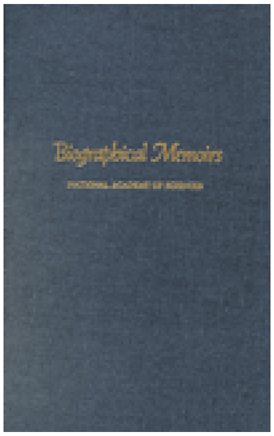

Biographical Memoirs V.77

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Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

Biographical Memoirs

VOLUME 77

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

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PREFACE

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.

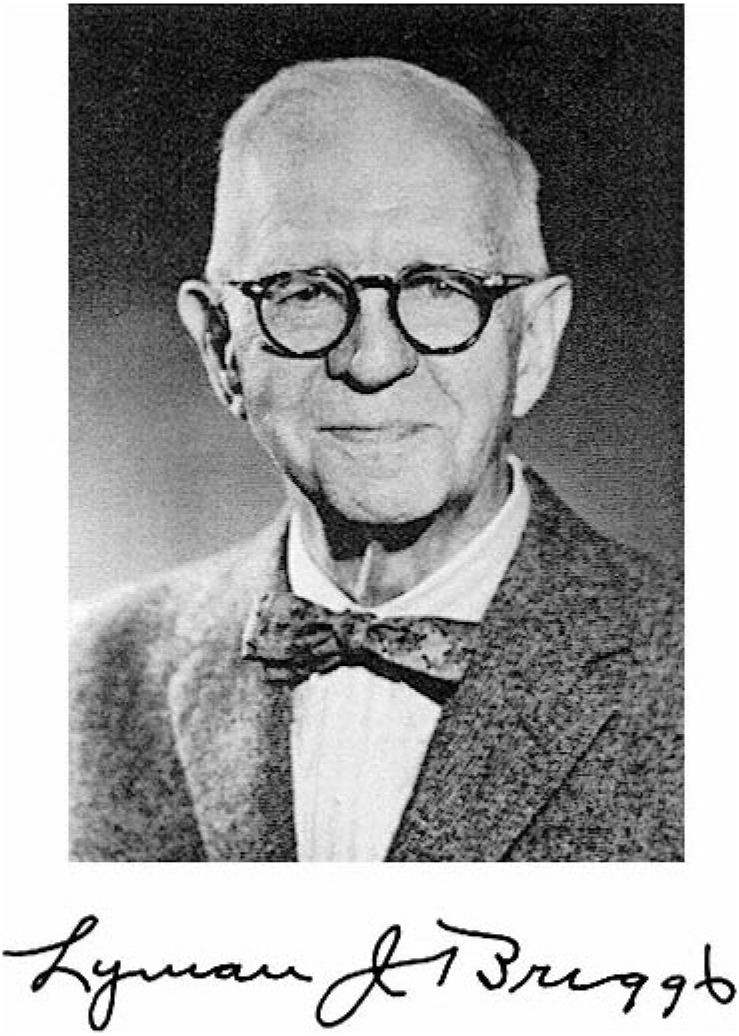
The *Biographical Memoirs*, begun in 1877, are a series of volumes containing the life histories and selected bibliographies of deceased members of the Academy. Colleagues familiar with the discipline and the subject's work prepare the essays. These volumes, then, contain a record of the life and work 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.

R. STEPHEN BERRY

Home Secretary

Biographical Memoirs

VOLUME 77



LYMAN JAMES BRIGGS

May 7, 1874–March 25, 1963

BY PETER BRIGGS MYERS AND JOHANNA M. H. LEVELT SENGERS

LYMAN JAMES BRIGGS APPEARED on the Washington scene at a time when the physical sciences, especially physics, were about to expand dramatically. His graduate professor at Johns Hopkins University, Henry A. Rowland, once commented to him that they were living in the final years when one individual could be knowledgeable across the field of physics. My (P.B.M.) grandfather became a scientist in the age when it was still possible for one person to make seminal contributions in diverse areas of science. The breadth of interests and contributions of Lyman Briggs, however, was exceptional even in his days, reaching from soil and plant science to atomic physics, from navigation to aerodynamics, and to the instrumentation of stratospheric balloons. For forty-nine years he was a civil servant in the federal government. The most important characteristic of his work was the systematic application of the principles and methods of physics in a variety of other fields. During his tenure (1933–45) as the third director of the National Bureau of Standards (NBS), he amply demonstrated his skills as an administrator and director of research in the years of the Great Depression and the Second World War. He chaired the top-secret Uranium Committee, which evolved into the Manhattan

Project for constructing the atomic bomb. In an unprecedented action, Briggs was appointed director emeritus of NBS at his retirement in 1945. He returned to research and to geographical exploration, while continuing as a gifted and prolific writer on scientific topics for science magazines.

PERSONAL HISTORY

Lyman James Briggs was born on May 7, 1874, on a farm in Assyria, Michigan, 12 miles north of Battle Creek. His childhood and early education are best told from a handwritten autobiography I (P.B.M.) persuaded him to write in 1952, when he was seventy-eight years old:

> Grandfather Briggs gave the land on which to build the district school and the Methodist Church. Through Grandfather, my forbears go back to Clement Briggs, who came to the Plymouth colony on the *Fortune* in 1621, the first ship to follow the *Mayflower*.
>
> My father, Chauncey Lewis Briggs, was the eldest of eight children. He went to the Briggs school built by my grandfather and later taught there, the same school that my younger brother Clifton and I were later to attend.
>
> My boyhood life on the farm was a happy outdoor life full of interesting things. I was early made to realize that the privilege of membership in my family entailed certain duties and responsibilities. Derelictions led to smarting consequences. Happily there were few, for I seemed to have sensed early the basic soundness of responsibility. It was the duty of my brother and myself to feed the chickens and the pigs; to gather the eggs; to keep the kitchen wood box filled; to drive the cows to pasture; to go to the post office for mail; and to market eggs at the country crossroads store in exchange for groceries my mother specified. These things were to be done first without prompting. Then we could play.
>
> In winter nearly every boy had a "schooner" for sliding down hill. It consisted of a stout oaken barrel stave to which was nailed a short upright, topped by a short crossboard for a seat. We became very expert in riding these schooners, going down a long hill like the wind, without once touching our feet. At one time my popularity in school was unchallenged, for I was the sole owner of the staves from a defunct vinegar barrel.

Lyman's father, who had served in the Union Army's Corps of Engineers during the Civil War, persuaded him to attend Michigan Agricultural College (now Michigan State University) in East Lansing, which he entered by examination in 1889 at the age of fifteen. Although he majored in agriculture, his interests centered on mechanical engineering and later physics. In his own words:

> Since this was a Land Grant college, courses were given both in agriculture and the mechanic arts. I elected the former, because I thought it would please my father. But by the end of the college year my interest had swung so strongly to the physical sciences that I wished I had elected mechanical engineering. It was too late to do so, however, and I still maintain my membership in the class of '93, to which I had become deeply attached. In later years I was glad I did not make the change, for I learned something of geology, physiology, biology, entomology, and botany, which otherwise I would not have acquired. In addition, I took all the courses in chemistry and mathematics that were available. But my absorbing interest was in physics. From the moment I saw the great glass cases in the physical laboratory filled with marvelous apparatus I knew I wanted to be a physicist.

After graduating second in his class in 1893 he went to the University of Michigan, where he received a masters degree in physics in 1895.

PROFESSIONAL HISTORY

In the autumn of 1895, Lyman Briggs entered Johns Hopkins University for further graduate study in the Ph.D. program in physics. While still an undergraduate, he had fallen in love with Katharine Cook, daughter of A. J. Cook, professor of entomology at Michigan Agricultural College. His desire to marry Katharine led him to secure a research position with the Department of Agriculture in Washington, D.C., in 1896; they were married the same year. While working in Washington, he traveled to Baltimore three times a week to pursue his research with the great Henry Rowland.

He spent appreciable time studying the newly discovered Roentgen rays, but he chose an agricultural topic for his thesis research and received his doctorate in 1903 with a dissertation entitled "On the absorption of water vapor and of certain salts in aqueous solution by quartz." That same year he was elected to the Cosmos Club at the age of twenty-nine.

In his first professional position as a physicist, in charge of the Physics Laboratory Division, later the Bureau of Soils, in the U.S. Department of Agriculture, he was one of the new breed of interdisciplinary scientists who made use of a much more varied background in order to understand the biology and ecology of plant life in terms of the physics involved. His research work at the Department of Agriculture concentrated on characterization of water retention by soil (1907, 1910). He was one of the founders of the science of soil physics and devised a soil classification technique based on centrifuging, which is still a standard method. He organized a biophysical laboratory in 1906, which later became the Bureau of Plant Industry. With H. L. Shantz (1911, 1912), he studied the effect of environmental factors on water uptake by plants, and became one of our early ecologists. Briggs thus pioneered the application of scientific methods and instrumentation to agricultural research.

Wars were to play a major role in defining Briggs's career. Mobilization pressures from World War I resulted in an Executive Order detailing him from the Department of" Agriculture to the Department of Commerce's Bureau of Standards in 1917. He was set to work on two topics: a stable zenith instrument for the Navy and the design and construction of a wind tunnel for aeronautical research. In a short time, he was successful on both scores, apparently having had no difficulty with this sudden change of field of research.

With John F. Hayford, he developed a gyroscopic instrument for maintaining an artificial horizon below deck as an aid in directing gunfire from battleships. The stable zenith instrument was used to synchronize the training of big guns, to point them in the direction of the target with the proper elevation. It was necessary to establish an artificial horizon, independent of pitch and roll of the ship, and then, by observation of the motion of the ship, the crew had to be informed when to fire. A model was developed, and installed in one of the battleships. Eventually, it was installed in the control rooms of all the main ships of the line. The confidential Hayford-Briggs report, turned over to the Navy, unfortunately was never published.

In 1920 Briggs resigned from the Department of Agriculture and permanently joined the Bureau of Standards. He became chief of the Engineering Physics Division (later the Mechanics and Sound Division). He appointed Hugh L. Dryden to head the Aerodynamics Physics Section, and together they did some of the best early research in aerodynamics in the nation. Their special study of the characteristics of airfoils in airstreams moving at speeds approaching the speed of sound (1925) found application in determining the blade form of aircraft propellers. Briggs was a member of the National Advisory Committee for Aeronautics from 1933 to 1945.

Retaining his interest in navigation, he invented, with Paul R. Heyl, the earth inductor compass (1922). The Heyl-Briggs earth inductor compass worked by spinning an electric coil in the magnetic field of the Earth, thereby determining the bearing of an airplane in relation to the Earth's magnetic field. For this invention, they received the Magellan Medal of the American Philosophical Society in 1922. A compass of this type was used by Admiral Byrd in his flight to the North Pole, and by Charles Lindbergh on his flight across the Atlantic in 1927.

