is converted to fructose 6-phosphate and then metabolized in the same way as glucose (Doudoroff et al., 1956,1,2). Doudoroff and Szymona (1960) found that *Rhodopseudomonas spheroides* contains enzymes of both the Embden-Meyerhoff and Entner-Doudoroff pathways of glucose degradation but apparently uses the latter pathway preferentially. Several enzymes catalyzing the above reactions and a novel mannose isomerase (Doudoroff and Palleroni, 1956,3,4) were purified and their properties determined.

Following the recognition of oxidative assimilation, the conversion of a large fraction of an oxidizable substrate into cellular components by washed suspensions of microorganisms, Doudoroff (1940,2) investigated this phenomenon in *P. saccharophila.* During this study, the ability of the organism to metabolize sucrose more rapidly than glucose and fructose was first observed. Subsequently, Doudoroff and his associates (Doudoroff and Whelton, 1945,1; Bernstein 1944) extended their studies of oxidative assimilation in *P. saccharophila* to compare the magnitude of assimilation in growing cultures with that in cell suspensions. These studies and those in other laboratories during the 1940s established the large magnitude of assimilation in several aerobic bacteria oxidizing a wide range of substrates but did little to clarify the underlying metabolic reactions. However, Doudoroff and Wiame (1951,4) made a solid contribution to knowledge of oxidative assimilation by studying the oxidation of ^{14}C-labeled substrates. They found that both carbons of acetate, carbons 2 and 3 of lactate, and the two methylene carbons of succinate are largely assimilated, whereas the carboxyl carbons of lactate and succinate are mainly converted to carbon dioxide. This indicated that the acetyl moieties derived from various substrates are probably a major source of assimilated carbon. Doudoroff and Stanier (1959,2) were stimulated to develop a more general explanation of oxidative assimilation by the observation of their colleague, Germaine Cohen-Bazire, that purple bacteria accumulate massive amounts of poly-β-hydroxybutyric acid during growth on certain organic acids. Poly-β-hydroxybutyric acid (PHB) was originally discovered by Lemoigne in 1927 as a major component of the cells of *Bacillus megaterium.* Doudoroff and Stanier examined the products of oxidative assimilation from glucose, acetate, and butyrate in *P. saccharophila* and of photosynthetic as similation from acetate and butyrate in *Rhodospirillum rubrum* and found with all those substrates that a major fraction (60 to 90 percent) of the assimilated carbon initially accumulated within the cells as PHB. When the external substrate was removed, the stored polymer was degraded intracellularly. These observations indicated that PHB can serve as an important reserve of carbon and energy as does starch or triglycerides in other organisms.

Doudoroff et al. (1959,1) also made a major contribution to understanding the role of organic substrates in bacterial photosynthesis. Earlier van Niel had concluded that organic substrates serve primarily, if not exclusively, as sources of reducing power for the conversion of carbon dioxide to cellular components. By using ^{14}C-labeled acetate and butyrate, Doudoroff et al. showed that the oxidation of these substrates and the reduction of carbon dioxide are minor reactions. Most of these and other substrates are assimilated directly as poly-β-hydroxybutyric acid or as polysaccharides.

Doudoroff and his associates investigated the enzymes involved in PHB synthesis and degradation. The immediate precursor of PHB was shown to be D-β-hydroxybutyrylcoenzyme A, presumably formed by reduction of acetoacetylcoenzyme A (Doudoroff and Merrick, 1961,1). The polymerase was found to be associated with granules of PHB and could not be obtained in soluble form. Although washed granules and associated enzyme convert β-hydroxybutyryl-coenzyme A to PHB in relatively high yields, the inability to separate enzyme and product prevented detailed analysis of the system (Doudoroff, 1966,2). *Rhodospirillum rubrum,* which stores PHB, contains a soluble intracellular enzyme system that degrades the polymer. This system also was found to be unexpectedly complex and refractory to analysis. Purified PHB is inactive as a substrate; only the PHB in washed granules, derived from cells that synthesize the polymer, can be hydrolyzed. Even the granules proved to be very labile. They are inactivated as substrates for the soluble hydrolytic enzyme system by mild treatments such as freezing and thawing or even repeated washing by centrifugation. The soluble intracellular enzyme system that degrades PHB was shown to contain at least three separable components, a thermostable activator, a thermolabile depolymerase, and an esterase. The activator, which appears to be a protein, causes no demonstrable chemical change in granules but is essential for the activity of the depolymerase. Together these two components hydrolyze PHB mainly to D-β-hydroxybutyric acid and smaller amounts of dimer. The esterase hydrolyzes the dimer. The β-hydroxybutyric acid was shown to be oxidized to acetoacetate by a DPN-dependent dehydrogenase that was purified from both *R. rubrum* and *Pseudomonas lemoignei* and studied in some detail.

Since large amounts of PHB synthesized by microorganisms must be available in nature, Doudoroff et al. (1965,3) investigated the ability of aerobic soil bacteria to use extracellular PHB as a major energy source. They isolated over 50 strains of PHB-using bacteria, most of which belong to the genus *Pseudomonas.* The most active polymer-using strains belong to a new species, *P. lemoignei.* This organism produces extracellular enzymes that hydrolyze purified polymer to a mixture of D-β-hydroxybutyric acid, the dimeric ester of this acid, and small amounts of trimeric ester. They also form an intracellular "dimeric hydrolase" that hydrolyzes the dimeric ester. All these enzymes are produced constitutively, regardless of the carbon source on which the bacteria are grown. Both the highly specific dimer hydrolase and an NAD-specific D-β-hydroxybutyrate dehydrogenase were partially purified from extracts (Doudoroff et al., 1965,3,4). Doudoroff and Lusty (1966,3) subsequently found that the extracellular PHB depolymerase of *P. lemoignei* can be separated into two fractions, one of which preferentially forms trimers and then hydrolyzes them to dimers and monomers in roughly equal molar quantities, whereas the other fraction preferentially forms dimers and relatively low yields of monomeric D-ß-hydroxybutyric acid.

In the early 1960s, Doudoroff was persuaded by his somewhat domineering colleague Roger Stanier to undertake a collaborative study of the taxonomy of the genus *Pseudomonas.* Initially he was reluctant to participate in this undertaking, which differed greatly from his previous research and could be expected to involve the collection of a vast amount of data obtained by relatively routine methods. However, he gradually developed enthusiasm for the project, and in collaboration with Stanier, Norberto Palleroni, and several graduate students and postdoctoral fellows made extensive contributions to knowledge of the taxonomy of pseudomonads and certain other aerobic bacteria.

The first publication resulting from this investigation was a massive survey of 169 phenotypic characters of 267 strains of *Pseudomonas* (Doudoroff et al., 1966,1). The ability to utilize 146 organic compounds as sources of carbon and energy was determined. Other characteristics studied included production of extracellular hydrolases, denitrifying ability, H_2 chemolithotrophy, pigment production, accumulation of poly-ß-hydroxybutyric acid as a cellular reserve material, biochemical pathways of aromatic ring cleavage, and the type of aerobic electron transport system. Analysis of the data so obtained permitted recognition of a relatively small number of species that can be distinguished from each other by multiple, unrelated phenotypic differences. Perhaps the most important result of these studies was the recognition that classification of aerobic bacteria by phenotype requires the determination and correlation of a much larger number of nutritionally diverse characters than had previously been used for this purpose.

Although the initial classification of pseudomonads was based entirely on phenotypic characters, Palleroni, Doudoroff, and associates later used the newer techniques of DNA-DNA and ribosomal RNA-DNA hybridization to investigate the genotypic and phylogenetic relations among species. On the basis of ribosomal RNA homologies they were able to divide thirty-five species or subspecies of *Pseudomonas* and one species of *Xanthomonas* into five major evolutionary lineages. Closer relationships among species within each lineage were established by DNA homologies. These comprehensive studies, which looked at bacterial classification from the point of view of phenotypic analysis, genetic relationship, and comparative biochemistry, served as a model for subsequent investigations. Doudoroff and Palleroni summarized their conclusions about the taxonomy of *Pseudomonas* in the *Annual Review of Phytopathology* (1972,3) and developed a practical scheme for identification of twenty-nine species for the eighth edition of *Bergey's Manual of Determinative Bacteriology.*

With Stanier and others Doudoroff investigated the taxonomy of other bacteria, including some denitrifying bacteria, H_2-utilizing bacteria, and organisms of the *Moraxella* group. The denitrifying bacteria, most of which had been previously classified as *Pseudomonas denitrificans,* were found to belong to several species and at least two genera on the basis of phenotypic characters and DNA and ribosomal RNA homologies. The H_2-utilizing bacteria, previously placed in the genus *Hydrogenomonas,* were shown to be a heterogeneous group; some organisms were assigned to the genus *Alcaligenes* and others to the genus *Pseudomonas.* The authors proposed that the genus *Hydrogenomonas* be discarded. Studies of the *Moraxella* group, done mainly by Paul Baumann, supported separation of these organisms into two genera, *Moraxella* and *Acinetobacter,* on the basis of a cytochrome c-dependent oxidase reaction, and recognized one species of *Acinetobacter* and several species of *Moraxella* on the basis of correlated phenotypic properties.

Doudoroff exerted a profound influence on the teaching of bacteriology at Berkeley. When he joined the department of bacteriology as an instructor in 1940, the courses of instruction emphasized mainly the medical and paramedical aspects of the subject. Doudoroff was given responsibility for teaching the introductory lecture and laboratory courses in general bacteriology, and he proceeded to reorganize them along the lines developed by C. B. van Niel and the Delft School of Microbiology. This involved the presentation of bacteria and other microorganisms as creatures whose structures, behaviors, and metabolic activities were worthy of study independently of their roles in agriculture, industry, or disease. Doudoroff brought great enthusiasm, a broad knowledge of general microbiology, and more than a touch of drama to his teaching. He was solely responsible for instruction in general microbiology for some years until R. Y. Stanier and E. A. Adelberg joined the department. Together they later wrote the excellent and widely used textbook, *The Microbial World,* based upon the courses Doudoroff had developed and which he continued to teach until his death. Thus, his influence on the teaching of bacteriology extended far beyond the university.

Doudoroff had a warm, outgoing personality. He loved conversation, the give and take of a lively discussion. He often enlivened seminars with penetrating questions or stimulating comments. As a scientist he was an accomplished experimentalist with insight to recognize and skill to solve a variety of biochemical and biological problems.

Doudoroffs contributions to microbiology and biochemistry were recognized by several honors and awards. In 1945 he received the first Sugar Research Award of the National Academy of Sciences with H. A. Barker and W. Z. Hassid. He became a J. S. Guggenheim Foundation fellow in 1949 and collaborated with Fritz Lipmann at Massachusetts General Hospital and Jacques Monod at the Pasteur Institute. In 1960-62 he held a Miller Research Professorship at the University of California, Berkeley, and in 1963 he was awarded a National Institutes of Health Special Postdoctoral Fellowship for studies with Professor Georges N. Cohen at the Centre National de la Recherche Scientifique, Gif-sur-Yvette, France. In 1962 he was elected to membership in the National Academy of Sciences.

In the late 1930s, Doudoroff married Mary Gottlund, a painter of considerable ability. They had one son, Michael John, now a professor of Spanish at the University of Kansas. The Doudoroffs were divorced about 1944, and he subsequently married Rita Whelton, who had been one of his graduate students. She died after a few years. His third wife, Olga Fowlks, had a son and daughter by a previous marriage. They formed a happy family. The death of Olga in 1974 was a crushing blow to Doudoroff. He died of cancer the following year after a short illness at the age of sixty-three.

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Photograph by George Brauer

Herbert Friedmann

HERBERT FRIEDMANN

April 22, 1900-May 14, 1987

BY S. DILLON RIPLEY

HERBERT FRIEDMANN, innovative museum director and long a productive zoological curator, was one of this country's most scholarly ornithologists. His technical specialties focused on the evolution of brood parasitism in birds and other aspects of bird behavior, avian taxonomy, cerophagy and wax digestion by honey guides, and the significance of animal symbolism in the art of the Renaissance and Middle Ages. Elected a member of the Academy in 1962, Dr. Friedmann was born in Brooklyn on April 22, 1900, and died, of cancer, at Saddleback Hospital in Laguna Hills, California, on May 14, 1987. He is survived by his wife, Karen Juul Vejlo, of Laguna Hills; one daughter, Karen Friedmann Beall (Mrs. Dale K. Haworth), of Northfield, Minnesota; and one brother, Ralph Friedman, of Manhattan.

In his eighty-seven years, Herbert Friedmann never ceased his pursuit of intellectual challenges, offered by a broad range of interests. These overshadow his less well-known achievements in the field of museum administration. His reorganization of the Los Angeles County Museum is testament to his leadership abilities. Yet he will be best remembered as a thoughtful scholar. In the following pages I provide a brief outline of his life and his major accomplishments and hope to impart a little of his gentle charm and wit, which I first came to know when, in 1941, I worked as an assistant curator under his benign tutelage at the then-named United States National Museum, in Washington, D.C.

Herbert Friedmann spent his childhood in Brooklyn, where his father was a druggist. The elder Friedmann had left his native Lithuania as a young pharmacist in the early 1890s and was followed by his wife-to-be (a teacher) a couple of years later. Herbert was the second of four sons. The demands of a drugstore left little time for family outdoor activities, but the great resources of New York City served the sons well, not only the three who gravitated to professions in law, medicine, and finance but the future naturalist as well. In addition to the strictly educational resources, the young Friedmann enthusiastically took advantage of available standing room at the city's centers of performing arts and treasured forever his memories of such luminaries as Caruso, Melba, and Isadora Duncan.

Friedmann's interest in birds developed gradually. By the time he was twelve years old, museums were his favorite haunts, in particular the Metropolitan Museum of Art and the American Museum of Natural History. In his high school years he and a brother joined a bird club, and he later started keeping detailed records of birds he saw in the parks of New York City and its vicinity. When he entered City College of New York at the age of sixteen, the thought of studying birds was taking shape. Summer jobs on a tobacco farm in Connecticut and a dairy farm in New Jersey gave him added opportunities to observe birds. Because the armistice was signed a few months after his enlistment in military service, his education was not delayed by overseas military service. His first serious ornithological study was carried out at the New York Zoological Society's Bronx Zoo and was later published under the title "Weav ing of the Red-Billed Weaver Bird in Captivity." This work so impressed Dr. William Beebe that he urged Friedmann to seek a scholarship to Cornell University to study birds. In three postgraduate years at Cornell, Friedmann completed his doctorate. His dissertation addressed the behavior of brood parasitism by cowbirds. The study was enlarged through field work abroad and appeared later in book form—The *Cowbirds: A Study in the Biology of Social Parasitism,* a work that after six decades continues to be cited as a key reference on this subject.

After teaching a summer course at the University of Virginia in 1923, Friedmann was affiliated with Harvard University as a postdoctoral fellow of the National Research Council, under the tutelage of William Morton Wheeler. Much of this fellowship was spent in the field, in Argentina (1923-24), on the Mexican border (May 1924), and then in Africa (1924-25). He made extensive zoological collections during his overseas expeditions (a tree with its remarkable composite nest of colonial weaver birds is still at the American Museum), and the African fauna in general captivated his imagination. Decidedly his primary interest, however, was bird behavior reflected in the social habits of the weaverbirds and brood parasitism of cuckoos, honey guides, and weavers.

Following his postdoctoral studies, Friedmann taught in the Biology Department at Brown University (1925-26) and at Amherst College (1927-29). While at Amherst he scheduled his teaching so that he could spend several days each week working at the Museum of Comparative Zoology at Harvard. During this period his research centered on African ornithology and brood parasitism.

Friedmann's studies of African birds made his appointment as curator in the Division of Birds, U.S. National Museum, a logical step toward his goal of devoting full time to ornithological research. His deep interest in museum research was evident from his close association with the American Museum of Natural History during his undergraduate years at City University and later his affiliation with the Museum of Comparative Zoology during his years in Massachusetts.

Friedmann had barely settled in at the Smithsonian when the 1929 stock market crash occurred and the heavy hand of the Depression began to make itself felt. A contemplated expansion of work into areas of bird biology had to be largely abandoned, and the lack of funds also brought retrenchment of publication schedules for larger works. Friedmann's publication of seventy papers and several books during this period indicates the amount of energy that he dedicated to his various ornithological pursuits.

In March 1942, on my own first day as an assistant curator of birds at the U.S. National Museum, I had the opportunity to meet Herbert Friedmann. As a new curatorial replacement for the late J. H. Riley, I shared a book-lined room with Herbert Deignan, and Friedmann himself, and began to learn the scope of my new position. The two Herberts showed me the Bird Division, its huge gallery of collections, and the windows that gave the best available natural light for examining bird specimens.

Returning to Mr. Riley's old rolltop desk, I heard a dull rolling sound out in the corridor. The rolling sound stopped in the doorway, and I could see the dim shine of brass, in heaps on the platform of a large dolly. In came an extremely cheery man carrying a brightly polished spittoon. He approached my desk, bent down, and reverently placed the brass object in the exact center of a square of rubber that I had noticed and wondered about earlier that morning. The rubber was stamped with a concentric series of circles, raised in the center. The man fitted the spittoon exactly into the center of the innermost circle.

At that point, Herbert Friedmann exclaimed, "Oh no, Dillon, you don't want that thing." I noticed that his desk lacked the government-issue rubber squares.

"What's it for?" I asked.

"Riley chewed," explained Herbert. Riley, the former assistant curator, was an elderly tobacco-chewing Virginian.

I asked the gentleman to leave the spittoon. At an annual salary of $2,600, it seemed to me that I had to grab whatever perquisites were offered, and so the spittoon stayed, refreshed each weekday until I departed the institution, with my doctoral thesis completed, three months later.

In the latter part of the thirties and during the war years, Friedmann found opportunity to pursue his interest in art—he took evening classes in drawing, painting, and sculpture at the Corcoran School of Art—and he developed a particular interest in the symbolic use of animals in art, spending a great many evenings in a reserved cubicle at the Library of Congress. The result was his widely recognized book *The Symbolic Goldfinch,* published in 1946 by the Mellon-financed Bolligen Press. It was a matter of more than casual interest to him—in fact, a source of some delight—that the National Gallery of Art was growing up on the Mall next to the U.S. National Museum at this time. John Walker, its second director, in his 1963 book *The National Gallery in Washington, D.C.,* describes the role Friedmann played in securing the Kress Collection for the gallery, one of only three collections to grace its walls when it opened in 1941.

Dr. Friedmann was to remain closely associated with museum-based ornithological research for the thirty-six years between 1925 and 1961. He served as curator of birds at the Smithsonian from 1929 to 1958 and was head curator of zoology from 1958 to 1961.

The research work of Herbert Friedmann had a number of foci, and yet the scope of his interest was truly without limit, as evidenced by the appended listing of his major publications. He is perhaps best known for his work on the evolution of brood parasitism in birds, as practiced by the cuckoos, cowbirds, honey guides, and weaverbirds. A brood parasite builds no nest of its own, but instead lays its eggs in the nest of another species. The hatchling brood parasites are provisioned and raised by the host species, entirely without assistance from the biological parents. Once fledged, however, they soon cease to interact with the foster parent and join their own species. Friedmann detailed the morphological adaptations that allow the parasitic species to successfully reproduce in this remarkable manner. For the parasitic cuckoos, he delineated the striking polymorphism in pattern and coloration of eggs that mimicked those of the dominant host species in different parts of the brood parasite's range. In addition, he detailed the behavioral specializations that permit the parasitic nestling to prosper in the nest of the host, often at the expense of the true offspring of the nest owner.

Friedmann's interest in nest parasitism led him to the study of honey guides (family Indicatoridae). After extensive research in European museums during the summer of 1950, he spent the fall and early winter of 1950-51 studying the guiding habit of the bird—by which it leads humans to bees' nests—by direct field observations supplemented by interviews with naturalists and other local observers of birds in South Africa and Southern Rhodesia (now Zimbabwe). In twenty-three successful guiding trips supplemented by interviews with local observers, he established that this astonishing habit is not native myth but fact. After the sym biont opens the nest and removes some of the honey, the bird, Friedmann found, feeds on the wax comb, and not as formerly assumed on honey.

This study, in turn, led to his interest in the remarkable relationship between these birds and the wax combs of bees' nests. The honey guide is unique in its consumption of beeswax, which forms a considerable part of the bird's diet. Since wax was thought to be indigestible by vertebrates, Friedmann, after he returned to Washington, set out to determine the process by which honey guides achieved this metabolic feat. With the help of contacts he had established in South Africa and Uganda, birds were trapped and, courtesy of Pan American Airlines, sent to Washington. With assistance from experts in bacteriology and biochemistry, whose interest he engaged, the problem was studied during the following years. It was discovered that the bird possesses a digestive enzyme that aids in wax breakdown and that the bird also has had a species of gut microbe that is capable of wax digestion. Together, the bacterium and the alimentary enzyme are able to extract as much as 50 percent of the lipid content of the beeswax, which is then mobilized for the bird's assimilation. Friedmann also discovered that the bacterium, which he named *Micrococcus cerolyticus,* inhabits wild combs and that presumably the birds obtain their wax-digesting microflora from the comb itself.

The ability of the *Micrococcus* to break down wax has led to the study of its utility in combatting the bacillus that causes tuberculosis. That microbe is protected by a waxy cell wall, which now has been shown to be susceptible to attack from the *Micrococcus.* The manner in which Friedmann's work on nest parasitism led to the study of honey guides, which in turn led to a new understanding of wax breakdown, demonstrates the perspicacity and diligence of his intellectual efforts. These faculties were also turned to his diligent studies in the realm of art history.

With his interest in animal life in general and birds in particular, and his love of art, Friedmann noticed the frequent depiction of animals in the works of the Old Masters. Studying the occurrence of wild creatures and the symbolism involved, he came to see the artists as early observers of nature, a link in the development of scientific natural history out of the mystical and allegorical beliefs dominating European culture's view of the natural world. His field of study was the art of medieval and Renaissance Europe. Friedmann focused on animals for their intrinsic interest rather than as ornamentation in the exclusively religious works.

A rich source of allegory for devotional art was St. Jerome and the "scorpions and wild beasts" which, according to a letter from his hand, were his daily companions. In 1,100 such art objects that he studied, Friedmann found that fifty-nine different animals had been used symbolically, some with great frequency. To represent them successfully, the artist had to know their outer appearance. Friedmann hypothesized that the rise of the natural science of zoology could occur only after this transformation in perception had occurred among the artists and intellectuals of the culture. This research was able to satisfy his native love of art while at the same time stimulating his still-questing intellect. It bespeaks a mind that remained remarkably vigorous even into his latter years.

As to the physical evidence of his intellectual efforts, Herbert Friedmann published seventeen book-length works, including *The Cowbirds: A Study in the Biology of Social Parasitism* (1929); Birds Collected by the Childs Frick Expedition to Ethiopia and Kenya Colony, Parts I and II (2 volumes, 1930, 1937); Notes on the Ornithology of Tropi cal East Africa (with A. Loveridge, 1937); *The Symbolic Goldfinch* (1946); The Honey-guides (1955); The Parasitic Weaverbirds (1960); The Host Relations of Parasitic Cowbirds (1963); The Evolutionary History of the Avian Genus Chrysococcyx (1968); and *A Bestiary for St. Jerome* (1980).

His technical and scholarly papers numbered more than 300. These can be grouped into several general areas of interest: nest parasitism by cuckoos, weaverbirds, and cowbirds; the avifaunas of Africa and South America; the behavior and wax digestion of the honey guides; taxonomy of North American birds; subfossil birds of archeological deposits; and zoological motifs in art. This listing, however, does not do justice to the full breadth of his contribution to the study of birds. He collaborated extensively with dozens of other researchers and made published contributions touching on nearly every bird group.

Dr. Friedmann was selected in 1961 to direct what was then the Los Angeles County Museum of Science, History and Art, succeeding another wonderfully knowledgeable and peripatetic ornithologist and aviculturalist, Captain Jean Delacour. At the time of his appointment, plans were nearly complete for separation of the art and natural history divisions into two museums. Subsequently, the Natural History Museum was granted a number of new positions—curators, exhibition staff, and so forth—and it inherited space vacated when art was moved out in 1965, offering great possibilites for expansion.

Friedmann carried on the innovations of Delacour and further strengthened the museum's potential for scholarly research. As director he sought a series of research and facilities grants that helped make the institution a world-class organization. Already at the Smithsonian he had actively served on a committee for the modernization of exhibits, and the Los Angeles Museum offered scope for such efforts. The list of accomplishments during this period is impressive: complete new halls for Pre-Columbian, South Pacific, and African ethnology as well as for geology, entomology, and vertebrate paleontology and in such diverse areas as the history of transportation and settlement of the American West. The hall of California history was renovated, eight new habitat groups were added to the mammal hall, dinosaurs were placed in the foyer, and many smaller exhibits were developed.

In addition, expeditions were undertaken, a docent program was initiated, and the collections grew tremendously, partly as a result of the expeditions and partly from transfer of some university collections to the museum. With unflagging interest in the African fauna, Friedmann secured funds from the National Science Foundation for surveys of the fauna of little-known, isolated, dwindling forests in western Uganda in 1966-70, and the resulting collections further enriched the museum's holdings. During these years as director, he was also closely associated with the University of California, Los Angeles, where he had given the Leida Scott Brown lectures in 1957. The public lectures were a success, as were his seminars for the zoology faculty and students and also for the departments of bacteriology and art. After his return to Los Angeles he was appointed professor in residence in the Department of Zoology and later on (more briefly) in the Department of Art. In this capacity he participated in graduate seminars, presented guest lectures, and consulted with faculty and graduate students.

Following his retirement from the museum in 1970, at age seventy, he was appointed by the National Science Foundation to evaluate biological research programs in Antarctica.

Friedmann's scholarly, scientific, and administrative achievements were recognized by the award of a number of hon ors, including a Guggenheim Fellowship in 1950; the Leidy Medal of the Academy of Natural Sciences, Philadelphia (1955); the Elliot Medal of the National Academy (1959); and the Brewster Medal of the American Ornithologists' Union (1964). He served as president of the American Ornithologists' Union (1938-39); as president, section F (Zoology) of the American Association for the Advancement of Science (1959); and as president, Biological Society of Washington (1960). Friedmann was elected a member of the National Academy of Sciences in 1962. He was awarded honorary membership in the Deutsche Ornithologische Gesellschaft, the South African Ornithologists' Union, the Sociedad Ornitologica del Plata (Argentina), and the Sociedad Poey (Cuba), and he was an active member or fellow of a number of other scientific societies.

A gentle humor was a pervasive trait until the very end of Herbert Friedmann's life, appreciated by friends and associates and expressed sometimes in limericks, though most spontaneously in conversation. He penned a limerick that accompanied an exhibit on honey guides in the Los Angeles County Museum:

There once was a bird in Nigeria
Whose chatter grew weary and wearier.
Its demeanor grew lax
As it gobbled up wax,
Which it stuffed in its little interior.

Herbert Friedmann's wife, the former Karen Juul Vejlo, was a Danish agricultural economist. He met her while she was a visiting scholar at the Brookings Institution in 193637. Her work for the U.S. Department of Agriculture and later the Food Research Institute at Stanford University included many scholarly endeavors, and her additional preoccupation with family and home left little time for participation in her husband's professional activities, but the associations they brought with naturalists and museums were always a source of delight to her. Seventeen years of retirement saw a constant sharing of mutual interests, whether with economists in Brazil, following birds in the Australian Outback, or pursuing St. Jerome in museums in Europe and the United States or, most of all, in the quiet hours at home.

The Friedmann's only child, Karen, followed her father's interest in art history and was for a number of years curator of fine prints at the Library of Congress. Now a research associate at Carleton College in Northfield, Minnesota, and an independent curator, she continues to work in the field of graphic arts.

Herbert Friedmann's ashes have been placed in a memorial step at the Oak Hill Cemetery in Washington, D.C., a five-minute walk from the Friedmann's first home in Georgetown.

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Photograph by Edward Hagar

Jacob Furth

JACOB FURTH

September 20, 1896-July 23, 1979

BY SIDNEY WEINHOUSE AND JOHN J. FURTH

THE TWENTIETH CENTURY has witnessed the phenomenal growth of medical science, and cancer research in particular has advanced from a field of descriptive anatomy to a flourishing, sophisticated biological discipline, pregnant with insights directed toward understanding and control of one of humankind's misfortunes. One of the leaders who helped to transform cancer research to a true scientific endeavor was Jacob Furth. In a career of fifty-seven years he contributed to diverse fields of cancer biology and experimental pathology. He was responsible for major advances in immunology, leukemia and radiation, and viral carcinogenesis. His pioneering work on hormonal effects in tumor development added new dimensions to our understanding of how tumors proliferate.

Jacob Furth was born in the city of Miscolcz, then part of the Austro-Hungarian Empire, in 1896. His father, Jonas, had seven children, four of whom died as infants. Jacob was the next to the youngest. His mother, Jetti Sussman, died when he was three. His father remarried Roza Farkas, and they had four more children. Roza was a simple woman and a devoted mother to both her children and her stepchildren. Jacob was particularly close to one brother, Lajos, with whom he played soccer and chess. Lajos came to the

United States in 1941, and they remained close until his death in 1969.

Jetti's children were all talented. One sister, Margit, wrote first-rate novels and poetry (in Hungarian). Jacob pondered whether to go into medicine or law. He chose medicine over law because, as he put it, "Lawyers can be hired to prove not only the right, but also that the wrong was right."[1]

This was at the outbreak of World War I; after his first year, having been captured in the first major battle in which his unit was engaged, Jacob spent three years in Russian military prisons. He returned to Hungary as part of a prisoner exchange and completed his medical training in 1921. His experiences as a prisoner during the turmoil of the Bolshevik Revolution, which extended into Hungary on his return, have been recounted by Murray Angevine.[2]

It was in Prague that Jacob met Olga Berthauer, a medical student at the Czech University, while he was attending the German University. She was to come to the United States a year after Jacob. The immigration authorities were reluctant to let her stay (as a single woman). However, they were married the next day, and, upon presentation of the marriage certificate, the immigration authorities relented. She was to be collaborator, colleague, homemaker, and confidant for over fifty years. Although maintaining a career, first as a pediatrician and later in school health, Olga's devotion to Jacob was complete. Without hesitation she gave up good jobs when he moved. She was good at her vocation, always able to get a good job in the new city (except Boston, for Massachusetts would not license a graduate of a foreign medical school). Her last job was with the School Health Department of New York City, a position she held until the mandatory retirement age of eighty. She died April 21, 1988.

EARLY YEARS IN RESEARCH, 1918-32

Jacob had an inquiring mind and a love of learning that attracted him to research, even while a medical student. Under the guidance of Edmund Weil, one of several great scientists who, he acknowledged, had an important influence on his career, Jacob began research in microbiology and immunology, directed to whether bacterial species could be characterized antigenically. This work required keeping open plates for one or two weeks in search of mutants, and these were subject to fungal contamination. In his autobiographical essay,[1] Jacob wryly confessed his chagrin at his presumption that the "damned fungi were preventing growth of the bacteria by depriving them of essential nutrients. The possibility that the fungi secreted a bactericidal substance did not cross my mind." He also regretted that he did not write up or follow up on work he started at that time on bacteriophage, just discovered by D'Herelle, recalling the statement by his dean and department chairman Oscar Bail that "this may be the greatest discovery of our period." However, a lasting contribution of the period was Jacob's introduction of the agar plate, which has now become standard practice in microbiology.

A turning point in Jacob's career was the sudden death of his mentor, Edmund Weil, and his senior associate, F. Breinl, from typhus, and his own illness and recovery from the same disease. Facing the dilemma of whether to choose a career in clinical research or stay in the laboratory, and weighing his future in postwar Europe, Jacob accepted an offer from Eugene L. Opie of the Henry Phipps Institute of the University of Pennsylvania in Philadelphia. He remained there for two productive years, 1924-26, working on acid-fast organisms and their antigens.

Jacob acknowledged with gratitude (naming one of his twin sons Eugene) the profound influence of Opie and an outstanding group of young scientists on his career. During his two years at the Phipps Institute he established immunologic relationships among acid-fast organisms, the antigenicity of microbial lipids, and at the suggestion of Opie he confirmed the observation of Zinnser that new antigens could be created by heat.

This last work caught the interest of Karl Landsteiner, the discoverer of the blood groups, who invited Jacob to the Rockefeller Institute as his assistant. His two years there, from 1926 to 1928, were a profound learning experience. From Landsteiner and daily contact with other great scholars at the institute, Jacob learned both the philosophy and strategies of research. It was at the institute that he was introduced to the cancer problem by the brilliant work of Peyton Rous, James Murphy, and especially Alexis Carrel, through whom he was introduced to tissue culture, a tool he used continuously in his future work.

With Landsteiner he attempted to transform saprophytic vibrios to cholera vibrios and to transform Drosophila strains. They chose the wrong organism. It was Avery who several years later successfully transformed pneumococcal strains, thereby revealing the hereditary role of DNA!

Under Landsteiner's instruction Jacob demonstrated differences in the blood groups among the anthropoid apes, and Landsteiner asked him to stay on to take on Philip Levine's work on minor blood subgroups. Restless to do independent work, however, Jacob accepted an offer in 1928 from Opie to return to the Phipps Institute in Philadelphia. A return to this institution was not without some regret. In his autobiographical essay[1] he points out ruefully, "Now that Philip Levine has attained greatness by the discovery of the Rh factor . . . I see what I have missed."

Opie had accepted a generous grant from E. Mallinkrodt, Jr., for experimental work on leukemia and placed Jacob in charge. It was here that Jacob began a long, wide-ranging, and uniquely productive study on leukemia, for which he received widespread recognition. He frequently expressed his gratitude to Mr. Mallinkrodt, who required no reports and even kept his support anonymous until 1957, when he relented on Opie's urging.

Jacob took two independent approaches simultaneously. One was to obtain leukemic mouse strains. He succeeded by inbreeding mice in which leukemia occurred spontaneously or was induced by radiation. One strain, the AKR mouse, carries a leukemia virus and has become one of the most common and widely used animals in many diverse experimental studies. The second approach was to attempt to isolate viruses from the various types of leukosis then available. Five leukosis viruses were isolated and studied, one of which was the fowl neurolymphomatosis virus, the agent of Marek's disease, an economic scourge of the poultry industry until its conquest by a vaccine in 1970.

Other notable findings of this period were the transmission of avian leukosis by blood-sucking insects, the occurrence of high concentrations of leukemia virus in the serum of leukemic chickens, and the viral nature of the common venereal sarcoma of dogs, transmitted through copulation.

It was in Philadelphia that Jacob's twin sons were born. He somehow found time to be a good father. Both sons emulated him and became physicians. One is now in North Carolina, the chairman of the Department of Medicine at East Carolina Medical School, and the other, a coauthor of this memoir, is a professor of pathology at the University of Pennsylvania in Philadelphia.

CORNELL, 1932-47

When Opie assumed chairmanship of the Department of Pathology at Cornell Medical College in New York in 1932, Jacob joined him as assistant professor of pathology. Acquainted with New York through his prior two years at the Rockefeller Institute, Jacob found the atmosphere exhilarating. Angevine, his colleague and sometime collaborator at Cornell, has succinctly described this period:

> The days spent at Cornell were among the happiest of J.F.'s career. The conditions and environment were ideal for investigation with fine, well-equipped laboratories, space for animals, a loyal and efficient technical staff, and adequate financial support.[2]

Angevine goes on,

> Initially, when full time was available for the studies on leukemia, [Jacob] was like a human dynamo, working so continuously and relentlessly that although he frequently appeared fatigued, his pace seldom slackened. He seemed to thrive in the midst of the restless activity and ferment of New York City, which served as a catalyst for his boundless energy. His enthusiastic approach to every problem attracted bright young men to his laboratory, most of whom worked hard and completed an *arbeit.* He was also strongly convinced of the value of student participation in research; perhaps this stemmed from his own experience as a student.

Jacob's fifteen years at Cornell were very productive. With Cole and Boon in the early forties he demonstrated important genetic factors in the heritability of leukemia in mice, and in cross-breeding experiments they showed that in some instances the influences of the low leukemia strain predominated, whereas in others the high leukemia strain predominated. He (and others) also showed that heritability was not due to a maternal "milk factor," as it was with high and low mammary tumor strains.

A role of the thymus in leukemia was shown by work with McEndy and Boon. In the high leukemia AK strain, removal of the thymus, a primary site in this strain, lowered the incidence from 78 to 11 percent. Removal of the spleen had no effect, and the effect of thymectomy was not inherited, since the offspring of thymectomized mice had the same high incidence as the high leukemia grandparent. Thymectomy also lowered the incidence of methylcholanthrene-induced leukemia but had no effect on the growth of grafted leukemic cells, which grow equally in high and low leukemic mice.

During his period at Cornell Jacob received support not only from the Mallinkrodt Fund but also from the Lady Tata Memorial Trust, the International Cancer Research Foundation, the Jane Coffin Childs Fund, and the Anna Fuller Fund.

Although research remained his foremost interest, Jacob enthusiastically entered into the other elements of the academic triad, teaching and service. Also in his mind was the tenuous nature of full-time research, particularly at that time. He was in a pathology department and felt he had to become a "compleat" pathologist. He took a minisabbatical to Vienna to learn anatomical pathology from Masters.

One of Jacob's major responsibilities was teaching experimental pathology, then a requirement for medical students, who had the choice of spending two semesters on an investigative study proposed by either the staff or the student. He derived much satisfaction in having indoctrinated future physicians in the methods and spirit of research and in some instances having encouraged students toward research careers. Additional wartime responsibilities as acting chairman of the department when Opie retired and other staff members left for wartime service drew him further away from research. He regretted this hiatus in his leukemia studies, causing loss in momentum to other investigators. Much of his most important work on viral causation of leukemia, such as genetic inheritance of the virus causing leukemia in AK mice, went unreported. Another important contribution inadequately reported during the pressure of wartime responsibilities were the effects of thymectomy in preventing viral expression and the use of thymus extracts to activate viral expression. This work, however, marked the beginning of a major career effort, to be described later, on the role of host factors in cancer development.