Briggs's character and his mode of operation in these early happy years at the Bureau are well described in *Measures for Progress*[1] from which we cite:

> Dr. Briggs was of slight, slender build and of warm, affectionate, and unfailingly kind demeanor and manner. [The first Bureau director], Dr. [Samuel W.] Stratton, when harassed by demands upon his time and attention or in a stormy mood, often sought out Briggs's company in his laboratory in West building, for as he once said, "You always have something nice to report to me and I appreciate it. These other fellows give me a lot of trouble." The "something nice" was usually a new and ingenious piece of apparatus or testing device, for, like Stratton, Dr. Briggs was strongly mechanical and an inveterate tinkerer. When he came from the Department of Agriculture, he brought with him his mechanic, Mr. Cottrell, and for years the two designed and constructed many special devices that Briggs used in his measurement studies. His laboratory was a wonderful clutter of apparatus in various stages of assembly, a tangle of piping and tubing and ticking instruments, but it was comfortable and a tranquil spirit filled it.
>
> His serenity of temper was Dr. Briggs outstanding characteristic, and he was to have a need of it under the frustrations of the depression years and the pressures and harassments of security in World War II.

In 1926 Bureau Director George Burgess appointed Briggs as the Bureau's assistant director for research and testing. On Burgess's death in 1932, Briggs was nominated to the position of director of the Bureau by President Herbert C. Hoover, but none of the president's nominations during the last session were acted upon by the Senate. He was renominated by President Franklin D. Roosevelt after the election. When Roosevelt was pressed to name "a good Democrat" to the office, he is said to have replied, I haven't the slightest idea whether Briggs is a Republican or a Democrat; all I know is that he is the best qualified man for the job."[1] Prior to his confirmation, however, the pressures of the Depression had caused a budget reduction of 50% at

NBS, so it was hard to imagine a less auspicious beginning for a new director. A major managerial and diplomatic effort on Briggs's part resulted in retention of about two-thirds of the career employees. He put many of his staff on part-time employment. Others continued their work at the Bureau as employees of the American Standards Association. He emphasized programs with direct economic relevance, including research in building materials and low-cost housing. He tried to convince the administration of the role of basic scientific research in furthering economic growth. The Bureau's important Mathematical Tables Project had its origin in the Depression when Briggs persuaded the Works Progress Administration to provide funds for unemployed mathematicians in the New York City area. Appropriations began to rise by the mid-1930s, due to Briggs's persuasive power with Congress and with the administration, and quite a few of the employees dismissed earlier were rehired.

As director of the Bureau, in 1934 Briggs succeeded in restoration of its original name, which had been lost in 1903: the *National* Bureau of Standards (NBS).

When war began to threaten Europe in 1939, Briggs sent Secretary of Commerce Daniel C. Roper a list of services NBS could provide in case of an armed conflict. At that time, the Danish physicist. Niels Bohr alerted American scientists to the discovery of fission of uranium by Otto Hahn and Fritz Strassmann in Berlin, as confirmed by Lise Meitner and Otto Robert Frisch, then refugees in Scandinavia. The fissionable uranium isotope could possibly be used to make a bomb. American scientists persuaded Albert Einstein to write a letter to President Roosevelt about this possibility. After receiving Einstein's letter of advocacy in October 1939, Roosevelt asked Briggs to head and organize a top-secret project, the Uranium Committee, to investigate the possi bility of using the energy generated by atomic fission of uranium. At that time, it was not even known which uranium isotopes could be split by slow neutrons, and the possibility of a chain reaction remained to be demonstrated. The first meeting of the Uranium Committee, chaired by Briggs, took place at NBS on October 21, 1939, with Eugene P. Wigner, Leo Szilard, and Edward Teller amongst the attendees. In 1940 the Uranium Committee became the S-1 Committee, chaired by Briggs, of the newly founded National Defense Research Committee. The S-1 Committee met regularly at NBS, and its work greatly expanded. Initial work on separating, purifying, and characterizing uranium isotopes was done at NBS, where about sixty civil servants were engaged in the project. In 1941 Briggs recommended an all-out effort to President Roosevelt, thus giving impetus to the Manhattan Project.

Meanwhile, other major defense assignments were accepted by Briggs, so that by 1942, 90% of the work at NBS was classified. It included the non-rotating proximity fuze; guided missile development; establishment of a Radio Propagation Laboratory; critical materials research on optical glass, for which Germany had been the sole supplier, and on quartz and synthetic rubber; and measurement and calibration services. To Briggs fell the unenviable task of modifying the Bureau's culture from open access and communication to secrecy.

In 1948 Briggs received the Medal of Merit from the President of the United States for his distinguished work in connection with World War II. On request of Secretary of Commerce Henry A. Wallace in 1945, he wrote a 180-page account on NBS war research (1949).

Briggs was elected to the National Academy of Sciences in 1942. The election biography lists his membership and/or presidency of over a dozen scientific associations, his recipiency of the Magellan Medal, and his scientific papers in soil science, aerodynamics, stratospheric research, and standards of measurement.

Upon his retirement in 1945, at the age of seventy-two, Briggs was appointed director emeritus of NBS after forty-nine years of' service in the federal government. NBS employees through their Employees Welfare Association erected a bronze sundial in his honor. At his explicit request, however, the names of the first *three* directors of NBS were cast onto the rim of the instrument: Samuel Wesley Stratton, George Kimball Burgess, and Lyman James Briggs.[3] The sundial presently graces the courtyard of the National Institute of Standards and Technology.

LATER YEARS

Lyman and Katharine Briggs had two children, a daughter Isabel, my (P.B.M.) mother, and a son Albert who died in infancy. I spent a month every summer with my grandparents in Washington, and have fond memories, as a child, of Sunday afternoons at the Bureau of Standards looking at exhibits and exploring the underground steam tunnels that connected the various buildings at the Bureau. In games of billiards with my grandfather I was introduced to the laws of physics and the beauty of classical music, two of his favorite hobbies and sources of relaxation. In 1942 I lived with my grandparents while attending George Washington University. My grandfather and I would canoe down the Potomac River to open-air concerts at the Watergate near the Lincoln Memorial.

My grandfather loved to tell stories and often used an appropriate anecdote to make a point. To help defuse a strained or angry situation, he would tell the story of an elderly gentleman, traveling across the country by train, who became incensed at breakfast when the dining-car stew ard told him there were no figs on board. "But your menu says clearly "fresh figs for breakfast," the gentleman insisted, "and I want fresh figs!" The steward was unable to persuade him to make another choice; as they were disputing, the train slowed for a station stop. The steward excused himself and disappeared; amazingly, he returned in a short time with a dish of fresh figs that he had managed to buy while the train was stopped in the station. He presented the dish with a flourish, only to be told, "Take them away, I prefer to be angry!"

After his retirement, Briggs happily returned to research and the pursuit of his diverse interests. At NBS, he established a laboratory for studying fluids under negative pressure. The choice of this topic was directly related to his early interest in water uptake by plants. He used a centrifuge, which had been his instrument of choice in his studies on water content of soils in the early 1900s, to study the stability of columns of liquids in fine capillaries subjected to tension. He measured the negative pressure (or tension) at which the column would break. At room temperature, the maximum attainable tension in water was over 250 bar, and in mercury close to 500 bar (1950–53). The 1950 paper on limiting negative pressure in water is a classic and is still cited regularly in the literature on metastable water.

Another remarkable piece of research was triggered by his lifelong passion for baseball. During the war, he had set up measurement methods for the performance of baseballs with government-mandated reduced rubber content and had demonstrated that the complaints voiced about the cork-filled surrogates were valid (1945). The question he now addressed was: is it possible to pitch the ball so that it curves away from the plane of the pitch? By using his 1917 wind tunnel, and with collaboration of two pitchers from the Washington Senators baseball club, he carefully studied the effect of spin and speed on the trajectory and established the relationship between amount of curvature and the spin of the ball. To measure the spin, he attached a light-weight tape to the ball and counted the number of completed turns in the twisted tape (1959).[1] The newspaper accounts of this work made it the most widely known of all his achievements.

The National Geographic Society had always been one of his interests. In 1934, while he chaired the society's Committee on Research and Exploration, he provided the instrumentation for two stratospheric balloon flights. The first flight was aborted, but the second broke a world record for altitude (1936) that was maintained until 1951. After retirement he became even more active in the society, and led the expedition to study the solar eclipse in Brazil in 1947. A prolific author, Briggs frequently wrote articles for the *National Geographic Magazine*, including a memorable story on carbon dating published in 1958, when he was eighty-four years old. At the Cosmos Club and in Washington science circles, he is fondly remembered for the exceptional range of his interests and for his many contributions to scientific research.

The significance of Briggs's life is best summarized by quoting a statement by his successor as director of the Bureau, Edward U. Condon:[2] "Briggs should always be remembered as one of the great figures in Washington during the first half of the century, when the federal government was slowly and stumblingly groping towards a realization of the important role [that] science must play in the full future development of human society."

KARMA BEAL, ARCHIVIST at the National Institute of Standards and Technology, formerly the National Bureau of Standards, made available to us material from NIST archives, including newspaper clippings

and biographical material assembled at NIST for special occasions, such as Briggs's retirement, his eightieth birthday, and the centenary of his birth.[2] A history of the National Bureau of Standards by Rexmond C. Cochrane[1] contains much biographical material on Briggs and his tenure at NBS, while NIST archives contain the original notes about the interview with Briggs conducted by Cochrane in 1961. The special issue of *Scientific Monthly* (May 1954) contains a biography by Wallace R. Brode (NBS) with a list of selected publications by Briggs. His former collaborators in soil science, atomic energy, aerodynamics, gravity, and geographic exploration wrote articles highlighting Briggs's contributions to these fields.[3] We made ample use of the biography written by Allen V. Astin on the occasion of the centennial celebration of Briggs's birth.[4] Karma Beal meticulously read the manuscript for accuracy of facts. Judson French made many suggestions for improvement of the manuscript.

NOTES

1. R. C. Cochrane. *Measures for Progress, a History of the National Bureau of Standards*. Washington, D.C.: U.S. Government Printing Office, 1966, 1974.

2. Narrative drawn from a centennial exhibit commemorating the 100th anniversary of the birth of Lyman J. Briggs. NIST archives, 1974.

3. *Sci. Mon.* May 7, 1954. Articles by W. R. Brode: "Lyman J. Briggs: Recognition of his eightieth birthday," pp. 269–74; V. Bush: "Lyman J. Briggs and atomic energy," pp. 275–77; E. C. Crittenden: "Some problems in national and international standards," pp. 278–82; N. E. Dorsey: "Spontaneous freezing of water," pp. 283–88; H. L. Dryden: "Supersonic travel within the last two hundred years," pp. 289–95; G. Grosvenor: "Earth, sea and sky: Twenty years of exploration by the National Geographic Society," pp. 296–302; P. R. Heyl: "Gravitation, still a mystery," pp. 303–306; L. A. Richards: "The measurement of soil water in relation to plant requirements," pp. 307–13.

4. A. V. Astin. Lyman James Briggs, 1874–1963. *Cosmos Club Bull.* March 1977. A manuscript of this title is in the NIST archives, but it differs in detail from the published version.