As recognition came and he advanced to full professorship, Jacob became increasingly in demand as a consultant and as a member of committees dealing with the evaluation of research projects and policies for such agencies as the Armed Forces Institute of Pathology, the Atomic Energy Commission, the National Institutes of Health, and the American Cancer Society. As a sometime member with him on such panels, the senior author of this biography saw firsthand how well Jacob performed in these roles. He was a superb adviser on these bodies—forthright in his opinions, backed always by adequate documentation; they were invariably the well-thought, considered, and eminently fair views of an erudite scientist well versed in biomedical science. These same qualities made for extremely effective service on editorial boards for a number of journals such as *Blood, Cancer Research, Journal of the National Cancer Institute,* and others, to which he gave much time and effort.

Jacob modestly attributed his productivity at Cornell to

> ... a good student body, associates and assistants. We reported on the individuality of various types of leukemia in mice; their transmission by a single cell; a method for preservation of living cells by slow freezing (with

> C. Breedis); the genetics of spontaneous leukemia (with R. Cole); the role of the thymus in leukemogenesis; the individuality of the monocytes, histiocytes and microglia cells (with H. Dunning) and their relation to reticulum cell sarcoma; differentiation of leukemia and leukemoid reactions (with W. A. Barnes); and the possibilities of experimental therapy of leukemias (with L. Reiner and C. Flory). We also expanded the list of different viruses causing leukemias and sarcomas, and indicated the essential identity of mouse and human leukemias and the neoplastic character of both. . . . The two years Elvin Kabat spent with us at Cornell were highly productive. In addition to work on high-speed sedimentation of leukemia viruses, the histochemical identification of alkaline phosphatases, and the localization of alkaline phosphatase (with C. Breedis) in the proximal convoluted tubules of the kidney led us to identify the site of nephrotoxic agents such as the mercurial compounds, then used in the therapy of syphilis.[1]

A SHORT STAY IN DALLAS, 1947-49

Disappointed at being passed over for chairmanship of the Pathology Department at Cornell after Opie's retirement, Jacob left to join the Veterans Administration Hospital in Dallas, Texas, in 1947. The Dallas position also carried an academic appointment at Southwestern Medical College, but several unforeseen setbacks made this position much less desirable than he had anticipated. Though accepted wholeheartedly by the faculty, the dean (a retired general of the Army Medical Corps) refused to appoint Jacob to a full professorship, a rank he held at Cornell; nor would the dean allow him to accept any of the research grants from the foundations that supported him in New York. (It would be hard to imagine any of today's deans conducting such an act of self-immolation.)

Although a heavy service load severely handicapped Jacob's research, notable observations were still made. With T. Bali he observed some remarkable changes in radiation-induced ovarian tumors, and he isolated a highly functional mast cell tumor.

When the Veterans Administration abolished its regional organization, a better opportunity arose in 1949 to chair a department in one of its university-based hospitals. At the same time, however, Jacob received a cordial invitation from Alexander Hollaender to join the Biology Division of Oak Ridge National Laboratory. He accepted on the advice of Shields Warren, a statesman of U.S. science who at the time was head of the Atomic Energy Commission Biology Division, and thus ended a seventeen-year period of broad-based pathology to return once again to the laboratory.

RADIATION BIOLOGY AT OAK RIDGE, 1950-54

Jacob had already contributed to radiation carcinogenesis with x-ray induction of leukemia in mice back in 1929, the induction of ovarian tumors as late radiation effects, and some attempts at γ-ray therapy of experimental leukemia. Enthusiastically plunging into the newly developing area of radiation biology, Jacob greatly expanded his perceptions by taking a course in radiation physics. Aided and guided by members of the Biology Division, he entered a renewed surge of research accomplishment in a highly charged atmosphere, of the same type that had nourished and sustained him at Cornell. A unique experiment in which he took part at Oak Ridge was a massive study of radiation effects on mice, resulting from an experimental explosion of an atomic bomb (Operation Greenhouse). As Shields Warren wrote,

> Jacob was the recipient of large numbers of mice, survivors from a Pacific nuclear test, placed with various degrees of shielding along radii from the point of explosion. He had the foresight to follow these animals to the time of their natural death. As a result of these studies, much new information was developed about the late effects of radiation, about biological dosimetry, and about the similarity of certain radiation effects to those of aging. The meticulous care with which these animals were kept free of

> epizootics and the painstaking observations on them, pre and postmortem, became a milestone in radiobiological research.[2]

The hitherto puzzling cause of the anemia associated with acute death from radiation occupied much of Jacob's attention during this period. This problem was solved by the discovery, with Storey, Wish, and others that erythrocytes enter the lymph ducts owing to radiation-induced destruction of platelets. Within minutes after platelet perfusion, the bloody lymph clears. With amusement Jacob described his difficulty in getting this important observation accepted by a prestigious journal.[1] This work led to the effective use of platelet perfusion in platelet deficiency disorders.

At Oak Ridge Jacob's senior associate was Arthur C. Upton. Among his various collaborations there, Jacob acknowledged that "in studies of radiation-induced leukemias of various types and the relative biological efficiency of diverse types of radiations, my senior associate, A. Upton, did a 'lion's share' of the work."[1] This collaboration, extending long beyond Jacob's tenure at Oak Ridge, resulted in many publications on detailed mechanisms of radiation-induced carcinogenesis and influences thereon of hormonal manipulation. As Upton has stated, "His name has become legend here. His intensity, devotion to science and impatience with imperfection are recalled vividly by all who knew him. My admiration for J.F. and my debt to him as my mentor are boundless."[2]

This period was especially notable for the beginning of Jacob's perhaps most important contributions—the role of hormones in neoplasia, a field in which he subsequently devoted most of his scientific efforts. Endocrine tumors had been observed in Operation Greenhouse. It also had been observed by Gorbman that radiation from ^{131}iodine induced pituitary tumors in mice. Further investigation with Burnett and others led to the finding of a thyroid-pituitary axis whose manipulation could produce at will either thyroid or pituitary tumors.

HARVARD AND THE CHILIDREN'S CANCER RESEARCH FOUNDATION, 1954-59

Despite the productivity of these Oak Ridge years, Jacob yearned for a more academic atmosphere and in 1950 welcomed an invitation by Sidney Farber to join him as associate director and chairman of the experimental pathology section of the Children's Cancer Research Foundation in Boston, supported by the so-called Jimmy Fund, named after a young cancer victim cured by chemotherapy. These were also fruitful years of research, which Jacob modestly attributed "to the fame of Harvard, which . . . channeled to my laboratories guest investigators from . . . Australia, England, Israel, India, Japan, ... and the U.S.",[2] and the unparalleled opportunity of collaborating with the many talented members of Harvard's faculty.

With Paul Hagen, the transplantable mastocytoma that Jacob had developed earlier was shown to produce heparin, serotonin, and histamine, a striking example of a transplantable tumor that retains considerable functional activity. Other notable Harvard faculty who contributed materially to Jacob's investigations of endocrine neoplasms were Jean Mayer on the obesity-inducing adrenotropic tumors, and Gregory Pincus and Eric Bloch on the steroids formed by these tumors. Several of the guest investigators—Donald Metcalf, now head of the Walter and Eliza Hall Institute of Melbourne, Australia; Nechama Haran-Ghera of the Weizmann Institute, Rehovot, Israel; Kelly Clifton, now at the University of Wisconsin; Gordon Sato, now at the W. Alton Jones Cell Science Center, Lake Placid; and Untae Kim of the

Roswell Park Memorial Institute, Buffalo-are among the current leaders in cancer research.

Gordon Sato was inspired by Jacob to develop completely defined media for cell culture to dissect the endocrine factors in cell growth in vivo being explored by Jacob. He acknowledged his debt by dedicating to Jacob Furth a conference on the "Growth of Cells in Hormonally-Defined Media."[7]

TWO YEARS IN BUFFALO

In 1959, approaching the Harvard retirement age, and with his hitherto cordial relationship with Sidney Farber having become strained, Jacob let it be known that he would like to move and so accepted an invitation from the Roswell Park Memorial Institute in Buffalo. Along with continuing unfinished work begun at Harvard, he was encouraged by Theodore Hauschka to examine chromosomal abnormalities in relation to hormone dependence of neoplasms.

In Buffalo a young Japanese pathologist, Kenjiro Yokoro, came to work with Jacob as a visiting scientist. Their association continued through the next seventeen years and included Yokoro's students and colleagues. Their appreciation was demonstrated by cordial receptions accorded Jacob (and Olga) on two lecture tours to Japan, the last one a year before he died.

THE FINAL YEARS, COLUMBIA, 1961-79

After two years in Buffalo, Jacob was invited by Alfred Gellhorn, then director of the Institute of Cancer Research, and Donald McKay, chairman of the Pathology Department at Columbia University's College of Physicians and Surgeons, to join the pathology department and to head the pathology department at the Francis Delafield Hospital, Columbia's cancer center. Despite his age of sixty-five, Jacob was warmly welcomed, and with youthful vigor he once again engaged in the broad responsibilities of service and teaching. However, he soon gave these up to concentrate on research and terminated his position at the Delafield Hospital to join the newly formed Institute of Cancer Research, an independent unit of Columbia University, headed by the late Sol Spiegelman. Here Jacob's research focused directly on the induction and properties of endocrine neoplasms, particularly of the thyroid, pituitary, and mammary glands, and on the role of the thymus gland on viral induction of leukemia. During this period, and after his retirement at age seventy, Jacob wrote a number of thoughtful essays on his conceptions of the neoplastic process.[3]-[6]

Jacob never really retired. His life was his work, except for several hobbies. In his Cornell years, when he lived in Pelham, he gardened, specializing in dahlias. He was interested in stocks and dabbled in the market—always long term; he would never buy and sell for the quick return. He consulted a broker and subscribed to several investment publications but made his own decisions. Overall they were excellent ones and on his death it was his investments, not his retirement pay, that left his wife financially secure.

One investment Jacob sold was some General Motors stock— to buy a farm in Surry, Maine. He thought he would spend the summers there and work at the house or at the Jackson Laboratory in Bar Harbor, about a forty-five-minute drive by car. He never spent more than a few weeks a summer there, often accompanied by colleagues. His children would visit him with their children and what with his frequent moves it became the family home. He called the place "Jake's folly," but it turned out to be his best investment. He died in Surry on July 23, 1979.

The previous day Jacob had attended a meeting at the Jackson Lab, visiting with old friends. This day, as was his custom, he spent the morning working on his latest manuscript. He then went out in the yard to pick up some brush that his son John had scattered about. After about an hour of yardwork he laid down to take a nap, also his custom. He never woke up.

The following tribute by Gordon Sato, abridged here, appeared in the proceedings of a 1982 Cold Spring Harbor conference dedicated to Jacob:

> Great artists and great scientists tend to be identified with more than one masterwork. Furth developed the first experimental leukemia system in mammals. This work involved the derivation of the AKR and RF mouse strains, the unequivocal demonstration that the experimental disease was analogous to human disease, and the transmission of leukemia with a single cell, the first demonstration that neoplasia can be monoclonal in nature. He demonstrated the necessity of the thymus in the genesis of lymphocytic leukemia and also defined the spectrum of neoplasms resulting from a specific virus infection in fowl, the role of genetics in the spectrum, and the incidence of neoplasms in mice following ionizing radiation. He also pioneered in cryopreservation of mammalian cells. Furth's interest in endocrine carcinogenesis was stimulated by the ovarian neoplasms that occurred in his early radiation studies, and this ultimately led to the investigation of radiation and hormone-induced functional pituitary tumors. The concept of conditioned and autonomous neoplasia emerged during these studies, a major insight into the importance of physiological feedback regulation in oncogenesis. Furth employed endocrine neoplasms as indwelling hormone sources in studies of normal and pathological growth, and he gave freely of his unique functional tumor cell lines to others for study *in vivo* and in culture. Perhaps the most important of his endocrine work was the establishment of the pivotal role of prolactin in mammary growth, differentiation, and neoplasia.[7]

Perhaps the greatest and most lasting influence of Jacob Furth has been on the crucial role of host factors in the induction and maintenance of tumors. Early on he recognized that the tumor and its host comprise a dynamic duo, in which the host and its regulatory mechanisms are pitted against the unremitting progression of tumors toward au tonomy and invasiveness. Foremost among the host factors are the hormones, and their interplay with each other and the tumor was Jacob's main research interest in the latter part of his career. Several examples from his classical studies with model endocrine systems illustrate how these systems interact.

Mammary tumors in rats produced by oral administration of chemical carcinogens, such as 3-methylcholanthrene, are responsive to the presence of the ovary and pituitary gland. Kim and Furth showed that if these organs are removed, the tumors regress, but they can be restored to growth even after months of dormancy by implanting a pituitary tumor secreting a hormone that stimulates mammary growth. If rats are given subcarcinogenic doses of either radiation or methylcholanthrene, or if mice are given a subcarcinogenic injection of milk containing a mammary tumor virus, no tumors occur. However, if the carcinogen is supplemented by mammatrophic growth-stimulating hormone, which is itself noncarcinogenic, mammary tumors arise. This promotional effect of hormones, Jacob concluded, is likely relevant to humans, where we are continuously exposed to small subcarcinogenic doses of multiple carcinogens.

Jacob thus amassed an impressive body of evidence for the hypothesis that hormonal imbalances caused by either overproduction of growth-enhancing hormones or underproduction of growth-restraining factors can lead to abnormal cell proliferation. Such growths are generally dependent on or responsive to the hormone that affects its behavior and can regress partially or completely if the imbalance is corrected. However, when to the hormonal imbalance there is added an alteration to the genome that removes intrinsic restraints on proliferation, autonomy ensues, leading to loss of hormone dependency and un controlled cell proliferation. This genomic alteration may occur first, causing the initiation of autonomous tumor development, or may be induced by cellular proliferation, thereby leading to the commonly observed gradual sequential progression resulting in partial and ultimately complete autonomy of growth.

Jacob was prominent in a host of professional activities. He was elected to the National Academy of Sciences in 1974 and was a member of its Advisory Committee to the Atomic Bomb Casualty Commission. He was on the Surgeon General's Advisory Committee on Smoking and Health, the Public Health Service's Advisory Committee on Tumor Viruses, and the Committee on Radiation of the Department of Health. He was president of the American Association for Cancer Research and the American Society of Experimental Pathology. He was a fellow of the American Academy of the Arts and Sciences and held honorary memberships in the Endocrinology Society of Chile, the Cancer Society of Peru, and the Pan American Medical Association (diplomate member).

Among his awards and honors were the Gold Medal of the American Medical Association, 1932; the Rosenthal Award of the American Association for the Advancement of Science, 1957; the Bertner Foundation Award from M. D. Anderson Hospital and Tumor Institute, 1958; the Robert Roesler de Villiers Award, 1959; the G. H. A. Clowes Award and Lectureship of the American Association for Cancer Research, 1962; the Semmelweiss Medal and Lectureship, 1962; an honorary doctor of science from the University of Pennsylvania, 1968; the Alessandro Pascoli Prize, 1973; and the Rous-Whipple Award of the American Association of Pathologists and Bacteriologists, 1974.

Jacob was a thoughtful statesman of science, with extraordinary vision that enabled him to see the process of cancer induction in its formidable complexity. With singular talent, a keen intellect, an inquiring and analytical mind, and experimental skill, he was able to unify otherwise disparate data to explain the development and maintenance of the cellular proliferation of cancer in terms of the operation of extrinsic and intrinsic factors. Our conceptions of cancer induction were broadened greatly in the decade following Jacob's death in 1979 with the discovery of a host of proto-oncogenes whose activation by various mechanisms leads to abnormal cell proliferation. The essential correctness of Jacob's views has been confirmed by the further discovery that the activated oncogenes encode a number of growth-promoting hormones and their cellular receptors.

Jacob brought to experimental pathology a high degree of scholarship, an appreciation of the contributions of the basic sciences to the understanding of disease, and an attitude of humility in the face of what still needs to be learned about life's processes.

His legacy is captured in the words of Henry Wadsworth Longfellow:

> When a great man dies, for years beyond our ken,
> The light he leaves behind him, lies upon the paths of men.

NOTES

The authors depended for this memoir on material in the following publications and are indebted to Arthur C. Upton for assistance.

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2. Murray Angevine, "Significant Events in the Life of Jacob Furth," *Cancer Research,* 26(1966):351-56.

3. Jacob Furth, "Influence of Host Factors on the Growth of Neoplastic Cells. The G.H.A. Clowes Memorial Lecture," *Cancer Research,* 23(1963):21-34.

4. Jacob Furth, "Pituitary cybernetics and neoplasia," in *The Harvey Lectures,* Ser. 63 (New York: Academic Press, 1969):47-71.

5. Jacob Furth, 'Hormones as Etiologic Agents in Neoplasia," in *Cancer: A Comprehensive Treatise,* vol. 1, ed. F. F. Becker (1975):75-120.

6. Jacob Furth, "A Meeting of Ways in Cancer Research: Thoughts on the Evolution and Nature of Neoplasms," *Cancer Research,* 19(1959): 241-58.

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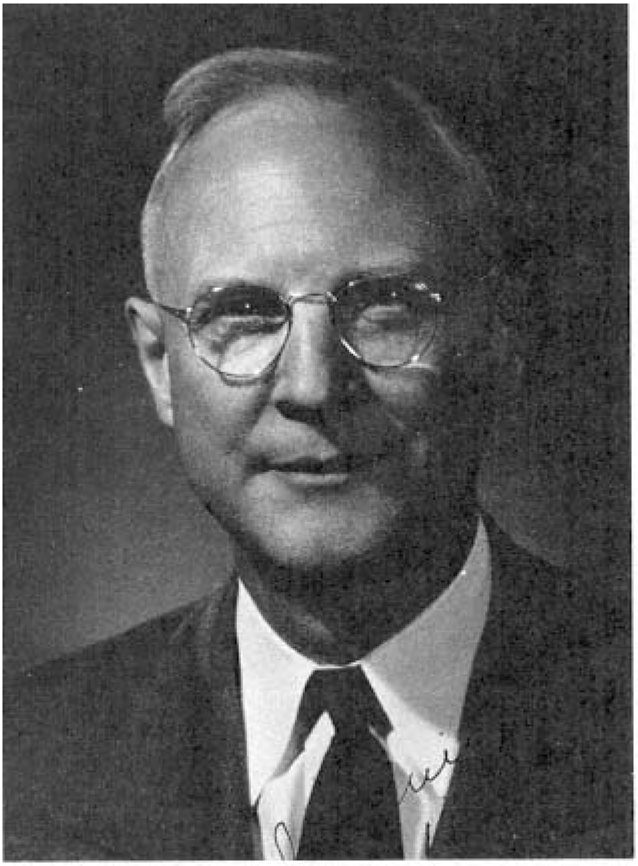

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J. P. Guilford

JOY PAUL GUILFORD

March 7, 1897-November 26, 1987

BY ANDREW L. COMREY

J.P. GUILFORD died at the age of ninety in Los Angeles on November 26, 1987, after a long series of debilitating illnesses. He is survived by his wife, Ruth; his daughter, Joan S. McGuire; three grandchildren; and three great-grandchildren. He was born on a farm near Marquette, Nebraska, on March 7, 1897, the son of Edwin and Arvilla Monroe Guilford. In 1914 he was graduated from Aurora High School as valedictorian of his class. After teaching elementary school for two years, he attended the University of Nebraska for a year, entered the Army as a private, and after being discharged returned to complete his B.A. and M.A. at Nebraska. During this period he served as interim director of the Psychology Clinic, where he administered intelligence tests to children. He was impressed with the unevenness of children's abilities in different areas, something he had already noticed while comparing his own and his brother's aptitudes. He became convinced that intelligence was not one monolithic, global attribute but a composite of different abilities. At this point in his training, therefore, he was already showing a strong interest in what was to be the dominant focus of his professional career—individual differences.

In 1924 Guilford entered the psychology Ph.D. program at Cornell University, where he studied with such famous historical figures as E. B. Titchener, Kurt Koffka, Harry Helson, and Karl Dallenbach. When Guilford was awarded the Ph.D. at Cornell in 1927, he had already published five papers. His doctoral thesis showed that variations in reported sensory experience with weak stimuli were due more to the characteristics of the limen itself than to fluctuations in attention, contrary to what was commonly believed at that time.

After short periods of time on the faculties of the universities of Illinois and Kansas, Guilford returned in 1928 to the University of Nebraska as professor of psychology, where he achieved an international reputation as one of America's foremost psychologists. In 1940 he moved to the University of Southern California. Except for a period of leave to serve in the U.S. Army Air Corps during World War II, he remained at USC until his formal retirement in 1962. This event represented little more than a milestone in his career since he continued to be very active in research and writing for twenty-five more years. As a teacher, Guilford trained dozens of graduate students who went on to make numerous contributions of their own to the psychometric literature.

During a productive research career that continued for more than six decades, Guilford published over twenty-five books, thirty tests, and 300 journal articles. Some of the honors and awards bestowed upon him include the following: president, the Psychometric Society (1938); president, the Midwestern Psychological Association (1939); president, the Western Psychological Association (1946); president, APA Division 5, Evaluation and Measurement (1947); president, the American Psychological Association (1949); president, APA Division 10, Aesthetics (1956); Legion of Merit for outstanding military service (1946); honorary degrees from the University of Nebraska (1952) and the University of Southern California (1962); membership in the National Academy of Sciences (1954); the APA Distinguished Scientific Contributions Award (1964); the Richardson Creativity Award (1966); president-for-life, the International Society for Intelligence Education (1978); and the Gold Medal of the American Psychological Foundation (1983).

During the early years of his career, Guilford focused on such classical research topics in experimental psychology as attention, psychophysics, autokinetic phenomena, eye movements, scaling methods, and the phi phenomenon. The crowning achievement of this period, however, was the publication in 1936 of his classic textbook, *Psychometric Methods,* revised in 1954. This book became required reading for practically all psychology graduate students for decades and provided for the first time in one source an encyclopedic but readable exposition of psychophysical methods, scaling procedures, and even factor analysis. After publication of the book, the focus of Guilford's research shifted more and more to the study of personality and ability traits.

L. L. Thurstone's *Vectors of Mind,* published in 1934, and related work on primary mental abilities provided a methodology that Guilford immediately began to apply to the study of personality. At the time, Carl Jung's extraversion-introversion construct was widely believed to represent a single unitary dimension of personality. Guilford and his wife, Ruth, developed thirty-five questionnaire items to measure attributes commonly assumed to represent extraversion-introversion and subjected them to a factor analysis using Thurstone's new method. They demonstrated that extraversion-introversion was not one global trait but a complex composite of several distinct personality attributes.

This influential investigation was quickly followed by many other empirical studies of a similar kind, which led to the identification of thirteen important factors of personality. Three of these were measured in the first published factored personality inventory, the Nebraska Personality Inventory (1934). This line of research culminated in the publication of the well-known Guilford-Zimmerman Temperament Survey (1949) and a scholarly book reviewing the personality literature from the factor analytic point of view, *Personality* (1959).

Guilford's new emphasis on correlational studies prompted him to give increased attention to statistical methods in his research and writing. In addition to developing many new statistical procedures of his own, in 1942 he published *Fundamental Statistics in Psychology and Education,* a popular textbook that was revised many times thereafter and is still in print today.

The arrival of World War II presented Guilford with a unique opportunity to apply his factor analytic methodology to the study of mental abilities. He had always believed that there are many important and relatively independent mental abilities. So, when he was asked to participate in the U.S. Army Air Corps World War II research effort to develop psychological tests for the selection of pilots, bombardiers, and navigators, he had a philosophy and a methodology ready to apply to the task at hand.

From 1942 through 1945 he directed a factor analytically oriented test development effort that dwarfed anything of the kind hitherto undertaken. He revolutionized job classification methods by factor analyzing performance criteria along with the tests themselves to provide more information about the aptitudes necessary for successful job performance. By the end of World War II, Guilford and his collaborators had identified and measured some twenty-five important mental ability factors. They used the psychological tests developed in this research effort as selection devices to reduce the failure rate in pilot training to one-third of what it had been at the start of the war. This epic work, described in his book *Printed Classification Tests* (1947), set the standard for all subsequent selection programs both in and out of the military.

In 1945, Guilford returned to teaching and research at the University of Southern California, where he continued with his investigations into the mental abilities that make up intelligence. Guilford was particularly aware of the absence of creativity measures in conventional intelligence tests. His 1950 APA presidential address emphasized the need for more research into the nature of creativity. Over the next twenty years he carried out numerous large empirical investigations that continued to expand the number of confirmed mental abilities. Many of these were related to creativity. Two major books on intelligence emerged from this period, *The Nature of Human Intelligence* (1967) and *The Analysis of Intelligence* (1971) (with Ralph Hoepfner).

By the early 1950s Guilford began to feel the need to develop a system for classifying the many mental abilities that had been and were continuing to be discovered. The first version of his now-famous Structure of Intellect (SOI) model was presented in 1955 to an international conference on factor analysis in Paris. From its first formulation, the SOI model became the main focus of Guilford's research and writing. He used the model to suggest where new abilities might be discovered, much as the periodic table had been used earlier to locate new chemical elements. The number of possible abilities represented by the model has increased over the years, and in the latest version (described below) there are 180.

As the SOI model developed, Guilford became more and more interested in applying it to improve education.

Despite the widespread popularity of the IQ Guilford never believed in the Spearman g-factor theory of intelligence, which implied that the IQ is based on a single monolithic ability trait. Furthermore, anticipating much recent controversy about the IQ concept, he doubted the immutability of mental ability. He believed that human abilities are differentiated into increasingly complex systems as a function of more and more education. He believed that children can be trained to be smarter; "Intelligence education is intelligent education" became his motto. His ideas in this area have been implemented in recent years, particularly in Japan, through the efforts of the International Society for Intelligence Education. This society and its affiliated schools rest on the foundation of Guilford's SOI model. In these schools students are trained, from an early age, to upgrade their SOI abilities in thinking, creativity, and many other areas through weekly exercises. In recognition of Guilford's enormous contributions to education, the International Society for Intelligence Education, headquartered in Tokyo, published in 1988 *An Odyssey of the SOI Model,* edited by A. Chiba. This volume contains Guilford's autobiography, several of his papers on the SOI model, tributes to Guilford by his daughter and others, many of his letters, a vita, and Guilford's bibliography as edited by his wife.

In his final version of the SOI, "Some Changes in the Structure-of-Intellect Model" *(Educational and Psychological Measurement,* 1988, vol. 48, pp. 1-4), Guilford described intelligence as being a systematic collection of a large number of abilities for processing different kinds of information in various ways. There are six kinds of operations (cognition, memory recording, memory retention, convergent production, divergent production, and evaluation); five kinds of contents (visual, auditory, symbolic, semantic, and behavioral); and six kinds of products (units, classes, relations, systems, transformations, and implications). The SOI model resembles a cube with contents, products, and operations each occupying one side. Each ability is defined by a conjunction of the three categories, occupying one cell in the three-dimensional figure. Many of these abilities are acknowledged to be correlated with each other. This 6 x 5 x 6 figure yields a total of 180 possible unique abilities, over 100 of which have been empirically verified.

It is not easy to single out one achievement as Guilford's most important contribution. His outstanding books on psychometric methods, statistics, personality, and intelligence; his personality and ability tests; his U.S. Air Corps personnel selection work; his discovery of new mental abilities; and his SOI model have all been extremely influential. What may be most enduring, however, is his influence on our way of thinking about intelligence. When Guilford began his career, intelligence was the IQ a monolithic global trait that was regarded as largely innate and immutable. Now, in large measure as a result of his research, intelligence has been shown to be incredibly complex. There may be as many as 180 separate abilities that can be individually developed through "intelligence education." The hereditary limitations placed on human intelligence are seen now to be far less restrictive than previously assumed. Guilford's conception of intelligence, if adequately heeded, will have a profound impact in the future on public perceptions about individual potential and upon the education of children.

A list of Guilford's accomplishments, impressive as they are, conveys very little about the man himself. What was he like? The following description of "J. P." is based on input from many different sources—family, friends, colleagues, students, acquaintances, and the writer's own personal contacts with him.

Words that come quickly to mind to describe J. P. Guilford the man are integrity, honor, dedication, devotion, kindness, fairness, patience, generosity, loyalty, dependability, and emotional stability. He worked with a great many people and had many work under his supervision. I have known many of these people personally, and I have never heard one word of criticism about the way J. P. treated them. He was always scrupulously honest and fair about giving credit where credit was due. Those who knew him could not imagine that he would ever do anything unethical, dishonest, or unfair, and, as far as I know, he never did. Others may have achieved national and international recognition by questionable means—politics, connivance, connections, maneuvering, exploitation, outright dishonesty, and so on—but in J.P.'s case every bit of it was earned fairly and squarely through inspiration, hard work, and honest achievement.

Many others have achieved fame and distinction in their work but at the cost of making a shambles of their personal life. Guilford, in contrast, was a devoted family man who loved, and in return enjoyed the lifelong love and devotion of, his one and only wife, their only daughter, and her three children. J. P. gave his wife a great deal of credit for what he was able to accomplish since she took on many responsibilities that otherwise might have distracted him from his career. In writing about her father in *An Odyssey of the SOI Model,* Joan S. Guilford paints a glowing portrait of the "daddy" she idolized who was always available to her, concerned about her welfare and happiness, helped her, and made her feel loved and respected. Few fathers could expect to receive such an appreciative tribute from their offspring, especially those fathers whose days had been so filled with work and heavy responsibilities.

This tribute from a family member is mirrored, if in a less dramatic way, by innumerable examples of a caring concern that Guilford showed toward all those with whom he was in close contact. He was never too busy to listen to someone's problem, to help out a student who was having difficulty, or to write a carefully composed letter of recommendation for someone who was trying to get a job or promotion. To mention only a couple of incidents, J. P. once wrote a sizeable personal check to a volunteer research associate to enable that person to represent himself as a paid member of Guilford's staff while making a trip around the country to contact important figures in the field. In another case a student relates how Dr. Guilford used a gentle, guiding question to rescue him from an embarrassing moment during his final orals when he was having difficulty with some equations.

Although he liked people and enjoyed being with them, Guilford was not gregarious or especially adept socially. He was very quiet and at times almost invisible, so much so that he was known by some of his U.S. Army subordinates as the "gray ghost." He would have liked to have been "one of the boys," but, although he had a few good friends, a basic shyness made it difficult for him to develop an easy camaraderie with others. The friends he had were usually professional colleagues who shared his interests and values. Raymond B. Cattell, in a personal communication, wrote, "Nevertheless, we soon became trusted friends, as he stood up like a rock for basic research amidst an endless flurry of fashionable nothings."

There was usually a somewhat awkward formality between Guilford and those around him, which neither wanted nor quite knew how to dispel. Few felt comfortable addressing him by anything other than "Doctor Guilford" or "Professor Guilford," even after decades of association. This formality was certainly not out of any fear of a negative reaction on his part. He was always most reluctant to criticize any student or subordinate, however much they might have deserved it, and was always very considerate and kind if he had to suggest any modification of others' behavior. I never saw him in an angry mood, and I never heard of him raising his voice to anyone. His disposition was always one of quiet friendliness and emotional calm. His daughter said about him that he evoked a kind of fear in her, "not because of any expectation of being punished or rejected, but rather from the possibility of disappointing an idol." I suspect that many others shared a similar feeling.

Although he was somewhat introverted, Guilford surprised people from time to time with his dry wit. After his final oral examination, one doctoral student thanked Dr. Guilford for the opportunity to have worked with him and for letting him make his own way and make his own mistakes. Dr. Guilford replied, "I didn't realize I let you make any mistakes."

In university circles, famous professors are sometimes prone to spend too much time bragging about their own accomplishments while tending to avoid activities that do not contribute to their own personal aggrandizement. Although he was very proud of his many accomplishments and honors, and justly so, Guilford never bragged about them. He usually spent most of his time with others listening to them rather than telling about himself. Furthermore, he earned a solid reputation for good citizenship among his colleagues by carrying more than his share of the work in making the university run. He dealt with other faculty members as equals and never acted like a prima donna who expected special treatment because of his star status. Nor did he try to influence or control the research and teaching of younger faculty members. One colleague recalls with gratitude that when he was a struggling young faculty member Dr. Guilford did not try to control or in fluence him but rather let him find his own way in both teaching and research. Another colleague confirms Guilford's tolerance for the teaching preferences of younger faculty members. Guilford was also a responsible public citizen who faithfully attended town hall meetings.

Despite all these wonderful qualities, so rarely found in one man, Guilford was not perfect. He appeared to be very modest and self-effacing but underneath that exterior lay a great deal of pride in himself and his achievements and an enormous confidence that in scholarly and scientific matters the way he looked at things was the correct way. Although he would rarely say openly that someone else was wrong, one got the impression that Guilford seldom entertained the notion that he himself might be wrong. He preferred to have around him people who accepted his own scholarly and scientific views. He was not one who loved to participate in the give and take of a public debate between those of divergent views. He marched to his own drummer, and once he made up his mind on a subject he felt little if any need to modify his views on the basis of what others who disagreed with him might think. Of course, this would not be an inaccurate description of many people who have achieved great success. This personality trait is noteworthy in Guilford's case only because it is somewhat out of keeping with the many sterling qualities that made him appear to be almost above human frailty.

In summary, then, Guilford will be remembered as an outstanding person as well as a gifted and productive scholar and scientist. To have made such an impact on the field of psychology while being such a revered teacher, father, husband, friend, colleague, and supervisor stamps him as a truly remarkable man. Few mortals have achieved so much in such an admirable way.

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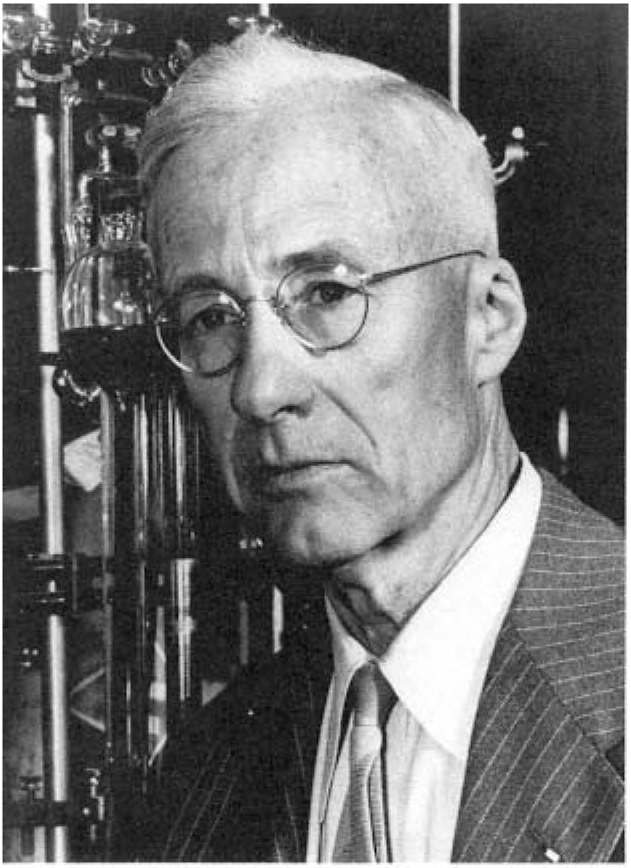

Joel H. Hildebrand

JOEL HENRY HILDEBRAND

November 16, 1881-April 30, 1983

BY KENNETH S. PITZER

THIS BIOGRAPHICAL SUMMARY differs from that for a typical scientist in many respects. First, there is the remarkable diversity of fields in which Joel Hildebrand made major contributions. To research scientists and engineers, his contributions to our knowledge of liquids and nonelectrolyte solutions are most important. But a substantially larger group recognize him as their outstanding teacher of freshman chemistry and often as the most inspiring teacher of their college experience. Others know him as mountaineer, lover of the outdoors, president of the Sierra Club, and coauthor with his daughter Louise of a charming little book, *Camp Catering*. There was his effective leadership in a variety of educational and scientific organizations far beyond chemistry, involving service as a member of the Council of the National Academy of Sciences (1949-52), dean of men and dean of the College of Letters and Science at the University of California, and member of the Citizens Advisory Committee on Education to the California Legislature. And, finally, he continued his active professional life past age 101.

It is a special pleasure to me to have the opportunity to write this biography. I first met Joel Hildebrand when I entered graduate school at Berkeley in 1935. His cordial ity to young people was immediately apparent. Although I did not do my thesis research with him, I did consult him frequently and the discussion was always helpful. With my appointment to the faculty, the association with Joel continued and expanded to a wide range of activities and to personal friendships including our families—with mine of the generation of his children.

Joel Henry Hildebrand was born on November 16, 1881, in Camden, New Jersey. His ancestors came to America before the revolution from the upper Rhine valley. When asked about his longevity, Joel replied, "I chose my ancestors carefully" and frequently added that most, if not all, lived well past eighty. His father, Howard Onid Hildebrand, was in the insurance business near Philadelphia, and Joel attended local schools. His intellectual interests were particularly stimulated by a grandfather who, although of limited schooling, had read widely and accumulated an excellent library. With his interest in natural phenomena aroused, Joel acquired and studied Dana's *Geology,* Newcomb's *Astronomy,* and similar books. After his high school mathematics was completed with solid geometry and trigonometry, he discovered independently the power and beauty of calculus. After he had learned as much chemistry as his teacher (the principal) knew, he was given the key to the laboratory, a college laboratory manual, and encouragement to learn more on his own. Joel told with justified pride about his experiment proving that nitric oxide gas was NO rather than N_2O_2—a result that demolished a theory in a book by a Harvard professor that he had been given. It is clear that this high school principal was a great source of encouragement also for opening to Joel broader horizons of interest in various cultural areas, including music.

Hildebrand entered the University of Pennsylvania in 1899 and wisely chose a double major in chemistry and physics in the College of Arts and Science rather than a more "professional" course in chemistry that emphasized recipes for analysis and similar details. He thereby had the opportunity to learn not only more physics but also history, literature, and mathematics while avoiding details of chemistry that were unimportant and sometimes even untrue.

After receiving his Ph.D. in 1906 in chemistry at Pennsylvania, Hildebrand was encouraged to spend a postdoctoral year in Germany learning the new science of physical chemistry before returning to teach it. He went to Berlin, where he attended lectures by J. H. van't Hoff and by Walter Nernst. He also did some research under Nernst and then returned to the University of Pennsylvania to serve on its faculty until 1913. In that year Gilbert N. Lewis invited Hildebrand to join the remarkable group of young chemists whom he selected and led in transforming the Chemistry Department at the University of California into a center of international eminence.

Hildebrand's doctoral thesis of 1906 was entitled "The Determination of Anions in the Electrolyte Way," and he continued with several papers on electrochemical methods in analysis. Herbert S. Harned was his first research student, and Harned's thesis was in this area. But Hildebrand soon shifted his primary interests to physical rather than analytical topics (as did Harned, who proceeded to a very distinguished career at Yale and was elected to the National Academy of Sciences).

The color of iodine solutions fascinated Hildebrand throughout his career; his first paper on that topic, "Uber die Farbe von Jodlosungen," was published in 1910. He soon noted (1920) that the deviations from Raoult's law of various violet solutions of I_2 formed a regular pattern. However, the curve for I_2 in benzene differed from this pattern, and the solution had a somewhat different color. This color differ ence suggested a more intimate interaction of the iodine with benzene.

These ideas were extended in many directions through the years. The concept of a regular pattern of positive deviations from Raoult's law grew into a general theory of "regular solutions." Such systems involve no specific solvation or association and the mixing of their molecules is essentially random. Equations for the activities of the components of such solutions had already been developed by several scientists, but these suffered either from the absence of relationships to the properties of the pure components or, in van Laar's case, to making these connections through an approximate equation of state. While the van der Waals equation was a great advance at the time and it gives a reasonable representation of gas imperfection, the quantitative deviations in the liquid region are large, and it is the liquid region that is pertinent to liquid solutions.

Scatchard published a paper in 1931 which, in his words, "may be regarded as a quantitative development of the treatment of Hildebrand, although it disagrees with his ideas in some important details, or as a method of freeing the van Laar treatment from the inadequacies of the van der Waals equation." Hildebrand and Wood derived the same equation two years later by a very different and modern method—by integrating the intermolecular pair potentials throughout the liquid weighted by the radial distribution function.

Both Scatchard's and Hildebrand's results yield the same working equation relating the deviation from ideal solutions (Raoult's law) to the cohesive energy density of the pure components, that is, $\Delta E/V$, where E is the energy of vaporization of a volume V of the pure liquid. More precisely, it is the square of the difference in the square roots,

$[(E_1/ V>_1)^{1/2}-(E_2/ V_2)^{1/2}]^2$, that determines the departure from ideality.

In recent years, this quantity, $(E/V)^{1/2}$, has been called the solubility parameter (or Hildebrand's solubility parameter) and given the symbol δ. The Scatchard-Hildebrand equation is quite successful—better than any other equation of comparable simplicity and generality. But it is not surprising that there are departures from perfect agreement, and from time to time Hildebrand presented tables of adjusted solubility parameters that yield improved agreement. These are always discussed in relation to aspects of the intermolecular forces that might explain the need for adjustment. Joel's effort to improve the theory of regular solutions continued with a final paper in 1979.

Hildebrand, always the effective teacher, summarized the current status of knowledge about nonelectrolyte solutions in monographs designed to interest and instruct chemists. Initially, these were general reports on the status of knowledge in the field and carried the title *The Solubility of Nonelectrolytes.* The successive editions of 1924, 1936, and 1950 (the last with R. L. Scott) grew in size along with the rapid advance of knowledge in this area. Opposite the title page of the third edition is a picture of a tube containing seven incompletely miscible liquids (heptane, aniline, water, perfluorokerosene, phosphorus, gallium, and mercury)—a beautiful example of Joel's flair for generating interest in and enjoyment of his topic for discussion. After 1950, Joel left to others the task of general review of knowledge concerning nonelectrolyte solutions, and he prepared smaller books concentrating on the areas of his particular interest. These were *Regular Solutions* in 1962 with R. L. Scott and *Regular and Related Solutions: The Solubility of Gases, Liquids, and Solids* in 1970 with J. M. Prausnitz and R. L. Scott.

The relationship of the color of iodine solutions to their other characteristics was noted. Joel maintained a continuing interest in the changes of color (or new spectral features) as an indication of bonding. The one case where I was coauthor with Joel of a published paper arose from a series of discussions in this general area; it is entitled "Color and Bond Character" and appeared in 1941. Hildebrand's most important discovery in this area came in a series of papers with H. A. Benesi in 1949-50 that related an intense ultraviolet absorption to the formation of electron donor-acceptor complexes. This type of complex, now more commonly called a charge-transfer complex, has been investigated extensively by others, is well understood theoretically, and is an integral part of our body of organized knowledge.

The "rule" that carries the Hildebrand name concerns the entropy of vaporization of a normal liquid. In 1915 he showed that, for a typical group of "normal" liquids boiling near or below room temperature, the entropy of vaporization was more nearly constant if compared at a constant vapor volume rather than on the constant pressure basis of Trouton's rule. With this considerably higher precision of agreement, the Hildebrand rule became a much more useful criterion of a normal liquid. In comparison, hydrogen-bonded or other highly polar liquids have larger entropies of vaporization than do "normal" liquids.

Another idea of Hildebrand's that has great practical as well as theoretical importance concerns the use of helium in deep diving. A diver at depth experiences high pressure and correspondingly increased solubility of breathing gases in his blood. The problem of the "bends"—the release of this gas as a bubble in a blood passage when the diver emerges—was well known. In the mid-twenties Hildebrand suggested that this problem could be ameliorated by substituting helium for nitrogen in mixture with oxygen for the diver's breathing gas. Not only is the solubility of helium much less than that of nitrogen at a given pressure, but also the diffusion rate is faster. These basic ideas have had a major role in improving diving capability and safety ever since.

Also in the mid-twenties, Hildebrand initiated precise physical-chemical studies of anhydrous hydrogen fluoride and fluorine. Among other studies, he and Simons measured the anomalous P-V-T behavior of HF and interpreted it on a polymerization basis. Simons proceeded from this beginning to a fruitful career of specialization in fluorine chemistry.

Through the years Joel took pleasure in demolishing concepts that he regarded as spurious or misleading. He was not fooled by "polywater." He was severely critical of theories of liquids that were based on complex assumptions about structural features for which there was no direct verification. With the deeper insight of molecular dynamics calculations, these complex assumptions have now been disproved in many cases. But Joel had refused to accept these theories, even if they were reasonably successful in representing the experimental data available at the time. Several of these situations are described in his 1977 paper, "Operations on Swollen Theories with Occam's Razor."

Another case of this type is the "hydrophobic effect" or, worse, the "hydrophobic bond." Joel objected to these terms because "phobic" implies repulsion. It is true that in aqueous solution a solute containing both alkyl (or other non-polar) groups and polar groups will arrange itself in a manner to favor water contact with the polar groups of the solute and alkyl group contact with other alkyl groups. But this does not mean that an alkyl group is actually repelled by a water molecule. Rather, as Hildebrand concludes, "There is no hydrophobia between water and alkanes; there is only not enough hydrophilia to pry apart the hydrogen bonds of water so that the alkanes can go into solution without assistance from attached polar groups."

In the period 1970-77 Joel gave considerable attention to the viscosity of liquids or, as he preferred, the fluidity that is the reciprocal of viscosity. These papers are collected in a small monograph, *Viscosity and Diffusivity: A Predictive Treatment,* published in 1977 with an introduction by J. O. Hirschfelder. In the introduction Hirschfelder writes of Hildebrand, "Somehow, he has the ability to sweep away all of the complexities and discover simple relationships which will take theoreticians another generation to derive." Indeed, Joel often presented new empirical relationships that were simpler and more accurate than those in common use. And he presented them in a simple qualitative theoretical framework that was free from inconsistencies or the complexities often contrived to circumvent inconsistencies. This book often elicited the comment that "Hildebrand is a genius in finding ways to present data so that they fall on a straight line." But Joel's were not merely functions yielding straight lines; he also required conformity to general ideas of molecular structure and behavior. Indeed, he was a genius in research of this type.

Hildebrand's impact as a teacher was just as important and in many respects more remarkable than his role in research. His freshman chemistry lectures, given regularly from 1913 until his "retirement" in 1952, were legendary. Thousands of alumni recall his vivid descriptions and dramatic demonstrations as well as his enlivening digressions into music, art, and mountaineering.

A single course was offered at Berkeley with total enrollment usually somewhat over 1,000, with lectures in a room seating about 500, but with laboratory, quiz, and discussion in groups of twenty-five. William Bray and Wendell

Latimer (both members of the Academy) took primary responsibility for the laboratory and wrote the book for it. Most of the regular faculty supervised freshman sections (in addition to other teaching) and thereby initiated the graduate students into their teaching assistant duties in an apprenticeship pattern. Thus, there was extensive involvement of most of the faculty with the general chemistry course and general agreement concerning its character. But Hildebrand gave the lectures, wrote the quizzes and examinations, and was in general charge of the course. He also wrote the central text, *Principles of Chemistry,* which was revised several times.

The course at Berkeley, as developed by Hildebrand, Bray, Latimer, and others, departed from the pattern of that time by much greater emphasis on principles, with reduced attention to memory of specific factual material. It was only after about twenty-five years that other textbooks began to appear that reflected a similar emphasis. Of course, the "Berkeley" books were used elsewhere in the intervening years.

As is often the case, the pattern has recently shifted farther (probably too far) toward dominance of theory and general principles and the near exclusion of "factual" material. The "Hildebrand" course maintained a balance; the student learned that, while important aspects of chemistry could be related to general principles through relatively simple equations, other experimental facts were best remembered, if important enough, or looked up when needed. To promote the habit of quick and convenient reference to this body of knowledge, Latimer and Hildebrand prepared their *Reference Book of Inorganic Chemistry* (1928). It was revised several times and was available combined with *Principles of Chemistry* in a single volume.

Joel was superb as a lecturer and thoroughly enjoyed it. There were many lecture experiments with an entertaining aspect and lots of humorous comments that the students enjoyed. But Joel never lost sight of the primary purpose of the lectures, and most of these entertaining features were tied into the primary lesson of the day. Joel's enthusiasm, combined with thorough knowledge and excellent lecture technique, was almost irresistible. There was never a problem of slack attendance at Hildebrand lectures.

From 1913 through 1952, Hildebrand had about 40,000 students in his freshman lectures. While only a moderate proportion followed chemistry professionally, many became engineers, physicists, or other scientists. Others became lawyers, business executives, and leaders in various fields, and they have a clearer picture of the role of science in the modern world because of their contact with Joel Hildebrand. His impact as a teacher was great indeed.

This fame as a teacher of chemistry gave Joel the credentials and brought invitations to influence educational matters more broadly. His former students, now in a multitude of positions of responsibility and influence, urged his inclusion on committees, boards, and conferences. A notable example was the Citizens Advisory Committee to the Joint Education Committee of the California legislature.

Joel had all of the qualifications of a good administrator or organizational leader. He never shirked such responsibilities when they were pressed upon him, but he never let such duties draw him permanently away from his primary interests in teaching and research. His preferences in this respect fitted very comfortably with the policies of the University of California, wherein academic administration was in the hands of distinguished professors, but there was no implication that a given individual would continue indefinitely as a department chairman or a dean. Indeed, the status of ex-dean was most highly regarded at the Berkeley Faculty Club.

Thus, Joel accepted appointments and served a few years in each case as dean of men (1923-26), dean of the College of Letters and Science (1939-43), chairman of the Department of Chemistry (1941-43), and dean of the College of Chemistry (1949-51). He also played a major role in the Academic Senate and served as chairman of important committees of the Senate.

In making an administrative decision, Joel collected and digested the pertinent information, consulted other individuals as appropriate, and then reached his conclusion promptly without emotional trauma. When he left his administrative office, he left those problems behind and was ready to discuss a problem in chemistry or to give a freshman lecture with full vigor and enthusiasm.

Other organizations frequently called on him to take positions of leadership, and he accepted when he believed he could make a significant contribution without undue interference with his work in chemistry. Thus, he became interested in the Sierra Club and was soon asked to be president (1937-40). He held various positions in the American Chemical Society but declined nomination as president until after his retirement from regular teaching; then he was elected and served in 1955. He also managed the U.S. Olympic Ski Team in 1936.

Hildebrand was elected to the Council of the National Academy of Sciences for a three-year term (1949-52) and to its Executive Committee for 1950-52. He also served by appointment as chairman or member of several important committees.

In both world wars Hildebrand was asked to undertake special duties. In 1918-19, he directed the chemical warfare laboratory of the American forces in France, with the rank of major and later of lieutenant colonel, and was awarded the Distinguished Service Medal. In 1943-44 he was a liaison officer in London for the Office of Scientific Research and Development. The British government also took advantage of Joel's presence in London to obtain his personal advice on many problems and awarded him their King's Medal for Service in the Cause of Freedom in 1948.

In 1908 Joel married Emily Alexander, whose continued good health and vigor were as exceptional as Joel's. Emily also lived to age 101. Their seventieth wedding anniversary in 1978 was a great occasion for all of their many friends. They had four children: Louise, Alexander, Milton, and Roger. Two are professors in the sciences—Milton in zoology at the University of California at Davis and Roger in physics at the University of Chicago. Both have been very successful in their own research and have held broader leadership roles within their universities. After outstanding research for the Standard Oil Company of California (now Chevron) in the area of oil discovery and production, Alexander took an early retirement and has been an active and successful farmer in the central valley of California. All of the children are married; there are twelve grandchildren and at least thirteen great-grandchildren. In their later years Joel and Emily frequently had a grandchild living with them while attending the university or beginning some new activity in the area.

In 1953, when Joel Hildebrand received the Willard Gibbs Medal, his son Roger was invited to help introduce him. The result was a most amusing and interesting insight into the Hildebrand family. Joel was always the enthusiastic teacher. As Roger tells it, "We were encouraged and instructed in any worthwhile pursuit. The most confirmed blockhead could hardly have withstood the assault of intellectual enthusiasm which we enjoyed. Any flair for science, athletics, music, arts or crafts on our part was noticed and the spark was fanned by a powerful hand. As a result, enough bonfires lit the sky to reduce any mother but mine to a cinder."

Another paragraph from Roger's introduction: "We learned a lot by watching him. He worked and played hard. He is justly proud of his physical condition. He once entered a grandfathers' swimming race. Now it takes him a quarter mile or so to get really warmed up, so he dove in and swam a few laps each of breast stroke, back stroke, and crawl. His competitors, who were watching from the bank, gradually disappeared and it is said that by starting time not a one of them could be found."

Among my multitude of memories of happy associations with Joel, I recall particularly the skiing trips of the mid-fifties. He was a member of a ski club with a lodge in the Sierra Nevada. All of the other members had given up skiing, even though many were younger than Joel. Thus, he had, in effect, a private ski lodge, and he invited his younger colleagues and sometimes their spouses on many occasions. Our skiing was primarily cross-country, which was also my preference, and most enjoyable. Equally wonderful were the evenings around the fireplace when we discussed all sorts of interesting topics. Joel had a stimulating comment on any topic, and we learned much from his rich experience.

An interesting Hildebrand story arose when the editor of *Who's Who in America* decided in 1975 to transfer Joel to the compilation "Who Was Who." This elicited a spirited response, of course, in which Joel listed five research publications from 1974 as well as others in press for 1975, together with copies of several comments about his current activities from others, including President Handler of the National Academy of Sciences. He concluded by saying: "Leave me out of *Who's Who,* if you must—*Europa* still lists me, but please postpone till a more appropriate time including me in Who *Was* Who. People would be writing to learn what happened to me." Needless to add, Joel continued to be listed in *Who's* Who—not "Who *Was* Who."

Nearly innumerable honors of various types came to Joel Hildebrand through the years. The more important honors are listed elsewhere, so I will comment only briefly. From the American Chemical Society came the award of the Nichols Medal in 1939; its teaching award in 1952; the Willard Gibbs Medal in 1953; and its highest recognition, the Priestley Medal, in 1962. Joel was elected to the National Academy of Sciences in 1929 and to the American Philosophical Society in 1951. He received an honorary doctorate after retirement from the University of California in 1954. I had the pleasure of presenting Joel on that occasion. When the citation was read, the audience immediately applauded Joel so enthusiastically that President Sproul at first forgot to confer the degree. After that omission was remedied, Joel received a second ovation. The warmth and enthusiasm of that occasion symbolize beautifully the high regard in which Joel was held by students, alumni, professional colleagues, and all others who had come to know him. Hildebrand's one hundredth birthday was celebrated by a special university convocation followed by a well-attended luncheon; it was a truly remarkable occasion.

CHRONOLOGY OF MAJOR ACTIVITIES AND HONORS

1881 Born November 16 in Camden, New Jersey
1903 B.S., University of Pennsylvania
1906 Ph.D., University of Pennsylvania
1906-07 Postdoctoral fellow, University of Berlin
1907-13 Instructor in Chemistry, University of Pennsylvania
1913-17 Assistant Professor of Chemistry, University of California, Berkeley
1917-18 Associate Professor of Chemistry, University of California, Berkeley
1918-52 Professor of Chemistry, University of California, Berkeley
1923-26 Dean of Men, University of California, Berkeley
1939 Sc.D., University of Pennsylvania
1939-43 Dean, College of Letters and Science, University of California, Berkeley
1941-43 Chairman, Department of Chemistry, University of California, Berkeley
1949-51 Dean, College of Chemistry, University of California, Berkeley
1952 Professor Emeritus, University of California, Berkeley
1954 LL.D., University of California

PROFESSIONAL SOCIETIES

1929 Member, National Academy of Sciences
1934 President, Pacific Division, American Association for the Advancement of Science
1946 Member, Royal Society of Edinburgh
1951 Member, American Philosophical Society
1953 Honorary Life Member, Faraday Society
1955 President, American Chemical Society
1957 Honorary Life Member, American Institute of Chemists
1960 Honorary Life Member, California Academy of Sciences

NATIONAL AND OTHER SERVICE

1917-18 Lieutenant Colonel, U.S. Army, Chemical Warfare Service

1922 Distinguished Service Medal, U.S. Army

1936 Manager, U.S. Olympic Ski Team

1937-40 President, Sierra Club

1942-43 Expert Consultant, Military Planning Division, Quartermaster Corps

1942-43 Member, Chemical Referee Board of Production Research and Development, War Production Board

1943-44 Scientific Liaison Officer, Office of Scientific Research and Development, American Embassy, London

1948 King's Medal for Service in the Cause of Freedom, United Kingdom

1958-60 Member, Citizen's Advisory Committee to the Joint Education Committee of the California Legislature

AMERICAN CHEMICAL SOCIETY AWARDS

1939 New York Section, William H. Nichols Medal

1949 Maryland Section, Remsen Award

1952 Scientific Apparatus Makers Association Award for the Teaching of Chemistry

1953 Chicago Section, Willard Gibbs Medal

1961 Northeastern Section, James Flack Norris Award in Teaching of Chemistry

1962 Priestley Medal

OTHER AWARDS AND LECTURESHIPS

1936 Faculty Research Lecture, University of California, Berkeley

1944 Guthrie Lecture, Physical Society, London

1944 Walker Memorial Lecture, University of Edinburgh Chemical Society

1952 Reilly Lectures, Notre Dame University

1953 Romanes Lecture, Royal Society of Edinburgh

1953 Spiers Memorial Lecture, Faraday Society
1954 Treat B. Johnson Lectures, Yale University
1956 Bampton Lectures in America, Columbia University
1957 W.A. Noyes Lecture, University of Illinois
1963 William Procter Prize, R.E.S.A.
1965 Joseph Priestley Award, Dickinson College
1971 Gilbert Newton Lewis Memorial Lecture, University of California, Berkeley
1974 S. C. Lind Lecture, Oak Ridge National Labaratory
1978 Clark Kerr Medal for "distinguished service to higher education," University of California Academic Senate

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Gerard P Kuiper

GERARD PETER KUIPER

December 7, 1905-December 24, 1973

BY DALE P. CRUIKSHANK

HOW DID THE SUN and planets form in the cloud of gas and dust called the solar nebula, and how does this genesis relate to the formation of other star systems? What is the nature of the atmospheres and the surfaces of the planets in the contemporary solar system, and what have been their evolutionary histories? These were the driving intellectual questions that inspired Gerard Kuiper's life of observational study of stellar evolution, the properties of star systems, and the physics and chemistry of the Sun's family of planets.

Gerard Peter Kuiper (originally Gerrit Pieter Kuiper) was born in The Netherlands in the municipality of Haringcarspel, now Harenkarspel, on December 7, 1905, son of Gerrit and Antje (de Vries) Kuiper. He died in Mexico City on December 24, 1973, while on a trip with his wife and his long-time friend and colleague, Fred Whipple. He was the first of four children; his sister, Augusta, was a teacher before marriage, and his brothers, Pieter and Nicolaas, were trained as engineers. Kuiper's father was a tailor.

Young Kuiper was an outstanding grade school student, but for a high school education he was obliged to leave his small town and go to Haarlem to a special institution that would lead him to a career as a primary school teacher.

The path to a university education in Holland was normally through proper high schools for that purpose, but Kuiper was intent on university admission and passed an especially difficult special examination that allowed him to enter Leiden University. In the same year, he passed an examination for certification to teach high school mathematics. Kuiper's, drive, persistence, and self-assurance, already well developed in his student days, moved him to succeed in spite of an atmosphere of discrimination at Leiden against poorer students and those who had not studied in the proper high schools.

At a young age, Kuiper's interest in astronomy was sparked when he read the philosophical and cosmological writings of Descartes. This interest was encouraged by his father and his grandfather, who gave him a small telescope. With his naked eye, Kuiper made sketches throughout an entire winter to record the faintest members of the Pleiades star cluster that he could detect. On his master chart, Leiden Observatory astronomers, to whom he sent the results, found the limiting magnitude 7.5, nearly four times fainter than those visible to the normal human eye. Even in his later years, Kuiper's visual acuity was exceptional.

Kuiper entered Leiden University in September 1924. His fellow student and long-time friend Bart J. Bok recalled the day they met as incoming students in the library of the Institute of Theoretical Physics. Kuiper explained to Bok that he intended to pursue astronomical problems of a fundamental nature, specifically the three-body problem and related questions about the nature and origin of the solar system. He completed a B.Sc. at Leiden in 1927 and immediately went on to postgraduate studies. Among Kuiper's professors at Leiden were Ejnar Hertzsprung, Antonie Pannekoek, and the theoretical physicist Paul Ehrenfest.

Kuiper became a friend of the Ehrenfest family in his role as tutor to the physicist's son.

Kuiper, Bok, and fellow student Piet Oosterhoff pursued their studies in astronomy together, learning from Willem de Sitter, Jan Woltjer, and Jan Oort, in addition to those named above. In his student years, Kuiper joined the Dutch solar eclipse expedition to Sumatra for eight months in 1929. He learned Malay and wandered among the native villages painting beach scenes and studying the local customs. Then, on the eve of the eclipse he discovered that another astronomer had incorrectly oriented the spectrograph slit on one of the cameras; the correction was made just in time to secure important data during the eclipse the next day.

In 1929, Kuiper began correspondence with the great double-star astronomer Robert Grant Aitken, at Lick Observatory of the University of California, and submitted his earliest measurements for criticism. He also outlined for Aitken the essence of the statistical study which was to occupy him for over a decade. Kuiper did his doctoral thesis on binary stars with Hertzsprung, and he received his Ph.D. on completion of this work in 1933. That same year he traveled to the United States to become a Kellogg Fellow (and then a Morrison Fellow) at Lick Observatory near San Jose on Mount Hamilton.

Under Aitken's tutelage, Kuiper continued his work on binary stars at Lick, where he systematically examined stars of large parallax for duplicity. He had delayed publication of his thesis until he could improve the observational data for double stars with large differences in brightness between the components. Observing visual doubles with the 12- and 36-inch refractors and making color-index measurements with the Crossley 36-inch reflector, he discovered numerous binaries and many white dwarf stars.

Kuiper always considered himself a double-star astronomer, and he was strongly influenced by Aitken. Aitken had learned from E. E. Barnard, and he in turn had learned the art from the great S. W. Burnham.

Concerning this work, in 1971 Kuiper recalled that at the beginning of his career he had been asked to review a book on the origin of the solar system.

> The analytical part of the book impressed me greatly. The second, synthetic part was entirely disappointing. After the review was written, I continued to struggle with this problem and had to conclude that the state of astronomy did not permit its solution. . . . I then determined to find a closely related problem, that with finite effort would probably lend itself to a solution . . . the origin of double stars.[1]

Eventually, Kuiper announced that at least 50 percent of the nearest stars are binaries or multiple-star systems. He more clearly defined the mass-luminosity relation for main-sequence stars and showed that the white dwarfs are high-mass objects departing from the empirical law. His 1938 paper in the *Astrophysical Journal* on the mass-luminosity relation is still considered a standard work on the subject.

Though intellectually stimulating and productive, the two years at the Lick Observatory were not an unqualified success. Sensitivities in the small, remote mountaintop community of astronomers were acute, and Kuiper was perceived by some as talented but somewhat outspoken and abrasive. He was not to become the heir to Aitken's legacy, and in August 1935 he left for a year at Harvard College Observatory.

At the time he arrived in Cambridge, Kuiper intended to go to Java to continue his career at the Bosscha Observatory. Instead, he met and married (on June 20, 1936) Sarah Parker Fuller, whose family had donated the land on which Harvard's Oak Ridge Observatory is built. During that year he was offered a position at the Yerkes Observatory of the University of Chicago by the director, Otto Struve. In November 1935, Kuiper telegraphed to Java that he would decline the position there. Kuiper felt that he could make an important contribution to astronomy by moving to Yerkes, but lamented in a letter to W. H. Wright of Lick Observatory (October 30, 1935) that ". . . it will mean a real sacrifice to me not to go to the beautiful and happy island of Java." In fact, he might not have been able to escape the Japanese prison camps after the invasion a few years later.

Even before Kuiper moved to Yerkes, Struve sought his advice on matters related to the addition of new senior staff members at that institution. In 1936 Kuiper was appointed assistant professor at the University of Chicago. He was associate professor from 1937 to 1943 and was then appointed professor.

As a new staff astronomer at Yerkes, Kuiper contributed heavily to what W. W. Morgan has called the renaissance of the Observatory. That rebirth was initiated by Struve, who, as the new director from 1932, brought Kuiper, S. Chandrasekhar, and Bengt Strömgren to the staff; Morgan was a graduate student there and was appointed instructor in 1932. Bok has noted that Kuiper's marriage and appointment at Yerkes Observatory were strong positive stimuli to his scientific work in the late 1930s and 1940s.

In his new post Kuiper worked with Struve and Strömgren on the eclipsing binary Epsilon Aurigae, proposing in a major joint paper in 1937 that a large star surrounded by a partly transparent gas halo eclipses an F supergiant whose ultraviolet radiation has ionized part of the larger star's tenuous atmosphere. This model spawned numerous additional observational and theoretical studies of the unique Epsilon Aurigae system.

To determine evolutionary tracks in the Hertzsprung-Russell temperature-luminosity diagram, Kuiper combined Strömgren's theoretical studies with Robert Trumpler's observations of clusters. In 1937, Kuiper published in the *Astrophysical Journal* a historic color-magnitude diagram for galactic clusters, interpreting the tracks shown as Strömgren's lines of constant hydrogen content and pointing out that this hypothesis explains several other observational results. The next year he derived the corrections needed to convert photographic stellar magnitudes, providing the basis for the stellar temperature scale that was in wide use until the advent of ultraviolet satellite astronomy.

Kuiper joined Struve and other members of the Yerkes staff in planning for the University of Texas 82-inch reflecting telescope, to be operated jointly by Yerkes and the University. McDonald Observatory near Fort Davis, Texas, was dedicated in 1939, and with the new instrument and its high-quality spectrographs, Kuiper continued his search for white dwarfs and spectroscopic studies of stars with large proper motions. He took up residence at the remote observing site in west Texas during the breaking-in period of the 82-inch telescope and began to acquire data on stars that had been too faint for the telescopes previously available to him. Although life at the remote and poorly developed observatory site was a strain on his family, the McDonald telescope was at that time the second largest in the world and a vital tool for the work he had set out to accomplish.

An early target of Kuiper's new work was Beta Lyrae. In his monumental 1941 paper on this double-star system, Kuiper introduced the term "contact binaries." In this work he also recognized that material accreted by the smaller star would form a ring around it; he thus anticipated the accretion disks of mass-exchange binary stars that are now known to be of great importance.

During World War II (1943 to 1945), Kuiper took a leave of absence from the University of Chicago and joined the faculty of Harvard's Radio Research Laboratory, where he was involved in radar countermeasures. In this connection he went to England with the Eighth Air Force Headquarters in 1944. He returned to Europe in January 1945 as a member of the ALSOS Mission of the U.S. War Department, to assess the state of German science. Kuiper accomplished a rather daring rescue of Max Planck, who he learned was in the eastern zone of Germany in dire circumstances and in danger of being captured by Soviet troops. He took a vehicle and driver and raced across the countryside to Planck's location, arriving only hours ahead of the Soviets. Planck and his wife were taken to the western zone and then on to Göttingen and to the care of friends and relatives.

During a brief respite from war work in the winter of 1943-44, Kuiper returned to McDonald Observatory and included in his observing program a spectroscopic study of the major planets, the Galilean satellites of Jupiter, Triton, and four of Saturn's satellites, including Titan. He found the 6,190-angstrom band of methane in Titan, the first detection of an atmosphere on a satellite. In 1944 he wrote, "It is of special interest that this atmosphere contains gases that are rich in hydrogen atoms; such gases had previously been associated with bodies having a large surface gravity."

Concerning the discovery of Titan's atmosphere, Kuiper noted in a letter to J. H. Moore (February 29, 1944), then director of Lick Observatory, "The only reason I happened to observe the planets and the 10 brightest satellites was that they were nicely lined up in a region of the sky where I had run out of program stars (stars of large proper motion and parallax)."

While the discovery of Titan's atmosphere may have been serendipitous, it stimulated the studies of planetary atmospheres which were to occupy Kuiper until the end of his life. His interest was further stimulated by contacts with Bernard Lyot in France during and just after the war, whence he brought back reports of excellent planetary work in progress at Pic du Midi Observatory. Kuiper was greatly impressed with the heroism of the French astronomers during the occupation and wrote of this in news notes to astronomical journals. In 1947, he was awarded the Janssen Medal of the Astronomical Society of France. In Germany following its surrender, Kuiper learned from German scientists details of the new lead sulfide infrared detectors being developed on both sides during the war, and he was impressed by their astronomical possibilities. The American detectors were declassified in September 1946, and Kuiper soon collaborated with the detector's developer, Robert J. Cashman, on construction of an infrared spectrometer for the study of stellar spectra in the wavelength region of 1-3 micrometers.

Kuiper's first near-infrared spectra of stars and planets, made with a two-prism spectrometer later the same year, were of low resolution, but they laid the groundwork for the study of planetary atmospheres for the next quarter of a century. Between 1946 and the 1980s the near-infrared spectral resolution increased more than 100,000-fold, with a corresponding improvement in knowledge of planetary and stellar atmospheres. Kuiper's earliest spectra in 1947 revealed carbon dioxide on Mars. He was the first to see the near-infrared spectra of Jupiter and Saturn, as well as those of the Galilean satellites of Jupiter and the rings of Saturn. His intuitive interpretation has largely been borne out as the data have since improved.

At the fiftieth anniversary of Yerkes Observatory in 1947, Kuiper organized a symposium on planetary atmospheres, assembling astronomers, meteorologists, and other specialists for the first meeting of its kind. An important outcome was the book edited by Kuiper, *The Atmospheres of the Earth and Planets* (Chicago: University of Chicago Press, 1949 and 1952). In his own classic paper, Kuiper summarized knowledge of the composition of planetary atmospheres, presenting a lucid account of their origins as primary (in the case of the Jovian planets) or secondary (the terrestrial planets) gas envelopes with regard to their interaction with the early solar wind (though the term "solar wind" had yet to be invented). From combined spectroscopic and thermal measurements, he gave the first interpretation of the atmospheric structures of Jupiter and Saturn.

Kuiper was among the first to think of planetary phenomena in terms of cosmochemistry, a subject that has been greatly developed in recent years. His table of atmospheric compositions of the planets in the Yerkes symposium volume has been the foundation of much subsequent theoretical and observational work. To this day, improved visible, infrared, and ultraviolet spectra are used to refine Kuiper's abundance estimates, lower the detection limits, and reveal new gases in the atmospheres of the planets.

Kuiper typically worked alone up to this time; according to Bok, he was too busy to work with students. With his own energetic self-motivation, and in the atmosphere of selfless service to science inspired by Otto Struve at Yerkes, Kuiper worked with great intensity on his science and in influencing the directions of the Yerkes and McDonald observatories. In the mid-1940s, however, Kuiper's work attracted Daniel E. Harris III, who was to become his first student of planetary astronomy. They collaborated on photometric studies of planets, satellites, and asteroids, and in 1949 Harris completed his Ph.D. work with a disserta tion on the satellite system of Uranus. (Harris died prematurely on April 29, 1962.) In February 1948, Kuiper found and named a fifth satellite (Miranda) of Uranus on photographs taken to determine relative magnitudes of the four known satellites. In a systematic search for outer and inner satellites of the planets, he found Nereid, the second moon of Neptune, in May 1949.

Succeeding his long-time friend Otto Struve, Kuiper became director of Yerkes and McDonald observatories in 1947, a post he occupied for two years and then resumed in 1957. His thoughts were returning to the origin of the solar system, as he described in the Kepler Medal discourse in 1971:

> I felt that I had come to understand the problem of double-star origin, at least in outline; that it was identical to the general process of star formation, from slightly turbulent prestellar clouds upon contraction, with conservation of angular momentum. It followed that the Solar System was no more than an "unsuccessful" double star with the companion mass spread out radially into a disk that in time developed the planets .... The *mass partition* between [the primary and companion masses] would be random mass fractions of the total, a result I had derived empirically from a statistical study of double-star ratios. Thus, planetary systems clearly had to originate as the low-mass extremity of the almost universal process of double-star formation.... A basis had thus been found for estimating the *frequency* of planetary systems in our galaxy.

In September 1949, Kuiper startled his colleagues and the public with the announcement that the frequency of planetary systems was at least one in a thousand. A year or two later he revised this to be at least one in a hundred, and then as many as half of the total number of stars in the galaxy; earlier workers had considered planetary systems extremely rare, perhaps one in a trillion stars.

That same year, Kuiper organized and initiated the Yerkes-McDonald asteroid survey, in order to provide reliable sta tistical information on the asteroid population down to magnitude 16.5. The main results from over 1,200 photographic plates were published in 1958, with one of the authors being Kuiper's second student of the solar system, fellow Dutchman Tom Gehrels. Gehrels had been given a student assistantship in 1952 with the formidable task of determining the brightnesses of the asteroids on the huge volume of photographic plates taken at McDonald Observatory. This work was the precursor to the later Palomar-Leiden survey to magnitude 20.5, from which essential statistical information on the asteroid population was derived.

By early 1950, Kuiper developed a theory of the origin of the solar system. In 1945, G. Gamow and J. A. Hynek had called the attention of American astronomers to a significant new work on this topic published a year before by C. F. von Weizsacker in war-torn Germany. Attracted by von Weizsacker's quantitative revival of the solar nebula theory of Descartes and Kant, and apparently influenced by the work of H. P. Berlage and D. ter Haar, Kuiper extended the work, arguing that large-scale gravitational instabilities could occur in the rotating solar nebula; these regions would be stable against the tidal shear by the Sun, and the material in them could begin to condense. The large condensations eventually became the protoplanets, of which there was one for each of the present planets formed in the central parts of the condensations, with satellites in the extremities. Uncondensed gas, constituting the majority of the initial mass of the protoplanets, was dissipated to the outer parts of the nebula by the solar wind. Except for the important role played by the solar wind, the Kuiper theory is basically a gravitational one in which angular momentum is lost by conventional means; more recent theories have shown that other mechanisms, such as magnetic and electrical fields, are needed to transfer momentum in the contracting and rotating nebula.

Kuiper recognized that certain satellites and some classes of asteroids did not fit the simple picture he had outlined, and in a lengthy series of papers published between 1950 and 1956 he explored the oddities of the solar system, the irregular planetary satellites, the Trojan asteroids, the comets, Pluto, the Earth-Moon pair, and other objects in an effort to understand their origins and their dynamical evolution.

The problem of the origin of the solar system attracted many investigators in the 1950s and 1960s, eventually leaving Kuiper's gravitational theory largely behind. It was with great delight that he returned to the subject in 1972, with a major work only partly finished and published at the time of his death.

In the course of his development of the theory of the solar system, Kuiper took a serious interest in the surface of the Moon, recognizing the information it contains for the early dynamic history of the terrestrial group of planets. Some early results of his careful visual and photographic observations of the Moon with the McDonald 82-inch telescope are interpreted in a series of papers in the *Proceedings of the National Academy of Sciences* (he had been elected a member of the Academy in 1950). Kuiper's interest in the Moon had been kindled in part by the appearance in 1949 of R. B. Baldwin's book *The Face of the Moon,* for which he often expressed great admiration. His studies of the time scale of formation on the lunar surface features attracted wide attention and generated a heated debate with Harold Urey in the pages of the *Proceedings.* For the next fifteen years, Kuiper would frequently defend his often rigidly held theories of the lunar surface in public and professional forums.

When Strömgren resigned as director of Yerkes and McDonald observatories in 1957, Kuiper was elected to the position for a second time, and it was in the next year, as a summer assistant at Yerkes, that I had my first contact with him. By that time he had begun work on a contract with the U.S. Air Force to produce an atlas of the best available photographs of the Moon selected from the collections at Lick and Mt. Wilson Observatories, augmented by Kuiper's own McDonald photographs. Soon thereafter, he invited the British lunar specialists E.A. Whitaker and D.W.G. Arthur to join him for continued work in selenodesy, lunar cartography, and photogrammetry. The *Photographic Lunar Atlas,* published in 1960, was the magnificent product of the enormous effort invested by Kuiper and his collaborators.

During this second period of Kuiper's directorship at Yerkes, his third student of planetary science, Carl Sagan, completed his graduate work at the University of Chicago.

Aware of the dearth of comprehensive literature on the physics of the solar system in the 1950s, Kuiper organized a large editorial project to produce four encyclopedic volumes on the Sun, the planets, the Earth as a planet, and the satellites and comets. The first, *The Sun,* was published in 1953, and the last, *The Moon, Meteorites and Comets,* was published ten years later. While still at Yerkes, he organized the production of a more comprehensive nine-volume compendium, *Stars and Stellar Systems.* Much of the work on these books was accomplished by Barbara Middle-hurst, who had joined Kuiper as an associate editor.

With the new interest in the Moon on the part of the U.S. Air Force and the recently created (in 1958) National Aeronautics and Space Administration, Kuiper received substantial support and encouragement for his solar system studies. He also saw opportunities for further studies of the solar system with NASA support as plans were begun for the first deep space probes to the planets.