SELECTED HONORS AND DISTINCTIONS

1916 President, Philosophical Society of Washington

1917 President, Washington Academy of Sciences

1932 Chairman, Federal Specifications Board

1933–1939 Chairman, Federal Fire Council

1933–1945 Director, National Bureau of Standards

1933–1964 Life Trustee, National Geographic Society

1934, 1935 Chairman, Special Advisory Committee for Stratospheric Balloon Flights

1935 Chairman, National Conference on Weights and Measures

1938 President, American Physical Society

1939 Chairman, Uranium Committee S-1 of the National Defense Research Committee

1937 Chairman, Research Committee of the National Geographic Society

1942 Vice-chairman, National Advisory Committee for Aeronautics

1945–1963 Director Emeritus, National Bureau of Standards

Briggs was awarded honorary doctorates by the following institutions:

1932 Science, Michigan State College

1935 Engineering, South Dakota School of Mines

1936 Law, University of Michigan.

1937 Science, George Washington University

1939 Science, Georgetown University

Science, Columbia University

Briggs received the following honors:

1922 Magellan Medal, American Philosophical Society

1942 Elected Member, National Academy of Sciences

1948 Medal of Merit by President Truman

1954, 1962 Franklin R. Burr Award, National Geographic Society

Briggs at various times served as president of:
American Physical Society
Washington Academy of Sciences

Philosophical Society of Washington
Cosmos Club, Washington, D.C.
Federal Club, Washington, D.C.

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1959 Effect of spin and speed on the lateral deflection (curve) of a baseball and the Magnus effect for smooth spheres. *Am. J. Phys.* 27:589–96.

LIQUIDS UNDER TENSION

1950 Limiting negative pressure of water. *J. Appl. Phys.* 21:721–22.
1953 The limiting negative pressure of mercury in Pyrex glass. *J. Appl. Phys.* 24:488–90.

Courtesy of Harvard University Archives

ROGER WILLIAM BROWN

April 14, 1925–December 11, 1997

BY JEROME KAGAN

ROGER BROWN COMBINED a remarkably creative mind, natural gentleness, and a passion for language into a persona marked by generosity, unfeigned humility, and a dazzling writing talent. Although he was the father of developmental psycholinguistics—Roger's students dominate this domain of inquiry—he moved away from the brightest spot on the stage to share credit with his students in order to promote their young careers. The title of his chair, "The John Lindsley Professor of Psychology in the memory of William James," was especially apt for a psychologist who probed the subjective frame with a sureness that reflected his faith in the validity of personal experience as a primary source of evidence. There is the hint of a paradox in Roger's wariness of Platonic abstractions that floated too far from their evidential origins. Roger loved language, but he distrusted words.

It is certain that biographers are most likely to select his summary of the first stage of the acquisition of English as his seminal work, while simultaneously praising his ability to attract so many bright scholars to the study of language development. Yet, his first teaching assignment in 1952 was as a social psychologist at Harvard University. Roger had good reason to be proud of his 1986 text *Social Psychology: The Second Edition*. Although colleagues regarded it as the most original in the field, instructors did not know how to use a book that was punctuated so frequently with the author's personal feelings—it broke too sharply with formulaic rules. The chapter on "Impressions of Personality," for example, contains a section that only Roger could write. After telling the reader that he was on a spring break in Nassau, he adds, "I think of myself as extremely nonracist and would unhesitatingly check that position on a self-descriptive inventory. . . . At the University Health Service, if I am assigned to a Pakistani woman physician with a diamond in her nostril, I do not flinch."

The uncanny ability to devise experiments that probed phenomena everyone had experienced but no one investigated was one of the most colorful threads in the elegant tapestry of his career. Two of the most famous were the study of the exquisitely detailed memories each of us has of events that were accompanied by an acute, strong emotion, and the frustration that accompanies the inability to retrieve a familiar word—often called "tip of the tongue." Roger explored the former phenomenon with James Kulik by asking informants of different ethnic groups to remember exactly where they were and what they were doing the moment they heard about some newsworthy event, like the assassinations of John F. Kennedy and Medgar Evers. Roger used the phrase "flash bulb memories" to convey the incredible detail contained in these episodic retrievals by those for whom the event was emotionally relevant. He even speculated on the evolutionary advantage this competence conferred on our species.

With David McNeill, Roger asked students to guess the word they were searching for when they were in the "tip of the tongue" state. Many of these guesses were near misses that had the same first letter, same number of syllables, and same syllabic stress as the correct word, suggesting that nonsemantic features are stored as inherent components of every word. A person searching for the word "sextant," for example, might guess secant, sextet, or sexton.

Although his fans, students, and colleagues alike admired his discoveries and smiled at his graceful sentences, fewer have commented on the moral authority he brought to academic settings. I watched Roger year after year listen quietly during a heated faculty discussion on a controversial issue, and then, at the perfect time, in a pace neither hurried nor hesitant, express his opinion. Usually his comment settled the issue as he had suggested. The reason for Roger's extraordinary persuasiveness was that his colleagues had awarded him the position of representing the unselfish voice. All of us understood that his remarks were not motivated by self-aggrandizement, but by his notion of what was best for faculty, students, and university. This role is not an automatic addition to the respect that follows fame, prizes, publications, and high intelligence. It is awarded—and rarely—by colleagues who have been pleasantly surprised by the fact that someone among them can be trusted.

Roger William Brown was born on April 14, 1925, in Detroit, Michigan, into a family of four brothers, together with two sons of the elder brother, that, by the time he was ready for school, began to suffer from the economic distress of the depression. Roger attended the Detroit public schools and, on graduation from high school, thought about becoming a novelist of social protest. He enrolled in the Navy during his first year at the University of Michigan. He was accepted into the V-12 Program, went first to Oberlin College and then to midshipmen training at Columbia University. Roger witnessed the battle of Okinawa and was on the first ship to enter Nagasaki harbor following the explosion of the atomic bomb. But there were many quiet days and nights at sea and much time to read. John B. Watson's book *Behaviorism*, which had attracted other budding novelists to psychology—B. F. Skinner included—also tempted Roger to replace a career in writing with psychology. He earned his bachelor's, master's, and doctoral degrees at the University of Michigan, the last in 1952. Although his doctoral research was on the social psychology of the authoritarian personality, a popular topic after the Second World War, the retention of a special curiosity about language rendered him receptive to chance events. He remembered—perhaps a flashbulb memory—a lecture on the phoneme by Charles Fries that fit his motivation perfectly. Uncertain about his talent as a novelist and discouraged by the complexity of the phenomena of social psychology, he discovered that an understanding of language appeared to possess the complexity and amenability to empirical inquiry that made it a perfect place to exploit his intellectual gifts and strongest passions.

Roger joined the faculty of Harvard University as an instructor in 1952 and was promoted to assistant professor the following year. He taught undergraduate courses in social psychology and language and became a participant in a research group on cognitive processes led by Jerome Bruner. The concepts of artificial intelligence, language, and cognition were replacing the antimentalist views of the behaviorists, and there was a heady excitement over mind and language swirling around MIT and Harvard in the mid-1950s. Listen to Roger describe the ambiance:

> Come with me, if you will, to Harvard and MIT in the early 1950s. American linguistics is still structural; Noam Chomsky is a junior fellow at Harvard, and we are all unaware of the surprise he is preparing. Skinner's behaviorism is strong at Harvard, George Miller is still interested in communication theory, and there is a lot of excitement in Jerome Bruner's cognition project

about the work on concept formation that would be described in *The Study of Thinking*.

In addition, Herbert Simon, Alan Newell, Marvin Minsky, and John McCarthy were applying the ideas of artificial intelligence to the study of mind. The new legitimacy of mind, thought, and language supported Roger's intellectual interests, and he and Eric Lenneberg, in one of their first collaborations, tested the highly publicized view of Benjamin Lee Whorf that the language of a community must influence the way the speakers remember and reason. Although these two young scholars found some initial support for that idea in a study of names for colors, four decades later Roger changed his mind following research by Brent Berlin and Paul Kay on the color words used by other cultures and a study by Eleanor Rosch-Heider on the Dani of New Guinea suggesting that some colors are naturally salient because of the brain's construction, rather than because of their names. The Dani showed good recognition memory for colors that their language did not name.

Roger wrote his first synthesis of the relations between language and mind during a sabbatical year in 1957. *Words and Things* was the first book on the psychology of language that was an obvious heir of this first stage of the cognitive revolution—and it remains in print forty years later. The ten chapters dealt with the nature of meaning, the relation between language and thought, and even the possibility that phonemes have symbolic connotations—for example, why most people feel that the word "ching" should mean light, while "chung" probably means heavy. Steven Pinker, one of Roger's students, admitted that *Words and Things* was one of the inspirations for Pinker's own book *The Language Instinct*.

The year Roger wrote *Words and Things* also marked the year he moved to MIT to teach popular courses on social psychology and language. Roger returned to Harvard in 1962 with the title of professor of social psychology and a five-year grant from the National Institute of Mental Health to study the language acquisition of three children he and his students named Adam, Eve, and Sarah. The strategy of this study was Baconian in the extreme, for it consisted of the careful collection, transcription, and analysis of a large corpus of spontaneous conversations between each mother and her child in the naturalness of the home setting. It turned out that Roger was wise to gather extensive data on a few subjects rather than more superficial information from a larger group. The extensive protocols were discussed with a gifted group of young colleagues that included Ursula Bellugi, Colin Fraser, Courtney Cazden, Jean Berko Gleason, David McNeill, Dan Slobin, Sam Anderson, Richard Cromer, and Gordon Finley. *A First Language: The Early Stages*, published by Harvard University Press in 1973, generated a large number of provocative generalizations, even though it did not explain completely how children acquired language. Although everyone knew that children acquire language at different rates, the data revealed that these rates appear more uniform when each child's attainment of a syntactic milestone is related to the average length of the child's utterances; that is, the mean number of morphemes in a large number of statements. This measure—the "mean length of utterance"—has become a standard index of a child's status in acquiring English.

Roger and his students also discovered that although children use abbreviated utterances like "dolly sit" or "hit cup," their minds hold a complex proposition. The problem is that the young child cannot express the whole idea in language. Roger also detected remarkable uniformity in the order of acquisition of English morphemes. For example, the *s* inflection on verbs—come versus comes—is acquired later than the s inflection on nouns. Tag questions (He can play, can't he?) emerge much later in development because these sentences require the grammatically complex processes of inversion, negation, and ellipsis. Most important for theory, the protocols revealed that parents do not praise sentences that are syntactically correct nor criticize those that are grammatically wrong. Rather, most parents react primarily to whether the child's sentence is true or false. That discovery meant that Skinner's contention, popular at the time, that grammar was learned in accord with the principles of reward and punishment had to be incorrect. Children learned the syntax of their language by relying on cognitive abilities, still poorly understood, with processes that remain a central puzzle in developmental psycholinguistics.

Roger spent a sabbatical year—it turned out unsuccessfully—trying to compose a summary of the later stages of language acquisition. The analyses of the older children's speech did not yield insights with the power of those inferred from the first stage. Roger decided there was little value in publishing the idiosyncratic details contained in the grammars that required so many hours of work. Equally important, linguistic theory had begun to expand and Roger admitted to some reluctance over devoting the time required to learn the new formalisms. He once noted, I have worked in different areas because I like beginnings, times when the curve of knowledge is rising steeply, when chunks of intellectual gold still lie on the surface to be discovered by whoever looks first. When the incremental curve levels off and new discoveries become hard to make, I tend to look elsewhere." Fortunately, his students, including Jill de Villiers, Helen Tager-Flusberg, and Kenji Hakuta, are now active in this domain.