In late 1959, the Yerkes staff voted not to reappoint Kuiper director and chairman, and while the intent was that he would continue his three-year term to its end in August 1960, his directorship in fact was terminated in January 1960. The growing tensions among the senior staff eventually resulted in Kuiper's resignation from his professorship later in 1960, whereupon he relocated at the University of Arizona in Tucson.

What follows is my own view of the state of affairs, gleaned from conversations with those involved and those who viewed the situation from outside the Yerkes staff.

Otto Struve, the hard-driving and autocratic director of Yerkes Observatory, had assembled a staff of extraordinarily talented observational and theoretical astronomers and astrophysicists. The scientific output was at the highest intellectual level, and the students educated in the program at Yerkes in those days became the senior astrophysicists of the next generation. With the talent came strong personalities, and Kuiper was one of several whose native brilliance was accompanied by an uncompromising self-assurance of the importance of his own work. Struve had the ability to mold the Yerkes staff into a highly productive entity, but by the time he left in 1950, the binding had unraveled. Natural tensions among individuals with strong personalities and scientific territorial instincts began to rise, resulting in a situation widely referred to as a "civil war." Professional criticism of Kuiper among the Yerkes staff members arose in part because of his demanding personality, in part because of his well-known conflicts with certain astronomers elsewhere, and also because of his shift away from "classical" astrophysics to problems of the solar system. One illustrative conflict is that with another white dwarf star specialist and fellow Dutchman W. J. Luyten, of the University of Minnesota. Struve mediated between Kuiper and Luyten in their proprietary conflicts over white dwarf stars to the extent that a written pact of collaboration drawn up by Struve was signed by the belligerent parties. When Kuiper shortly thereafter published a paper that Luyten considered a violation of the pact, Luyten tore up his copy of the document.

In the late nineteenth century, the study of the planets was a significant part of the work of most observatories in America and abroad. An element of fantasy entered this work with the publicity and voluminous popular writing about the canals of Mars, largely by Percival Lowell in the United States and Camille Flammarion in France. The popularity of the space-related science fiction of Jules Verne and others also contributed to the perception of many professional astronomers that the study of the planets was outside the realm of serious astrophysics. Those very few astronomers who applied the developing observational techniques to the planets did so in a very low key throughout the first half of the twentieth century. It was Gerard Kuiper who endeavored to return the physical study of the solar system to respectability on the strength of his own reputation, previously established in stellar studies, and through application of infrared techniques to the study of planetary atmospheres.

In 1960 Kuiper established the Lunar and Planetary Laboratory (LPL), first as an adjunct to the Institute of Atmospheric Physics and later as a separate entity of the University of Arizona. Kuiper conceived of the Lunar and Planetary Laboratory as a multidisciplinary approach to the study of the solar system, drawing upon astronomy, geology, atmospheric physics, and chemistry, all of which were well established at the University of Arizona. With this move, when he was fifty-four, his career accelerated to a most extraordinary pace. Many Yerkes staff members accompanied or ultimately followed Kuiper to Arizona. Whitaker, Arthur, Elliott Moore, Middlehurst, van Biesbroeck, and others were all at one time important members of the LPL staff. Four new students, Tobias C. Owen, William K. Hartmann, Alan B. Binder, and I, joined Kuiper's group in those first years in Tucson, all seeking an education in planetary studies and all going on to independent research after several years of association with him in Arizona.

With new developments in infrared detectors and diffraction grating technology, Kuiper's attention at once returned to stellar and planetary spectroscopy. By the end of the first year in Tucson, his group produced a new spectrometer with nearly 100 times the performance of the device used in his earliest infrared work. No time was lost in putting the new instrument to work on the McDonald 82-inch telescope and on the recently completed 36-inch telescope at Kitt Peak National Observatory. As he described in his summary of the first three years of LPL *(Sky and Telescope,* January and February 1964), the results were quick to follow: measurement of the isotopic ratios and the discovery of several "hot" bands of carbon dioxide on Venus, experimental verification of the low pressure on Mars, and detection of new absorption bands in the spectra of Alpha Orionis, Chi Cygni, and other cool stars. Later came the first observations of the infrared lines of hydrogen in the spectra of early-type stars. The cool stellar spectral bands were soon identified as carbon monoxide and, in the case of Omicron Ceti, water vapor.

From the beginning, Kuiper had emphasized laboratory studies of gases in long-path absorption cells as an essential part of planetary spectroscopic work, an approach clearly resulting from his association with Gerhard Herzberg at Yerkes in the late 1940s and early 1950s. The expanded LPL facilities included a 40-meter multiple-path cell, one of the largest in the world.

Shortly after he founded LPL, Kuiper began a search for high-quality sites for astronomical observatories especially suited to work in the infrared and on the planets. Together with Harold Johnson, who joined LPL in 1962, he established a family of telescopes in the Santa Catalina Mountains near Tucson. The major instrument was Kuiper's 61-inch NASA-funded telescope, put into operation in late 1965. The lengthy series of high-resolution planetary and lunar photographs initiated by Kuiper with this fine telescope include the best ever obtained from the ground.

In the early LPL years, Kuiper observed regularly at McDonald and Kitt Peak, made balloon spectroscopic observations of the Earth's atmosphere, and conducted observatory site surveys in Hawaii, Mexico, and California. He was influential in initiating what is now the Cerro Tololo Interamerican Observatory in Chile. It was largely his strong interest in 13,800-foot Mauna Kea in Hawaii as an infrared observing site that led to its development and the installation of a 2.24-meter NASA-funded telescope there in 1968; Mauna Kea now holds the largest collection of large telescopes in the world. He remained continuously engaged in the search for and study of superior observing sites, as he was in Mexico at the time of his death.

At LPL, the lunar work required a special effort with the interpretation of NASA's Ranger and Surveyor Moon probe results, which were precursors to the manned-landing program. The fundamental work on selenographic coordinates, *The Orthographic Atlas of the Moon,* done at Yerkes by Kuiper, Whitaker, and Arthur, was published in Tucson in 1960. In 1963 came the *Rectified Lunar Atlas,* during the preparation of which Kuiper and Hartmann discovered many lunar concentric basins and opened a new era of interpretation of early lunar impact history. Kuiper was principal investigator on the NASA Ranger program and served as an experimenter on the Surveyor program in the mid-1960s, eventually editing the voluminous photographic atlas of Ranger pictures. In 1967, Kuiper and his colleagues published an atlas of the best lunar photographs made from several ground-based observatories.

In addition to the special atlases published in Arizona, Kuiper had in 1962 begun publication of the *Communications of the Lunar and Planetary Laboratory,* an observatory annals-type journal which he edited and for eleven years used as his main line of communication to the astronomical community.

Kuiper took a keen interest and played an influential role in the development of infrared astronomy in the 1960s. In 1967 the NASA Convair 990 aircraft with a telescope aboard became available for infrared studies from an altitude of 40,000 feet, and Kuiper at once began observations of the planets and stars. In 1967 and 1968, he conducted a program to make an atlas of the infrared solar spectrum at high resolution above most of the atmosphere. The atlas was published in ten articles in the *Communications.* In recognition of the impetus he provided to airborne infrared astronomy, the successor to the Convair 990 ("Galileo"), a 1-meter telescope in a C-141 aircraft, was named the Kuiper Airborne Observatory in 1975.

Throughout the Arizona years, Kuiper served on NASA committees and panels, and he briefed high government officials on aspects of the space program. His scientific input to NASA officials at many levels influenced the course of ground-based and space probe investigations of the solar system.

Kuiper had a remarkable ability to recognize opportunities for advances in astronomy afforded by the develop ment of new instruments and techniques. Seeing an opportunity for an advancement in astronomy, Kuiper would seize upon it and push it to a self-sustaining degree of development, using all his prodigious energy.

As an observer, Kuiper was indefatigable. I accompanied him as a student assistant on many long observing runs at McDonald and Kitt Peak observatories in the 1960s when the full nighttime schedule of infrared spectroscopy was supplemented by daytime observations of bright stars and planets. During these periods, sometimes amounting to fourteen consecutive clear days and nights at McDonald, Kuiper would function on three to four hours of sleep on some days, while another assistant and I would work ten-hour shifts. When heavily fatigued at some time during the day or night at the telescope, he would occasionally lie down on the observing platform only to awaken twenty minutes later appearing fully refreshed and ready to press on for another four to six hours. In retrospect, it seems not at all surprising that Kuiper independently discovered Nova Puppis visually, *after* completing a full night's observing (in November 1942), or that he then reopened the telescope and obtained four spectra and a position determination before the Sun rose.

Gerard Kuiper was a demanding individual who thrived on a daily routine of hard work and long hours, and he expected the same from his subordinates and associates. His European formality both attracted and repelled many of the people with whom he had contact. He was in complete command of his manner, which was by nature outwardly kind and friendly, though reserved and serious. But he could become instantly cool and acerbic in some unpleasant confrontation, which he greatly disliked and carefully avoided whenever possible. He sought loyalty in his associates, and in return his sense of fair play prevailed in his supervisory capacity as director at Yerkes and LPL. However, as an intensely driven man, Kuiper's perceived hauteur occasionally strained the patience and loyalty of his colleagues and friends.

Two children were born to Gerard and Sarah Kuiper, Paul Hayes in 1941 and Sylvia Lucy Ann in 1947. Both grew to become intelligent, talented, and perceptive adults in the loving household that was the rock of stability in Kuiper's personal life.

Kuiper had a delightful eagerness to share his knowledge and perceptions, a fact that many anecdotes could substantiate. He had a strong aesthetic sense, and on the numerous topics of interest to him Kuiper was a marvelous conversationalist, with a ranging curiosity and a perceptible wit. He commanded wide respect for his achievements, his organizational abilities, and his passion for science. His gift of prodigious energy was matched by one of acute intuition. Kuiper's approach to science was, in fact, highly intuitive, propelled by first-order computation from the first principles of physics, and always a drive for new data.

As an individual who initiated physical studies of the solar system, sometimes in the face of professional criticism, Gerard Kuiper can truly be considered the father of modern planetary astronomy. At the same time, his contributions to stellar astronomy remain a fundamental part of the literature of double-star studies, the nature and statistics of white dwarf and high-proper-motion stars, and infrared stellar spectroscopy.

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The principal archive of Kuiper's correspondence, papers, and notebooks is held at the University of Arizona in Tucson. His correspondence and papers related to the years at Yerkes and McDonald observatories are located in the Archives of the Yerkes Observatory, Williams Bay, Wisconsin, while certain early letters are contained in the Mary Lea Shane Archives of the Lick Observatory, University of California, Santa Cruz.

THIS BIOGRAPHICAL MEMOIR is based in part on my article about Kuiper in *Sky and Telescope* 47 (1974):159-164. In preparing this memoir, I have benefited from interviews and correspondence with the late Bart J. Bok, Martin Schwarzschild, E. A. Whitaker, Donald M. Hunten, W. J. Luyten, S. Chandrasekhar, J. Oort, W. W. Morgan, and especially Tom Gehrels. Numerous colleagues and students of Kuiper commented on early drafts. I thank Mrs. Sarah F. Kuiper Lansberg for her friendship, interest, and support in the preparation of this memoir.

NOTE

1. Quotation from Kuiper's address on December 28, 1971, when he received the Kepler Gold Medal at a joint meeting of the American Association for the Advancement of Science and the Franklin Institute. For the full text, see *Communications of the Lunar and Planetary Laboratory* 9(1972-73):403.

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L. A. Maynard

Leonard Amby Maynard

November 5, 1887-June 22, 1972

BY DAPHNE A. ROE

EARLY LIFE AND FORMATIVE YEARS

LEONARD AMBY MAYNARD was born on a farm in the Township of Hartford, New York. It was on this farm and also in the nearby village, where his family moved when he was eight, that he acquired an appreciation of plant and animal life. He was also exposed early to books on agricultural science, which were owned by the family and used in a practical manner for their farm activities.

At the age of five he started school in a one-room red schoolhouse near the farm, and later he attended the elementary school in Hartford village, where he remained until he reached eighth grade. At this time his parents sent him to the Troy Conference Academy, a coeducational boarding school in Poultney, Vermont. Here he received an excellent education in a limited range of subjects, including language skills, literature, and mathematics, but training in the sciences other than mathematics was not provided. While still at this school, he took examinations which enabled him to teach in a New York State district school, and for a year he taught in the same school where he had begun his own education.

In 1907, he entered Wesleyan University in Middletown, Connecticut. As a freshman, he continued to take courses in the subjects he had learned in prep school, but he also took courses in physical, biological, and social sciences. His first course in chemistry determined his future career interests. Chemistry was indeed an exciting discipline at Wesleyan at that time; it was there that Professor W. O. Atwater had recently shown how a knowledge of chemistry could be applied to enormously increase knowledge of human and animal metabolism. Although Atwater had died the year before Maynard entered Wesleyan, Atwater's pupil, Professor W. P. Bradley, gave him his first exposure to chemical knowledge and explained in the Atwater tradition how this knowledge could be applied both to animal and human nutrition as well as to agriculture. Maynard received his A.B. degree from Wesleyan in 1911 cum laude and was elected to Phi Beta Kappa.

After graduation, he was assistant in chemistry for a year at the Iowa Experiment Station. During that year he pursued part-time graduate study, which he also did during the following year when he was assistant chemist at the Rhode Island Experiment Station.

In 1913 he came to Cornell as a graduate student in the chemistry department, and in 1915 he received his Ph.D. in chemistry. During his two years of graduate work at Cornell, he was most academically stimulated by Dr. Wilder D. Bancroft, who greatly widened Maynard's knowledge of chemistry literature and initiated many of his later research interests (1972,2). However, his later concern for dissemination of nutrition knowledge to the public may have arisen not only from his earlier work in agricultural experiment stations in Iowa and Rhode Island but also from his exposure at Cornell to faculty and students who were studying physiological influences on the nutritional need of farm animals. During the period that Maynard was a graduate student at Cornell, the extension activities of the university were being established. These endeavors may also have shaped his future concern with nutrition education of farmers and their families (1940).

Maynard's academic career was interrupted by his period of military service. He served with the American Expeditionary Force in France in 1917 and continued with the World War I armed forces until 1919 when he was discharged, having served as a major in the Chemical Warfare Service (1972,2).

FIRST CONTRIBUTIONS TO THE NUTRITION PROGRAM AT CORNELL UNIVERSITY

Maynard obtained his first academic position at Cornell University in 1915, immediately after obtaining his Ph.D. It was then that Professor Elmer Seth Savage invited him to establish a laboratory for the study of nutrition in small animals. At that time Maynard also received an appointment as assistant professor of animal nutrition in the Department of Animal Husbandry in the New York State College of Agriculture. In 1920, after his discharge from the armed forces, he was promoted to a full professorship.

In 1926, he took sabbatical leave and carried out studies at Yale University with Professor Lafayette Mendel. It was there that he met Clive McCay, who subsequently became his scientific collaborator at Cornell. Maynard's exposure to Mendel's method of teaching graduate students (by making them report on published scientific papers in biochemistry and nutrition) influenced his decision to employ this method in his own teaching at Cornell. In both institutions, the students had immediate feedback from the professor on their performance, which encouraged them to do the necessary homework thoroughly. This system of instruction is still used in Cornell's Division of Nutritional Sciences; indeed, these seminar performances are the only stipulated efforts for students in that graduate program.

LEADERSHIP IN ESTABLISHING CORNELL AS A CENTER FOR HUMAN NUTRITION RESEARCH AND TEACHING

Maynard accepted a personally imposed challenge to establish a program at Cornell in which graduate students would be prepared for careers in human nutrition research and teaching. His genius was in knowing that in developing such a program, it would be necessary to broaden currently held definitions of what should be termed "human nutrition." He also knew that he would have to work with a multidisciplinary faculty to accomplish his goals.

A unique feature in his building of this program was the manner in which he formed linkages between the different units at Cornell, units that were then carrying out nutrition research with goals dictated by their separate leadership and allegiance. Maynard's intent was to bring about a necessary unification of the different programs, at least to the extent that the different units would all have concern for studies which provided insight into the nutritional needs of human beings. In certain instances, this unification came about because he initiated the research endeavors; in other cases, he was successful in persuading his colleagues to focus their work on the solution of human nutrition problems.

In 1939 the Agricultural Research Service of the U.S. Department of Agriculture established the U.S. Plant, Soil and Nutrition Laboratory on the Cornell campus, and Maynard became its first director, serving in this capacity until 1945. During his years of leadership of the Laboratory, the focus was on research which better defined methods of food production to meet the needs of the public.

In 1941 he was appointed the first director of the Graduate School of Nutrition. In this new unit of the university, he collaborated with Dr. Norman Moore, director of health services for students at Cornell, to establish a program in which effects of both environmental and dietary factors on human nutritional requirements could be investigated. For this purpose, Dr. Moore set aside ten beds in the students' inpatient unit to be used for metabolic studies. Further, he provide the medical coverage and expertise which made such studies possible.

Another undertaking, encouraged by Maynard and for which he provided technical support, was a nutrition survey of a nearby township which was later to be involved in follow-up studies. Both the survey and the studies in the metabolic unit were actually carried out by Charlotte Young, one of Maynard's most distinguished proteges.

He also forged relationships in the Department of Poultry Husbandry, where Leo Norris had assumed leadership. In pursuing this research linkage, Maynard was able to harness the knowledge of poultry scientists to focus on meeting human dietary needs with their products and also on using chickens as animal models for human nutritional diseases.

In the years 1940-45, when the war effort necessitated outstanding collaboration to improve the availability of the domestic food supply, Maynard brought the resources of many different units at Cornell together to solve the problems of food preservation by freezing. These units included not only those already involved in nutrition research, such as the College of Agriculture and the College of Home Economics, but also the College of Engineering and the Agricultural Experiment Station in Geneva, New York. He also worked with the major food companies, food freezer manufacturers, and utility companies to bring the food freezing endeavors at Cornell to practical use.

RESEARCH ACCOMPLISHMENTS

Much of Maynard's research concerned effects of diet on lactation. In 1929, he and Clive McCay reported that the removal of lipids from a grain mixture fed to milking cows and the replacement of these lipids with an isocaloric amount of starch resulted in a marked loss of milk yield (1929). Those results were later confirmed by Maynard and colleagues in additional experiments involving cows and goats (1939, 1941). Later he investigated whether these findings of the effect of reducing dietary fat content on lactational yield would apply to animals that were not herbivores. For example, in 1942 he and a colleague examined the effect of reducing dietary fat on lactational performance in rats (1942). They showed that the young from mothers on a high-fat diet grew better than those on a low-fat diet.

PUBLIC SERVICE DURING WORLD WAR II AND IN THE POSTWAR YEARS

Maynard served as commissioner for nutrition of the Emergency Food Commission beginning in 1943. He also served as United States nutrition expert on Inter-Allied Food Missions to London from 1943 to 1945 and to Germany in 1945. Following the war, he was a liaison member of the New York State Food Commission until its termination in 1948.

It was during World War II that he began to shape major national food policies. This he accomplished as a member of the Food and Nutrition Board, which was particularly powerful at that time. He served as chairman of the Board from 1951 until 1955. His first concern with the Board, in which he gave outstanding leadership during the war years, was promotion of the consumption of dairy products as milk rather than as butter or cream. He also initiated the Board's investigations of the uses of dairy by-products such as whey and buttermilk (1943).

After the war, he served as consultant in nutrition to various national and international agencies, including FAO, WHO, and UNICEF.

INTERPRETATION OF IMPORTANT ADVANCES AND RELEVANT ISSUES IN NUTRITION FOR POLICY MAKERS

Maynard's ability to comprehend and utilize complex data on nutrient metabolism derived from both animal and human studies, and to point out the relevance of findings to other nutritionists and to those in the agricultural sector and health care fields, indicates that he had an unusual facility for productive literature analysis outside his own field of research (1947,2). He could write equally clearly for audiences of legislators or others making decisions about foods and feeding of the U.S. population (1944,2). He could use language understood by economists to explain to them the reasons for diet-related nutritional deficiencies existing in the United States in war years, and he suggested practical means for combating such deficiencies (1947,1). He could also point out that differences in the mode of calculation of food energy requirements that existed in the 1940s between the United Kingdom and the United States imposed barriers to the international decision-making process with regard to desirable calorie levels of diets. To overcome this lack of understanding, he wrote an extensive account of the Atwater system for the calculation of the energy value of diets, which was then published as an editorial review in the *Journal of Nutrition.*

Maynard was ahead of his time in pressing for an international system for determining not only the food energy but also the nutrient content of diets. Indeed, as early as

1946 he recognized that such a system would greatly facilitate the work of the United Nations Food and Agriculture Organization in developing food policy for the Far East as well as for other parts of the world.

ENCOURAGEMENT OF FACULTY COLLEAGUES

Examples of his relationships with colleagues provide explanations for his success as the head of a major department of nutrition. He had a facility for demonstrating his confidence in junior faculty such that it improved their performance, but also in making the necessary liaison for them so that they could more easily start new programs. For example, he assisted Professor Charlotte Young in starting a "diet table" for students at Cornell by negotiating a formal arrangement with the director of the University Health Services, Dr. Norman Moore, so that Dr. Young would have an appointment at the clinic and would have responsibility for students with nutritional and metabolic problems. This diet table subsequently served as a model for similar programs set up in other institutions.

As previously indicated, Maynard was able to bring the work of his colleagues to the attention of top officials in government agencies making food policy, as well as to food manufacturers interested in developing new products based on new findings in nutrition. Further, he recognized the genius of those around him and often knew when his colleagues had made fundamental contributions to biology which were not yet understood by others. As an example of the value of this quality, it is important to cite his acclaim of Clive McCay, Maynard's colleague at Cornell for 40 years. In his biographical sketch of McCay, published as a posthumous introduction to McCay's *Notes on the History of Nutrition Research* (1973), Maynard emphasized McCay's extraordinary contributions to the field of geron tology. Effects of diet on aging had not previously been studied, and the fact that McCay's findings in rats of a beneficial effect of food restriction in retarding aging and age-related diseases was, as Maynard pointed out, fundamental.

HONORS

In 1944 Maynard was elected to the National Academy of Sciences in recognition of his distinguished achievement in nutrition research. In 1947 he received the Borden Award from the American Institute of Nutrition, again for his outstanding research in nutrition. In 1952 he received the Award of Distinction from the Grocery Manufacturers of America, and in 1954 the American Institute of Nutrition honored him with the Osborne and Mendel award. This award, which is given in recognition of outstanding basic research accomplishments in the science of nutrition, was given to Maynard also for his other achievements. Indeed, the citation at the time the award was made read ". . .[given for] investigations on biochemical and nutritional aspects of metabolism and lactation and for his contributions as a teacher, administrator and public servant in the field of nutrition" (1978).

In 1957 his extraordinary contributions to the furtherance of home economics were recognized by two awards: he was chosen a National Honorary Member of Omicron Nu (and it was indeed a rare honor for a male to become a member of a women's honor society!). That year he was also elected an honorary member of the American Dietetic Association. In 1958 he received an honorary degree from the University of Rhode Island. In the following year his outstanding contributions to international nutrition were recognized when he was elected to the Order Rodolfo Robles by the Republic of Guatemala and was also pre sented with an honorary degree of doctor of science. In 1960 he became a Fellow of the American Institute of Nutrition.

THE HOME AS AN ENVIRONMENT FOR DISCUSSION OF NUTRITION-RELATED INITIATIVES

Maynard's wife and life companion was Helen Hunt Jackson Maynard. She ensured that their home was always open to her husband's students and to his colleagues, who, even when junior, know that they would feel at ease there. It was in this setting that new research, teaching, and public service endeavors could be planned. Later, when Maynard was retired, it was in his home that he felt comfortable in giving advice to the newer nutrition faculty of his old department. He took the wise approach that it was better to impart the gems of his own experience in this informal setting rather than at faculty meetings.

MAJOR ACHIEVEMENTS IN CHANGING PUBLIC AWARENESS OF NUTRITIONAL ISSUES AND IN PROMOTING THE TEACHING OF NUTRITION

In retrospect, Maynard's major achievements were in advancing the teaching of nutrition and in making nutritional issues relevant to those who plan programs at the international, national, and local levels. He also increased linkages between the fields of biochemistry, animal husbandry, home economics, and human nutrition. Indeed, through his encouragement of women trained in schools of home economics to become distinguished nutritional scientists, and his acknowledged respect for contributions of people working in the area of extension, he brought about a great change in the public image of women as nutritionists and in the attitude of farmers toward extension teaching. He also improved the linkages between in dustry and academe in that he brought to the attention of the food industry the fact that advances in product development could best be made with advice from those in nutrition who were knowledgeable about nutrient requirements and how these requirements could best be met to avoid not only deficiencies but also nutrient excesses and imbalances.

However, to those who knew Leonard Maynard well, he will best be remembered (as he was described in the citation when he was given honorary membership in The American Dietetic Association) as "a tireless public servant concerned with utilization of scientific knowledge of food and nutrition for the promotion of human welfare."

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I. I. Rabi

I. I. RABI

July 29, 1898-January 11, 1988

BY NORMAN F. RAMSEY

SOME SCIENTISTS MAKE their greatest contribution through their own personal research, while others are best remembered for their general wisdom and their influence on others. A few, including Rabi, excel in both respects. His own discoveries, which led to his Nobel Prize in 1944, are of great importance, including the invention of the molecular beam magnetic resonance method, which he and his associates used to measure magnetic moments and electric quadrupole moments of many atomic nuclei and to show the existence of a previously unsuspected tensor force between the neutron and the proton. But Rabi's influence extended far beyond his own laboratory. He was a creative scientist, an innovative statesman, and a philosopher. Proposals first made by Rabi have led to many of the most successful ventures in national and international cooperation in science.

THE EARLY YEARS

Rabi was born on July 29, 1898, in Rymanow, a small town in Galicia, a province in the northeast of the Austro-Hungarian empire. His parents were Orthodox Jews who gave their son the name Israel Isaac Rabi. Soon after Rabi was born, his father moved to New York City and within a few months had earned enough money for his wife and son to join him on the Lower East Side of Manhattan. At home the young Rabi was called "Izzy," but when his mother gave this name at the time he was first enrolled in public school, the name was recorded as Isidor and the error was never corrected. Throughout most of Rabi's professional life he was known as I. I. Rabi except to his closest friends, who called him either Rabi or Rab. The Rabi home was in a Jewish ghetto in the Manhattan slums. Rabi's education began in Hebrew school at age three. His father worked in a sweatshop making women's clothes by day, and at night he operated a small but unsuccessful grocery store that was tended by Rabi's mother during the day.

When Rabi was nine his parents moved to the Brownsville section of Brooklyn, which was crowded but still somewhat rural. Rabi attended New York public schools but did not find school inspiring. Rabi's fascinating childhood and early education have been well described by Jeremy Bernstein[1] and John Rigden.[2] Rabi was an avid reader and gained much of his education and interest in science through books borrowed from the public library. He was for several years particularly interested in books on socialism, science, radio and technology. His first scientific paper, written while he was still in elementary school, was on the design of a radio condenser and was published in Hugo Gernsback's *Modern Electronics.* In 1916, after graduating from the Manual Training High School in Brooklyn, Rabi entered Cornell University, starting in electrical engineering but graduating in the field of chemistry. After three years of uninteresting jobs, he returned to Cornell to do graduate work in chemistry, moving a year later to Columbia University and to physics. At Columbia, Rabi did his doctoral research on magnetic susceptibility with A. P. Wills, but, characteristically, it was on a subject of Rabi's own choosing and with a novel technique which greatly simplified the experiments. The day after he sent in his doctoral thesis, he married Helen Newmark, who remained his lifelong companion and became the mother of his two daughters, Nancy Lictenstein and Margaret Beels.

The Rabis soon went on a traveling fellowship to Europe, where he worked intermittently with Sommerfeld, Heisenberg, Bohr, and Pauli. The Stern-Gerlach experiment demonstrating the reality of space quantizations had earlier sparked Rabi's keen interest in quantum mechanics and so, while working in Hamburg with Pauli, Rabi became a frequent visitor to Stern's molecular beam laboratory. During one of these visits Rabi suggested a new form of deflecting magnetic field; Stern in characteristic fashion invited Rabi to work on it in his laboratory, and Rabi in an equally characteristic fashion accepted. Rabi's work in Stern's laboratory was decisive in turning his interest toward molecular beam research.

RESEARCH AND TEACHING AT COLUMBIA

Rabi returned from Europe to join the faculty at Columbia University and to begin atomic beam research in his own laboratory. In 1931 he and Gregory Breit developed the important Breit-Rabi formula, which showed how the magnetic energy of an atom and its effective magnetic moment vary with the strength of the external magnetic field. These changes occur because the atomic configuration varies from the electron angular momentum being primarily coupled to the nucleus at a low external field to being principally coupled to the external magnetic field at a high field.

Utilizing the Breit-Rabi formula and an atomic beam apparatus which deflected the atomic magnetic moments with inhomogeneous magnetic fields, Rabi, V. Cohen, and others[3] were able to determine the strengths of the electron nucleus interaction and the magnitudes of nuclear spins and magnetic moments. Rabi further improved the precision of the measurements by noting from the Breit-Rabi formula that the effective magnetic moments are zero at certain magnetic fields, which give marked identifiable rises in the intensity of the undeflected atoms passing through an inhomogeneous field. Since the strengths of the fields giving zero moments depended on the hyperfine interactions and nuclear spins, Rabi, Fox, and other students and associates determined a number of hyperfine interactions by measuring the zero moment magnetic fields. Although the zero moment method did not work for atoms with nuclear spin 1/2, Rabi devised an alternative refocusing technique which did.

Rabi also showed that the molecular beam deflection method could be adapted to measurements of the signs of nuclear magnetic moments by determining which transitions occurred when atoms went through a region of space in which the directions of the magnetic fields were successively reversed.[3]

Rabi developed the theory of such transitions in his important paper entitled "Space Quantization in a Gyrating Magnetic Field" (1937). In this paper Rabi assumed for simplicity, that the applied field changed its direction ("gyrated") at a fixed frequency. As a result, this paper provides the theoretical basis for all subsequent magnetic resonance experiments.

Rabi initially applied his theory to fields which changed only in space rather than in time. A few months after the publication of that paper, following a visit by C. J. Gorter, Rabi directed the major efforts of his laboratory toward the development of the molecular beam magnetic resonance method with the magnetic fields oscillating in time. As shown in Figure 1, a molecular beam was deflected by one inhomogeneous magnetic field and refocused by a similar field. In passing between the two fields, the molecules were subjected to a weak oscillatory magnetic field at frequency v. When v equaled the Bohr frequency $v_0 = (W_f - W_i)/h$, transitions could take place with a consequent refocusing failure and a reduction in beam intensity. By measuring the beam intensity as a function of frequency, one could thereby determine the spacing of the molecular energy levels.

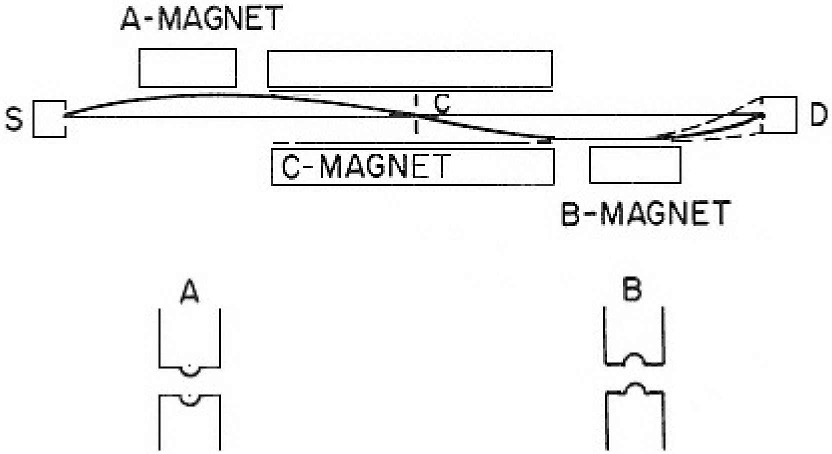

FIGURE 1

The first successful molecular beam magnetic resonance experiment was that of Rabi, S. Millman, P. Kusch, and J. R. Zacharias in 1938, which determined the nuclear magnetic movement of Li. Soon thereafter, J. M. B. Kellogg, Rabi, N. F. Ramsey, and Zacharias applied the method to molecular hydrogen and discovered a multiplicity of resonance lines, whose separation arose from the magnetic interactions of the nuclear moments with each other and with the magnetic field caused by the rotation of the molecule. They found that the separations of the resonances for D_2 were much greater than could be attributed to such magnetic interactions but could be fitted by assuming that the deuteron had a nuclear electric quadrupole moment, i.e., had an ellipsoidal shape like an American football rather than a spherical shape; an ellipsoidal shape would result from the existence of a previously unsuspected tensor force between the neutron and proton.

In subsequent years Rabi, with his students and associates, successfully applied the beam resonance method to single atoms as well as to polyatomic molecules and in such experiments measured numerous nuclear spins, nuclear and atomic magnetic moments, atomic hyperfine interactions, and nuclear quadrupole moments. The Rabi laboratory at Columbia was a major research center providing new physics data, new ideas, and outstanding and creative young scientists who later went on to establish their own research programs in a variety of fields. Although most of Rabi's graduate students were experimentalists, some, including Julian Schwinger, were theorists.

WORLD WAR II

World War II interrupted the molecular beam research at Columbia University from 1940 to 1945, when Rabi was actively involved with the development of microwave radar at MIT. There Rabi was a major source of new research ideas and an influential advisor to his research associates. He headed the magnetron group at the MIT Radiation Laboratory and later became deputy director. He was particularly active in developing shorter wavelengths, first from 10 centimeters to 3 centimeters at MIT; later he initiated the establishment of the Columbia Radiation Laboratory, which pioneered in the development of 1-centimeter-wavelength radar.

Rabi originated the plans for the writing of the twenty-eight-volume Radiation Laboratory Series, which for many years was the major reference for microwave and electronic technology. Rabi also served as an influential consultant to J. Robert Oppenheimer, director of the Los Alamos nuclear research laboratory.

Shortly before the end of the war, in his 1945 Richtmeyer Lecture, Rabi discussed the possible use of an atomic beam magnetic resonance apparatus as the control element of an accurate clock. The *New York Times* report on this lecture is the first published account of atomic clocks, which have now become so accurate that they are the basis of the international definition of the second.

RETURN TO COLUMBIA

Following the end of the war, Rabi, Ramsey, and Kusch returned to Columbia to reestablish the molecular beam laboratory. Nierenberg and Ramsey rebuilt an old apparatus and measured the radiofrequency spectra of a number of alkali halides. Rabi, with his students J. Nafe and E. Nelson, successfully applied the magnetic resonance method to atomic hydrogen and discovered that the hyperfine separation due to the interaction of the magnetic moment of the proton with the electron was slightly different from the theoretical expectation from the Dirac quantum theory; this result was the first indication that the magnetic moment of the electron was different from the expected Dirac value, an observation later confirmed at Columbia by Kusch's direct measurements of the electron magnetic moment. These observed anomalies were the principal stimuli to the development of relativistic quantum electrodynamics, the first successful quantum field theory.

Another important molecular beam development was the adaptation by Rabi and his student H. Hughes of the resonance method to electric deflecting and oscillating fields. With subsequent improvements by Rabi, J. Trischka,

V. Hughes, and others,[3] the electric resonance method has been used for many precise measurements of the spindependent internal interactions within molecules in specific rotational states.

Most of Rabi's experiments were with molecular beams, but he also participated with W. Havens and J. Rainwater in a neutron-electron scattering experiment which provided the first experimental evidence for the neutron-electron interaction.

Rabi's classroom lectures were often chaotic, but he was a stimulating teacher who made his students think. He was an inspiring supervisor of Ph.D. students whose research experiments were innovative and fundamental. Rabi and his wife Helen were personally very helpful to his students and associates, most of whom remained lifelong friends. Rabi gave his students freedom and independence while maintaining high standards for the research. The influence of Rabi has been extended by his students, such as Zacharias, Ramsey, Nierenberg, Schwinger, and Hughes, who in turn have had a number of excellent students; many of the scientists now active in physics can trace their scientific ancestry back to Rabi or his former students.

Rabi not only contributed innovative new resonance techniques to the field of atomic physics but imparted to his students and associates high standards for both the quality and the interest of the research. His lifelong interest in scientific taste and standards is illustrated by his last publication, written just a few weeks before he died. In paying tribute to Wolfgang Pauli and Otto Stern, Rabi wrote:

> From Stern and Pauli I learned what physics should be. For me it was not a matter of more knowledge. I learned a lot of physics as a graduate student. Rather it was the development of taste and insight; it was the development of standards to guide research, a feeling for what was good and what is not

> so good. Stern had this quality of taste in physics and he had it to the highest degree. As far as I know, Stern never devoted himself to a minor question.

For a number of years Rabi was a highly effective chairman of the Columbia Physics Department; his critical and stimulating presence was clearly responsible for much of that department's greatness.

Although Rabi was not directly involved in physics experiments after 1960, he retained an active and critical interest in the field and was a regular and stimulating participant in atomic physics meetings and seminars until a few months before he died from cancer on January 11, 1988, at the age of eighty-nine.

SCIENTIST STATESMAN

Rabi's influence extended far beyond his own and his students' research through his membership on important committees, his presidency of the American Physical Society, his many public lectures, and his innovative proposals for new means of cooperation among institutions and nations. Discussions late in 1945 between Rabi and Oppenheimer led to the Acheson-Lillienthal-Baruch plan proposed by the United States for the international control of atomic energy. One of Rabi's greatest disappointments was that this forward-looking plan, after initial favorable consideration, was never adopted by the United Nations.

Rabi was a member of the AEC General Advisory Committee and joined with Enrico Fermi in writing a strong memorandum supporting the controversial GAC recommendation against a crash program for the development of a hydrogen bomb. Later, Rabi became chairman of the GAC and an eloquent defender of Oppenheimer in the AEC hearings that culminated in the removal of Oppenheimer's security clearance.

Rabi and Ramsey initiated the first proposals for Brookhaven National Laboratory and were early strong proponents for the construction of the Cosmotron. Later, with the model of Brookhaven in mind, Rabi pioneered in advocating the European collaboration that led to CERN. Rabi was the initiator of the International Conferences on the Peaceful Uses of Atomic Energy and a principal speaker at them.