Roger served as the last chairman of Harvard's Depart ment of Social Relations from 1967 to 1970. This was the time of student rebellion against the Vietnam War and elitism in the academy, and Roger could not avoid confrontations with students who transferred their unbound anger at society to anyone in a position of authority. I remember sitting in a large auditorium the afternoon Roger was speaking to hundreds of students waving flags and stamping their feet to complain about a policy that Roger had instituted. After a flow of abusive language, Roger replied, I think that I make a very unlikely Fascist pig." A long laugh broke the mob's hostility and the afternoon was won. I dropped in on Roger later that day to congratulate him for creative handling of a messy situation.

Roger was remarkably active during the last two decades of his academic life. He wrote a draft of a book, never published, called, "A New Paradigm of Reference," that revised earlier notions on meaning by accommodating to the work of his student Eleanor Rosch. He also composed thoughtful papers on the varied moods conveyed by music, the differences between novels and songs, and drafted a theory of politeness that profited from the work of Penelope Brown and Steven Levinson. Roger admitted to feeling delighted when he discovered that some verbs ascribe causality to their targets—Bill admires Sam because Sam is admirable—whereas others ascribe causality to their subjects—Bill charms Sam because Bill is charming. However, he learned later that this provocative insight had been detected earlier by Alfonso Caramazza and Catherine Garvey.

His health began to decline during the last decade of his life; he had a bypass operation and was diagnosed with prostate cancer. Despite the chronic discomfort of these illnesses, Roger retained his civility and gentleness, continued to supervise students, and taught a popular course on the relation between psychology and fiction until retirement in 1994.

When Roger's partner of forty years, Albert Gilman, died of cancer in 1989, he confessed to the unexpected difficulty of coping with the pain of that loss. During the next half dozen years, he tried to conquer each day's dark mood of isolation with a series of relationships with younger men. He summarized those terrible years in a short book, published in 1996, called *Against My Better Judgment*. Roger's friends were simultaneously puzzled and saddened by the honesty of this memoir and could not understand why he had chosen a confession that created impossible levels of dissonance in those who loved him most. Jean Berko Gleason, Roger's first student, ended her comments in a 1998 memorial service by saying that the Roger Brown she would remember was tall, handsome, brilliant, kind, incredibly generous, and a wickedly funny man.

Eric Wanner, a former student and president of the Russell Sage Foundation, offered a speculation at the memorial service that may help to explain why Roger rubbed sadness into each page of the memoir. Wanner suggested that a suspicion of psychological theory and its unconstrained descriptors permeated Roger's scholarship, and he wished to remind psychologists and his close friends of the easily suppressed truth that each person's anima is so hidden and resistant to logical analysis, one must reject simple stereotypes as appropriate descriptions of the warren of feelings, thoughts, and symbols that are the skeleton of the human psyche. If one reads *Against My Better Judgment* with that assumption it becomes a revealing psychological document. I suspect this was one of Roger's motivations for writing it.

Roger Brown was a fellow of the American Academy of Arts and Sciences and was elected to the National Academy of Sciences in 1972. He was awarded the G. Stanley Hall

Prize in developmental psychology of the American Psychological Association in 1973 and received honorary degrees from York University, England, in 1970, a D.Sc. from Bucknell University in 1980, and a D.Sc. from Northwestern University in 1983. He was awarded the Distinguished Scientific Achievement Award of the American Psychological Association in 1971, and in 1985 was awarded the International Prize of the Foundation Fyssen in Paris.

I THANK STEVEN PINKER for sharing with me his obituary to be published in *Cognition* in 1998.

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PHILIP PACY COHEN

September 28, 1908–October 25, 1993

BY ROBERT H. BURRIS

PHILIP P. COHEN HAD a distinguished career in biochemical research that emphasized nitrogen metabolism in the animal body. He was a pioneer in studies of transamination reactions and in the investigation of urea production. His career in research and administration was spent predominantly at the University of Wisconsin, where he served as chairman of physiological chemistry for twenty-seven years and as acting dean of the medical school for two years. In addition he functioned on many national boards and had a substantial impact on a number of Asian, Mexican, and South American institutions through his collaborative associations.

Philip Pacy Cohen was born September 28, 1908, in Derry, New Hampshire. He died on October 25, 1993, in Portland, Oregon. He attended high school for a year in Everett, Massachusetts, and then attended Boston English High School for three years. He graduated in 1926. Cohen received his B.S. degree from Tufts in 1930. He attended graduate school at the University of Wisconsin from 1930 until he received his Ph.D. in 1937. His major was in the Department of Physiological Chemistry and his minor in physiology. He continued, and received an M.D. degree in 1938. Professor

E. J. Witzemann served as major professor for Cohen's Ph.D. work, and Professor Harold C. Bradley was on his examining committee. E. B. Fred, a member of the National Academy of Sciences, signed Cohen's thesis as dean of the graduate school. The thesis was entitled "Studies in Ketogenesis," and it covered preparation of tissue slices and breis, the synthesis of several hydroxy and alpha-keto acids, the determination of acetoacetic and beta-hydroxy butyric acids, and the oxidation of a variety of fatty acids, amino acids, and keto and hydroxy acids. The studies were published in the *Journal of Biological Chemistry* (1937, 1938).

In June 1935 Cohen married Rubye H. Tepper, who had been a student in the University of Wisconsin Medical School. They had four children: Philip T., David B., Julie A., and Milton T., all of whom were students in the Madison, Wisconsin, school system.

After completing his Ph.D. and M.D. degrees, Phil Cohen was awarded a National Research Council Fellowship, and he spent 1938–39 in the laboratory of Hans A. Krebs in Sheffield, England, before returning to Yale University to complete the fellowship. The Krebs laboratory was a very lively research center, and Krebs and Cohen published a paper in *Nature* (1939) on glutamic acid as a hydrogen carrier in animal tissues. In 1939 they also had a paper in the *Biochemical Journal* on the metabolism of -ketoglutaric acid in animal tissues. Thus, Cohen's many papers on nitrogen metabolism in animal tissues were launched with a distinguished investigator (Krebs later received the Nobel Prize) in prestigious journals.

Cohen (1939) also was sole author of two papers in the *Biochemical Journal*. The first detailed a method for the microdetermination of glutamic acid, which involved the conversion of glutamic acid to succinic acid with chloramine T plus acid hydrolysis, followed by the manometric determi nation of succinic acid with succinoxidase. The second paper concerned transamination in pigeon breast muscle. He found the transamination reactions of aspartic acid and alanine with -ketoglutaric acid to give glutamic acid were far more active than other transamination reactions.

During his stay in the Yale University Department of Physiological Chemistry, Cohen did his research in the laboratory of Cyril N. H. Long. During this period he published two papers in the *Journal of Biological Chemistry* on transamination (1940). If you read them now, many years later, you are impressed with the thoroughness of the papers and the insight of the author. The papers certainly were important in placing studies of transamination on a solid footing. Transamination studies were initiated by Aleksander E. Braunstein and M. G. Kritzman (*Enzymologia* 2:[1937]:129–46), but Cohen pointed out a number of problems with their earlier methods and interpretations. They considered transamination a very general reaction, whereas Cohen reported it was highly active with only a limited number of amino acids. It was particularly active in the reversible reactions of glutamic acid plus oxalacetic acid to -ketoglutaric acid plus aspartic acid; glutamic acid plus pyruvic acid to -ketoglutaric acid plus alanine; and aspartic acid plus pyruvic acid to oxalacetic acid and alanine. Cohen and G. Leverne Hekhuis (1941) examined transamination rates in a variety of animal tissues. Cohen (1940) stated, "Braunstein suggested the names of glutamic aminopherase for the former, aspartic aminopherase for the latter. Since the original term *Umaminierung* used by these workers has been accepted with the English (and French) equivalent of transamination, it is suggested here that the enzyme (or possibly enzymes) catalyzing the transfer of amino nitrogen be termed transaminase. The latter term is more euphonious and does not suffer from a mixed etymology."

With his return to the University of Wisconsin, Phil Cohen continued his research on transamination and branched into other aspects of nitrogen metabolism in animal tissues. Much of his work was with pigeon breast muscle, but he also used rats and guinea pigs as sources of liver, spleen, testis, kidney, brain, and intestinal tissues. As a plant biochemist, I am pleased to find, in reviewing Cohen's work, that he published a couple of papers on plant materials (oat seedlings in 1943 and Jack beans and soybeans in 1946). He was careful to establish reliable methods, and his work was well planned and analytical.

Cohen became interested in the changes in nitrogen metabolism that occur during metamorphosis, and he chose the conversion of the tadpole to the adult frog as his object of study. His investigations, which spread well over a dozen years, are nicely summarized in his Harvey Lecture (1966) and his review in *Science* (1970). As he pointed out, the metamorphosis of the frog in the postembryonic period is marked by extensive morphological, cytological, and chemical changes. During this period the tadpole adapts from an aquatic life to a terrestrial life, and its developmental biochemistry undergoes many interesting modifications. The physical changes are apparent, but what is happening to the liver and other organs? Apparently the liver does not undergo cell division during metamorphosis, but the liver cells change in their biochemistry. Tadpoles excrete ammonia before metamorphosis (ammonotelic), but after metamorphosis starts they shift toward urea production (ureotelic). Cohen and coworkers developed methods so that they could follow these changes and the alteration in enzymes that promoted these changes.

The rate-limiting enzymes for urea production appeared to be carbamyl phosphate synthetase and arginosuccinate synthetase, and their activity appeared to correlate directly with urea excretion. Thyroxine can induce metamorphosis, but the changes in the urea-producing enzymes were the same whether metamorphosis was natural or thyroxine-induced. A marked increase in carbamyl phosphate synthetase preceded the morphological changes accompanying metamorphosis. The rate of formation of this enzyme was increased by added thyroxine and also was increased by raising the temperature of the tadpole's bath. At 15° C only about half as much carbamyl phosphate synthetase was formed as at 25° C. Cohen's group observed new synthesis of ribosomal and soluble RNA promptly after thyroxine treatment and before the induction of synthesis of carbamyl phosphate synthetase. Further studies suggested to them that thyroxine treatment modified chromatin in some way to make it a more effective template for RNA synthesis. They concluded that the effect of thyroxine on induction of carbamyl phosphate synthetase involves transcriptional as well as translational events.

Cohen and colleagues carried out a systematic study of the ultrastructural changes in the liver of tadpoles during metamorphosis with or without thyroxine induction, and they also followed changes in the adult liver. These changes observed by electron microscopy in liver sections from tadpoles exposed to thyroxine could be reproduced in vitro by adding thyroxine to pieces of liver taken from tadpoles before metamorphosis. They observed that the so-called cubed-liver preparations could survive under appropriate conditions for forty-eight hours with cellular integrity and capacity for biosynthesis.

Cohen's group also investigated glutamic acid dehydrogenase in the developing frog. The crystalline enzyme from frog liver and from tadpole liver exhibited different kinetics and substrate specificity and different molecular weights. This suggested that studies should be directed to differences between enzymes from the fertilized ovum stage to the tadpole stage, as well as to the changes that had been observed between the tadpole and the adult frog.

Cohen continued his interest in urea synthesis and the interconversions of ornithine, citrulline and arginine in urea production. When Krebs and Kurt Henseleit (*Hoppe-Seyler's Zeitschrirft für Physiologische Chemie* 210 [1932]:33-66) had first proposed their theory of urea synthesis, they described it only in intact liver cells, but Cohen and Mika Hayano (1946) were able to demonstrate the cycle in liver homogenates by adding glutamic acid, ATP, cytochrome C, and magnesium ions, and operating aerobically. They concluded that glutamic acid was an obligatory intermediate in the introduction of ammonia at the citrulline to arginine conversion step of the urea cycle. Others had reported no transamination in homogenates of kidney but had found transamination in kidney slices. Cohen and Hayano (1946) found such activity: in a variety of preparations in decreasing order of activity: liver homogenates, kidney slices, kidney homogenates, and liver slices. The homogenized preparations required added ATP, glutamic acid, cytochrome C, Mg ions, and oxygen. They found no transamination in brain, testes, or heart homogenates. Cohen and Santiago Grisolia (1948) could not demonstrate the ornithine to citrulline conversion under anaerobic conditions.

Grisolia and Cohen (1948) employed $^{14}CO_2$ in exploring CO_2 fixation in the synthesis of citrulline. They demonstrated the incorporation of ^{14}C into the carbonyl group of citrulline and into urea, and they concluded that the reactions were vigorous enough to establish citrulline as an obligatory intermediate in the urea synthesis cycle, a role that had been questioned by some investigators. The conversion of ^{14}C-citrulline to the carbonyl of urea yielded urea with essentially the same ^{14}C specific activity as the ^{14}C-citrulline supplied. Cohen and Grisolia (1950) concluded that in the synthesis of citrulline from ornithine, carbamyl-L-glutamic acid was an intermediate; it was not active in the absence of ammonium ion. The Cohen group and the Henry A. Lardy group (Patricia MacLeod, Grisolia, Cohen, and Lardy) (1949) joined forces to investigate the role of biotin in the synthesis of citrulline from ornithine. Biotin-deficient rat livers were only about half as active as livers from pair-fed controls. Injection of biotin twenty-four hours before testing the rats restored their livers to normal activity.

Earlier Cohen had questioned the conclusion of Braunstein and Kritzman that transamination was a general reaction among the amino acids, as he had found much higher activities among glutamic acid, aspartic acid, and alanine. P. S. Cammarata and Cohen (1950) reexamined the issue and reversed their opinion on the generality of transamination when they found that twenty-two amino acids in addition to alanine and aspartic and glutamic acids transaminated with aqueous extracts of pig heart, liver, and kidney. They stated, "Each transamination reaction appears to be due to a different transaminase." They had devised new methods for these tests and had found that pyridoxal phosphate accelerated the reactions.

Phil Cohen rather promptly established himself as an expert in the area of nitrogen metabolism in animal tissues, and in 1945 he contributed a chapter on proteins and amino acids to the *Annual Review of Biochemistry* . This review had very broad coverage of the topics of protein synthesis, plasma and tissue proteins, amino acid requirements, intermediary metabolism, deamination, transamination (a rather short treatment considering this was an area of special expertise for Cohen), and about a dozen other topics. Later he contributed a wider ranging review with Henry J. Sallach (1961) in a book entitled *Metabolic Pathways*. As they pointed out, they were concerned primarily with enzymatic systems involved in the transformation or transfer of the amino, amine and amide nitrogen moiety of amino acids, amines, and amides, and they gave preference to studies that emphasized the enzymatic aspects of chemical transformations. With this definition of their interests they then covered the field rather thoroughly in a 78-page treatment supported by 511 references. They even brought the subject up to date with an addendum.

A good summary of his extensive work on the changes of nitrogen metabolism during metamorphosis of tadpoles to frogs is given by George W. Brown, Jr., and Cohen in a symposium presentation (1958). By 1958 Cohen's group had studied this intriguing transformation of the ammonia-excreting tadpole to the urea-excreting frog rather extensively. They had concluded that the Krebs-Henseleit ornithineurea cycle was induced during metamorphosis. They pointed out that the nitrogen metabolism observed involved a highly integrated system consisting of nine enzymes operating in three subcycles. The net reaction at steady state kinetics converted two moles of ammonia and one mole of bicarbonate to urea at the expense of three moles of ATP. The investigators used tadpoles of the giant bullfrog, which often have livers that weigh over a gram. As the ratio of the hind limb/tail increased, the percentage of nitrogen recovered as urea increased dramatically. Brown and Cohen followed changes not only in the ammonia to urea ratio with development but the changes in a variety of other pertinent enzymes as well. They present an interesting discussion of the evolutionary development of the urea cycle.

Phil Cohen was a graduate student at Wisconsin during the 1930s and did his postdoctoral stints with Krebs in England and then at Yale. When he returned to Wisconsin it was an era of great research activity on respiratory enzymes.

Conrad A. Elvehjem and Perry W. Wilson had done postdoctoral work at Cambridge University, and they had their graduate students busily investigating respiratory processes with manometric methods. They organized a very active seminar group and as a product published a book in 1939 entitled *Respiratory Enzymes* with Elvehjem and Wilson as editors. They also staged a successful symposium concerned with respiratory enzymes on the Wisconsin campus in 1941 and published the proceedings as a book. Phil Cohen contributed to both volumes. There was good interaction between the biochemistry and physiological chemistry departments, and a number of joint papers came from research collaboration between the two departments. Later members of the group published a book on manometric techniques that went through five editions.

After returning to Wisconsin from his postdoctoral period in England and at Yale, Cohen moved rapidly through the ranks. During 1941–43 he was research associate in physiological chemistry, 1943–45 assistant professor, 1945–47 associate professor, and in 1947 professor. In 1968 he was appointed Harold C. Bradley professor of physiological chemistry. Cohen succeeded Bradley as chairman of physiological chemistry. Bradley had held the chairmanship for many years and was well known locally not only for his university functions but also for his support of skiing before it was a widely accepted sport, and for the accomplishments of his seven sons on the ski slopes and elsewhere. Cohen filled the transition between chairmen seamlessly and continued as chair for twenty-seven years; obviously, he was well accepted by his colleagues. Along the way there were some turbulent years in the medical school, and Cohen aided in smoothing out the problems by accepting the post as acting dean of the medical school for two years. He also served on and chaired the University Committee, the most influential and demanding committee on the campus. He chaired the Wisconsin Section of the American Chemical Society and was president of the Wisconsin Section of Sigma Xi.

Cohen's skills were recognized far beyond his department. For the National Research Council he served on the Committee on Growth (chairman), the Executive Committee of the Division of Medical Sciences, and the Review Committee on Biomedical Research in the Veteran's Administration. He served as an advisor to the Commission on International Relations of the National Academy of Sciences. For the National Institutes of Health he chaired the Physiological Chemistry Study Section, was a member of the National Advisory Cancer Council, Advisory Committee to the director of NIH, and National Advisory Arthritis and Metabolic Disease Council. He functioned on the Research Advisory Council of the American Cancer Society and on the Board of Scientific Counselors of the National Cancer Institute. He was a member of the National Board of Medical Examiners, Biochemistry Test Committee, and served on the Advisory Committee on Biology and Medicine for the U.S. Atomic Energy Commission. He was on the President's Public Health Service Hospital Commission and dealt with the Commonwealth Fund Award for Oxford University. Cohen was a consultant in biochemistry for the U.S. Department of State to the University of Mexico and on the Advisory Committee on Medical Research to the Pan American Health Organization. He chaired the Battelle-Northwest Biomedical Advisory Committee and was on the Advisory Committee on Biology and Medicine for the Los Alamos National Laboratory. Cohen was consultant to the administrator of the U.S. Energy Research and Development Administration and was a member of the National Commission on Research. In 1978 he chaired an ad hoc Committee for Review of Basic

Research in the Life Sciences for the U.S. Department of Energy.

Phil Cohen also was active in a number of scientific societies: the American Chemical Society (Executive Committee, Division of Biological Chemists), American Society of Biological Chemists (treasurer and member of the council), American Association for the Advancement of Science (fellow), Biochemical Society, Sigma Xi (president, Wisconsin Chapter), and the National Academy of Sciences. He also was an honorary member of the Chiba Medical Society of Japan, the Harvey Society, the Medical School Faculty of the University of Chile, the National Academy of Medicine of Mexico and the Japanese Biochemical Society.

Cohen traveled rather extensively, not only to committee meetings in the United States, but also to Japan, Korea, and Taiwan. He had visitors and students in his laboratory from these countries. His many ties with Mexico and South America were very important to him. He traveled widely there and served on committees concerned with research problems south of our borders. A number of investigators from Mexico and South America did research in Cohen's laboratory. Best known of these was Santiago Grisolia, who spent an extended period with Cohen and published at least ten papers in various aspects of nitrogen metabolism from Cohen's laboratory. Guillermo Soberon was another distinguished associate who studied with Phil Cohen and published four papers with him during the period 1957 to 1963; these papers were concerned with the formation and metabolism of uric acid and its role in diabetes. Soberon later served as president of the University of Mexico, one of the largest universities in North and South America. Cohen's contributions in Mexico were recognized with an honorary doctorate, and he was made an honorary member of the medical school faculty of the University of Chile.

Phil and Rubye Cohen and their four children lived for many years in Shorewood Hills. This is a Madison, Wisconsin, suburb directly west of the university campus, and it always has been well populated with members from the university staff. It is an independent village except for its association with the Madison school system. Many married graduate students reside in the Eagle Heights housing units adjacent to Shorewood Hills, and their children attend the Shorewood Hills grade school. Currently these children represent twenty-six foreign countries, so the Cohen children were exposed to a broad cultural spectrum while attending the Shorewood Hills school.

Dr. Cohen was a dedicated member of the faculty who focused on research, teaching, and administration. However, he did enjoy his Friday evening poker sessions with an equally intense group of faculty members from diverse departments. He was also a trout fisherman. I cannot testify to his prowess, because my own trout fishing talent was hardly exemplary. At least I did learn to avoid the alder bushes (nitrogen-fixing nonlegumes) on the backcast, as alders were dominant along many Wisconsin trout streams.

Phil and Rubye Cohen had a happy marriage of fifty-seven years. Rubye predeceased Phil by about a year. When Phil's health started to fail, he moved to Portland, Oregon, where he stayed with his daughter Julie until his death on October 25, 1993. He had a full and very diverse career in research, teaching, and public service. His role in the university was exemplary and he left many admiring and firm friends when he passed on.