Through his friendship with President Eisenhower, Rabi was largely responsible for the establishment in 1957 of the President's Science Advisory Committee and the Office of Special Assistant to the President for Science and Technology. For many years Rabi was the U.S. representative to the NATO Science Committee, where he effectively advocated the establishment of the highly successful NATO supported Summer Schools and Fellowship program.

Rabi was the recipient of many honorary degrees and awards, including election to the National Academy of Sciences in 1940, the Japan Academy in 1950, and the French Legion of Honor in 1956. He received the 1944 Nobel Prize, the 1948 U.S. Medal for Merit, Britain's 1948 King's Medal, the 1964 Priestley Memorial Award, the 1967 Niels Bohr International Gold Medal, the 1967 Atoms for Peace Award, the 1982 Oersted Medal of the American Association of Physics Teachers, the 1985 Roosevelt Four Freedoms Medal, the 1985 Public Welfare Medal of the National Academy of Sciences, and the 1986 Vannevar Bush Award of the National Science Foundation.

Rabi's carefully prepared public lectures were stimulating and presented fresh points of view, as illustrated by his words at the Fourth International Conference on the Peaceful Uses of Atomic Energy:

> Real peace is more than the absence of violent war. To fulfill human expectations peace must be a condition which permits the release of the

latent creative energies of all the people to the end of enhancing and elevating the quality of human life on this globe.

NOTES

[1] Jeremy Bernstein, "Profiles: Physicist," *The New Yorker* (October 13 and 20, 1975).

[2] J. Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, 1987).

[3] N. F. Ramsey, *Molecular Beams* (Oxford: Oxford University Press, 1956, 1985).

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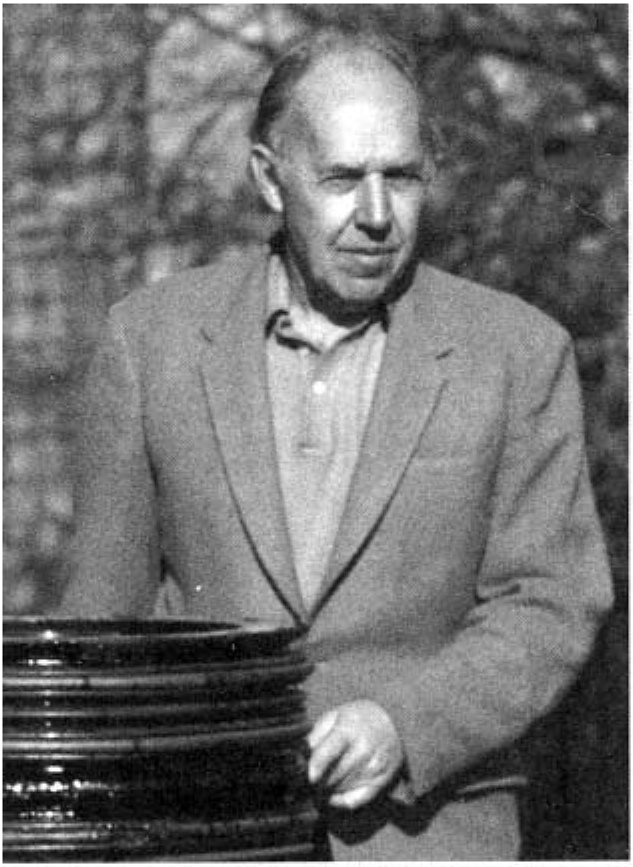

Richard Brooke Roberts

RICHARD BROOKE ROBERTS

December 7, 1910-April 4, 1980

BY ROY J. BRITTEN

Dr. Richard Brooke Roberts spent most of his career in the biophysics group at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. Dick contributed importantly to many scientific advances in this period in microbiology, the beginnings of molecular biology, and study of the brain. One high point was the proof (with Kenneth McQuillen and me) that in *Escherichia coli,* protein synthesis occurred on ribosomes. Dick also named the ribosome. Dick started out as a nuclear physicist and among several discoveries showed that delayed neutrons were emitted in uranium fission (1939,5). This discovery was of great practical consequence because delayed neutrons slow the responses in a pile enough to permit control by mechanical movement of cadmium rods. This made fission piles practical for all of their uses in weapons making as well as power. As a result, Dick was involved in early planning of what became the Manhattan Project, although he decided that it was too long range a project for the emergency. He chose to work on more practical weapons and showed that vacuum tubes would survive being fired from a gun; he also developed a radio-controlled proximity fuze which made antiaircraft guns very effective (the first "smart" missile), forever changing the course of war. He earned the Congressional Medal of Honor.

Dick's career was marked by an independence of mind and a very practical style. He was a professional physicist and biologist with few equals and a severe and irreverent critic of the illogical and imperfect. Nevertheless, his attitude was close to that of amateur in the best sense. He had a love of what he did and a noncompetitive desire to help everyone else achieve "good" science. Perhaps the greatest of his contributions were the ideas and cooperation he gave to others.

FAMILY

Dick Roberts was fortunate in his forebears, many of whom must have had some of the same definite, practical, and inventive cast of character. In the early days of oil in Pennsylvania, his grandfather's brother, Colonel Edward A. L. Roberts, invented and patented shooting explosives in an oil well to improve the flow. He and his brother formed the Roberts Torpedo Company, and the time was ripe since the wells were beginning to clog. The family fortune was helped by the $200-a-well charge and by many successful suits against infringers. When they heard of nitroglycerin, they immediately started manufacturing it in 100-barrel lots in old barns for well shooting. Dick wrote a document[1] (AB) which has been useful for this memoir, and I quote from the first paragraph.

> I have just given 9 volumes of the Academy Biographical Memoirs to the library. The sight of these volumes always provokes the horrible thought that someday someone will have to prepare one of them for me. The thought of being dead is not horrible at all. I have had a very fine serving of life and would not feel cheated if I went tomorrow. However, writing such a piece is not easy and I have often wondered how I could handle such an assignment for somebody—.... And so it seems almost mandatory

> to write out quite a bit myself so that the poor devil will only have to cut out the meanderings and leave the hard core.

As ever, I continue to appreciate Dick's help.

The next paragraph of AB, entitled "Genetics," starts: "Since I am convinced that the genetic endowment is by far the most important factor in an individual I will begin by recording a few items about my ancestors." There was a Roberts on General Washington's staff and there was Lucius Quintus Cincinnatus Roberts who traveled to China and set up trade. L. Q. C.'s father-in-law (Mr. van Braam) gave the set of china to Martha Washington which is now at Mount Vernon. Dick's grandfather, Walter B. Roberts (brother of Edward A. L.), was a businessman and state senator; the brothers were dentists who invented and sold dental equipment, and there was a banker in the family as well. His grandmother on the Roberts side was Emily Titus, and Titusville was where Dick was born. The family on his mother's side was also involved successfully in Pennsylvania oil, having started a refinery, sold to Standard Oil in 1870. In AB, Dick states, "For a memoir probably all this would boil down to one sentence like[:] His ancestors were active, intelligent, well educated etc. Active is the key word for LQC Roberts . . . for W.B. Roberts.... Intelligent is the key word for the Titus family and my father."

CHILDHOOD AND EDUCATION

Dick was born on December 7, 1910, in Titusville, Pennsylvania, the third child with two much older (perhaps twelve and fifteen years) brothers. The family moved to Princeton in 1916 and to New York in 1921 but continued to summer every year in Titusville into the 1930s. His schooling was that of well-to-do families of the time, never setting foot in public school—Miss Fine's School in Princeton, Lawrence Smith School in New York, St. Paul's School, and Princeton. His interest in science and math started at St. Paul's School, and the reasons given in AB were that he enjoyed them and was not so good at sports (though his love of golf started there) and that the supervision was not too strong. He and a friend (Charlie Thayer) forced the school to give them a calculus class for two, which was rare before college in that era. A quote from AB: "I liked to use the calculus for physics problems and baffle the physics teacher who didn't know calculus."

In AB, Dick raises the question of why none of the other very bright youngsters in the school went into science and answers it as follows: "Probably it was because most were very rich. The class list read like the NYSE. Our family was always very comfortably well-off but I felt like a pauper at SPS. For example I was the only one of the 6th form who did not have a raccoon coat. The free time to play in the lab. was particularly valuable. Too much supervision may be deadly."

For Dick, Princeton was a great success as he grew up, but there is no comment in AB that suggests that great interests in physics were formed or that the classes were particularly good. Apparently, in his senior year he did drift down to the basement at Palmer lab, where research was going on, and got hooked. There seemed no possible choice but to do graduate work at Princeton and go for a Ph.D. Of course, his older brother Walter, who was an RCA radio engineer (with valuable circuits named after him), had been living in Princeton for many years, having graduated from Princeton during the First World War and returned for a Ph.D. (Later he often worked for RCA in a lab attached to his Princeton house.) One quote from AB about mathematics is interesting:

> I think math is one subject that has to be learned in the classroom. The assignments are essential. Who has the will power to go through it without some compulsion. I have frequently tried to go through the book on group theory but never got beyond the first chapter. History, economics, etc. even biochemistry were not hard to pick up but not math. And it's not that I was dumb at it. I never had worse than 1st group. But my math stopped with the differential equations and complex functions or whatever it was that junior year. Possibly this was because math was an applied subject for me. I liked to solve problems with it but did not care much for elegant proofs. Heaviside's approach appealed to me (the man who used operators without formal proof...).

Dick did graduate first in the class shared with a St. Paul's School comrade (Lew Van Duzen). For those who don't know, the quality of students is high at Princeton and the competition is strong.

There was never any doubt in his mind that he was as intelligent as anyone around. Meeting him, one would quickly recognize that this was a man of importance, yet there was a complete lack of pomposity. As with all such persons, he made demands on the world around him and instantly recognized a bore. His circle of friends was large, and the parties at Linnean Avenue, with barrels of oysters and steamship roasts, are to be remembered. It was hard for him to take much of anything seriously except for a prime list: science, family, golf, new weapons, the fate of humanity, and money. His open-mindedness was remarkable. As a minor example, ESP caught his eye as a student at Princeton and remained a lifelong interest, with total objectivity as far as I could tell. But in writing the history of genetics, some have tried to cut him down for this. Dick's informal approach to mathematics was just right for the main part of his career in biophysics, where a major contribution was an analytical quantitative approach. His skill was in crossing the line between a problem or sets of ob servations and the mathematical formulation. An important part of his time at Princeton was spent in the ROTC, and he became a first-rate artillery officer.

About the start of graduate school, AB states: "Arriving at the beginning of the fall term 1932 I was told that I was assigned to work with Prof. Ladenburg in Nuclear Physics. My reaction was fine, but what is nuclear physics? .... It had not been included in the undergraduate work. Thus began 4 years of battle to get the degree." There is detailed reminiscence about troubles with high-voltage equipment (Cockcroft-Walton) and ion sources. This was ultimately all resolved, and his thesis was on deuteron-deuteron reactions. By virtue of Ladenburg's extensive absences and a lot of independence, he had become an experimental nuclear physicist. There is, of course, more to it, and I quote AB again: "Somehow the theorists did not resonate with the experimental people. I picked up more from Ed Salant while working with him than any other time. ... . 32-34 were big years in nuclear physics. Artificial disintegrations, the neutron, the positron and induced radioactivity. And the deuteron. At Princeton we had enough equipment to follow along closely but not enough insight to contribute anything. Ladenburg had 1 curie of radium and so he could add a little beryllium and show neutrons within a week after the announcement arrived." It may be worth remembering the fate of that curie which Ladenburg was assembling into a sealed brass cylinder, quoting AB again: "Just before the solder hardened the water pressure (or rather steam pressure) blew the top and his whole curie. He had to take treatment to reduce his radium dose, the whole chemistry stockroom . . . where the explosion occurred ... was sealed off.. ." It was still sealed off when I arrived as a graduate student thirteen years later.

PREWAR YEARS AT THE DEPARTMENT OF TERRESTRIAL MAGNETISM OF THE CARNEGIE INSTITUTION OF WASHINGTON

The thesis was finished in late 1936, and on a visit to his wife Adeline's family in Washington he drove up to see Merle Tuve and told him of his plan of measurements for a scheduled year at Cavendish; Merle said, "Why not do them here? We have better equipment." Dick accepted the temporary arrangement, and later a fellowship was squeezed out of the Carnegie Institution of Washington (CIW) administration by Merle. So easily began a lifetime. The early work was on scattering experiments (protons, deuterons, and helium), which was hard fundamental work with N. P. Heydenburg. There followed some lithium work with Rumbaugh, and "the main event scientifically of 1938 was pinning down the Be7. This was really very satisfying as isotopes were being discovered right and left and this was my first (and last)" (AB).

The Department of Terrestrial Magnetism (DTM) pressurized Van de Graaf generator split its first atoms on December 23, 1938. A quote from a January 1939 letter to Dick's father is included in AB:

> We have had a very exciting week in Physics. The annual theoretical conference started Thursday with an announcement by Bohr that Hahn in Germany had discovered a radioactive isotope of Barium as a product of bombarding uranium with neutrons.... Fermi also discussed the reaction and described an obvious experiment to test the theory. The remarkable thing is that this reaction results in 200 million volts of energy liberated and brings back the possibility of atomic power. Hafstad and I left the meeting as soon as Fermi finished to go to the lab to try the reaction. We had some trouble with a leak in the tube so it wasn't till Saturday afternoon that Meyers and I finally made the test. We had Uranium in our ionization chamber and bombarded with neutrons. We soon observed tremendous pulses corresponding to very large energy release. [There follows a drawing.] I told Tuve after supper and he immediately called Bohr and Fermi

> and they came out Saturday night and we ran the test again for them and they were immediately convinced. What we did of course is of no particular credit to us but it is nice to be the first to observe the actual splitting of a uranium atom.

Another quote from AB to keep the history exactly straight: "We later found out that the Columbia group had done the same on Wednesday and Johns Hopkins on Thursday. Frisch was two weeks ahead."

One witness that Saturday evening, Enrico Fermi, had done the same experiment in 1936, using a radium beryllium source of neutrons and had (for a good technical reason) placed a very thin aluminum foil between the uranium and the ionization chamber, which stopped all of the fission fragments. But for that foil the Italians, the Germans, and possibly the world would have known about atomic power and explosions much longer before the Second World War.

Continuing to quote valuable history from AB:

> The following weeks were also hectic. The key to atomic power was the neutron emission that might accompany fission. Since it was technically difficult to observe the relatively few additional neutrons released during bombardment we looked for (and found) neutrons emitted after the beam was turned off.... In March Adeline and I went to Florida for a few days on the beach with my brother Walter . . . he recorded all our long discussions about uranium fission, how to make a pile, and particularly arrangements to control the pile as the delay in the neutron emission gave ample time for control. On his return to New York in May he presented his usual stack of applications for circuit patents. He then said here are a few along a different line and handed over the applications for the pile and its control. His superiors (?) decided RCA was not interested. I had no interest in the patent side as CIW had a policy of no patents ....
>
> After Florida I continued work with Salant on neutron scattering but my main efforts went into measuring cross-section for fission for neutrons of various energies. These were essential in calculating whether a chain reaction would run. By summer I wrote a long paper on the possibility of a

> chain reaction based on these cross sections but it was never published because of an agreement to keep such data out of the journals. I did however write one article which I believe is the first serious discussion of the possibility of atomic power for the Journal of Applied Physics. It concluded "The day of free atomic power is probably not in sight."
>
> One other historical note on fission. Tuve was scheduled to be present at the first meeting between the scientists and the government following the letters from Einstein and Sachs to Roosevelt. For some reason he could not attend and sent me along in his place. The meeting decided to appropriate $6000—an enormous sum in those days—for graphite so Fermi could estimate the possibilities of a graphite moderated pile. Also I remember an Army colonel saying a 20 KT bomb wouldn't do much—he had survived such a explosion (at Halifax?). In short he was not impressed.

The next phase is 1939 to 1940, when Dick mostly helped in building the cyclotron at DTM, which was supposed to be a supplier of isotopes for half the days and for nuclear physics the other half. He designed the RF system and much else. In this period, many contacts were made that set up the future biophysics group at DTM, including Philip Abelson and Dean Cowie. The biophysics work had actually started much earlier, including a study with Louis Flexner on the transfer of ^{24}Na from rat mother to embryo. The cyclotron did useful work during the war but, of course, it was short lived as a supplier of isotopes. From AB: "At the end of the war the cyc supplied isotopes around the world for the period before the AEC was ready to sell them.... Abelson and I used it for my one post-war physics experiment which showed that the neutron (s) from D plus light elements were mainly forward." It is worth noting that this was the first evidence that the Bohr compound nucleus did not apply everywhere and it motivated the theory of stripping reactions. AB continues: "In retrospect the cyclotron was a fine machine that came at the wrong time." The permanent benefit was the cyclotron building which housed the biophysics group for nearly thirty years.

WARTIME

In AB, Dick states that the idea for a proximity fuze came from England by way of the Tizard mission in the summer of 1940. After development by a group Dick led (that grew to 1,000 people), the fuze was very effective against German V1 bomb attacks on London, as well as for many other uses. One afternoon in 1940, Merle Tuve had asked Dick to find out whether a glass radio tube could be designed to stand 22,000 g and that evening Dick ran an 8,000 g test on an old 38 tube which survived. In the morning, a 954 acorn tube survived a 20,000 g test attached to a lead sphere dropped from the top of the cyclotron building, and "Section T" was off to a quick start. Merle Tuve was boss and put Dick in charge of the radio fuze, while others got projects such as photoelectric and acoustic. In two years, production was going and tests of improvements were being run, a remarkable record. Just one quote for atmosphere from AB: "The next problem was to get some action out of Crosley. They told us that two weeks were required to put a change into effect. New blue prints (of resistors) were required and seven approvals plus signatures. On the assembly line I found that all that was needed was a different basket of components to introduce the change."

Dick with Section T under Merle's direction (which became a part of the Applied Physics Laboratory of Johns Hopkins University) developed many things such as radar jammers and fire control. Later he went on to guided missiles and to ramjets. This period was very important to Dick's life, and his contributions were first rate but will not be described in detail since they involve military engineering and not science. He kept in touch, and his commitment deepened during the Korean War. In review, it is appropriate to say that the proximity fuze made a difference in the survival of England by protection against German aircraft and V1 bomb attack. It was also a factor in turning the Battle of the Bulge, as well as in much naval activity.

PEACE, ARMS CONTROL, AND SOME POLITICS

This section is not sequential in time but attempts to encapsulate what was a main force in Dick's life. Many scientists, realizing what a disaster modern warfare could become and somewhat guilty over their part in the creation of the weapons, have attempted to forestall the disaster and improve the chances for disarmament. It is fair to say that Dick met with some success (much more than most of us), through writings, his military contacts, and his role in a science advisory group for the Democratic Party (the Science and Technology Committee of the Democratic Advisory Council). The committee initiated something called the National Peace Agency, which ultimately evolved into the Arms Control Agency, in which Dick played a role in its early days (1963). From AB:

> For a year previous to this time there had been a voluntary unilateral ban on testing while the negotiations were in progress. Then I heard one day from friends in the Pentagon that Eisenhower had decided to resume the tests and had given orders for the preparations to begin. This seemed a bad step backwards to me so I called all the members and received unanimous agreement that we should issue a quick statement. This was prepared—I think by the Washington group of McClure, Lapp and me—and then I took it to the Council which was meeting in New York. It was approved and issued by the Council as one of the main items from their meeting. I don't remember the exact words but it was to the effect that resumption of testing at that time would be terrible. Evidently this unexpected blast shook Eisenhower because he countermanded the order to resume testing.
>
> This was very satisfying as I felt (and still do feel) that my efforts in

> calling our committee, going to the council, etc., had changed history a slight bit in the right direction.

He was a strong proponent of U.S. and U.S.S.R. submarines as the only deliverers of nuclear missiles as the ultimate safe strategic deterrent. The logic, at least, is still sound. From AB:

> The idea was to have two Polaris fleets. Ours would be stationed in the Caribbean and the Russian one behind Japan. Both would be out of range but could move out to attack after a week's cruise. Both sides would want the other to know the fleets were at home and out of range so both sides would allow the other to observe and verify that there was no danger. But if necessary they could move out. It was like the solution to a double dummy bridge problem. Levering Smith, who was then in command of the Polaris fleet, said it would suit him. We published it but of course nothing happened. Implementing a good policy is far more difficult than inventing it.

THE BIOPHYSICS GROUP AT THE DEPARTMENT OF TERRESTRIAL MAGNETISM

This group grew out of a wartime cyclotron-oriented biophysics group, and initially Phil Abelson was group leader. It was really created when Dick Roberts joined Abelson and Dean Cowie after convincing Merle Tuve (director of DTM) and Vannevar Bush (president of CIW) that a permanent group might do good science. Soon it was joined by Ellis Bolton and later by me. For a long period all of us were jointly listed as the authors of all of the work in the annual reports. The group members were all very cooperative, and the research interests ran in parallel for many years as a result of continuous discussion, but it never operated as a research team with appointed jobs. Dick had chosen to give up personal power when he turned down a good offer from the Kellex Corporation and left the Ap plied Physics Laboratory. He could afford to just be interested in science. That attitude and the opportunities offered by CIW mostly suppressed personal ambition. The five of us formed the core of the group for the longest period, though many others, including Brian McCarthy, Dave Kohne, Bill Hoyer, Nancy Rice, Tom Bonner, and many important fellows and visitors, were part of it.

Dick Roberts was, as I remember it, responsible for the "philosophy" or underlying set of attitudes which set the strategy. One was that basically simple processes were responsible for biological complexity and another was that (anathema to many biologists) a physicist could step in and devise ways to isolate these processes. Whatever one may think about their validity, these are fruitful attitudes and have changed the face of biological knowledge. This summary is best divided into three periods corresponding to two volumes which record the research published in 1955 and 1964 and then the succeeding decade or so. For a fuller history, see page 656 of the second of these volumes or page 172 of *CIW Year Book 74* (1975).

STUDIES OF BIOSYNTHESIS IN E. COLI

Biophysics was redefined as "quantitative research in biology carried out by investigators trained in physics" *(CIW Year Book 50* [1951]), in preference to the customary meaning of the time which was instrument development in support of biological research or medicine. After Dick took the phage course, the attitude developed that the host *E. coli* was more interesting and should be studied during exponential growth so that "normal" pathways of synthesis and processes could be examined. The early interest was in transport and permeability and later moved to biochemical pathways. They both represented opportunities for new insight deriving from radioactive tracers. It is hard, even for me, to sympathetically reimagine a period in which it was not yet proved that DNA stored the genetic information and in which the only useful radioactively labeled materials were targets bombarded in the cyclotron and from Oak Ridge, ^{14}C as barium carbonate, ^{32}P as orthophosphate, and ^{3}H as hydrogen gas or water. Anyone interested should get hold of this volume (1955,3), which became known as the *E. coli* bible. Dick initiated this writing project and was the driving force, though it cost us all a year. I met someone only this year at a meeting in Cambridge who took the trouble to come up and say how much it had helped him in the lab. While the term "feedback inhibition" was devised by others, its existence was proved by the work of the group during this period. A high point of Dick's contributions might be the quantitative analysis and proof that the Krebs cycle (previously recognized as a component of carbohydrate metabolism) was important in the synthetic activities of *E. coli.*

MACROMOLECULAR BIOSYNTHESIS

The next period is reflected in a book which Dick put together (1964,1) including all of the reprints of the group for the period and selected annual report sections with comments interpreting their current significance. It reports the transition from investigation of pathways of synthesis of small molecules to studies of ribosomes. In this period, Dick became interested in the code and particularly studied a doublet code, which reflected what we would now refer to as degeneracy, but the influence of this work was minor. The application of the sucrose density gradient to macromolecules came out of Dick's attempts to use layers of sucrose to fractionate ribosomes for kinetic studies. The measurements (with Kenneth McQuillen) of the presence on ribosomes of nascent protein were driven by

Dick's enthusiasm to manually carry out experiments fast enough to catch the process where the turnover time was only a few seconds. The result was the proof that ribosomes rather than some other component of the microsome fraction were responsible for protein synthesis.

THE BRAIN

Over the years, Dick's interests moved to the intractable subject of the operation of the brain and the mechanisms of memory, and much of the work of this period was in cooperation with Louis Flexner. The rest of the group never became closely involved. The experiment Dick and Louie initiated was an attempt to determine whether protein synthesis was involved in the establishment of long-term memory. Mice were trained, and their brains were injected with puromycin. They obtained positive conclusions but later withdrew this interpretation, since memory was restored by intracerebral injections of saline. This was pioneering work which I am assured by experts is now carried out successfully. Dick states in 1974 *(CIW Year Book 75,* p. 178): "Puromycin blockage appeared to be caused by the formation of puromycin-peptides, which adsorbed to receptor sites and blocked certain synapses. Presumably these are receptors for catecholamines as the puromycin has a structural resemblance to these compounds. Thus, experiments designed to demonstrate a role for protein synthesis in memory formation ended in implicating the catecholamines. . . . Roughly 20 papers have been published. .."

In the same review of the history of the group, Dick stated: "We are pleased to have participated in this exciting period in the development of biology. We believe that we did make significant contributions and that, since some of us will carry on in different places, our history is not complete. We are especially pleased to note the contributions being made by 22 Fellows who received a part of their training with us."

During graduate school, Dick married Adeline Furness (November 1935), and their children, Dick junior and Julie, were born during the early days at DTM. After a divorce, Dick married Irena Zuzanna Eiger (December 1948) and they had a son, Tommie. After Irena died, Dick married Josephine Taggart Rice (January 1967). I have known all of Dick's children, and though they did not always agree with their father and have not gone into science, they are excellent human beings and reflect credit on Dick. On Saturday, April 4, 1980, Dick was playing golf, his lifetime favorite game, and collapsed of a heart attack, dying as he might have preferred.

NOTE

[1] The document is called "Autobiography of Richard Brooke Roberts," written from 1977 to 1979. It has sections called "Genetics" (describing his family), "Chronology," etc., and I will quote freely from it, referring to it as "AB." It is typed by Dick's own hand, mostly on paper headed "Quarterly Review of Biophysics." I have asked the Academy to include the whole in their files because it creates a sense of presence in many circumstances that I will not be able to quote. There is a freedom of style to it that reflects his sense of fun and is responsible for some of the looseness in this memoir. It is not a balanced autobiography (35 pages) but is the sort of thing historians may value for certain gems.

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Kenneth D. Roeder

KENNETH DAVID ROEDER

March 9, 1908-September 28, 1979

BY V. G. DETHIER

Kenneth David Roeder was born in Richmond, a suburb of London, England, on March 9, 1908. His father, Carl David Roeder, grew up in Germany and was of Scots and German parentage; his mother, Grace (Phillips) Roeder, spent her childhood in Australia, her parents having migrated there from England.

His first school was Bruce Payne School in Bishops Stortford, Essex, where his education was strict and formal. From there he advanced to Bembridge School, Isle of Wight. The headmaster, Mr. Howard Whitehouse, who was a Ruskin enthusiast, active in the Liberal Party, and interested in American education, made this school a happy compromise between British and American systems and awakened Roeder's interest in ideas and pleasure in working with his hands. He leaned toward physics and chemistry through the enthusiasm of a science teacher, Mr. E. J. Baggaley. In 1926 he entered St. John's College, Cambridge University, and received the degrees of B.A. (1929) and M.A. (1933). He was awarded an honorary doctor of science from Tufts University in 1952.

As a child he had become "imprinted" on insects, and at the age of ten, learning from his father the joys of collecting insects and surgaring for moths, he amassed a large collection of British butterflies and moths. This zoological bent followed him to Cambridge, where he was trained in classical zoology. His special interest was insect metamorphosis. At this time there was little interaction between the departments of zoology and physiology; nevertheless, he became interested in what was then called experimental zoology. He took Part II of the Natural Science Tripos under the tutelage of James Gray and C. M. A. Pantin. He also received superb instruction in entomology and invertebrate zoology from L. E. S. Eastham, L. A. Borradaile, F. A. Potts, and Stanley Gardener.

In 1930 he was appointed teaching assistant at the University of Toronto. In 1931, in the nadir of the Great Depression, he returned to Europe and married Sonja von Cancrin of the Weiberhof, a farm in Bavaria, Germany. He then moved to Tufts College as instructor in biology. He became successively professor of physiology (1951), chairman of the Department of Biology (1959-64), research professor on a National Institutes of Health Career Award (1964-75), and professor emeritus (1976).

In the summer of 1932, while he was enrolled in the physiology course at the Woods Hole Marine Biological Laboratory, his interest in invertebrate nervous systems was stimulated by C. Ladd Prosser, the instructor in the course. The most significant outcome of a laboratory demonstration involving ablation on the brains of dogfish, worms, and lobsters was, from Roeder's point of view, the augmentation of certain kinds of behavior that followed reduction in the mass of central tissue. He decided to investigate this phenomenon with praying mantids as experimental animals. While at Toronto he had mailed fifty cents in response to an advertisement for eggs and had become intrigued with the behavior of these attractive insects. Now, building upon what he had learned from Prosser, he began to investigate the consequences of various brain lesions on behavior. He found that continuous copulatory activity in males and locomotion in both sexes could be released by removing specific parts of the brain. Control of behavior seemed to be exercised mainly through inhibition of inappropriate patterns. Roeder was invited to describe this work at the Cambridge Entomological Club at Harvard at this time when entomology in America concerned itself almost exclusively with taxonomy and natural history. The potential of insects as "guinea pigs" for solving basic physiological questions was little appreciated.

This was also a period in physiology when the work of Charles Sherrington and Jacques Loeb was generally interpreted dogmatically. Animals were conceived of as reflex input-output machines. On the other hand, E. D. Adrian had already described spontaneous electrical activity in the isolated nerve cords of caterpillars, and Prosser was finding the same phenomenon in isolated crayfish ganglia. The consensus of vertebrate physiologists of the time was that ongoing activity was just physiological "noise." In reflecting on these matters, Roeder sensed some connection between continuous sexual and locomotor activity in his operated mantids and the spontaneous electrical activity observed by Adrian and Prosser.

About this time, George H. Parker at Harvard urged Roeder to become one of his graduate students. Roeder declined. He explained later that a friend who had gone from Toronto to undertake graduate work at Harvard was so busy taking courses which he did not care to take and having someone else tell him what experiments to do that he was not having any fun. Roeder valued his freedom highly and looked upon research as his play. He wanted to approach experiments on his own, free from the biases and preconceived ideas of others.

Tufts College provided a congenial milieu and an opportunity to embark upon electrophysiological experiments. The opportunity came in 1938 when Leonard Carmichael became president of that institution. As Roeder wrote,

> He brought with him from Rochester some very erratic amplifiers and a string oscillograph accompanied by an electronics technician (Bertram Wellman) to nurse them. Carmichael found that his presidential obligations almost precluded research with the result that I practically had the set-up to myself. I went to work on the isolated nerve cords of various arthropods, studying (without much logic, I feel) the action of a wide variety of drugs and cation concentration on long-term changes in the spontaneous level of spike activity. The outcome was several papers in the 1940s which showed that the level of activity in deafferented insect and crayfish cords was much more sensitive to chemical changes in the medium than were the more popular phenomena of neurophysiology such as action potential parameters and simple synaptic transmission.

These experiments were carried out with equipment that was extremely primitive by modern standards. Nearly all the apparatus was home-built and ingeniously tailored to the projected experiment. Roeder was a master in the design and construction of experimental set-ups. He was known to his students as an inveterate "tinkerer." On the wall overlooking one of his self-designed pen recorders for action potentials was a quotation from T*he Rubáiyát of Omar Khayyám:*

> The Moving Finger writes; and having writ,
> Moves on; nor all thy Piety nor Wit
> Shall lure it back to cancel half a Line,
> Nor all thy Tears wash out a Word of it.

This sentiment reflected Roeder's honest and uncompromising approach to even the most recalcitrant data.

A longtime colleague wrote, apropos of mechanical and experimental ingenuity, "He encouraged each of us to build black boxes and nerve chambers, realizing the appropriate and ingenious experiment was often the key to solving a research problem." This trait of mechanical ingenuity had also appeared early in his life. His hilarious stories of struggles and pranks with electric generators and automobiles belonging to his father and his headmaster offered ample proof of this. We cannot help but remember with pleasure the loving care expended in making microelectrodes, the artificial eye with compensating pupil, the mealworm cannon for free-flying bats, the homemade or modified cameras, the artificial electrical cockroach, and outdoor electrophysiological studies pursued in woods and fields using the heavy and clumsy equipment of the 1950s and early 1960s.

With this equipment he pioneered on many fronts. He used motion picture films and single-frame analysis to elucidate fast movement sequences performed in prey capture by praying mantids; he modified cameras for photographing at night the maneuvers executed by moths evading hunting bats; he confirmed J. W. S. Pringle's discovery of the myogenic properties of insect flight muscle by combining the recording of fast thoracic oscillations by means of a crystal phonographic pick-up with the recording of electromyograms in rapidly flying Diptera; he was one of the first to use thermistors as differential anemometers for measuring the turning tendency of moths in stationary flight in the presence of ultrasounds.

Although early electrophysiological experiments on the central nerve cord were interesting to Roeder, none seemed to him to have obvious relevance to animal behavior until he read the 1950 essays of Konrad Lorenz and Erik von Hoist. As he remarked, these seemed much more heuristic and acceptable to a zoologist than the then current Pavlovian rat psychology. He was stimulated to return to his earlier studies on spontaneous activity and to attempt to prove that spontaneous nerve activity could generate adaptive behavior. In 1960 he published two papers showing conclusively that coordinated patterns of motor spikes destined for the phallic apparatus of mantids and cockroaches are generated endogenously in the last abdominal ganglion and are suppressed most of the time by descending signals from the brain. This demonstration of central inhibition came long before the cellular basis of inhibitory control could be established. His demonstration that rhythmic copulatory and locomotory movements are organized by central pattern generators had already been made before the importance of pattern generators in insects was beginning to be realized.

During this period he was making annual visits to the country of his father's birth, where he relaxed in the hospitality, peace, and freedom of the Weiberhof, Sonja's family home. While in Germany he was able to maintain personal contact with European ethologists, especially Konrad Lorenz, and with Erik von Holst and Nicolaas Tinbergen of The Netherlands. These contacts came at a time when American psychologists were only just becoming aware of European ethology, and confrontations between the two schools of behavior were on occasion extremely acrimonious. At the same time there was minimal interaction between neurophysiologists and behaviorists of either camp. Roeder, in his quiet, forthright, scholarly, and often humorous manner, served as a bridge of understanding. His fluency in German, his European cultural background, and his training in classical zoology at Cambridge played an important role in unifying the thinking in the field of animal behavior. At meetings in Freiburg and Seewiesen he not only enriched his own understanding of animal behavior but also influenced greatly by his quiet wisdom and intellectual honesty the course of neuroethology.

In the 1960s Roeder turned his attention to afference and the central control of acoustic evasive behavior of moths. As with so many of his studies, this interest had its genesis in his field observations of the behavior of moths when hunting bats were in the vicinity. He set up in his backyard in Concord, Massachusetts, an experimental arrangement that was simple and elegant. He reasoned that certain families of moths could hear the ultrasonic cries of bats by means of paired tympanic organs in the thorax. Thereupon he dissected a moth's "ear" and attached recording electrodes connected to a portable oscilloscope. At the same time he rigged a flash camera capable of photographing the field of battle when triggered. At dusk a stream of bats emerged from their roost in a neighbor's barn. The cries of any bat that reached the precincts of the garden were detected by the moth's ear and flashed on the oscilloscope, whereby Roeder was notified that a bat approached in pursuit of a moth. As the work progressed it became clear that the bat changed the character of its cry when echos were received from the moth. When the cry changed, Roeder triggered the camera. In this manner he obtained simultaneously a record of the bat's cry as perceived by the dissected moth ear and a motion picture photograph of the flight path of the moth being pursued. At one point Roeder and Asher Treat hauled about 300 pounds of electronic gear up a grassy hillside in the Berkshires of Massachusetts, where bats were known to feed, and conducted their experiments there.

From these field experiments Roeder moved on to investigate the acoustic properties of the two receptor cells associated with the tympanic organ. He plunged farther and farther into the central nervous system as he sought to understand how sensory signals were perceived at different levels and how this related to evasive flight behavior. In the 1960s he was recording from single cells in the brain. He considered himself fortunate if he found the acoustically sensitive brain cells 50 percent of the time.

The monumental body of work that led Roeder from mantid and cockroach behavior, to spontaneous activity in central nervous systems, to central inhibition, to neurophysiological analyses of prey-predator relationships, and finally to central processing of ultrasonic sounds was conducted uninterruptedly over a period of forty-two years in a cramped, cluttered laboratory in the basement of Tufts College's Barnum Museum, the home of the Biology Department and of Barnum's stuffed elephant Jumbo. In this ecological niche between the years 1933 and 1945 he worked alone with practically no research funds beyond those provided by the department plus a small grant from the American Academy of Arts and Sciences. Toward the end of World War II the Office of Scientific Research and Development, and later the Army Chemical Corps, the National Science Foundation, and the National Institutes of Health, provided support for studies of the mode of action of DDT and other insecticides. During this period, which dealt mainly with insect pharmacology, Roeder viewed the various drugs and toxic compounds mainly as levers for prying out information about the normal machinery of the insect nervous system. In the process, much was learned about the operation of insect synapses, sense organs, and muscles, particularly those concerned with flight. Of the eighty-eight major papers published during his career, two-thirds represented work done by himself alone. In contrast to the modern mode of working with teams of collaborators, Roeder preferred to work alone; nonetheless, the basement laboratory was the home of a small, closely knit group of students, postdoctoral fellows, and visitors from widespread parts of the world. There was a heady atmosphere here, a joie de vivre, an excitement, a unique camaraderie. Ken was always available to discuss, challenge, and offer opinions.

A former student and colleague captured the spirit of the enclave and the personality of Ken when she wrote the following:

> He taught us to ask questions, even in the face of established authority, to tinker and invent, to laugh at ourselves, to believe evidence, to play and to take joy in research, to teach and most of all to love. No teacher can have been so able in guiding and encouraging his students without excessively challenging them or overwhelming them. He cared about each of us personally—Tufts was his world, the kingdom he was looking for. He traveled far, taught many and learned much, but no faculty member was more loyal, devoted, or conscientious in performing his university chores or in serving either his adopted country or his intellectual discipline when the need arose. Honors came and they pleased him, but he cared most for the opinions of his family, his friends and his scientific colleagues.

His interactions with people, his mien, his philosophy were all of a piece whether he was at the college or at home with family and friends. The latchstring was always out at his home in the Concord countryside where, he once remarked in reference to the Battle of Concord and his nationality, "I live on the wrong side of the river."

Over the years, in all seasons, I spent many relaxed hours with Ken and Sonja in that setting, where the conversation ranged through experimental science, personal philosophy of science, music, art, literature, and religion. Always lurking in the background was Ken's acute wit. It expressed itself on one occasion at a seminar in reply to a fulsome introduction wherein the audience was enjoined to be prepared for "the incredible science that Professor Roeder will describe." Ken's opening sentence was "I trust that what I have to tell you will be credible."

Although much of his work was physiology and some concerned basic physiological processes, his approach was essentially that of a zoologist. He described his approach as follows:

> Questions dealing eventually with the whole intact animal in relation to its natural environment hold more interest for me than those of a more atomistic kind that lead to the physico-chemical basis of life. Psychology interested me, although it seemed to me to avoid the central problem of animals' nervous systems in relation to behavior. Consequently, I found more in common with the European ethologists and animal physiologists.

He was not one to lose the animal in the machinery or in the wetlands of ion channels, neurotransmitters, or secondary messengers.

Even though he worked exclusively with insects, he saw them as subjects and models from which one could extract information relative to a mechanistic understanding of behavior. He was frustrated by the lack at that time of some synthetic concepts, ethological or psychological, which would enable him to move up from neuronal levels of analysis to complex behavior.

Among his lasting contributions are his books *Insect Physiology* (1951,2), which he conceived, edited, and contributed to, and *Nerve Cells and Insect Behavior* (1963). The first established him as the founding father of insect physiology in America; the second presented a synthesis of his own physiological work and his broader views about the control of animal behavior.

In 1975 he wrote:

> Today's apprentice scientist is confronted by such a flood of objective literature that he is apt to lose sight of the fact that this public outpouring is the work of very human and fallible creatures like himself. *Logic* determines the framework on which he arranges scientific data, and the scientist must assume that cause and effect operate throughout the material universe.... But the doing of science is a very human endeavor, and the direction taken by this expanding edge of this logic framework is often

> influenced by human bias, insight, blindness and imagination as well as by chance. When he is reporting research the scientist rightly attempts to discount these imponderables-in fact, he does all that he can to limit their influences on his conclusions. But when he is doing research they play a vastly important part both in his successes and his failures. Not to recognize and admit to, perhaps even to court, one's subjectivity at this time is to delude oneself; it is also to miss the special joy of scientific discovery and to reduce the adventure to a form of computation.

THE MATERIAL WHICH FORMED the basis for this memoir came from my own files, my personal reminiscences, biographical documents from the National Academy, documents supplied by Dr. Nancy Milburn, and a memorial article by Dr. Franz Huber.

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Franz Schrader

FRANZ SCHRADER

March 11, 1891-March 22, 1962

BY KENNETH W. COOPER

The only child of Franz Schrader and Hedwig Dorothea Rohde, Schrader received his early schooling in the cloister of the magnificent Magdeburg Cathedral and the Magdeburger Bürgerschule. In 1901 he came to the United States with his mother, who was divorced from his father, and her second husband, Friedrich Wille, a prosperous specialist in mining enterprises. The family lived on Staten Island (the borough of Richmond, New York City), where Schrader attended grammar and high school. Left to his own devices, on that still unspoiled island he continued the natural history pursuits that had so delighted him on vacations with his father in the Harz Mountains. That zest for fieldwork, and especially for the natural history of insects and fish, never abated; it gave a unique and significant stamp to Schrader's cytological research.

Schrader attended Columbia University: first its School of Mines (1910-1912), at his stepfather's urging; then, yielding

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to his own interests, Columbia College, from which he graduated (B.S., 1914). Following college and two summers (1915, 1916) as assistant in the Bureau of Fisheries at Woods Hole, Massachusetts, Schrader served in Minnesota as supervisor of fisheries for the U.S. Bureau of Fisheries (1916-1917). There he carried out investigations on the feeding behavior of mussels and on parasites of fish. In 1917 he returned to Columbia as assistant in zoology, completing his doctoral degree in 1919 under the preeminent cytologist Edmund Beecher Wilson. His dissertation, published in 1920, was a landmark contribution, for it proved for the first time that sex may be determined by haploidy or diploidy of a zygote; that fertilized females can produce male progeny parthenogenetically; and that these males, unlike their sisters, are fatherless, for their single set of chromosomes comes only from their mother.

After receiving his doctorate, Schrader was appointed chief pathologist of the U.S. Bureau of Fisheries in Washington, D.C., where he applied his cytological skills to an analysis of nuclear changes in oocytes that accompany the loss of fecundity of penned pike-perch. Though all went well in that post, he resigned in 1920—a pivotal year. That year Schrader accepted appointment as biological associate at Bryn Mawr College, and on 1 November he married Sally Peris Hughes, doctoral student under Wilson and lecturer at Barnard College of Columbia University, whose skills and interests in cytology and natural history matched his own.

At Bryn Mawr, Schrader's research was largely devoted to chromosomal problems of sex determination, an outcome of which was the comprehensive treatise *Die Geschlechtschromosomen* (1928). Remarkably, he found species of coccids that provided possible evolutionary intermediates between ordinary diploid species with sex chromosomes and haplo-diploid parthenogenetic species. In these coccids, one set of chromosomes of the diploid male undergoes heteropycnosis, leading, he surmised, to inactivity and ultimately to effective haploidy of the nominally diploid male. Schrader pointed out that all of one haploid set in these cases may be viewed as a compound X chromosome (an X chromosome consisting of more than one element), and all of the other set as a compound Y. On this basis a hypothesis for the evolutionary origins of haplo-diploid parthenogenesis was formulated (Schrader and Hughes-Schrader, 1931), a hypothesis that is still in the forefront though mistakenly attributed by some to M. J. D. White (1954).

In 1930, as had both Wilson and Thomas Hunt Morgan before him, Schrader left Bryn Mawr to become professor in the department of zoology at Columbia University, assuming the post vacated by Wilson at retirement. Again like Wilson, Schrader was named Da Costa professor (1949) in recognition of the world renown he had attained in chromosomal cytology. During his twenty-nine years at Columbia, Schrader served twice as executive officer (chairman) of the department of zoology (1937-1940, 1946-1949), as member of the National Research Council Fellowship Committee (1939-1943), as an editor of the Columbia Biological Series (1930-1962), as one of the founding editors of *Chromosoma* (1939-1962) and of *Journal of Biophysical and Biochemical Cytology* (later *Journal of Cell Biology,* 1954-1961), as member of the editorial boards of *Journal of Morphology* (1932-1935) and *Biological Bulletin* (1939-1954, 1959-1962), and as trustee of the Marine Biological Laboratory at Woods Hole (1934-1951). He was elected vice president of the American Association for the Advancement of Science and chairman of the Section of Zoological Sciences (1947), president of the American Society of Zoologists (1952), and member of the National Academy of Sciences (1951) and of the American Academy of Arts and Sciences (1953).

The man who gained these distinctions was of cosmopolitan outlook and aristocratic mien. Sensitive, perceptive, intuitive, with a penchant for fine foods and wines, game fishing, Near Eastern rugs, art, classical music, literature, pre-Columbian archaeology, and travel, he had exceptional social grace and was a delightful host. His research and the trips to the tropics it entailed were pursued as most satisfying pleasures. Critical, dispassionate in judgment, with a vast range of learning, regarding continuity with the past as a necessity, and, like Wilson, keenly interested in the diversity of chromosomal behavior, Schrader's publications were never trivial or given to polemics. Like his book *Mitosis,* they stand apart with their clear, engaging, and individual style, with full consideration of the findings and ideas of his forerunners.

From the age of twenty-four Schrader was intermittently troubled by a weakened heart. Nevertheless he made many field trips with his wife to Central America for specially sought species. Those out-of-the-way organisms made his (and Sally Hughes-Schrader's) work unique, and so it remains today. The 122 different species upon which he published over the years, representing four phyla and some 64 genera, nearly all so different from the common grist of cytological research, added greatly to what is known of the diversity of chromosomal behavior in somatic and meiotic mitoses.

Schrader's research at Columbia may arbitrarily be viewed as comprising two periods of emphasis: 1932-1953, interrelations of spindles and chromosomes, and—overlapping the former—1945-1961, the diversity of meiotic systems, comparative cytochemical studies, and evolutionary considerations of DNA content of nuclei among related species and genera, all of which he interrelated.

The regular occurrence of very strange yet functional spindles at meiotic mitoses of some coccids he had investigated (monopolar spindles in *Pseudococcus,* tubular spindles within which chromosomes are aligned in single file in *Protortonia),* as well as the remarkable compound spindles discovered by Hughes-Schrader, diverted Schrader from problems of sex determination to an extensive research and review of spindles and the relations between chromosomes and spindles. One outcome of that preoccupation was his widely influential book *Mitosis* (1944; 2nd ed., 1953), which placed what was known of these subjects under searching analysis and offered new directions for research on chromosomal movements.

In 1930 it was generally believed that mitotic spindles are real entities, of short half-life, that originate independently of chromosomes yet are necessary for their movements. Since spindles of healthy, living cells appear nearly optically homogeneous, the fibrous structure that appears within spindles when cells become moribund, or are fixed, was widely regarded as artifactual, especially by physiologists. However, on the basis of his study of the consistency of paths taken by the fibrous components of spindles in fixed cells (1932), and on the results of his carefully controlled centrifugation experiments with living cells (1934), Schrader concluded that spindles in living cells do indeed have a fibrous structure, albeit an invisible one. In bipolar spindles he described these fibrous arrays as consisting of(1) those running from a spindle pole to each chromosome or chromatid, namely, chromosomal fibers or half-spindle components; (2) continuous fiber systems from pole to pole; and, in some organisms (3) interzonal fibers of quite different nature connecting the ends of separating chromatids at anaphase. Final proof of the validity of Schrader's conclusions came with Shinya Inoué's remarkable time-lapse mo tion pictures of cells undergoing consecutive mitoses, taken by means of Inoué's vast improvement of polarization optics. In these now classical films, first widely shown to audiences in 1949, the strikingly birefringent groups of chromosomal and continues "fibers" stand out for all to see.

Before Schrader's studies of mitosis, most took for granted that the relation of spindles to chromosomes is fundamentally the same throughout eukaryotic organisms, and that spindles play the determining role in mitosis. However, both Schrader and Hughes-Schrader had described clear instances in which it is the chromosome that determines development of the spindle. Furthermore, Schrader had shown in *Pseudococcus* that the state of a chromosome can be critical in the formation of a spindle. In Schrader's view, nonchromosomal spindle initiators and chromosomes are clearly interactive and may show various degrees of predominance at certain developmental stages and in certain cells.

Schrader (1935) contrasted those spindle "attachments" located at a fixed point on a chromosome, or localized kinetochores of complex morphology and common to most organisms, with holokinetic (or "diffuse") kinetochores first discovered in homopterous and heteropterous bugs. The diffuse kinetochore is of no known special morphology, the chromosome appearing to have spindle fibers normal to its entire poleward surfaces. These two sorts of kinetochores appear to be mutually exclusive, for all chromosomes of an organism's complement in a given cell have either localized or diffuse kinetochores. The properties that Schrader described imply that all, or nearly all, fragments of holokinetic chromosomes, unlike all but one of those of a chromosome having a localized kinetochore, should be capable of spindle fiber association and undergoing mitosis. That holokinetic chromosomes do indeed have that property was first proved by Hughes-Schrader and Hans Ris (1941).

The discoveries and their interpretation by Schrader are now regarded as expressions of fundamental properties of chromosomes in relation to spindles. Chromosomal "spindle fibers" have been shown to have their physical basis in ultramicroscopic microtubules, along with associated molecules, which in some cells appear to be formed first in fibrous arrays at kinetochores, and in others at future spindle poles or centrosomes. The distinctions between holokinetic and localized kinetochores are borne out by electron microscopy, but the evolutionary relationship between the two remains a Gordian knot. Finally, as Schrader predicted, the main advances in the resolution of these problems are now being made physico-chemically.

Unlike the seminal contributions made by Schrader to sex determination, mitosis, and chromosome structure, his other studies have not been so readily assimilated or assimilable. Cyril D. Darlington's "precocity theory" (1932) was the prevalent cytogenetic theory, an eclectic one ingeniously constructed but not from wide-ranging comparative cytology. It imposed rules of behavior upon chromosomes, a behavior guaranteed to conform to classical genetics, which is based on few kinds of organisms. If chromosomal behavior could not be fitted to the "rules," it was ignored. Despite those cardinal inadequacies, in Schrader's time Darlington's theory dominated cytological work on meiosis, and Darlington's following among geneticists and cytologists was immense.

A chain of relations is required under Darlington's theory for segregation at meiosis, with failure at any link automatically resulting in random assortment of chromosomes. That chain is as follows: precocious onset of prophase ` synapsis ` crossing-over (chiasma formation) ` conjunc tion as bivalents ` segregation. From his very early work (1923) to nearly his last (1960), Schrader had found unassailable cases in many diverse organisms in which segregation occurs despite failure of synapsis in some, of chiasmata in others, and of bivalent formation in still others. What is more, molecular geneticists proved that the first meiotic prophase is not "precocious," as Darlington's theory requires, for chromosomal DNA is doubled before synapsis; the theory in its original form has thus collapsed. However, its former adherents now hold crossingover, which they equate with chiasmata, to be the sine qua non of conjunction and segregation, so most of Schrader's demonstrations of the remarkable diversity of meiotic phenomena among some forty organisms remain unassimilated by current cytogenetic theory, and accordingly are ignored by most.

Schrader's 1945-1960 studies of meiosis in what he called the "harlequin lobe" of the testes of twenty-one tropical or semitropical pentatomid bugs bear importantly on the complexity of meiosis. In that special lobe in each species, there occurs a species- or genus-specific, fantastic distortion of meiosis, wholly unlike the regular meioses in cells of the other testicular lobes. In all of these species highly aneuploid spermatozoa result that no longer can have an ordinary gametic function at fertilization. In some, only the sex chromosomes undergo their customary meiotic maneuvers and segregate. Autosomal behavior is exceptional in all, variously but regularly—according to species—involving combinations of asynapsis, desynapsis, chain formation, clumping, lateral displacement on the spindle, non-random segregation, and so on. Differentiation of the harlequin lobe in related species has evidently brought about different uncouplings of events of normal meiosis that are both numerous and complex, and regular in their consequences.

The harlequin lobe phenomena therefore are highly significant, for, side by side, they present an aberrant meiosis and the normal meiosis from which it was derived.

Schrader believed that theoretical resolution of these and other problems would require painstaking comparative physico-chemical investigation of the astonishing variety of cytological conformities and nonconformities. Accordingly he welcomed T. Caspersson's introduction of cytochemical tests quantifiable by photometry. With Cecile Leuchtenberger (1946-1956) and others, Schrader carried out a variety of photometric studies on DNA content of cells in relation to quantities of RNA and protein, on infertility in man and in dwarf bulls, on development of nebenkern and acrosome, and on apparent exceptions to DNA constancy.

Of this series, several studies of pentatomid bugs by Schrader and Hughes-Schrader (1956,1958) are specially notable. A study of six species of *Thyanta* showed that five species, like the majority of pentatomids, have six pairs of autosomes; the sixth species, *T. calceata,* however, has twelve pairs. Remarkably, all six species have "a surprising uniformity in nuclear content of DNA." This unexpected state of affairs is accounted for by assuming that pentatomid chromosomes are polytenic, and that a longitudinal, equal separation of strands of each autosome doubles the number of autosomes without increasing the total amount of DNA per nucleus. Somewhat more complicated, but similar, relations among ten species of *Banasa* are also interpreted in terms of polyteny and "chromatid autonomy"—attributes that are suggested to have played important roles in chromosomal evolution.

The notion of polyteny of mitotic chromosomes of course flies in the face of the widespread belief that chromatids of all mitotically competent chromosomes are composed of but one bineme of DNA. The explanation given by the

Schraders to these and still other cases is not the only possible one, but the chief basis today for rejecting their hypothesis lies in the fact that it is counter to widespread belief. That dogma, however, is not based on widespread investigation; rather, it depends upon analyses of chromosomes of a very limited range of organisms. In any case, the findings for *Thyanta* and *Banasa* are of such potential significance regarding both chromosome structure and evolution that it is to be hoped that these cases will be reinvestigated with molecular methods.

At retirement as Da Costa professor emeritus in 1958, Schrader was invited to Duke University as visiting professor and Hargitt Fellow, privileges he gladly accepted and much enjoyed. His last research (1960-1961) was published from Duke's laboratories, continuing with characteristic gratification his exploration of harlequin lobe meioses as well as an experimental study of the properties of holokinetic chromosomes and their fragmentation products at meiosis. In 1961 the editors and publisher of *Chromosoma* presented him with a Festschrift. He died in the following year of renal neoplasia, and Sally Hughes—Schrader-whose last research paper was published in 1983 (at age eighty-eight)—died in 1984. They were a remarkable pair of outstanding cytologist-naturalists.

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Photograph by William Simmons, New York University

W S. Tillett

WILLIAM SMITH TILLETT

July 10, 1892-April 4, 1974

BY H. SHERWOOD LAWRENCE

As happens so often in science, it is one of life's little ironies that William S. Tillett's discovery of the bacterial protein streptokinase and his revolutionary idea of enzymatic therapy of thromboembolic disease had to take so long to reach its full flowering and current successful clinical application in the treatment of coronary thrombosis on a global scale. Yet Tillett never doubted the ultimate outcome or flagged in his pursuit of this idea. He had conceived, explored, and fostered this enzyme's unique thrombolytic applications with the broad vision, gifted intuition, and unerring precision which characterized all of his investigative work. This early major discovery was to become a constant preoccupation and a source of particular pleasure in his mature scientific life, yet he had discovered joy in nature long before.

Tillett's love and reverence of nature had its origins in his childhood in Charlotte, North Carolina, where he was born on July 10, 1892. The youngest of four sons, his early years were spent happily midst a warm, devoted, and loving family reinforced by close ties with his older brothers, whom he admired greatly. His father, Charles Walter Tillett, was a successful and highly respected lawyer, and his mother, Carrie Patterson, was a physician's daughter. As a young ster he used to spend each summer with his three older brothers on his grandfather's farm in North Carolina. Here he savored fully the rich experience of country life, the animals, the garden blooms, and the farm itself. And he cherished the memories of making a country practitioner's rounds in a horse and buggy with his grandfather, Dr. Patterson.

His public schooling began in Charlotte, and then he moved on to a private preparatory school, Webb School, in Bell Buckle, Tennessee, for which he continued to have a particular fondness. He remained proud of the fact that at the completion of studies at Webb School students were eligible for admission to any college in America without qualifying examinations. The school was noted for its strict discipline and a curriculum consisting of English grammar, Latin, Greek, history, and mathematics. He was a willing student who enjoyed being taught, yet was most happy when discovering new ideas on his own, a preference which was to characterize his career and persist for a lifetime.

Upon graduation from Webb School in 1909, Tillett enrolled at the University of North Carolina, where he excelled as a scholar as well as an athlete; his selection as All-American quarterback earned the high esteem of faculty and classmates alike.

More importantly, it was here that Tillett experienced his first exposure to the sciences; from the very outset, the course in biology fascinated him. He was mesmerized by the thrill of watching frog's eggs divide and redivide to finally emerge as tadpoles in his laboratory exercises. This fascination with biology and the orderly, predictable sequence of life unfolding caught his imagination and charted his future course in science and in medicine. Many years later, in his retirement, it would give him the warmest feelings of pleasure to recall anew these still vivid images and recount them with an undiminished awe of the marvelous.

Upon graduation from the University of North Carolina in 1913, he entered Johns Hopkins Medical School, which fostered in him a profound and enduring admiration and affection both for the school and for its stellar faculty. As a result of this exposure he emerged strongly attracted to an investigative career in medicine. Tillett's pursuit of these plans after graduating with the M.D. degree in 1917 were put in abeyance with the advent of American participation in World War I. He enlisted promptly, before completing an internship at the Baltimore City Hospital, and was commissioned a first lieutenant in the Army Medical Corps. Upon completion of two years' service as medical officer, much of it in combat with a battalion of engineers in France, he returned to America unscathed and was demobilized with the rank of captain. In 1919, back at his beloved Hopkins, he took up where he had left off, first as an intern and then successively as a house officer and then an assistant in medicine. This experience was capped by a year in Europe visiting medical institutions in London, Vienna, Paris, and Rome to round out his education.

Tillett was always a great believer in chance providing the destiny that shaped one's career. Chance frequently smiled on him with coveted job opportunities, as we now learn, as well as in his scientific investigations, as will become evident. Upon his return to New York City from the European grand tour, he invited to dinner an old friend and former classmate from Hopkins who was then chief resident physician at the hospital of the Rockefeller Institute. As the evening waned, this physician invited Tillett back to the hospital to spend the night in the guest bedroom of the residents' quarters. He was delighted to accept the invitation and the opportunity to see the fabled Rockefeller Institute at firsthand. The next morning the chief resident told him of a new clinical department that was being formed for which a resident physician was needed to care for the patients on that service. Would he be interested in the position? Of course he would. And so, as Tillett often recalled in later years, he went to the Rockefeller Hospital to spend the night and remained for eight years. Thus chance and happy circumstance put Tillett, an ideal mind, in the ideal place at the most propitious of times and wound the spring that propelled his scientific career from that moment forward.

Coming to the hospital of the Rockefeller Institute as assistant resident physician in September 1922, he was originally assigned to work on the program of viral diseases with Dr. Thomas M. Rivers. This association resulted in his first publications coauthored with Rivers on the transmission of lesions and the immune response of rabbits to varicella infection. Prophetically this work presaged Tillett's lifelong interest not only in the properties, characteristics, and behavior of the specific microbe which caused infection but also in the nature and scope of the host's immune response so critical to a favorable outcome.

By 1924 Tillett had moved on to become resident physician on Dr. Rufus Cole's pneumonia service and was assigned to do laboratory research with Dr. O. T. Avery. As he often proudly and affectionately proclaimed in later years, this was the high-water mark of his career. It was at this juncture that he encountered the most propitious chance of all, the golden opportunity and cherished privilege to work and learn with Avery—outstanding scientist and warm human being, revered by his colleagues and affectionately known to all as Fess.

Thus it chanced that Tillett entered on the happiest days of his career, enthralled by Avery, whom he held in reverential awe and the warmest affection then and forever after. It was here that Tillett flourished and emerged as an innovative and resourceful scientist under the gentle guidance and inspiration of Avery. And it was here that his lifelong preoccupation with the pneumococcus and the streptococcus was forged. He became intrigued with the behavior and chemical properties of these microbes as critical determinants of the host response to infection and thereby the ultimate outcome of the encounter.

In his first work with W. F. Goebel and Avery, he set out to analyze the components of the pneumococcus and in the course of these studies he discovered that in addition to the type-specific polysaccharide, all pneumococci contained a distinctive and unrelated carbohydrate, the somatic C-polysaccharide, dubbed the C-fraction. Tillett, working with Thomas Francis, Jr., then observed that an "antibody" to the C-fraction appeared in the sera of patients with pneumonia during the acute phase of their illness and was not detectable in convalescent sera. This "antibody" became known as the C-reactive protein. Their subsequent studies revealed that the acute-phase sera obtained from patients afflicted with the broad range of infectious diseases as well as noninfectious, inflammatory syndromes had the property of precipitating the C-fraction. These findings resulted in the development and widespread clinical application of the C-reactive protein (CRP) test as an indicator and guide to the presence and course of acute inflammatory disease. The C-reactive protein that is precipitated by the C-fraction is currently still under intensive investigation by immunologists and other students of mechanisms of inflammation and host responses as a potent mediator of the inflammatory cascade. It is now recognized as a unique acute-phase protein which may participate in host defenses concomitant with but independent of the immune response.

It was during this period (1922-30) that Tillett accomplished a prodigious volume of work chiefly in collabora tion with his close friend and esteemed colleague Thomas Francis, Jr., and each of their oeuvres, like that of the C-reactive protein described above, led to new findings of broad significance which proved milestones in the progress of elucidating the repertoire of host responses to infection.

Still preoccupied with the properties of the carbohydrates of the pneumococcus as the major determinants of the host response to that infection, Tillett and Francis next turned to studies of the properties of the type-specific polysaccharides. They observed that intradermal injection of such purified protein-free polysaccharide fractions resulted in an immediate wheal and erythematous cutaneous reaction in patients convalescent from pneumonia caused by that specific pneumococcal type. They also observed that purified protein fractions of pneumococci injected intradermally gave rise to a delayed type of cutaneous reaction and that such reactions to this material were not type specific.

Further pursuit of these observations in normal individuals revealed that the type-specific polysaccharide was antigenic and resulted in type-specific antipneumococcal antibodies following repeated injection. Such immunized individuals would also respond with the wheal and flare cutaneous reaction when tested intradermally with the related type-specific polysaccharide just as they had observed in the patients convalescent from pneumococcal pneumonia. This intradermal test for type-specific immunity to the pneumococcus later became known as the Tillett-Francis test. Their observations also provided the first demonstration that carbohydrates free of protein contaminants could function as potent antigens.

Colin MacLeod, in an appreciation of Dr. Francis[1] upon his death, said of this work:

> On coming to Avery's laboratory, Francis and William Tillett worked together on cutaneous and serological reactions to products of pneumococcus, particularly the specific capsular polysaccharides and the 'C' or somatic carbohydrate, now known to be a constituent of the bacterial cell wall. Over the three-year period of their collaboration two remarkable findings came forth.
>
> The first of these was that there occurs in the blood of patients with many acute infections a new substance, not an antibody in the usual sense, which reacts specifically with the 'C' carbohydrate of pneumococcus to give a precipitation reaction. During recovery from the disease the "C-reactive protein," as it came to be known, diminishes in amount and within a few days disappears entirely. This is an enigmatic reaction whose function in man and animals is still unknown but which provides a useful clinical test to measure the activity of a variety of infectious processes, for example the activity of the inflammatory process in rheumatic fever.
>
> Francis and Tillett also discovered that minute amounts of specific capsular polysaccharides of pneumococcus injected intracutaneously in man cause the development of specific antibodies and that the antibodies are protective...

These seminal observations of Tillett and Francis thus provided the background for the idea of producing a type-specific polysaccharide pneumococcal vaccine which was conceived, initially tested, and proven effective in the prevention of pneumonia in the field by Colin MacLeod and Michael Heidelberger in 1944.[2] The vaccine was brought subsequently to its current acceptance and widespread clinical application as a result of the carefully designed and personally monitored trials of its efficacy by Robert Austrian, who had been a research fellow in MacLeod's laboratory at New York University at an earlier time.

In 1928, while at the Rockefeller Institute, Tillett met and married Dorothy Stockbridge, who had become and remained forever the brightest light and lodestar of his life. He was then an associate of the Rockefeller Hospital, where he remained until 1930. In that year a daughter, Elizabeth, was born, his and Dorothy's only child, whom he idolized. Then, after eight exciting and productive years spent at the Rockefeller Institute, Tillett was lured back to Johns Hopkins as associate professor of medicine and director of the Biological Division, newly formed in the Department of Medicine by Warfield T. Longcope, who was then chairman of the department.

It was here that he continued his studies on acute-phase reactants and shifted attention from the pneumococcus to the streptococcus, and here again that chance intervened to lead his receptive mind to the discovery of enzymatic fibrinolysis, for which he is most acclaimed and renowned. It all developed innocently enough from what seemed to be routine experiments with hemolytic streptococci arising from his earlier observation that the organisms were agglutinated by normal human plasma but not by serum. He deduced that the fibrinogen component present in plasma and absent in serum was the prime candidate for this agglutinating activity. This led Tillett to take oxalated human plasma containing fibrinogen that was unable to clot because of calcium depletion and to add hemolytic streptococci as potential absorbents of the soluble fibrinogen. He wished to observe whether upon the subsequent addition of calcium the anticipated fibrin clot formation would be negated by the binding of fibrinogen to the streptococci. The results of this experiment were uniformly negative; all of the tubes containing plasma clotted following the addition of calcium whether streptococci were present or not.

Tillett recalled his disappointment at this result and leaving the test tubes in the rack without even bothering to clean up or discard them. A nagging curiosity about nature's failure to respond to such a good idea led him to examine the tubes again at a later time. To his unbounded amazement and delight, he observed that the clots in those tubes which contained streptococci had lysed and become liq uid. He repeated this experiment a number of times with the same result and concluded that the hemolytic streptococci elaborated a fibrinolytic principle, streptococcal fibrinolysin, which dissolved fibrin clots. The principle turned out to be an enzyme activator, which was subsequently isolated and identified following Tillett's move to New York University and named streptokinase. Thus, as with his appointment to the Rockefeller Institute, once again chance intervened on Tillett's behalf, but this time the essential ingredient of destiny was the prepared mind.

The results of these initial observations were published in 1933, yet Tillett saw the meaning of it all clearly and precisely and conceived the idea of applying this fibrinolytic principle to the dissolution of the tenacious fibrinous clots so devastating to patients with empyema and meningitis. However, the clinical application of his discovery would not be realized until he moved to New York University a few years later. In the interim he continued to study the phenomenon and began analysis of the fibrinolytic principle with characteristic insight and resourcefulness, and by 1934 he and his young colleague, R. L. Garner, had characterized the fibrinolysin further and delineated some aspects of the mechanism of the reaction.

In 1937 Tillett was recruited to New York University School of Medicine to become professor and chairman of the Department of Bacteriology by John Wyckoff, its dean. He remained at that post for only one year; when the chair in medicine became vacant in 1938, and he was unanimously selected by the faculty to become professor and chairman of the Department of Medicine and director of the Third (NYU) Medical Division of Bellevue Hospital.

It was probably no accident that he was succeeded in the chair of bacteriology in 1938 by his long-time friend and collaborator from the Rockefeller Institute, Thomas

Francis, Jr. Happily also for New York University, it evolved that when it came time for Francis to move to his new post as the first dean of the School of Public Health at the University of Michigan in 1941, he was succeeded in turn as chairman of microbiology by Colin MacLeod of the Rockefeller Institute. This felicitous succession of unusually talented and gifted scientists, Tillett, Francis, and MacLeod, were all students and proteges of O. T. Avery, and each enriched and strengthened our faculty at New York University as well as science and the progress of mankind in his uniquely creative ways.[3]

With respect to the "Rockefeller connection," it is of interest that one of Tillett's first appointments as chairman of medicine was that of Dr. Maclyn McCarty as a research fellow in 1940. McCarty had been in Tillett's laboratory at New York University for a year when he was awarded a National Research Council fellowship in the medical sciences. With the letter of notification came the suggestion of the chairman of the Medical Fellowship Board that McCarty consider the possibility of working with Colin MacLeod of the Rockefeller Institute to broaden his experience.[4] McCarty showed the letter to Tillett, who knew that MacLeod would be leaving Avery's laboratory in July to assume the chairmanship of the Department of Microbiology at New York University. Nothing daunted, Tillett telephoned Avery promptly and recommended that he take on McCarty as a fellow in his laboratory. Avery agreed, and McCarty moved to the Rockefeller Institute, where he began to pursue the course that would lead to his pivotal contributions to the delineation of the biochemical nature and establishment of the pneumococcal transforming principle as DNA. In later years Tillett would recall this incident with great admiration and affection for McCarty, modestly adding that as much as he would have liked McCarty to stay with him, he knew that working with Avery would be so much more productive that it was no contest.

Once settled in the chair of medicine, Tillett began the pursuit of his two main objectives: first to recruit a cadre of bright, young full-time investigators to the department and then to press on with the elucidation of the biochemical nature of the streptococcal fibrinolytic principle and explore its therapeutic applications to human disease. Recruitment of full-time investigators was no easy task for a clinical department in the lean years of limited private foundation support and before the advent of NIH support and its guiding star, James Shannon, had arrived to revolutionize the course of biomedical sciences on a grand scale.

Fortunately Tillett had inherited from his predecessor, John Wyckoff, a strong faculty of clinical investigators to build upon: Joseph Bunim and Currier MacEwen in rheumatology; Herbert Chasis and William Goldring in renal disease and hypertension, along with their collaborators Homer Smith and James Shannon of the Department of Physiology; Charles Kossmann in cardiology; Joseph Connery in hematology; and Norman Jolliffe in hepatology.

An additional asset to attract talented investigators was the backup afforded by a preeminent faculty in the basic sciences: Homer Smith and James Shannon in physiology; Severo Ochoa and Otto Loewi in pharmacology; Thomas Francis, Jr., and then Colin MacLeod and Alwin Pappenheimer, Jr., in microbiology; Keith Cannan in biochemistry; Donal Sheehan in anatomy; and William von Glahn in pathology.

An early acquisition to the Department of Medicine was Ludwig Eichna in cardiology, who was to work full-time to establish investigation in cardiovascular hemodynamics. Additional strength was achieved with the acquisition of David Earle and Saul Farber, who formed the nucleus of the full-time investigators in the renal division.

Other full-time academic investigators were recruited through the Bellevue Hospital House staff training program: Sol Sherry and Alan Johnson, who collaborated with Tillett on the streptokinase-streptodornase studies; Henry Kunkel, who later moved on to the Rockefeller Institute; Saul Farber, later to succeed Lewis Thomas, who had followed Tillett as chairman of medicine; and myself.

With a strong and thriving department in place, Tillett now felt free to pick up the skein of his investigative pursuits. He had established earlier that the fibrinolysis induced by streptokinase resulted in the breakdown of fibrin. It was also determined by Milstone in Tillett's laboratory that the presence of a euglobulin was a requirement for the reaction to proceed. In subsequent studies done in collaboration with Tillett and later with MacLeod, L. R. Christensen showed that the fibrinolytic principle was an enzyme precursor possessed of both proteolytic and fibrinolytic properties. This precursor, a proenzyme named plasminogen, was detected in mammalian plasma and was found to be activated by streptokinase to become the enzyme plasmin.[5] Then in 1948 Tillett, Sherry, and Christensen discovered a new activity in the filtrates of broth cultures derived from several strains of hemolytic streptococci. They found that the addition of such filtrates to thick purulent exudates resulted in their prompt dissolution. This activity was isolated and identified as an enzyme distinct from streptokinase which was subsequently proven to be a streptococcal deoxyribonuclease and named streptodornase.

Prior to this discovery Maclyn McCarty, in the course of his investigations to establish beyond cavil that the pneumococcal transforming principle was indeed DNA, had independently isolated, purified, and described for the first time in 1946 the existence and properties of bovine pancreatic deoxyribonuclease.[6]

Tillett and his colleagues Sherry, Christensen, and Johnson proceeded with their studies of the effects of this new streptococcal enzyme streptodornase on DNA and on purulent exudates in vitro and in vivo. They went on to delineate the essential biochemical reactions involved and show that the dissolution of exudates resulted from the progressive depolymerization of the viscous DNA, and to detect in the crude streptococcal preparations additional nucleotidases and nucleosidases as well as deoxyribonuclease. They also demonstrated that the end result of the reaction was degradation of DNA into its constituent purines and pyrimidines. It was then established that the enzymes did not penetrate living cells and lysed only the extracellular nucleoprotein debris in patients with empyematous pulmonary exudates in vivo, resulting in the transudation of fresh polymorphonuclear phagocytic cells.

Tillett was anxious to press on with clinical trials of the efficacy of streptokinase and of streptodornase but had to await further purification and production of the enzymes in adequate quantity. Finally, the Lederle pharmaceutical company laboratories undertook the task of mass production and purification under Tillett's guidance and gentle prodding, and the ultimate success of this collaboration allowed a series of clinical studies to be launched. The general targets of this therapy undertaken by Tillett and Sherry were hemorrhagic and purulent pulmonary exudates such as those seen in hemothorax, acute pneumococcal empyema, and chronic empyemas of various bacterial etiologies. The results of intrapleural injection of the enzymes were prompt, unequivocal, and most impressive; there was dissolution of the thick viscous pleural exudate to fluid which could be aspirated, with resultant reexpansion of the lung. This outcome prevented the development of fibrothorax and the inevitable morbid consequences of surgical thoracoplasty.

Tillett named this revolutionary new therapeutic concept "enzymatic debridement" and established the general principles of its successful therapeutic applications. With his colleagues he went on to extend this new approach successfully to other refractory purulent diseases such as chronis osteomyelitis with draining sinuses and to pyogenic and tuberculous meningitides.

Encouraged by these successes, Tillett turned again to his initial goal and the more challenging and difficult problem of the lysis of intravascular thrombi by the systemic administration of streptokinase intravenously. With Alan Johnson in 1951, Tillett demonstrated that clots produced locally in the veins of rabbits would undergo lysis following systemic administration of streptokinase. This observation was extended gradually until in 1955 an intravascular lytic state was achieved in humans following streptokinase administration. It was these pioneering studies conceived and pursued by Tillett and independently by his former colleague Sherry and his collaborators A. P. Fletcher and Alkjaersig which laid the foundation and charted the way for the current global clinical application of streptokinase in the treatment of acute coronary thrombosis. For example, in a recent large randomized clinical trial, 11,806 patients with acute myocardial infarction received either conventional treatment or streptokinase intravenously. The favorable results achieved in the streptokinase treated group at 21 days was impressive: 23 percent reduction of mortality in those patients treated within three hours of onset and 47 percent reduction of mortality in those patients treated within an hour of onset.[7]

This report would have pleased Tillett immensely, as would have the cumulative favorable experience reported in a series of studies such as this which stimulated and accelerated efforts that have resulted in the recent cloning and production of recombinant tissue plasminogen activator (r-TPA) and prourokinase. Although the strategy has been refined and new mediators developed, the principle of enzymatic thrombolysis discovered by Tillett and pioneered by him and his students prepared the way.[8]

Thus enzymatic lysis of thrombotic disease has revolutionized the treatment and tipped the scales in favor of life in coronary thrombosis, a killer of the dream that had replaced lobar pneumonia as the "captain of the men of death" in western civilizations. How ironic, then, that this felicitous outcome had its origins in Tillett's dogged pursuit of an observation that had emerged from his dedication to unraveling the biology of the pneumococcus and the streptococcus. That a bacteriologist and infectious disease clinician should contribute so much to cardiology is in itself noteworthy, particularly since the outcome was so much less predictable in Tillett's case than that of the virtual eradication of rheumatic heart disease which followed upon the discovery of penicillin.