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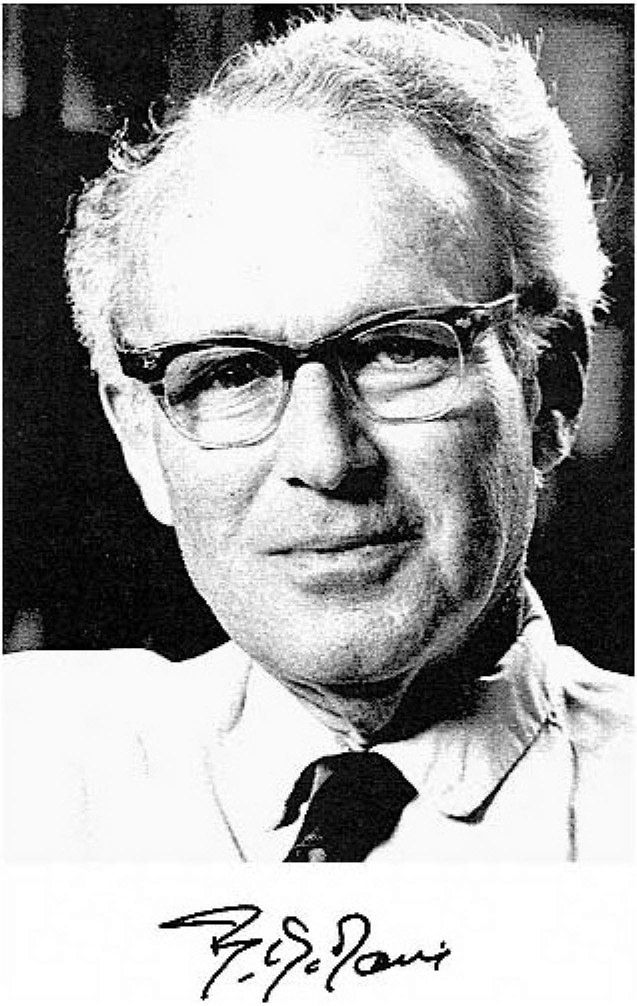

BERNARD DAVID DAVIS

January 7, 1916–January 14, 1994

BY WERNER K. MAAS

BERNARD DAVIS'S MAJOR scientific contributions were in microbial physiology and metabolism. During the late 1940s he discovered an ingenious method for isolating mutants of *Escherichia coli* that were deficient in individual steps of biosynthetic pathways. His approach was based on the previous work of Beadle and Tatum with the mold *Neurospora*, but his efficient method permitted him to isolate in one year an arsenal of mutants that in its number and variety surpassed the *Neurospora* mutants isolated over a previous seven-year period. Furthermore, *E. coli* was in several ways more suitable for metabolic studies than *Neurospora*.

Davis used his mutants mainly for working out the steps of biosynthetic pathways. A major advance was his elucidation in the early 1950s of the complete biosynthetic pathway of aromatic amino acids from a common precursor, shikimic acid. Further studies with mutants led also to the clarification of other biosynthetic and catabolic pathways, and to basic findings in the areas of antibiotic action, drug resistance, active transport of metabolites and protein synthesis.

Bernard Davis became a leading figure in biology through his ability to carry out incisive experiments that answered basic questions. His wide knowledge and penetrating analytical powers made him a superb expositional writer, teacher, and author of a first-rate, inspiring textbook.

Bernard D. Davis was born on January 7, 1916, in Franklin, Massachusetts, where his parents, immigrants from Lithuania, had settled. "Bernie" (I hope that the reader will forgive this familiarity, but I have known him by this name over a forty-five-year period of close association) was raised, together with three siblings, in the close-knit environment of a Jewish family. There was great emphasis on learning and intellectual achievement. All four children graduated as valedictorians from the local high school. Despite the limited financial resources generated from his dry goods store the elder Davis managed to provide his children with an education at Harvard (for the two sons) and Radcliffe (for the two daughters).

From early childhood Bernie had a penchant for rational explanations. This turned him off at an early age from the rituals of the Jewish religion, and he became an agnostic. In high school he excelled in science and mathematics. His valedictorian address dealt with "creative chemistry." A complication arose because the year of his graduation coincided with the bicentenary of George Washington's birth. His teachers insisted that the valedictorian address should deal with the first president. Bernie overcame this obstacle by starting his speech with, "Little did George Washington dream that chemistry. . . ." This diplomatic approach satisfied his teachers.

At Harvard, after an abortive attempt to broaden his education by taking courses in history and literature, Bernie settled down to a hard-science curriculum with a concentration in biochemistry. His undergraduate honors thesis dealt with the oxygen dissociation curve of hemoglobin. Twenty years later he was able to point out the generality of this S-shaped curve at a Cold Spring Harbor symposium, since the same kind of curve was found in the allosteric regulation of enzymes, as proposed by Jacques Monod.

After graduation he vacillated between graduate work in chemistry and medical school. For practical reasons he decided for the latter and enrolled at Harvard Medical School. During the medical curriculum he also worked in the laboratory of E. J. Cohn, a pioneer in protein chemistry. Thus, as well as his medical qualifications, he acquired during this period a solid background in biochemistry. He graduated in 1940 with the very rare degree of M.D. summa cum laude.

Following medical school Bernie went to Johns Hopkins Hospital where he had been offered a research fellowship combined with a part-time internship. He set up a Tiselius apparatus for the analysis of plasma proteins. During this time he carried out a significant study on the consequences of the binding of sulfa drugs to plasma proteins. His medical duties did not generate a great deal of interest and enthusiasm and he decided to discontinue the practice of medicine.

In 1942 he began a research career as a commissioned officer in the U.S. Public Health Service. After a brief period in aviation medicine at the National Institutes of Health in Bethesda, Maryland, he was assigned to work on serological tests for syphilis. He felt he needed more experience in immunochemistry and spent the next two years in the laboratories of Elvin Kabat at Columbia University and Jules Freund at the Public Health Research Institute in New York City. In 1945 the U.S. Public Health Service offered Bernie his own laboratory to work on basic science problems related to tuberculosis. He prepared himself for this by spending two years in the laboratory of René Dubos at the Rockefeller Institute. During this time he contracted a mild case of tuberculosis and had to undergo surgery, followed by a protracted recovery period. It was during this period of reading and reflection that he formulated the plans for his future research. A deciding factor was a review by George Beadle on the use of biochemical mutants of the mold *Neurospora* as tools for genetic and biochemical studies. As he stated in his autobiographical memoir in 1992, "It seemed to me that such work on universally distributed biosynthetic pathways should be deeply satisfying because it was near the trunk of the evolutionary tree, while attempts to grow bigger and better tubercle bacilli were only twigs." Thus, after five "Wanderjahre," Bernie had found his niche.

The period at the tuberculosis research laboratory between 1947 and 1954 represented the flowering of Bernie's research career. He set up his new laboratory in the Department of Preventive Medicine at Cornell Medical College in an obscure corner, yet in a scientifically central position, near the Rockefeller Institute in New York City. The direction of his research was set by his early discovery of the penicillin method for the isolation of biochemically deficient mutants. The important contributions that resulted from his work with mutants of *E. coli* paved his way to becoming a widely recognized figure in microbial physiology. As a consequence of his achievements he was appointed, first, chairman of the Pharmacology Department at New York University School of Medicine in 1954 and, three years later, chairman of the Department of Bacteriology and Immunology at Harvard Medical School, a position he held until 1968.

While at New York University Bernie married Elizabeth Menzel, who, as he stated, "brought a great deal of balance to my life," and who remained a supportive and gracious companion until his death. They had three children: Franklin Arthur born in 1956, Jonathan Harry born in 1958, and

Katherine Judith born in 1960. They acquired a summer home in Woods Hole, where Bernie taught in the prestigious physiology course from 1955 to 1960. Subsequently they spent most of their summers in Woods Hole, and Bernie became a prominent member of the local scientific community. During this period, among many other tokens of recognition, he was elected to the National Academy of Sciences in 1967.

One of Bernie's main tasks in the 1960s was the writing, with Barry Wood, Renato Dulbecco, Herman Eisen, and Harold Ginsberg, of a new kind of microbiology textbook (1967) for medical students. It emphasized the use of bacteria as a model for the new genetic and molecular biology. Through four editions in twenty-three years this popular textbook bore the marks of Bernie's lively and clear expository style.

In 1968 Bernie resigned from the chairmanship of the department and set up a separate Bacterial Physiology Unit to carry out his research with a small group of investigators. At the same time he developed an interest in the relationship between science and society, which became increasingly dominant during subsequent years. In many articles and several books he addressed problems created, directly or indirectly, by the impact of science on human relations. He played an important role as an outspoken critic of problems that arose inside and outside the scientific community, but at times his candor created difficulties for him. For example, in commenting in an article in 1976 on the dangers of affirmative action in lowering the standards of medical education, he attracted the wrath of the dean of the Harvard Medical School and other members of the faculty, and was denounced as a racist.

Bernie retired from his laboratory in 1984, but continued an active life as a lecturer and writer. He was a visiting professor at Tel Aviv University in 1985, University of California, Berkeley in 1986, and National Taiwan University in 1987; he was a Fogarty scholar at the National Institutes of Health in 1988. At the time of his death Bernie was writing a book about the "Baltimore affair," in which he defended David Baltimore against the unfair treatment he had received in his defense of his collaborator Imanishi-Kari against accusations of alleged scientific fraud by a congressional committee (later shown to be groundless).

Bernie's insistence on exposing the truth outlived him. An obituary in the *New York Times* described his role as a critic of affirmative action, but hardly mentioned his many other positive contributions. A number of his colleagues and friends (myself included) wrote a letter of protest to the *Times* describing Bernie's achievements, and as a result the *Times* published a second obituary in which it rectified its previous omission.

THE TUBERCULOSIS RESEARCH LABORATORY AND NEW YORK UNIVERSITY SCHOOL OF MEDICINE (1948–1957)

I was associated with Bernie during this period, first as a member of his group at Cornell and later as a faculty member of his department at New York University. I was, therefore, intimately acquainted with his scientific activities in directing the laboratory, including my own research.

The work on elucidating biosynthetic pathways was based mainly on the use of mutants as "living dissecting needles" for individual reaction steps. The principle is that a mutant blocked in a given step accumulates the substrate of the blocked reaction and can use the product of the blocked reaction as a growth factor. Thus, the substrate of a blocked reaction step becomes a growth factor for a mutant blocked at an earlier step. In this fashion the order of reaction steps in a pathway can be determined. To carry out this kind of analysis it is necessary to identify chemically the intermediates of a pathway. Consequently, our laboratory included, in addition to Bernie and myself, associates with training in organic chemistry.

The major project in the laboratory was the elucidation of the pathway leading to the synthesis of the aromatic amino acids tyrosine, phenylalanine, and tryptophan from the common precursor, shikimic acid. This compound had originally been isolated from the fruit of the oriental shikimi tree. Shikimic acid, besides being the precursor of aromatic amino acids, was found to give rise to the then unknown growth factor, parahydroxybenzoic acid. The cellular origin of shikimic acid from intermediary metabolites did not yield to the mutant methodology but was determined in collaboration with David Sprinson of Columbia University by using radioactive isotopes labeled in specific atoms. It was shown that three of the atoms of shikimic acid came from phosphoenol pyruvate and the other four from erythose-4-phosphate.