Although Tillett's main investigative pursuits focused on the basic and clinical studies of streptokinase and streptodornase in the period detailed above, he also made a number of important contributions related to studies of penicillin therapy of pneumococcal lobar pneumonia from 1942 to 1945. He had been selected by Chester Keefer to evaluate the newly discovered antibiotic penicillin in the treatment of this disease. Studies on antibiotic therapy were then in the thrall of the limited experience with bacteriostatic agents like the sulfonamides and the dogma that the daily dose administered was governed by the determination of the level of drug in the patient's serum. Tillett's findings using penicillin in pneumococcal lobar pneumonia led him to promulgate the idea that the tissue level of antibiotic, rather than the serum level, was the important factor. He went on to show that it was not the total daily dose of penicillin which resulted in recovery from the disease, but the duration of therapy which determined the outcome. He also showed that this result was dependent upon the development by the patient of type-specific antipneumococcal polysaccharide antibodies by the seventh to tenth days after infection. Additionally, he established that if penicillin therapy was interrupted before this time, the patient would relapse and experience a recurrence of the disease. These studies provided the first clear-cut demonstration of a seminal principle of antibiotic therapy, namely, that the antibiotic serves to limit the growth of the infecting microbe until the appropriate immune responses of the host can be marshaled and result in its eradication.

Another series of Tillett's clinical observations concerned the complication of pneumococcal empyema whereby he showed that in addition to systemic administration, a single intrapleural injection of penicillin eradicated the pneumococci in the pleural fluid promptly and more effectively than did systemic penicillin therapy alone. Ultimately, he demonstrated that with the combination of an intrapleural injection of penicillin plus streptokinase-streptodornase, this life-threatening complication of pneumococcal pneumonia was replaced by a curable illness. This was a triumph of clinical science carried out and witnessed on a daily basis by a host of interns at Bellevue Hospital who had come to take such a favorable outcome as a predictable consequence of the regimen employed.

While making these significant contributions, Tillett always had an eye out for young physicians with investigative potential. This unique talent has been well phrased in the following excerpt of an appreciation by his long-time collaborator Sol Sherry:[9]

> While Tillett also made important contributions to the early investigation of penicillin in the treatment of pneumococcal pneumonia and its complications, his impact at N.Y.U. was not restricted to his own research. He played a major role in the emergence of this institution into the front ranks of scientific medicine, and he provided many current leaders in academic medicine with their initial opportunities. He had an uncanny knack for spotting the potential in young people long before others could recognize it and, in many cases, offered suggestions as to a worthy problem. For example, having sensed very early the biological importance of Chase's work on the cellular transfer of delayed hypersensitivity in guinea pigs, he encouraged Lawrence to begin his career by determining whether cellular transfer could be accomplished in man using viable white blood cells; thereafter a beautiful series of studies on transfer factor emerged.

The author was at the tail end of a long list of such young physicians who benefited from Tillett's influence and unselfish guidance. The list was headed by Maclyn McCarty and included Ludwig Eichna, Sol Sherry, Saul Farber, Herman Eisen, Henry Kunkel, and Morris Ziff, all of whom have gone on to distinguished careers in science.

In addition to this talent as an investigator and clinician, Tillett was an educator with a very effective approach. He set the example, provided the support, and then allowed students the individual freedom to achieve their full potential in academic medicine. This quality is best illustrated by the following men who became chairmen of the Department of Medicine in their respective medical schools: David Earle (Northwestern University); Ludwig Eichna (State University of New York, Downstate); Saul Farber (New York University); Edmund Pellegrino (University of Kentucky); Sol Sherry (Temple University); and Gene Stollerman (University of Tennessee).

Tillett continued to play an active and integral role in the basic and clinical investigations of streptokinase and streptodornase up to the time of this retirement in 1958.

At that time, after many years of patient negotiation with the hospital authorities, he managed to acquire half a floor for laboratory space in the Administration Building adjoining the wards of Bellevue Hospital. The other half of the floor was shared for a similar purpose with the First Medical Division of Columbia University's College of Physicians and Surgeons at Bellevue Hospital, headed by Nobel laureates Dickinson Richards and Andre Cournand.

Upon Tillett's retirement in 1958, the Department of Medicine had constructed and dedicated a suite of laboratories in his honor designated the William Smith Tillett Laboratories. Although he was the recipient of many prestigious honors and awards, nothing gave Tillett so much joy and fulfillment as this legacy to the future. The laboratories and the research they fostered were a source of constant pleasure and enthusiastic anticipation for him. The only regret he ever expressed was that in our headlong dash to accommodate a growing cadre of investigators in crowded space, we had preempted a room he had earmarked for thinking.

This felicitous turn of events was capped by an invitation from the National Institute of Allergy and Infectious Diseases for Tillett to become director of a newly conceived training program for young physicians with an interest in allergy and infectious diseases. Of course he accepted, and nothing made him happier than guiding and advising a succession of bright young physicians whom the program attracted to be launched on a full-time academic investigative career. At a time when most men of his age were gearing down, Tillett was gearing up to participate in the shaping of the future. He continued in this position alert and productive, with an unerring instinct for selecting the right individual and the right problem, until just a few years before his death, cheerful and content in doing that which he loved to do above all else and that which he did so well. I never saw a man derive such pure, unalloyed enjoyment and such vicarious pleasure in the daily ups and downs of scientific pursuits as well as in the successes and achievements of his proteges. In his own investigative pursuits as well as in his training of others, Tillett set high standards of accuracy and excellence and was a commanding presence in the laboratory. Yet above all else he was a generous, understanding, and inspiring mentor who gave unstintingly of himself, of his ideas, and of his guidance. He never flagged and was happiest in ensuring the young investigator's identification with the scientific achievement and fostering the progress of his career.

Tillett's scientific contributions and leadership in biomedical science did not go unrecognized. He was the recipient of many honors and awards, notably the Lasker Award with L. R. Christensen (1949) and the Borden Award (1952) for his discovery of streptokinase-streptodornase and delineation of its clinical applications. He was also the recipient of honorary doctor of science degrees from his alma mater, the University of North Carolina (1942), from the University of Chicago (1951), and from Northwestern University (1959).

This high regard of his colleagues and peers was also evident in his election to the National Academy of Sciences (1951) as well as in positions of eminence which marked the various phases of his career. He was elected successively president of the American Society for Clinical Investigation (1937); the Association of American Physicians (1958); and the Harvey Society (1957). He was also a member of the American Society of Bacteriologists (representative to National Research Council), American Association of Immunologists (member of the editorial board of *Journal of Immunology),* Society for Experimental Biology and Medicine (member of the editorial board of *Proceedings),* New York Academy of Medicine (member of the Committee on Medical Education), American Association of Advancement of Science, and American Medical Association. During and after World War II, he served as chairman of the Streptococcal Commission of the Armed Forces Epidemiological Board and on committees of the National Research Council dealing with research into problems of military importance. He also served as a consultant to the Secretary of War on Epidemic Diseases of the U.S. Army (1941), member of the Pneumonia Commission of the Armed Forces Epidemiological Board, and chairman, Executive Committee, Division of Medical Sciences, National Research Council.

And what of the man himself? Tillett was the quintessential courtly Southern gentleman—urbane, well-mannered, courteous, and charming. Although he could be stern, he was at his softest with the patients he cared for at Bellevue Hospital. On grand rounds he would often think "There but for the grace of God go I" with an acute understanding of the suffering and the despair of the rejected and unwanted, and the patients sensed the empathy in his heart. He was devoted to them and they cherished him.

Tillett loved his family. A devoted husband and a proud father, he was also a loyal and staunch friend who would stick through thick and thin. While he was quite formal and all business in the laboratory, away from it he was a jovial, relaxed host and companion. A witty conversationalist, he had wide interests in literature, drama, and sports. He derived great pleasure in all that he did and lived each day with great gusto. And in addition to a wide coterie of friends in academic and scientific circles, he had friends in all walks of life: writers, newsmen, actors, cartoonists, literary figures. He prized his membership in the Player's Club, composed of a select group of prominent playwrights, authors, dramatists, and actors, and he had a lively interest in the arts. Most of all, he loved to relax and recharge his spirit with his family in their cottage in Deer Isle on the Maine coast—to plant his garden and watch it grow and to prune and engraft his trees. Tillett's final days were serene and spent with his beloved wife Dorothy in a well-run convalescent home on a pleasant cove in Essex, Connecticut.

We who were his close friends still miss him deeply. We miss his humor and gusto, his love of life and nature, his incisive mind, and his great heart. He was a generous and gallant man with a reserved exterior which cloaked a soft heart deeply touched by the plight of the less fortunate brought low by disease.

The most prized and fitting memorial in addition to his impressive scientific achievements still stands on a plaque in the corridor of the Tillett Laboratories in Bellevue Hospital, his bequest to the young people of the future which he cherished above all his accomplishments:

> "These laboratories for
> Medical Research are named in Honor of
> William Smith Tillett
> Professor of Medicine
> New York University College of Medicine
> Director Third Medical Division
> Bellevue Hospital 1938-1958

They are a symbol of his guiding principle that research in the problems of disease is essential to good medical care of patients and proper instruction of students and physicians."

Thus we remember him best as he would have wished, not in headstones or mausolea but in the hearts and lives of those he touched and in the scientific achievements of the next generation which meant so much to him.

This memoir draws on and expands upon the obituary notice I composed for the Infectious Diseases Society of America (J*ournal of Infectious Diseases* 130(1974) :311-12) and similar notices prepared by Sol Sherry *(Transactions of the Association of American Physicians* 88(1975):32-34) and A. McGehee Harvey (*The Interurban Clinical Club (1905-1976)— A Record of Achievement in Clinical Science* (New York: Saunders, 1978), pp. 201. Additionally, the materials supplied by the Archives of New York University Medical Center, by Dr. Richard Ross, Dean of Johns Hopkins Medical School, and the office of the Home Secretary of the National Academy of Sciences were most helpful sources of additional biographical information. I am also indebted to the following of Dr. Tillett's colleagues and friends for careful reading of this memoir for accuracy and significant detail: Drs. Maclyn McCarty, Sol Sherry, Saul Farber, Michael Heidelberger, and Herbert Chasis.

NOTES

[1] Colin M. MacLeod, "Thomas Francis, Jr., 1900-1969." *Archives of Environmental Health* 21(1970):226-29.

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[3] See also J. R. Paul, "Thomas Francis, Jr.," *Biographical Memoirs* 44 (1974):57-110; W. MacDermott, "Colin M. MacLeod," *Biographical Memoirs* 54(1982):183-219.

[4] Maclyn McCarty, *The Transforming Principle* (New York and London: W. W. Norton Co., 1985):43-50.

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[7] Gissi, "Effectiveness of Intravenous Thrombolytic Treatment in Acute Myocardial Infarction." *Lancet* 1 (1986):397-401.

[8] Sol Sherry, "The Fibrinolytic System and Its Pharmacologic Activation for Thrombolysis," *Cardiology Clinics* 5(1987):1-1 1; "Appraisal of Various Thrombolytic Agents in the Treatment of Myocardial Infarction," *American Journal Of Medicine* 83(1987):31-46.

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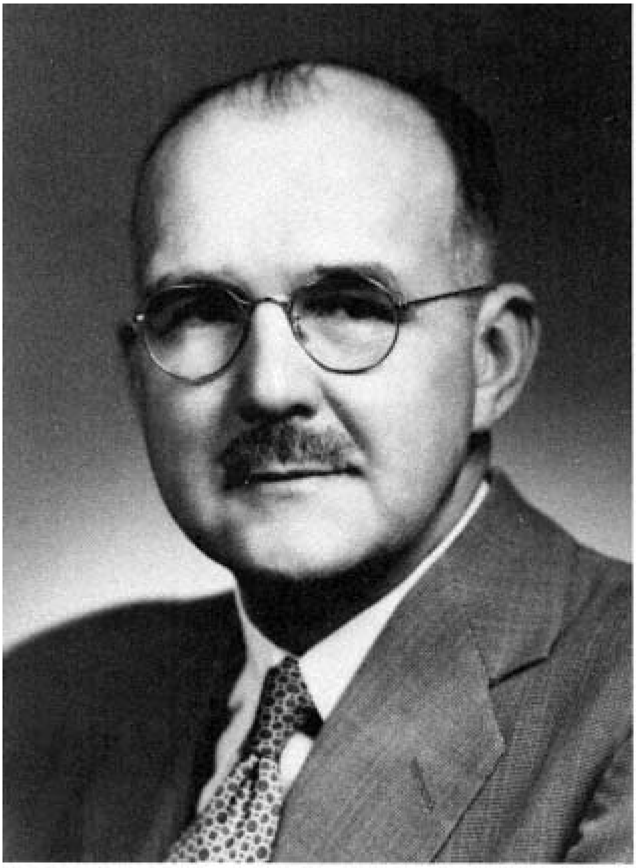

Courtesy of the American Physiological Society

Maurice B. Visscher

MAURICE BOLKS VISSCHER

August 25, 1901-May 1, 1983

BY HORACE W. DAVENPORT

Maurice Visscher was a physiologist who made two important contributions to his science. His own early opinion was that the more important was his demonstration that heart muscle becomes less efficient as it fails and that cardiotonic drugs tend to restore its efficiency. Later he concluded that his pioneer use of isotopes to define and measure the absorption of electrolytes by the small intestine was more important. Knowledgeable physiologists, including some of his own students, agreed with Visscher's revised judgment.

Maurice Bolks Visscher was born on August 25, 1901, in Holland, Michigan, the fourth of six children of Dutch Calvinists whose own parents had been members of a large group that had migrated to western Michigan in the 1840s to escape religious and economic oppression. Those immigrants had established churches, schools, and colleges before they finished building their own homes. Maurice Visscher attended high school and Hope College in his home town, and he thought himself fortunate in having stimulating biology teachers in both high school and college. His teacher at Hope College made Visscher a teaching assistant and charged him to do a research project, studying pollution of local streams and a lake caused by the town's discharge of raw sewage. His father became incapacitated while Visscher was in high school, with the result that the boy had the responsibility of running the family's two farms. He resented the loss of time he could devote to his studies, but he thought the experience taught him the importance of hard work and the necessity to budget his time if he were to succeed as a student while helping his family.

When Visscher was about to graduate from Hope College in 1922, he was encouraged by his biology teacher and inspired by the example of a physician uncle to apply for a scholarship to study medicine or a preclinical science. One application went to the University of Minnesota, where Elias Potter Lyon was both dean of the medical school and head of the Department of Physiology. Lyon, who himself had graduated from a small Michigan college, was on the lookout for similar applicants, and he gave Visscher a teaching assistantship in physiology so that Visscher could work for the Ph.D. and M.D. at the same time. Visscher earned his Ph.D. in three years, presenting a thesis on the transport and storage of carbohydrate in the animal body on May 9, 1925. Minnesota's flexible medical curriculum allowed Visscher to pick up clinical experience on a catchas-catch-can basis, so that he received his Minnesota M.D. in 1931 while he was head of the Department of Physiology and Pharmacology at the University of Southern California. Visscher later lamented that the rigidity imposed on medical studies by "curriculum reform" prevented many others from following his example.

Visscher received a National Research Council fellowship for two years in 1925, and he spent the first year working with Ernest Henry Starling at University College London and the second year in the Department of Physiology at the University of Chicago. Immediately afterward he was appointed associate professor and head of the Department of Physiology at the University of Tennessee. He moved to the University of Southern California in 1929, and from there he migrated in 1931 to the University of Illinois College of Medicine in Chicago, where he was head of the Department of Physiology. Lyon retired from physiology at Minnesota in 1936, and Visscher was brought back to Minneapolis to head the Department of Physiology. He became successively Distinguished Service Professor and Regents' Professor. When he became emeritus in 1970, he moved to another laboratory to continue his physiological research until just before he died of cancer on May 1, 1983.

When Maurice Visscher arrived in Starling's laboratory at University College London in the autumn of 1925, Starling had returned to the study of the behavior of the heart-lung preparation of a dog he had developed in 1912. In a heart-lung preparation, blood flows in a closed circuit from a reservoir into the right heart. From the right ventricle blood is ejected into the pulmonary artery and then into the lungs, where it is aerated by mechanical ventilation. Blood flows from the lungs through the pulmonary vein into the left heart, and upon being ejected by contraction of the left ventricle into the aorta, it passes through an adjustable resistance, then through a device by which the rate of flow of the blood is measured, and then into the reservoir, from which it returns to the right heart. The heart's rate of contraction is raised or lowered by warming or cooling the sinoatrial node, and the pressure by which it is filled is adjusted by raising or lowering the reservoir. The pressure against which the left ventricle works on each stroke is measured by a manometer, and the pressure is varied by adjustment of the resistance through which the blood then flows. The output of the ventricle on each contraction is calculated by dividing the rate of blood flow by the heart rate, and consequently the static work done by the ventricle by raising the pressure of the blood is calculated. Using this preparation, Starling had found what he said was the Law of the Heart: that "the mechanical energy set free on passage from the resting to the contracted state depends on the area of chemically active surfaces, i.e., on the length of the muscle fibres which in turn is determined by the volume of blood in the ventricle during diastole."

Starling and Visscher modified the heart-lung preparation by inserting a spirometer into the circuit ventilating the lungs, and by that means they could measure the rate of oxygen consumption by the preparation and could calculate the average oxygen consumption of the heart at each contraction. The calculation was not entirely correct, for the lungs as well as the heart consumed oxygen. Starling and Visscher thought the lung metabolism was probably constant and that in the face of the large changes in oxygen consumption by the heart, the lung oxygen consumption introduced little error. Likewise, the calculation of work done was not entirely correct. Starling and Visscher did not at that time measure pulmonary artery pressure or coronary blood flow. They also neglected kinetic work done in accelerating the blood. Nevertheless, they could obtain a reasonable estimate of the energy liberated by the oxygen consumed in relation to the work done. At this distance in time it is impossible to tell how much Starling and how much Visscher contributed to performing the experiments and drawing conclusions. Starling's own heart was beginning to fail, but Visscher wrote later "that [Starling] . . . was experiencing recurrence [of his disability], but he refrained from talking much about it and instead went right ahead with his research and writing program."[1]

Starling and Visscher found that in a sound heart whose output was maintained constant, the rate of oxygen consumption was directly proportional to the aortic pressure and therefore to the work done. When Starling and Visscher varied both the cardiac output and the aortic pressure, the rate of oxygen consumption was directly proportional to the diastolic volume and therefore to the diastolic length of the ventricle's muscle fibers. In a failing heart there was only a random relation between diastolic volume and work done, but here again there was a linear relation between the rate of oxygen consumption and diastolic ventricular volume. Starling and Visscher wrote:

> Under all conditions we have studied, the oxygen consumption of the isolated heart, maintained under constant chemical and temperature conditions, is determined by the diastolic volume, and therefore by the initial length of its muscle fibres. This rule applies whatever the physiological condition of the heart. During the whole of an experiment the oxygen consumption at a given diastolic volume is always the same, whatever the work the heart is performing at this volume.[2]

Visscher often restated the conclusion: the energy derived from oxidative metabolism is directly proportional to the diastolic volume of the heart, and the efficiency of the use of that energy decreases as the heart fails.

In Chicago Visscher refined the heart-lung preparation, and in particular he improved the accuracy of measurement of oxygen consumption. He added calculation of kinetic work to his estimate of work done by the left ventricle, and he arbitrarily substracted 20 per cent from the oxygen consumed to correct for its use by the lungs. When he used the value of 5 calories liberated per cubic centimeter of oxygen consumed, he found the efficiency of a sound heart to be about 6 per cent and that of a failing heart to be about 3 per cent. Such a calculation is critically depen dent upon the assumed energy equivalent of oxygen, for if carbohydrate is burned the value is near 4 and if fat is burned the value is near 9. In Starling's laboratory Visscher had found that insulin "free of an adrenalin-like impurity" provoked an increase in oxygen consumption; when he was in Tennessee he found that the glycogen content of a dog's heart did not fall in an experiment lasting as long as six hours and that the amount of glucose taken from the blood could not account for more than 6 per cent of the heart's metabolism. In California Visscher enlisted the help of Richard Barnes and Eaton McKay, two experts on fat metabolism, and together they determined that oxidation of ß-hydroxybutyric acid could account for between 22 and 82 per cent of the heart's oxygen consumption. The lungs alone used 80 per cent of their oxygen consumption to oxidize the ketone body.

While he was still in Chicago, Visscher had found in a few experiments that adding a digitalis glucoside increased both the heart's oxygen consumption and efficiency at constant diastolic volume. Soon after he arrived in Minnesota, Visscher had Gordon K. Moe as a graduate student and collaborator, and together they made a systematic study of the effects of digitalis preparations on the heart-lung preparation. First they found that a dose of digitalis insufficient to evoke irregularities of rhythm prevented or delayed failure. When Moe and Visscher allowed a heart to fail, they saw in one experiment that over fifty minutes the heart's efficiency dropped from 6.76 per cent to 3.88 percent. A dose of 2 milligrams of a digitalis glucoside restored efficiency to 4.91 per cent, and an additional 0.5 milligram raised it to 7.00 per cent.

During his tenure as head of the Department of Physiology at the University of Illinois College of Medicine in Chicago in 1931-36, Visscher substituted a study of absorp tion from the intestine for much but not all of his work on the heart. There is a story that Visscher became interested in absorption when a physician asked him whether perfusion of the intestine might be used as a means of dialysis in patients with kidney failure. That story may be true, but the most important factor in Visscher's study of absorption was that at Illinois he had the collaboration of Raymond C. Ingraham, who had just earned his Ph.D. in chemical engineering at Cornell. Papers by Ingraham and Visscher contain first- and second-order differential equations. Such mathematical formulation had not appeared before in Visscher's work, nor did it appear after his collaboration was broken by Visscher's return to Minnesota in 1936.

When Visscher began to study intestinal absorption, the topic was dominated by investigators attempting to replace what they conceived to be Rudolf Heidenhain's vitalism by strictly physical and chemical concepts. In the 1880s and 1890s Heidenhain had demonstrated that intestinal absorption cannot be attributed to osmotic and diffusive forces alone. Homologous serum, he showed, is absorbed by a tied-off loop of intestine of an anesthetized dog, although the serum has almost the same composition as the plasma into which it is absorbed. Even serum concentrated twice by evaporation in vacuo is absorbed. Likewise, chloride is absorbed against a concentration gradient, and its absorption is abolished by 0.04 per cent sodium fluoride. Heidenhain concluded that in addition to diffusion and osmosis, absorption is effected by a driving force, Treibkraft, residing in the cells of the intestinal epithelium.

Although Heidenhain repeatedly and vigorously asserted that by Treibkraft he did not mean "anything more . . . than [that] the chemical and physical events occurring within the cells produce demonstrable alteration within the cells or their surroundings,"[3] he was accused of vitalism by those who did not read his papers carefully. Physiologists in the first third of the twentieth century reacted against imputed vitalism by invoking the many anomalies of diffusion and osmosis through artificial membranes uncovered by physical chemists like Jacques Loeb. One physiologist reviewing intestinal absorption in 1921 wrote that "factors other than osmotic may be active in modifying [absorption] .... All the evidence points to their physico-chemical nature, and much useful research may still be expended before we need to seek refuge in that resort, 'vital cell activity.'"[4] Visscher, working first with Ingraham at Illinois and a few years later at Minnesota, laid the foundation upon which many others erected the elaborate structure called "active transport." For some reason, the term "active transport" introduced by Visscher seemed more mechanistic than "Treibkraft."

Visscher improved on Heidenhain's experiment by demonstrating absorption of autologous rather than homologous serum, and he and Ingraham analyzed luminal fluid and plasma for cations as well as for anions.

Sulfate ions are poorly absorbed by the intestine, and when Igraham and Visscher placed a mixture of half-isotonic sodium sulfate and half-isotonic sodium chloride in a loop of terminal ileum of an anesthetized dog's intestine, they found that in 1.5 hours the chloride concentration fell to less than 0.5 per cent of that in the dog's plasma. Bicarbonate concentration in the luminal fluid rose from zero to a concentration above that in plasma. Magnesium ions are also slowly absorbed, and when Ingraham and Visscher placed a mixture of magnesium chloride and sodium chloride in a similar loop, they saw that the sodium concentration fell to 5 mN, far below plasma concentration. When they divided the small intestine into four segments and repeated the sodium sulfate experiment, Ingraham and Visscher found that very little difference in chloride concentration was established in the jejunum. Chloride was more completely absorbed in descending segments. Because absorption of both chloride and sodium was abolished by metabolic inhibitors, Visscher concluded that their absorption is effected by active transport. Thus, Visscher's early experiments established that both anions and cations can be absorbed against a diffusion gradient and that there is a lengthwise gradient of absorption and secretion in the small intestine.

The Physics Department of the University of Minnesota had constructed a Van de Graaf apparatus before Visscher arrived in 1936, and Visscher could use short-lived $^{24}Na+$ and $^{38}Cl^-$ produced in the machine. He used a homemade counter to measure radioactivity and a simple density method to measure D_2O. Later A. O. C. Nier, the head of the Physics Department, provided Visscher with a mass spectrometer. Because his work was done before methods of handling isotope data were generally agreed upon, Visscher had to invent his own method of using his data, and consequently his two papers published in the June and November 1944 issues of the *American Journal of Physiology* are hard to understand. Visscher had wrestled with his data for years before submitting the papers, and editors reluctant to accept papers with mathematical expressions caused additional delay in publication.

Visscher established good working relations with Owen Wangensteen, head of Minnesota's Department of Surgery, and young surgeons in training worked in Visscher's department, bringing with them their surgical skills.[5] Visscher, who had been trained in an era when no physiologist was happy unless he was up to his elbows in a 60-kilogram anesthetized dog, could begin to use dogs with chronically prepared Thiry-Vella loops of the small intestine or gastric pouches. He soon found that absorption is more rapid in a loop of an unanesthetized dog than it is in the corresponding loop of an acutely operated anesthetized dog, probably because of substantial sympathetic nervous activity in the latter.

When Visscher repeated on an ileal loop of an anesthetized dog his experiment of filling the loop with a mixture of half-isotonic sodium sulfate and half-isotonic sodium chloride, he added radioactive chloride as well as D_2O. Net chloride absorption, he found, was the difference between chloride being absorbed from the lumen and chloride being delivered to the lumen from the blood. Thus he established that net flux across the intestinal wall is the result of unidirectional fluxes in each direction. From the D_2O data Visscher calculated that the concentration of chloride in fluid leaving the gut was half that in fluid entering the gut. Furthermore, when he measured the effect of osmotic pressure of luminal fluid upon absorption by putting 53 mN NaCl, 160 mN NaCl, or 480 mN NaCl in loops, he found that the rate of chloride moving from lumen to blood increased with luminal concentration of chloride but the rate at which chloride moved from blood to lumen was unaffected by the osmotic pressure of the fluid into which it moved. When luminal osmotic pressure was low, there was a net flow of water from lumen to blood, and when luminal osmotic pressure was high, the net flow was from blood to lumen. Using his D_2O data, Visscher calculated that unidirectional flux of water from blood to lumen was essentially independent of luminal osmotic pressure, whereas flux from lumen to blood was high when luminal osmotic pressure was low and low when luminal osmotic pressure was high. The difference between the two fluxes accounts for whether net flow was in one direction or the other.

Visscher used chronically prepared Thiry-Vella loops of the duodenum, jejunum, ileum, and colon for his work with radioactive sodium. When he put labeled isotonic sodium solution in each loop, he found that the concentration of sodium in the lumen remained nearly constant in the lumen of the duodenum but that its specific activity decreased rapidly. The specific activity of sodium decreased less rapidly in the lumen of the jejunum than in the duodenum, and it decreased still less rapidly in the ileum and only very slowly in the colon. In corresponding experiments in which Visscher injected radioactive sodium into the dog's blood, he saw the specific activity of isotonic sodium solutions in the lumen rise rapidly in the duodenum, less rapidly in the jejunum, still less rapidly in the ileum, and only slowly in the colon. This observation established that there is a substantial two-way traffic of sodium between blood and lumen of the small intestine and colon and that there is a gradient of decreasing traffic from duodenum to colon. Visscher had demonstrated how the intestine handles electrolytes. Chyme is brought to isotonicity and neutrality in the duodenum and upper jejunum by a brisk flow of electrolytes in both directions across the intestinal mucosa. In the ileum there is net absorption of sodium, chloride, and water, with secretion of bicarbonate replacing chloride in the lumen. The colon performs the essential function of maintaining the volume of extracellular fluid by net absorption of the sodium that escapes absorption in the ileum.

After the Second World War, many persons newly arrived in physiology and biophysics greatly elaborated on the study of intestinal absorption by means of isotopes, and they developed perfusion methods permitting study of absorption by the human intestine. In the great flood of enthusiasm for this work, only a few remembered that Maurice Visscher deserved credit for starting it all.

At Minnesota, Maurice Visscher built a large Department of Physiology that under his leadership trained thirty-six Ph.D. students and provided research experience for more than fifty residents in training from clinical departments. Many of these students became distinguished physiologists or physiologically oriented clinical professors. In addition, Visscher's Department of Physiology contained the Division of Physiological Chemistry until it became an independent department in 1946. A Division of Cancer Biology was made a part of the Department of Physiology in 1942, when John J. Bittner, who had described the milk factor in mammary carcinogenesis, came to Minnesota from the Jackson Memorial Laboratory in Bar Harbor, Maine. Ancel Keys's Laboratory of Physiological Hygiene was housed in Visscher's department until 1946. Consequently, Visscher could assign his numerous graduate students to one or another of those programs as well as to his own multiple research programs in cardiac, gastrointestinal, and respiratory function. As a result, Visscher's name is on some 200 research papers published from Minnesota. His name is first in order on only the most important papers, and it is usually last on the others. One of his best students said Visscher put his own name last in order to give his students advantage in acquiring reputation and perhaps also in conformity with the English practice of listing names in alphabetical order, as exemplified by the Starling-Visscher paper. Now it is impossible to discover the magnitude and nature of Visscher's own contribution to papers on cancer, nutrition, aging, circadian rhythms, arteriovenous fistulas, acid-base balance in hyperventilation, edema of the lungs, kidney function, endotoxin shock, hypertension, and the rest that have a student's name first. When queried on this point, one of Visscher's students, a man who was with Visscher at Minnesota almost from the first and who had a long and brilliant career of his own, wrote: "I would be surprised if Maurice did not make a significant contribution at least to design and interpretation of any investigation in which he was involved."

Maurice Visscher's most remarkable administrative accomplishment at Minnesota was to foster close relations between his Department of Physiology and the Department of Surgery, an accomplishment all the more remarkable when one remembers that in most medical schools in the United States physiologists and surgeons scarcely know each other by sight. This relationship was promoted by three factors. The first was that the University of Minnesota had a Graduate School of Medicine that encouraged research and research training in all clinical departments, with the result that a large number of physicians and surgeons spent a year or two in basic science laboratories during their residency training, earning a master's or a doctor of philosophy degree. The second is the enthusiasm of Owen H. Wangensteen, head of the Department of Surgery from 1930, for physiological research. Wangensteen, who had himself earned a Ph.D. in 1925, believed that basic research has immediate application in treatment of patients before, during, and after surgical intervention, and his own research earned him election to the National Academy of Sciences in 1966. Wangensteen maintained surgical research laboratories, and he supported them by gently extracting donations from rich patients. As a result of his example and precept, 115 surgical fellows earned the Ph.D. degree, and Wangensteen himself was the major advisor of 69. The third was Visscher's urge to apply physiology to the solution of clinical problems. He had learned during his own clinical training that problems in patient care might be solved by application of basic science knowledge, and he encouraged his graduate students to earn an M.D. as he had done so that they could appreciate the potential applications of physiology to medical practice.

Cooperation between the two departments paid off handsomely. Wangensteen said that open heart surgery performed by the surgical staff could not have been done without the physiological background the surgeons had acquired, but there were many less spectacular benefits. For example, continuous monitoring of oxygen, carbon dioxide, and anesthetic gases day to day in the operating room made possible by the use of physiological equipment resulted in greatly improved condition of surgical patients in the recovery room.

Maurice Visscher abandoned the Calvinistic faith of his Dutch forefathers, and he stopped believing in an authoritarian underpinning of ethics provided by revealed religion. "Science and technology," he wrote, "contribute to rejection of values of yesterday."[6] When Visscher looked for a scientific and universally applicable ethical principle, he turned to the Unitarian Church, of which he became a trustee, and he adopted the Unitarian "effort to make the world better for all human beings." For him, improvement in the human condition is the desired good. Nevertheless, Visscher wrote that the rigidity of Calvinist doctrine promoted in him a sense of urgency so that he doubted whether he would have been as active in promoting humanistic values if he had been raised in a Unitarian family.

As a scientist, Visscher believed that the ethical imperative is "complete truthfulness, fearlessness in defending unfettered scientific inquiry and the necessity to communicate one's findings to the world." He thought that the growth of scientific knowledge occurring in his lifetime brought severe problems in scientist-to-scientist communication, and he helped to solve some problems by improving the effectiveness of abstracting and indexing and by supporting publication of reviews and critical compendiums. He became president of the board of directors of *Biological Abstracts,* and largely through his efforts *Biological Abstracts* was rescued from financial difficulties to become once more an important member of the information retrieval system. As a member of the board of *Annual Reviews,* he promoted a program of annual publication of critical review volumes not only in physiology but in thirteen other major areas of science. After Visscher finished his term as president of the American Physiological Society, he became a member and then chairman of the Society's board of publication trustees. As the result of the success of *Physiological Reviews* published by the board, a substantial surplus had been accumulated and wisely invested. Visscher persuaded the board and then the council of the Society to use the surplus to publish a series entitled *Handbooks of Physiology* to replace the German *Handbücher,* whose publication had been terminated by the war. Visscher served as chairman of the *Handbook* editorial committee for ten years, and he recruited editors and authors for authoritative surveys of each field of physiology. The multivolume *Handbooks* were an enormous success from the first, and the long row of blue-bound *Handbooks* are on the shelves of all university and medical school libraries as well as in the offices of progressive college teachers who want to keep up with the progress of physiology.

Maurice Visscher believed the antivivisection movement was a major threat to unfettered scientific inquiry, and for forty years he was an active and effective opponent of restrictive antivivisection legislation in the United States. As a board member and then vice-president and president of the National Society for Medical Research, the medical scientists' organization for combatting antivivisection, Visscher was tireless in attacking what he called "little old ladies of both sexes" who put their sentimental version of animal welfare before human welfare. In particular, Visscher lobbied in Minnesota and Washington against restrictive legislation, and in doing so he turned the ethical argument of antivivisectionists around, saying that man at the pinnacle of evolution of animals on earth has a moral obligation to use animals in humanely executed experiments to solve human problems.

Maurice Visscher fervently believed that he had a duty as a citizen as well as a scientist, and it would probably be easier to list good causes, if any could be found, other than those into which he threw himself with irresistible energy. Many were related to his professional work. He was an officer of the American Physiological Society, the Society for Experimental Biology and Medicine, the American Heart Association, the American Cancer Society, the Minnesota Polio Research Commission, the International Organization of Medical Sciences, and the International Union of Physiological Sciences. In 1938 he helped to organize the Minnesota Medical Foundation, an early health maintenance organization, against the formidable opposition of organized medicine. He was an officer of the American Association of Scientific Workers, of the American Association of University Professors, and of the Special Committee on the Civil Liberties of Scientists of the American Association for the Advancement of Science. For ten years Visscher ran the Prospect Park Consumers' Co-op out of the basement of his home. He worked for Hubert Humphrey in Humphrey's first campaign for mayor of Minneapolis, but he later broke with Humphrey over his support of the Vietnam War. Visscher was a member of the Unitarian Service Committee and of the United Nations Relief and Rehabilitation Medical Mission to Italy in 1945-46, and he went on a medical teaching mission to Austria for the World

Health Organization in 1947. At the time of atmospheric testing of atomic bombs, Visscher demonstrated pollution of food in Minnesota by radioactive fallout, and he became a member of Minnesota's Governor's Commission on Atomic Energy Development Problems and of the National Committee for a Sane Nuclear Policy.

As the inevitable consequence of some of these activities, Visscher was suspected of disloyalty during the period that followed President Truman's Loyalty and Security Executive Order of February 15, 1947. His telephone was tapped, and he was subjected to harrassment by the Loyalty Board of the Federal Security Agency. He received official notice on February 15, 1949, "that no reasonable grounds exist for belief that you are disloyal to the Government of United States." Thereafter Visscher refused to accept any appointment that would involve loyalty clearance. That, he believed, gave him greater freedom of action.

Maurice Visscher was a member of many senior committees of his medical school and university, and he expressed his views on medical school and university governance with his characteristic devotion to high principles and his disregard for any unfavorable consequences to himself. He even broke with his old friend Owen Wangensteen over a question of university policy. His dean wrote that "Dr. Visscher is an energetic and compulsive man about his own plans, projects and ideas. He rarely gives any evidence of self-doubt about his position in discussion of these or hesitancy in their promotion. The result is that he can be more than a little vexing at times."[7] Those who dealt with Maurice Visscher on other fields of action sometimes agreed with the dean.

Maurice Visscher was elected to the National Academy of Sciences in 1956. He was also a Fellow of the American Academy of Arts and Sciences and a member of the Ameri can Philosophical Society. He married Gertrude Pieters on August 12, 1925. She and two daughters and two sons survived him.

I am grateful to Nathan Lifson, Gordon K. Moe, Clara M. Szego, and Leonard G. Wilson for providing me with documents concerning Maurice B. Visscher, for making comments on his personality and career, and for answering questions about him.

NOTES

[1] Quoted by C. B. Chapman, "Ernest Henry Starling: The Clinician's Physiologist," *Annals of Internal Medicine* 57(Suppl. 2) (1962):42.

[2] E. H. Starling and M. B. Visscher, "The Regulation of the Energy Output of the Heart," *Journal of Physiology* (London) 62(1927):243-61, p. 260.

[3] R. Heidenhain, "Beiträge zur Histologie und Physiologie der Darmschleimhaut," *Pflugers Archiv* 43(Suppl.) (1888):63.

[4] S. Goldschmidt, "Absorption from the Intestine," *Physiology Review* 1 (1921):421-53, p. 449.

[5] Visscher's admiration for Wangensteen and something of the collaboration between Visscher and Wangensteen in research and research training are described in Visscher's biographical memoir of Wangensteen.

[6] Ethical principles and quotations attributed to Maurice Visscher are taken from his writings on ethics listed in the bibliography. I have occasionally paraphrased him. I am grateful to Dr. Kenneth W. Phifer of the Ann Arbor Unitarian-Universalist Church for advice on Unitarian ethics.