Besides the aromatic pathway, the pathways of proline, lysine, methionine, histidine, and pantothenate biosynthesis were investigated with the use of mutants. Other people who at one time or another were members of the Bernie Davis laboratory engaged in these studies included Ulrich Weiss, Ivan Salomon, Edwin Kalan, Charles Gilvarg, and Henry Vogel.

The work on biosynthetic pathways constituted the bread-and-butter research of the laboratory. The studies with mutants also led in directions that were not as well defined as biosynthetic pathways, but they were of perhaps greater general interest in foreshadowing developments in molecular genetics. Such studies included the work of Charles Gilvarg and Howard Green, which demonstrated the existence of specific transport systems for metabolites (later named "per meases" by Monod); my own studies, with a temperature-sensitive pantothenate-requiring mutant, published in 1952, demonstrated that a mutation could alter the structure of an enzyme. The following year the double-helix structure made it clear how a gene could determine the structure of a protein molecule; and Bernie's work on sulfonamide-resistant mutants indicated that such mutations can involve an alteration of the enzyme that is the target of the drug. This notion, now generally accepted, was entirely novel at the time.

HARVARD MEDICAL SCHOOL (1958–1984)

Bernie's work at Harvard veered from the systematic use of mutants to explore biochemical pathways and concentrated on specific problems that he considered important. One of these problems was the mode of action of streptomycin that had aroused his interest in connection with his work on tuberculosis.

Streptomycin was known to kill bacteria by irreversible inhibition of protein synthesis. Visibly, cells remain intact, but on examining these cells it was found that many processes went awry. The question was: what is the primary action of streptomycin that leads to the killing of the cell?

Bernie and his associates found that there were two major areas of damage: ribosomes involved in protein synthesis and the integrity of the cytoplasmic membrane responsible for the uptake of metabolites. Bernie spent many years trying to disentangle the chain of events that led to killing. In the course of these studies, he and Luigi Gorini, a member of his department, discovered that streptomycin causes misreading of the genetic message, resulting in the production of faulty proteins. Bernie finally solved the problem in 1987 after he realized that killing depends not on one key step but on a series of interlocking multiple steps.

Two other major problems occupied Bernie during this period: the reactions of the ribosome cycle and the mechanism of protein transport across cell membranes. For these studies he used conventional biochemical rather than genetic methods. In the former project he worked out details of the cycle and in the course of this discovered that the initiation factor IF3 acted as a dissociation factor for maintaining 30S and 50S subunits. For the latter process he and David Rhoads showed that the incorporation of protein into membrane vesicles requires the setting up of a membrane potential.

In summary, Bernard Davis's scientific contributions can be arranged under three headings:

1. Protracted effort on one topic that resulted in a solid and complete piece of research, such as the elucidation of aromatic biosynthesis and the determination of the mode of action of streptomycin;
2. More limited projects that led to significant findings that opened up a new area of research for future investigators. His work leading to the recognition of specific membrane transport systems falls into this category.
3. The publication of papers that presented original interpretations of experiments carried out by others. For example he offered a reasonable explanation for the occurrence of adaptive mutations observed by John Cairns and his associates (1989). His papers in this category were on a wide range of topics and were indicative of Bernie's intellectual mastery of biology.

I can do no better than let Bernie himself speak about his contributions and his role as a scientist:

> I clearly have "internalized the canons of science," emphasizing rationality

> and reality more than most. I think my strongest suit in science has been critical, logical analysis, leading to a single but decisive experiment. And although a systematic program, pursuing the shikimate pathway, has probably contributed most to my scientific reputation, I have tended not to pursue programs at length but to skim the cream from a variety of problems.

This is a portrait of a "romantic" scientist. His contributions, like the sometimes more spectacular contributions of the contrasting class of "classical" scientists, will find a permanent place in the edifice of science.

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Michael J. S. Dewar

MICHAEL J. S. DEWAR

September 24, 1918–October 10, 1997

BY JOSEF MICHL AND MARYE ANNE FOX

MODERN ORGANIC CHEMISTRY would not have been the same without Michael J. S. Dewar. He was one of the first, if not the first, organic chemist to master molecular orbital theory and to apply it to problems in organic chemistry. His sparkling intellect and theoretical insight introduced many of the fundamental concepts that are now taken for granted, and his ceaseless efforts over four decades produced the semi-empirical methods of computation that are still used the world over. He is remembered as a man of marvelously original and unorthodox ideas, a man of impeccable integrity, and a formidable debater.

Michael James Steuart Dewar was born in Ahmednagar, India, on September 24, 1918, of Scottish parents. His father was a district commissioner in the Indian Civil Service, the British government of India. Michael was sent to a boarding school in England at the age of eight, and as a holder of a prestigious scholarship he then studied at Winchester College. He entered Balliol College at Oxford in 1936 and undertook classical studies before developing his intellectual pursuit of organic chemistry. After earning a first-class honors undergraduate degree and his doctoral degree, he stayed at Oxford as a postdoctoral fellow with Sir Robert

Robinson in the Dyson Perrins Laboratory. At Oxford he met, and in 1944 married, Mary Williamson, a historian who later became well recognized as a scholar of English Tudor history. They had two children, Robert and Steuart. Their marriage was one of the world's truly perfect matches, filled with much mutual admiration and support in every part of their personal and professional lives.

In 1945, Dewar accepted an industrial position as a research director at Courtaulds in Maidenhead near London. Although this position permitted him to work with Bamford, the assignment of a newly minted organic chemist as the director of a physical chemistry laboratory was without precedent. Simultaneously, he wrote his first influential book *The Electronic Theory of Organic Chemistry*, which appeared in 1949. The book represented a landmark, as it was the first treatment of organic chemistry in terms of molecular orbital theory.

In 1951, at the age of thirty-three, Dewar accepted a chair at Queen Mary College at the University of London. He was instrumental in establishing a credible research program by hiring aggressively and forming a sound intellectual foundation that has persisted and grown. He stayed until 1959, when he moved to the University of Chicago. In 1963 he accepted the first Robert A. Welch chair at the University of Texas at Austin. Largely because of the presence of Dewar and his former student Rowland Pettit, the University of Texas at Austin became an internationally accepted destination for visitors and sabbatical visitors who wished to work at the frontiers of theoretical chemistry or on physical organic mechanisms. In 1980, the Dewars became U. S. citizens, and in 1983, Michael was elected to the National Academy of Sciences. In 1989, he moved to a half-time appointment at the University of Florida at Gainesville, from which he retired in 1994. He passed away in 1997.

Dewar's reputation for providing original solutions to vexing puzzles first developed when he was still a postdoctoral fellow at Oxford. In 1945, he deduced the correct structure for stipitatic acid, a mold product whose structure had baffled the leading chemists of the day. It involved a new kind of aromatic structure with a seven-membered ring for which Dewar coined the term "tropolone." He then correctly suggested that a similar structure would account for the properties of another problem compound, the alkaloid colchicine. The discovery of the tropolone structure launched the field of non-benzenoid aromaticity, which witnessed feverish activity for several decades and greatly expanded the chemists' understanding of cyclic -electron systems.

Also in 1945, Dewar devised the then novel notion of a complex, which he proposed as an intermediate in the benzidine rearrangement. This notion turned out to be extraordinarily fruitful, as it also automatically accounted for the ease of 1,2-shifts in carbocations, as opposed to radicals and carbanions. It also provided a simple explanation for the structure of "non-classical" carbenium ions for which Winstein was starting to provide experimental evidence at the time, and offered the first correct rationalization of the electronic structure of complexes of transition metals with olefins, later known as the Dewar-Chatt-Duncanson model. While at Courtaulds, Dewar displayed an appreciation for the utility of models in practical chemistry. For example, he measured the first absolute rate constants in a vinyl polymerization and in an autoxidation, and performed an array of other kinetic and mechanistic studies, coming close to describing the modern concept of photoinduced electron transfer.

In this period, Dewar developed the key ideas discussed in *The Electronic Theory of Organic Chemistry* at nights and on weekends. When this revolutionary book was published, it was the start of the conversion of organic chemists to a new creed. By 1951, Dewar had succeeded in formulating the molecular orbital theory of organic chemistry in a semi-quantitative form, later termed "perturbational molecular orbital theory." This approach was clearly superior to the purely qualitative resonance theory then still in use, but the papers were written in such a condensed manner that the theory was virtually incomprehensible to practicing bench chemists, the intended users. Unlike his more qualitative concepts, it was never enthusiastically adopted by organic chemists as the back-of-the-envelope tool it was designed to be.

At Queen Mary College, Dewar continued his work on the theory of organic chemistry. His startling and provocative views on hyperconjugation stimulated an active debate about the nature of the chemical bond. Although some may feel that his original position later turned out to be somewhat exaggerated, his work served to correct the simplistic descriptions prevalent at the time and formed the basis for the more complicated picture that is accepted today. A list of his experimental projects from the time reveals the incredible breadth of his interests and talents. His group performed a series of significant quantitative experimental studies on aromatic substitution, designed to test theoretical predictions based on his book and on perturbation theory. He elucidated the electronic structure of phosphononitrile chlorides and started a long series of experimental studies of new stable heterocycles, the borazaromatic compounds. He performed the first studies of self-assembled monolayers of thiols on a metal surface, a field that has grown in popularity immensely in recent years. He started a series of investigations of the structure and properties of liquid crystals and developed a novel analysis of substituent effects in aromatic and aliphatic compounds, showing that the classical inductive effect is insignificant. He built an electron paramagnetic resonance spectrometer for use in his research when this kind of spectroscopy was just beginning to be recognized as useful for chemistry applications.

In spite of these burgeoning scientific successes, Dewar did not enjoy the increasingly administrative duties of chairing a modern research-focused department. When they became too much of a burden, he decided to concentrate on chemistry fully by accepting a professorial offer from the University of Chicago. The Dewars had already come to know the United States during a 1957 half-year visit to Yale, which they combined with a long automobile trip all around the country. During that trip, they met many American scientists and established many happy friendships and fruitful collaborations. They were exuberant in praising the United States and the spirit of her best chemists. Even his election as a fellow of the Royal Society in 1960 could not lure him permanently back to England, notwithstanding his frequent summer visits to London to escape Texas summers.

At Chicago, Dewar continued some of his earlier projects and added new ones. In a series of experiments, he showed that charge transfer makes a relatively unimportant contribution to the stability of charge-transfer complexes, contrary to the general belief at the time. Most importantly, at Chicago Dewar started the development of a series of increasingly sophisticated semi-empirical molecular orbital methods for organic chemistry, for which he is probably best known today. Access to the large amounts of computer time that this work required was the main motivation for Dewar's next move. This new direction brought him to the University of Texas at Austin, where he was to spend nearly three decades. He served there as a fulcrum for a strong theoretical chemistry institute that continues to this day.