[7] J. A. Myers, *Masters of Medicine* (St. Louis: Warren H. Green, 1968): 394-95.

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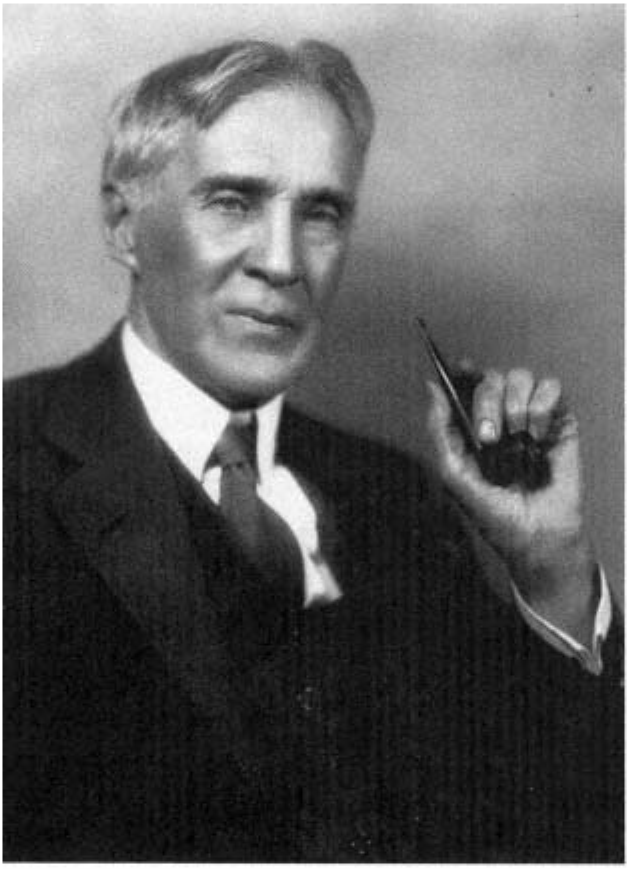

R. W. Wood.

ROBERT WILLIAMS WOOD

May 2, 1868-August 11, 1955

BY G. H. DIEKE

When Professor R. W. Wood died on August 11, 1955, he was to the younger generation of physicists a colorful legend, a representative of the past. He was, however, by no means forgotten, as a lesser man might have been at the age of eighty-seven. Many stories were still circulating about him. When I recently paid a visit to the University of Wisconsin, which Wood had left in 1901, his exploits were being discussed there as if they had happened just recently instead of more than half a century before. His solid scientific achievements are now a matter of record. His active mind refused to accept retirement, and he visited his old room in the Physical Laboratory at Johns Hopkins University regularly until nearly the end, even though the infirmities of old age had gradually made themselves felt. He never gave up his curiosity about things and was still actively engaged in the revision of his book *Physical Optics.* Death came to him peacefully; he passed away during his sleep without any severe illness.

Wood's active period of scientific productivity coincided with the rise of atomic physics, and he made important

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contributions to the increasing knowledge of the structure of the atom, chiefly through his experimental research in physical optics. He was, however, far from one-sided and penetrated into many fields. He went wherever his insatiable curiosity led him, whether this was into different branches of physics or into all sorts of other activities such as engineering, art, crime detection, spiritualism, psychology, and archaeology.

Wood may have inherited his interest in scientific matters from his father, Robert Williams Wood, who was born in Massachusetts in 1803, grew up in New England, and was a physician in Maine until 1838. Then he went to the Hawaiian Islands, where he stayed until 1866 as physician and pioneer in the sugar industry, after which he returned to New England. He was active in the American Statistical Association.

From his childhood R. W. Wood, Jr., showed an absorbing interest in all sorts of phenomena in natural science, and he soon started finding out for himself what made things work by trying experiments of his own. He went to Harvard University from which he graduated in 1891 with a bachelor's degree in chemistry. He was apparently bored by the regularly prescribed studies but made up for the poor marks in required subjects like languages and mathematics by extra work in fields that interested him, such as geology, astronomy, and psychology. After graduation he went to Johns Hopkins University for further studies in chemistry. There he found that he was more interested in what went on in Henry Rowland's laboratory in the Department of Physics than what he was supposed to be doing in the Chemistry Department. The following year (1892) he went to the newly founded University of Chicago, intending to get a doctor's degree in chemistry. He was not too well pleased with the routine of chemistry as it was taught there and went to Germany in 1894 for further study of chemistry. In Berlin he once more was attracted by what went on in physics, and under Ruben's influence changed permanently to a career in physics. After two years in Europe, he returned to the United States, spent a short time at the Massachusetts Institute of Technology, and in 1897 accepted an instructorship at the University of Wisconsin.

During the four years he spent at Wisconsin, his many-sided talents came into full blossom and he became known as one of the most promising young physicists. When Henry Rowland died in 1901, Wood was appointed his successor at Johns Hopkins University as full professor of experimental physics. Rowland was probably the most eminent physicist in the United States during the final quarter of the nineteenth century, and to be chosen his successor came as a high honor to the thirty-three-year-old Wood.

Wood remained at Johns Hopkins for the rest of his life, although his stay in Baltimore was interrupted by frequent summer trips to Europe and elsewhere and by occasional sabbatical leaves. He was professor of experimental physics until his official retirement in 1938 and was then appointed research professor, a position he held until his death.

When one tries to describe Wood's lifework and his impact on contemporary physics, one is immediately struck by the pure chemistry to contributions in almost every branch of physics, be it acoustics, electricity, heat, or optics. He did not confine himself to the purely academic side of the subject but often came forward with clever practical applications. One of the early examples (and a typical one) is the way he helped to relieve the hardships of the severe winters in Wisconsin. It happened regularly that the water pipes froze. Getting them thawed out was a long, drawn-out, and costly procedure. Wood suggested connecting the leads of a high-current transformer to the water faucets in neighboring houses and letting the electric current produce the heat necessary for the thawing-out process. This extremely simple procedure worked so well that it established Wood's reputation as a person who could be counted on to solve practical problems, something of no small importance in a midwestern state university, one of the chief purposes for the founding of which had been to help the general population meeting all the problems confronting a frontier society.

During his Wisconsin time Wood showed clearly the beginning of the particular genius that later made him famous with physicists all over the world. Here came the start of his studies in physical optics which were to become the main substance of his lifework. The first attack on the problem is typical for Wood. The wave theory of light was, of course, well established by the end of the nineteenth century. The concept of waves, wave fronts, etc., was, however, somewhat abstract. Elegant mathematical methods had been developed for demonstrating all the properties of the waves and could serve for the solution of practically any problem. These, however, meant nothing to Wood, to whom mathematical abstractions were without significance. In order to demonstrate to his students and to his own satisfaction the wave properties of light, he photographed (with the Schlieren method developed by Toepler) the actual wave fronts of sound waves which have analogous properties and demonstrated vividly all the familiar phenomena which are observed when waves are reflected or refracted. These photographs apparently received wide attention, and he was invited by Sir Charles Boys to demonstrate them before the Royal Society in 1900.

When Wood came to Johns Hopkins University, he found a favorable atmosphere for continuing his research in ear nest. His teaching duties were light. During the next three decades a series of papers on the optical properties of simple gases appeared, which placed his name among those of the great physicists. The modern theory of the atom was then in its early stages. Scientists realized that the spectra of the elements would form the chief source of information about the detailed structure of the atoms, although the key that would make all this information accessible was not found until the fundamental discoveries by Rutherford and Bohr in 1912. Wood had started on a detailed investigation of the spectra of such simple gases as sodium, mercury, and iodine. He discovered resonance radiation and studied its many puzzling features with great thoroughness and amazing experimental ingenuity. How it was polarized and how it behaved in a magnetic field or in the presence of foreign gases were things at first completely incomprehensible. Wood's experiments stimulated those of many others. During the 1920s the theory had advanced far enough to be greatly aided by these experiments in its further developments. The experimental work of Wood and the increase of our knowledge about the structure of the atom were inseparable. In the late 1920s, after the Raman effect had been discovered, it became apparent that the most significant results would be obtained with gases. The strong background scattering seemed to make experiments with gases impossible. Wood showed that by a ridiculously simple modification the difficulties could be completely overcome.

No one who knew Wood could ever get the mistaken idea that he would confine himself to one subject. Whenever he found something that interested and puzzled him, he went to his laboratory to clear the matter up. He usually could devise some simple experiment that went to the heart of the matter. Wood undoubtedly was one of the greatest experimenters of our time. Nevertheless, he never worked with anything in the least complicated. His apparatus was usually made by himself, often improvised and crude looking but always near perfection in its essentials and capable of extraordinary performances in his own hands. A typical example was the large spectrograph which he used during the summer months. He did amazing things with it, using the sun as his chief light source.

Wood and his family spent the summers on an old farm on Long Island. In a barn he had improvised a laboratory, and one of its features was this forty-foot grating spectrograph, probably the largest then in existence and certainly capable of giving better results than anyone had ever seen before. It was constructed from sewer pipe laid by the local stonemason. During the long months between summers when the instrument was not used, all sorts of wildlife used it as a shelter, and the optical path became cluttered up with spider webs. Wood's method of cleaning the tube has become a classic. He put the family cat in one end and closed this end so that the cat, in order to escape, had to run through the whole length of the tube, ridding it very effectively of all spider webs.

Wood's work was certainly not confined to the academic aspects of science. Whenever he saw a practical application, he would pursue it and see to it that others knew about it. The thawing of water pipes in Wisconsin was mentioned earlier. About the same time he invented a process for color photography and later made other contributions to photography. He was also very much concerned with the applications of the invisible ultraviolet radiation to all sorts of problems. During the First World War he developed an efficient filter which, combined with a mercury lamp, suppressed the visible light while trans mitting freely the ultraviolet. This filter is still called by many Wood's glass or Wood's filter. During the war such invisible light was used for signalling without the knowledge of the enemy. Many substances fluoresce brightly when subject to ultraviolet light, and the possibility of interesting effects for stage illumination was not lost on Wood. The famous Ziegfeld, who put on spectacular stage shows in New York with a swarm of chorus girls in resplendent costumes (or sometimes without them), was Wood's neighbor on Long Island. Many of Wood's ideas on lighting tricks with ultraviolet light found their way to Ziegfeld's stage.

During the war he became acquainted with high-frequency ultrasonic waves and made many experiments with them. Much of this work was done in the private laboratory of his friend, Alfred Loomis, in Tuxedo Park near New York, to which also his forty-foot spectrograph was transferred (without the sewer pipes, however).

Wood made many inventions, but he was a scientist rather than inventor and did not follow them up with development work and promotion; thus, he rarely reaped any financial benefits from them. His work on the production of hydrogen atoms in discharges, which he had undertaken in order to obtain a well-developed hydrogen spectrum, led in Langmuir's hands to the hydrogen welding process. He was one of the first, if not the first, to make successful photographs in infrared and ultraviolet light and applied them to photographing the moon and the planets in order to discover details on their surface invisible to the eye or ordinary photography. He applied this technique also to the detection of forgeries. He made numerous other improvements in photographic techniques, particularly as they applied to the solution of astronomical problems. He is credited with having been the first to propose the use of tear gas and the use of air spaces around warships to dissipate the destructive power of torpedoes when the ship is attacked.

Another example shows what his combination of curiosity and experimental skill could do to solve a problem completely outside of his own field. In 1931 Wood and his wife took a trip to Egypt, where he saw in the Cairo Museum gold ornaments that had come out of Tutankhamen's tomb. The gold looked decidedly purple, and the reason for this was a mystery. Some thought that the purple color had been produced purposely by the Egyptian goldsmith by a process since lost; others believed that age had affected the gold this way. No one had a clue to the mystery. Wood was allowed to take a few specimens with him and made tests on them when he returned to Baltimore. He found that the purple color was due to a thin surface film and by spectroscopic experiments found that the gold of this film was mixed with iron and some arsenic. He produced evidence that the films were produced purposely either by mixing the necessary ingredients or by using natural gold from a locality where iron and arsenic were present as impurities. Wood was able to reproduce in his laboratory the exact appearance of the purple gold and thus solved an archaeological mystery.

When Wood became seriously interested in physical optics and began to teach this subject, first at the University of Wisconsin and then at Johns Hopkins University, he keenly felt the lack of a book which would approach the subject as he did himself. He therefore set out to write one, and the first edition of *Physical Optics* appeared in 1905. It differed from any other book in this field by the large number of experimental details it included, all from Wood's own experience and therefore original, fresh, and full of interest. Wood admitted in the preface that he had taken the more mathematical parts largely from other books. The experimental parts were what appealed to the reader; a second edition became necessary in 1911, a third was published in 1934, and Wood was working on a revision for the fourth edition when he died. In the preface of the third edition, he stated his aim: "I have attempted to give, in as many instances as possible, a physical picture of the processes usually described by equations." In this he succeeded, spectacularly well in many instances. In others he perhaps overlooked the fact that not everyone had the same aversion to mathematical formulae as he had himself, and in a few cases his physical picture is unnecessarily elaborate, for a simple mathematical derivation would have conveyed the idea quite well.

I had occasion to discuss with him many parts of his book during the revisions for the third and fourth editions, and the attitude he took was perhaps typical of his relations with science during his whole life. It was only natural that in a subject which had progressed as much as physical optics had done, largely because of his own endeavors, there were some phases with which he had not kept abreast but which he nevertheless wanted to include in the new edition. It would have been easy for him to ask one of his associates to write these parts for him. He never did this. He wrote the parts himself and then went to one of his friends for criticism and suggestions, which he always took freely. He then rewrote the part and had it read again. This would go on until he finally felt that he had absorbed the matter and was reproducing it in his own words. Occasionally this process resulted in something that others felt was more elaborate than the subject warranted, but Wood refused to let others do the thinking for him.

There is an extensive biography of R. W. Wood by William Seabrook (New York: Harcourt, Brace & Co., 1941) which was written in such close collaboration with Wood that large portions of it are nearly autobiographical. This biography gives many details about his career. Much of his experimental work is conveniently assembled in his *Physical Optics.* Space does not permit a presentation here of all details, but one subject should be singled out because through his work in this field he affected the work of many other scientists. This is his work with diffraction gratings.

Henry Rowland, in his early days at Johns Hopkins University, had made the optical diffraction grating into a precision instrument and had invented the concave grating which made possible the modern high-resolution spectrograph. Rowland's engines for the rulings of such gratings were in the Physics Department of Johns Hopkins University. Wood had access to them and later was in complete charge of them. He made many studies of the gratings ruled with these engines. At the same time he made many improvements and saw to it that the machines were kept going so that gratings continued to be produced for other scientists to use. For many years these engines under Wood's care furnished practically the only source of supply for high-quality diffraction gratings, without which precision spectroscopy was virtually impossible. For this reason nearly every spectroscopic laboratory in the world and most astronomical observatories had some of Wood's gratings. Only in relatively recent years have other sources of supply for such gratings become available. Against the expectations of many, the demand for gratings has increased with the years, and now they are used in industry as well as in physical and chemical laboratories and astronomical observatories.

The ruling of 15,000, 30,000 or even more lines per inch, with the highest uniformity of spacing, is no small task, and when there are even minute flaws in the ruling, the resulting grating may be useless. The gratings produce a "spectrum" of light, and the literal meaning of the word is, of course, "ghost." However, use of the English word "ghost" does not refer to the spectrum that is to be produced by the grating but flaws in the spectrum which cause spectrum lines to appear where none are supposed to be. Wood found that one class of such ghosts, the so-called Lyman ghosts, was produced by the shock to the ruling engine caused whenever the seam in the drive belt hit the driving wheel. By designing a more flexible drive, he removed this difficulty entirely.

Wood designed the echelette grating, which is a grating with the grooves ruled with such a shape that they throw practically all the light into one narrow wavelength band. He also experimented with replica gratings so that one successfully ruled grating could provide many replicas with the same properties. The size of a grating is limited by the design of the engine. The astronomers in particular were looking for larger gratings to be placed in front of their large telescope objectives. Wood showed that this problem could be solved by making a composite large grating out of several smaller ones, and he was successful in making several of these mosaic gratings. He made many other improvements in the art of ruling gratings. Perhaps more important than this achievement is the fact that he kept the engines going and supplied a constant stream of gratings without which much of the research that contributed to the progress of physics would have been impossible.

Wood was a fascinating lecturer and a superb showman. This was already manifest during his early days in Wisconsin, where he went out of his way to enliven the teaching of physics by instructive and sometimes spectacular experiments. For Wood no phenomenon became real until he had devised an experiment that clearly demonstrated it. Abstract reasoning never appealed to him; nevertheless everything he did was carefully reasoned out. He never engaged in experimenting at random just to see what would come out but always had a carefully thought out plan. He knew all the tricks of the showman and could prepare carefully and in detail an experiment that was intended to give the impression of being improvised. He knew how to build up to a climax, and it was customary that wild applause would break out even in gatherings where no one would have expected any enthusiasm. He loved stunts such as taking a mouthful of liquid air and spitting it out or breathing in hydrogen and then speaking with a changed pitch to his voice.

Wood never formed a school; he was too individualistic for that. He had, however, many students from all over the world, and even now whenever two of them come together they can spend hours telling anecdotes about their former teacher. He was very fond of practical jokes. As a student he had a landlady who he felt showed too much interest in his affairs. One day when the streets were muddy, he took his shoes and placed footprints in his room beginning at the window, up the wall, across the ceiling, and down the other wall. The reactions of the landlady are not recorded.

When he gave demonstrations with ultraviolet light, often before gatherings with ladies present, he used to call attention to how brightly teeth fluoresce in ultraviolet light. Everyone would open his mouth for a demonstration until Wood remarked casually, "And you notice that all the false teeth remain black"; suddenly the mouths would snap shut again.

Wood, who worked over his own experiments until whatever he wanted to show became crystal clear, had no patience with scientific quackery and frauds. He occasionally went to considerable lengths to set matters straight. This he sometimes did by unconventional means and often with dramatic results.

Perhaps the most famous of these cases was that of the "N-rays," discovered by a reputable French physicist, which caused a considerable stir in 1903 and 1904 because of the remarkable properties attributed to them. Other investigators appeared to confirm the original findings, and a whole series of papers appeared in reputable scientific journals dealing with these sensational rays. In fact, the French Academy awarded a gold medal and a large prize to the discoverer. Other investigators, among them Wood, could not duplicate any of the experiments. At the request of other scientists, but undoubtedly also because the subject appealed to him, Wood went to France to investigate the matter. The rays were supposed to be deflected by an aluminum prism. While the inventor was demonstrating the effects in a darkened room, Wood took the prism out of the apparatus and put it in his pocket without any change being observed by the inventor. Wood published this finding immediately in *Nature* and thus ended the N-rays for good.

As far as I know, no one ever suggested that fraud was involved in the affair of the N-rays. It apparently was a case of self-delusion on the part of the inventor and the other scientists who claimed that they could observe them.

Wood also had ample experience with outright frauds and fakers. Some claimed to have secret inventions for which they sought financial backing, others claimed a cure for strange ailments, and still others could communicate with the spirits of the departed. Wood had no patience with any of them; he used much ingenuity to trap them and had spectacular success in many cases. Mediums soon became wary and after such experiences did not want to have anything to do with Wood.

The following example has all the imprints of Wood's method, though I cannot vouch that it originated with him. During the Second World War there was a self-styled inventor who claimed to have discovered a powerful explosive which in his opinion would supersede all hitherto used explosives, while the military experts considered it worthless. The inventor had, however, powerful friends among the politicians in Washington, and therefore the Aberdeen Proving Ground, where Wood was a consultant, could not refuse to give the inventor the opportunity for a demonstration. It was agreed that the effect of the explosive should be tested with goats to be tethered at various distances from the explosion. The day of the test arrived, with senators and generals present and the newspapers full of the importance of the occasion. The explosion went off but the goats, even the nearest ones, calmly continued to graze as if nothing had happened. Nothing more was ever heard of this particular explosive.

What had not been made public was that on Wood's advice the goats had been made to graze for weeks in the immediate vicinity of the big guns that were tested at Aberdeen so that noise did not startle them. While undoubtedly the worthlessness of the explosive would eventually have been demonstrated the more usual methods, Wood's way presented a shortcut which saved a great deal of time, which the explosives experts could then use for better purposes.

Wood could attack, with the same zest that he bestowed on the most important problems in physics, other questions that caught his fancy whether they were on the fringes of science or had nothing to do with it.

He started as a boy and kept it up until his death. As a student at Harvard he became interested in hallucinations. To gain firsthand experience he secured a quantity of hashish, swallowed it, and recorded in minute detail the visions and hallucinations he experienced; and he reported the results as a thesis in a course of psychology he was then taking. He also sent the account to a New York newspaper and was disappointed when it was published without sensational headlines.

As to the matter of publication, Wood was never a publicity seeker but was so interested in whatever he was doing that he felt others should be just as interested to read about it. He often sent his scientific papers simultaneously to journals in the United States, Britain, and the Continent and saw to it that the daily press was also informed. When he was very impatient, he sent communications to *Nature* by cable.

To come back to Wood's extrascientific activities: they were too numerous for all to be mentioned here, because he was interested at one time or other in practically everything. He was a skilled boomerang thrower and introduced the Hawaiian surfboard to the Long Island beaches. He traveled before 1900 through the Wisconsin countryside in one of the first automobiles seen in that part of the country. He was a gifted painter in watercolors. For a time he tried his hand at science fiction in collaboration with Arthur Train, an author of popular stories, but after getting a few stories published he gave up this sideline.

He was, however, very successful with a series of poems, first published in 1907, and probably many who never heard of Wood the physicist know him as the author of *How To Tell the Birds from the Flowers,* for this volume went through about twenty editions. He had written these nonsense verses to amuse his children and illustrated them by clever drawings.

It is not surprising that Wood with all of his scientific curiosity should be fascinated by crime detection, and he lent a hand in the solution of a number of criminal cases in New York and Baltimore.

The scientific curiosity which he showed in all matters manifested itself even when, a few years before his death, he suffered a heart attack. As these things go it was not a very severe attack, but he must have driven his doctor to distraction by the intense interest he showed in his own symptoms. It was a great disappointment to him when he realized that he was not the first one to go through this experience.

Wood always had close ties with British scientists. These dated from 1900, when he was invited to lecture on his process of color photography before the Royal Society of Arts and received at the same time an invitation for a demonstration of his photographs of sound waves before the Royal Society. In 1904, in connection with the Cambridge meeting of the British Association for the Advancement of Science, he was the guest of Lord Rayleigh at his country house and private laboratory, and there was from then on a frequent exchange of ideas between the two great scientists. In 1919 he was elected Foreign Member of the Royal Society. He received his first honorary degree from the University of Birmingham and one of his last from Oxford University, with one from the University of Edinburgh in between. He was an honorary member of the Royal Institution and of the London Optical Society and an honorary fellow of the London Physical Society. He received in 1899 a medal of the Royal Society of Arts for his invention of the diffraction color process in photography and in 1938 the Rumford Gold Medal of the Royal Society for his achievements in physical optics, a distinction which he valued perhaps most of all. The *Philosophical Magazine* and later the *Proceedings of the Royal Society* were his favorite journals for his own publications.

Thus Britain, perhaps more than any other country, showed appreciation for Wood's genius, although he did not lack distinctions in his own country and abroad. He was a member of the National Academy of Sciences in Washington, the Academia dei Lincei in Rome, the Russian Academy of Science in Leningrad, the Royal Swedish Academy in Stockholm, and the Indian Association for Science in Calcutta, among many others. The University of Berlin awarded him an honorary doctor's degree in 1934, and Johns Hopkins University honored him in the same way when in 1951 he had finished his fiftieth year as professor at that institution.

During that same year his closest associates in Baltimore honored him at an intimate dinner to which the wider circle of his friends had been invited to send messages. The messages poured in from all corners of the world. They showed how much Wood and his achievements were still in the minds of physicists.

From the foregoing, the reader will have gathered that Wood was by no means a one-sided scientist but that he had wide interests and made contributions to many diverse fields.

In his private life he was far from being a recluse. In 1892 he married Gertrude Ames, who also came from a New England family and who was his constant companion for more than sixty years, although she herself had no interest in scientific things. The Woods led a very active social life in Baltimore, at their summer place near East-hampton on Long Island, and during their travels abroad. They had a very wide and interesting circle of friends. Mrs. Wood provided the family the stability without which a man of Wood's temperament might have found life occasionally very difficult. He is survived by Mrs. Wood, their three children, and many grandchildren.

His biographer, Seabrook, called him "a small boy who never grew up," and there is probably a good deal of truth in that statement, for though he had much sophistication, his fundamental approach to everything was that of a small boy fascinated by something new that he wants to take apart so he can see what makes it work.

The secret of Wood's greatness is probably that he could recognize a problem that could be dealt with by experimental methods, that he could then reduce the experimental technique to something very simple, and that in carrying out the experiment he could distinguish the essential points from the confusing details. Physicists will look back with nostalgia to this era when one person working practically by himself could make major contributions to the frontier fields of his science, and they will think of R. W. Wood as the protagonist of this era.

Perhaps techniques in physics, like so many other things, have changed since the two world wars. Now we have become accustomed to a state of affairs where even a moderate effort to get beyond what is already known often involves the expenditure of millions of dollars and requires the cooperation of many persons, scientists, technicians, and engineers, and an organization that will make such cooperation effective. Many will think back to the times of R. W. Wood as the good old days. Perhaps there will again be persons who can make this era revive.

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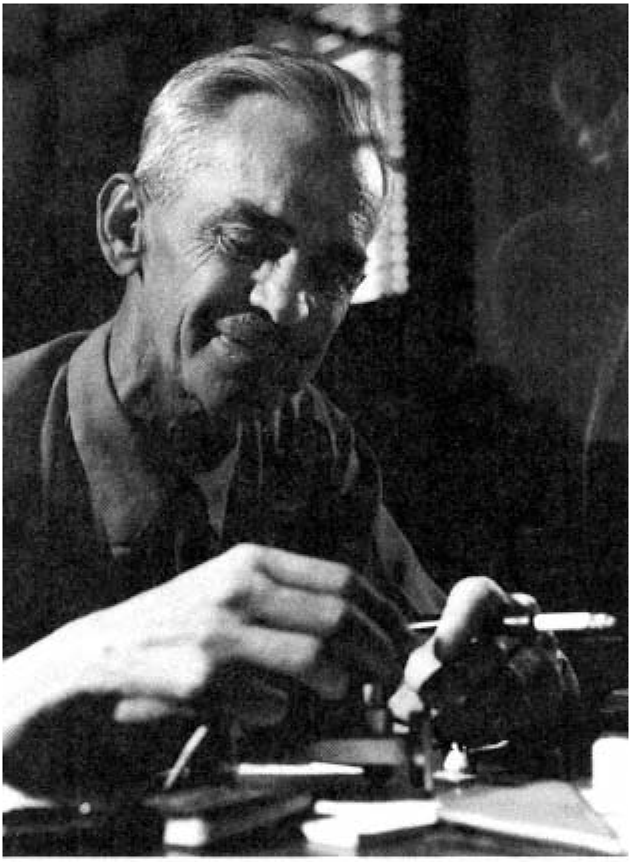

Don M Yost

DON MERLIN LEE YOST

October 30, 1893-March 27, 1977

BY JOHN S. WAUGH

Don Yost who spent his entire professional career at the California Institute of Technology, was one of the leading American chemists of the period between the two world wars. He brought to inorganic chemistry the rigor of the physical chemist's approach, following the Berkeley school and the A. A. Noyes tradition in which he had been educated. He remained in the vanguard of new developments in chemistry and physics, pioneering the exploration and applications of Raman spectroscopy, the third law of thermodynamics, chemical applications of radioactive isotopes, fast reaction kinetics, and microwave spectroscopy and magnetic resonance. He was the author of influential monographs on inorganic chemistry and on the rare earth elements.

Yost was born on a strawberry farm near the small town of Tedrow, Ohio. After a succession of moves within the middle west, the family settled permanently on a ranch near Boise, Idaho, in 1902. There Yost finished elementary and high school. In that frontier environment no formal courses in chemistry, physics, or any other science were available. However, Yost and his boyhood friends formed a radio club and designed and built working radio stations. The receivers had diode detectors made from galena crystals found in the surrounding mountains. Yost's vector to ward a career in science originated from these experiences and from an apparently unusual high school teacher of mathematics, a man named Sawyer. On graduation, it was evidently taken for granted that Yost would attend college, where he intended to pursue his interests in electricity. In private correspondence he relates, "An ex-cowboy told me that Harvard was too old fashioned, so I finally chose Berkeley." There he traveled in August 1914, by rail to Portland, Oregon, and thence by steamship to San Francisco.

In his first year at Berkeley Yost studied chemistry. The lectures were given by Joel Hildebrand, and the laboratories and recitations were conducted by Gerald E. K. Branch and Richard Chace Tolman. It is scarcely surprising that he switched his major to chemistry. He also took a minor in mathematics, a subject which was to remain an avocation throughout his life. During this period he met Susan Marguerite Sims, also a student at Berkeley. They were married on March 7, 1917, at her parents' house in Salt Lake City. Yost had dropped out of college, and when the United States entered the war he enlisted in the Navy. After the war ended he returned to Salt Lake City, where he spent a semester at the University of Utah. Their daughter Helen Marguerite was born on October 16, 1918. The family then went back to the ranch in Idaho, probably for lack of money for college.

It was not until the fall of 1921 that Yost was able to return to Berkeley. He had the opportunity to assist William C. Bray—today we would call it undergraduate research— in his monumental researches on the rare elements. On graduating in June 1923, Yost was offered a graduate fellowship at Utah by Walter Bonner, who had befriended him during his brief stay in 1918. After a year there Bonner evidently thought the young man needed broader horizons, and he helped Yost get a fellowship at Caltech. Yost wrote of the stimulating environment he found in Pasadena in 1924: ". . . the faculty in mathematics and mathematical physics were outstanding, as well as the visiting professors. H. A. Lorentz, Albert Einstein, Schroedinger, Raman and others lectured.... For a country boy from rural Idaho to be involved actively in all this very modern and highbrow scientific endeavor struck me as almost unbelievable. I enjoyed and appreciated it more than I can express."

After two years of research Yost received a Ph.D. degree, magna cum laude, in chemistry and mathematics, and was given an appointment as instructor. His son Max Cayley Yost had been born on August 17, 1927. His thesis supervisor was probably A. A. Noyes. However, no acknowledgment or other indication appears in Yost's thesis, which consists only of reprints of his four papers published in 1926 (1926,2) under a cover page with the title "The Mechanisms and Rates of Certain Oxidation-Reduction Reactions in Aqueous Solution. The Existence of Trivalent Silver."

It was still important in those days for an ambitious young scientist to study in the scientific capitals of Europe. In 1928 Yost applied for and received a Rockefeller Foundation fellowship. He spent a half a year at the University of Uppsala working on X-rays with Manne Siegbahn, followed by a half year working in the University of Berlin with Peter Pringsheim on the newly discovered Raman effect. He was especially stimulated by Pringsheim, and also by Walter Nernst, the head of the physical institute in Berlin. Both of these influences are evident in Yost's bibliography for the following twenty years.

Between his return to Caltech and the outbreak of World War II, Yost built up a very active program of research and teaching. His interests were broad: we find publications on Raman spectroscopy and low-temperature heat capaci ties, growing from seeds planted in Berlin; on X-ray absorption edges (recall Uppsala); on chemical equilibria and cell potentials, following the traditions of A. A. Noyes and of the Berkeley school of physical chemistry; and on radioactivity and neutron physics, no doubt reflecting the interest in nuclear physics and chemistry stimulated at Caltech by R. A. Millikan, R. C. Tolman, C. C. Lauritsen, and J. R. Oppenheimer. We also find a growing interest in reaction kinetics and catalysis and even in nuclear medicine. All of these varied researches, leading to perhaps fifty publications, were carried out with simple homemade apparatus; the sophisticated commercial instrumentation of today did not exist. A conspicuous quality of Yost's papers was an insistence on rigorous and quantitative characterization of chemical substances and reactions, quite different from the descriptive flavor of much work in inorganic chemistry at the time.

The chemistry of rare or "difficult" elements was always a challenge to Yost. Fluorine chemistry was no exception. Indeed, Yost earned an international reputation for his work on the volatile inorganic halides. Apparently the notion arose in the early 1930s (probably from Linus Pauling) that xenon, a "noble gas" guaranteed by all the textbooks to be chemically inert, might form chemical compounds with fluorine, the most electronegative element. Yost (who would not have used the word electronegative) and Albert L. Kaye describe in a 1933 paper a failed attempt to prepare such compounds. Neil Bartlett, who won fame many years later for preparing xenon fluorides, considers it nearly certain that such compounds must have been created under the conditions used by Yost and Kaye. We can only speculate on the reasons for their negative result on an experiment which might have had a revolutionary effect.

Late in the 1930s, after World War II began in Europe, Yost took a commission as lieutenant commander in the

U.S. Naval Reserve. In 1940 he applied for a government contract—the first of several—to pursue research on chemical warfare at Caltech under the National Defense Research Committee (NDRC). After the United States entered the war, and the NDRC gave way to the Office of Scientific Research and Development, Yost became steadily more involved and responsible for a larger and larger program, supervising research teams at Caltech and Northwestern University and maintaining liaison with groups in the United Kingdom. These efforts were ultimately to win him a Presidential Certificate of Merit. Then, in July of 1943, he abruptly quit this work. The reasons are not completely clear; there had been squabbles with government accountants over reimbursement of minor travel expenses, and one of these may have provided the trigger. At about the same time Yost was struck by osteomyelitis of the jaw, which might have ended his life except for successful efforts to obtain for him the newly developed antibiotic penicillin. Even with treatment the disease persisted for months, causing great pain and robbing Yost of most of his physical vigor. His intellectual effort, however, continued unabated. This was the period in which he published, with Horace Russell, his book *Systematic Inorganic Chemistry* and wrote sections of a projected text, "Advanced Inorganic Chemistry," which was never completed. With Russell and Clifford S. Garner he wrote the influential book *The Rare-Earth Elements and Their Compounds,* which was published in 1947. The two books were regarded for years as authoritative critical treatises on their subjects. (It is interesting that this should have been so of the rare earth book, in view of the fact that Yost's bibliography contains not a single paper on any of the lanthanides up to that time.) Yost was elected to the National Academy of Sciences in 1944. In July 1945, Yost again became involved in war research, this time under the Manhattan Project. The work was connected with high explosives and was conducted at Caltech and Los Alamos.

In the years after the war, Yost returned to basic research. He still maintained an interest in reaction kinetics, especially as studied by radioactive tracer methods, but he also saw the promise of the new spectroscopies at radio frequencies. His last students worked in the fields of microwave spectroscopy of gases, nuclear magnetic resonance, and electron spin resonance. He remained physically frail, and his vision and mobility suffered further as a result of two cataract operations. During convalescences he kept in touch with his students through long and far-ranging afternoon conversations at his home (a house on San Pasqual Street near the campus, no longer standing, which had earlier belonged to A. A. Noyes). When on the campus he spent his time reading or experimenting with war surplus electronics in his office. Visitors would be greeted by a slight, cheerful figure, always dressed in a neatly pressed blue tweed suit, blue shirt, and red and blue striped tie, shading his eyes with one hand and waving either a cigarette or a six shooter with the other.

Always a supremely independent person, Yost refused to adapt to the new style and scale of scientific research that followed the war. He would not accept government grants or contracts, likely because of unpleasant experiences with bureaucrats during the war. In a 1950 letter to Kenneth Pitzer, then Director of Research of the Atomic Energy Commission, he wrote, ". .. for a working scientist the cost of research contracts in self respect and equanimity is great, but the incentive is uninspiring." He sneered at the new fashion for "gang research." Toward the end of his career he had only a small grant from the Newmont Mining Company; sometimes he would secretly buy supplies for his students out of his own pocket. He withdrew from active participation in departmental affairs at Caltech, and he had little good to say of the research in structural chemistry which was flourishing there under Pauling and his school.

In his last years, especially after his retirement in 1964, Yost's scholarly interests expressed themselves in an avocation for mathematics. He was perhaps the first student in fifty years to devote himself to the algebra of quaternions and the properties of discriminants, catalecticants, and evectants. While he published little (only a short encyclopedia article on quaternions), he gave rein to his thoughts on these and a variety of other subjects in an informal but intellectual correspondence with members of the Iron Nail Club. This was a society of his own invention, composed of former' scientific colleagues and personal friends, each of whom was given a nom de plume (Yost was Cisco, his boyhood friend Ivan Nelson from Boise was Pancho, I was Currito, etc.). He also became famous for his perceptive and colorfully written book reviews, which often were vehicles for his views on science and society. In a 1950 review of *The Transuranium Elements* by Seaborg, Katz, and Manning, he wrote:

> ... the reader may well pause to reflect that the books are of little use to anyone except those few having access to the materials described; there is no normal, healthy way to check the many measurements and statements made. This field of scientific endeavor is highly monopolized; and extreme monopolization, like compartmentalization, is one of the sordid forms of state controlled enterprise. It is not free, and is not in the spirit of that part of the Atomic Energy Act quoted by Mr. [David] Lilienthal in his foreword. Accordingly, the reader, after further charitable meditation, could justly class the whole content of the books along with W. C. Fields' fabulous, three legged ostrich. And if it were not for the sobering undercurrent of both pleasant and unlovely fact now enmeshed with the new elements, which would require the deep insight of another Saint Jerome to evaluate, he, the reader, might well dismiss the whole matter from his mind and, with Shakesepeare, say "Mucho Ruido y Pocas Nueces."

Abbreviated references to those book reviews that could be found appear at the end of the bibliography.

Yost's lecturing style in the undergraduate classroom was awkward and uninspiring, and in later years he tended to hand these lectures off to his postdoctoral associates. However, he had a strong effect on his research students. His own palpable independence encouraged them to pursue their own ideas, learn from their own mistakes, and take credit for their successes. When they needed help, he would provide it or point the way toward someone who could. At other times his conversation tended to avoid the research at hand and range over a variety of subjects, scientific, historical, political, and personal. At times he seemed to speak obliquely or in parables. His students were most influenced in the long run by his scientific taste, his wit, and his utter intolerance of pretense. He was called by one colleague "the foremost anti-stuffed-shirt in American Science."

I am indebted to Terry Cole, to Verner and Judy Schomaker, and to the California Institute of Technology archivists for their assistance.

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