In Austin, he supplemented his work in theory by continuing his interest in experimental chemistry, examining a wide array of problems ranging from carbenium ions, semiconductors, and liquid crystals to various forms of spectroscopy that now included nuclear quadrupole resonance and photoelectron spectroscopy. Although his experimental program was prolific, the pull of theory gradually became irresistible and dominant. Dewar began to focus his energy totally on the development of increasingly sophisticated semi-empirical methods for quantitative molecular orbital computations on large organic molecules, including novel ions, conjugated systems, and sigma-bonded arrays. This resulted in the development of a series of progressively more accurate methods that have been tested in a wide variety of applications. Although both structure and many molecular properties were treated in these methods, Dewar's primary interest always was chemical reactivity, and he devoted most of his effort to the study of transition states of organic reactions. He was a major contributor to our current understanding of pericyclic reactions, especially in his provocative studies of cycloadditions, electrocyclic rearrangements, and sigmatropic shifts. His work on hydrogen bonding and sigma conjugation continues to be stimulating to experimentalists. Because his methods were computationally much simpler than the non-empirical methods used by others, he was able to treat much larger and more realistic reaction models and to perform full geometrical optimizations of equilibrium and transition state geometries for large and chemically meaningful molecules well before others could even dream of such calculations. In the last decade of his life, he explored a variety of complex phenomena, including superconductivity, the structures in organometallics and reactivity in such biologically relevant compounds as enzymes and carbohydrates. His development of a semi-ab ini tio (SAM-1) approach to chemistry was based on the calculation of electron-electron repulsions scaled to approximate internuclear distances to allow for electron correlation. He frequently spoke of the utility of his methods, vigorously defending them as "models that work" in real situations.

Today continued advances in computer technology and computer codes permit ab initio calculations for large organic molecules at a level of sophistication that was inconceivable when Dewar developed his semi-empirical methods. Because of this progress, many applications of his semi-empirical approach may have now become obsolete. However, his models are still in use for very large molecules and for rapid preliminary scans. Indeed, in many ways his sequence of SCF-MO, MINDO, MNDO, DEWAR-PI, AM-1, and SAM-1 methods prefigured a whole new generation of parameterized methods, based on density functional theory, that have emerged as a modern replacement of the procedures introduced by Dewar. He would have been pleased to witness this development and the increased sophistication this achievement reflects.

Dewar was a brilliant conversationalist who enjoyed shocking others with his unorthodox ideas, just as he did in chemistry. For instance, he would declare firmly that everyone ought to be taught Latin at school. When challenged to explain why, he would give three reasons with disarming charm: (1) Latin is sufficiently complex to provide superb intellectual training, teaching young people how to reason through intricate problems; (2) children tend to hate Latin, so they learn at an early age to face and overcome adversity; and (3) almost without exception Latin will be totally useless for them in later life (one would not wish to make children hostile to something that might be useful to them later!). He offered similar analyses on such topics as traffic speed limits, the use of DDT, and preferred techniques for the reform of incarcerated convicts.

Dewar viewed argument as a stimulating intellectual challenge and as the best way of arriving at a reasoned solution, much in the spirit of medieval scholasticism. To be worthwhile, an argument had to be led ruthlessly, with no holds barred, but also without malice or anger. For all his bluster and disdain for the politically correct, he was a warm and happy person, truly dedicated to science and his students. Dewar's wide interests in chemistry were matched by an equally wide range of interests outside his discipline. He was keen on many subjects—from astronomy and geology to oriental cooking—and he loved to discuss them all with wit and passion. In his early years, Dewar was also an avid outdoorsman, but a back injury forced him to abandon rock climbing and other physical exercise other than carrying large pitchers of Manhattans and martinis at the legendary parties he and Mary loved to give.

Dewar's outspokenness was not limited to science, and he found it increasingly difficult in his later years in Austin to deal with blind bureaucracy and arbitrary regulations. The disruption associated with his move to Gainesville preempted the completion of what might have been some of his finest work, and the premature death of his beloved wife Mary left him personally devastated. The irony of her death by lung cancer after lifelong opposition to smoking was particularly painful. Some of the difficulty of his transition to retirement is reflected in his memoirs published by the American Chemical Society, "A Semi-Empirical Life," which uncharacteristically provides a seemingly embittered and convoluted picture of a truly great man.

Dewar's professional recognition started early with his scholarships to Winchester and Balliol and his designation as a Gibbs Scholar in his second year at Oxford, a univer sity-wide prize never previously won by so young a student. His first major award (from the Chemical Society in 1954) was largely in recognition of the influence of *The Electronic Theory of Organic Chemistry* and a stunning 1952 series of six back-to-back articles in the *Journal of the American Chemical Society* that explained the implications of molecular orbital theory in organic chemistry. Elected a fellow of the American Academy of Arts and Sciences in 1966, and a member of the National Academy of Sciences soon after accepting U.S. citizenship, Dewar was also named an honorary fellow of Balliol College (Oxford) and of Queen Mary and Westfield College (University of London).

Despite his dislike of flying, Dewar accepted 32 named lectureships and visiting professorships around the world and served as a stimulating consultant to industry both in the United States and abroad. His list of professional society awards serves as a nearly complete list of those available to organic chemists:

1954	Tilden Medal of the Chemical Society
1961	Harrison Howe Award of the American Chemical Society
1974	Robert Robinson Medal, Chemical Society
1976	G. W. Wheland Medal of the University of Chicago (first recipient)
1977	Evans Award, The Ohio State University
1978	Southwest Regional Award of the American Chemical Society
1982	Davy Medal, Royal Society of London
1984	James Flack Norris Award of the American Chemical Society

1986	William H. Nichols Award of the American Chemical Society
1988	Auburn-G. M. Kosolapoff Award of the American Chemical Society
1989	Tetrahedron Prize for Creativity in Organic Chemistry
1990	World Association of Theoretical Organic Chemists Medal Chemical Pioneer Award, American Institute of Chemists
1994	American Chemical Society Award for Computers in Chemistry

As a recipient of the Davy Medal, he is one of only six Americans to have been so selected. But his principal personal reward was in the achievements of his more than 50 doctoral students and 60 postdoctoral fellows, whose collaborative work is reflected in the more than 600 refereed scientific papers and 8 books that Dewar published.

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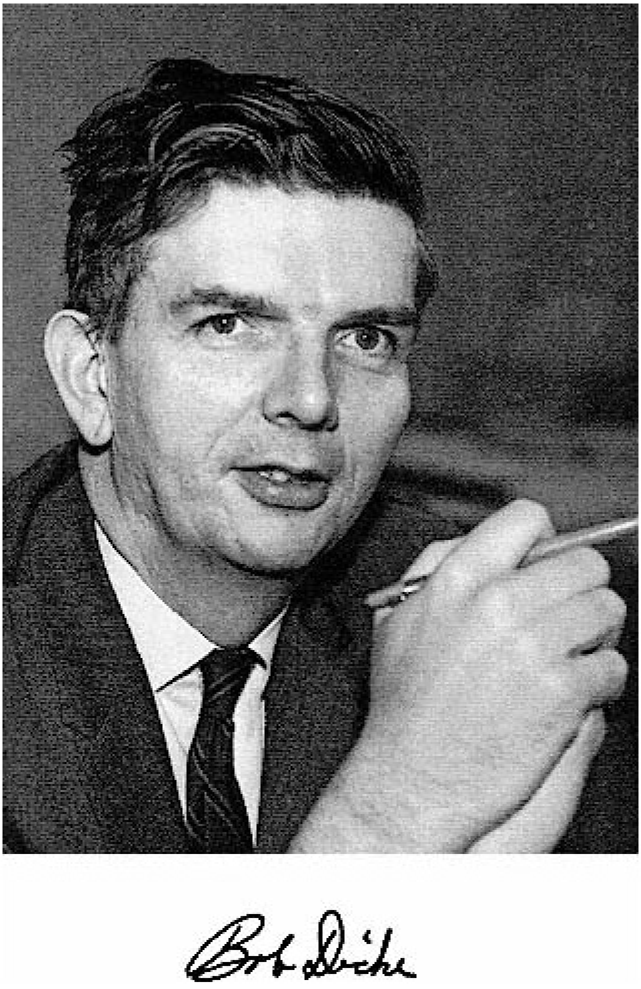

ROBERT HENRY DICKE

May 6, 1916–March 4, 1997

BY W. HAPPER, P. J. E. PEEBLES, AND D. T. WILKINSON

BOB DICKE CONTRIBUTED to advances in radar, atomic physics, quantum optics, gravity physics, astrophysics, and cosmology. The unifying theme was his application of powerful and scrupulously controlled experimental methods to issues that really matter. Though Bob sometimes had to hide his amusement at theorists he found poorly grounded in phenomenology, he did not hesitate to speculate where the experimental ground is thin; the condition was that there had to be the possibility of a measurement that could teach us something new. He wrote:

> I have long believed that an experimentalist should not be unduly inhibited by theoretical untidiness. If he insists on having every last theoretical T crossed before he starts his research the chances are that he will never do a significant experiment. And the more significant and fundamental the experiment the more theoretical uncertainty may be tolerated. By contrast, the more important and difficult the experiment the more that experimental care is warranted. There is no point in attempting a half-hearted experiment with an inadequate apparatus.[1]

Bob held some 50 patents, from clothes dryers to lasers. He recognized that two mirrors make a more effective laser than the traditional closed cavity of microwave technology. In the company Princeton Applied Research he and his students packaged his advances in phase-sensitive detection in the now-ubiquitous "lock-in amplifier." With its successors this probably has contributed as much to experimental Ph.D. theses as any device of the last generation. Bob predicted and experimentally showed that collisions that restrict the long-range motions of radiating atoms in a gas can suppress Doppler broadening. The physics is the same as that of Mössbauer narrowing of gamma-ray lines; it is used in the atomic clocks of the Global Positioning System. He contributed to the concept of adaptive optics in astronomy. He was among the first to recognize that the accepted gravity theory, general relativity, could and should be subject to more thorough tests. His series of gravity experiments mark the beginning of the present rich network of tests. He set forth the idea[2] of the anthropic principle that now plays a large part in speculation on what our universe was doing before it was expanding. Bob's visualization of an oscillating universe stimulated the discovery of the cosmic microwave background, the most direct evidence that our universe really did expand from a dense state. A key instrument in measurements of this fossil of the Big Bang is the microwave radiometer he invented.

Bob left us a challenge: discover whether or how laboratory physics is related to the universe at large. At the turn of the century Ernst Mach argued for such a relation, that distant matter determines local inertial frames. Mach's principle led Einstein to general relativity. In this theory the mass distribution does influence inertial motion, but it has no effect on local laboratory measurements. Bob felt Mach's principle likely expresses more than this, and he and Carl Brans[3] gave an example, a generalization of general relativity in which the expansion of the universe causes the strength of the gravitational interaction to decrease. Experimental advances in gravity physics ruled out their approach, but the theory reappears in superstring models. And we are left to wonder what to make of Bob's belief. "the laboratory, Earth and Solar System could not be isolated even in principle from the rest of the universe."[1]

PERSONAL HISTORY

Bob recalled his early life as follows:

> I was born in St. Louis, Missouri, in 1916, but my earliest recollections are of Washington, D.C., where my father worked for the U.S. Patent Office as a patent examiner. Later, when my father became a patent attorney for the General Railway Signal Corp., we moved to Rochester, New York. It was there, at an age of 5, that I had my first contact with the fascination of science. An old spectacle lens fell into my possession and I was both fascinated and puzzled by its behavior. Later my childhood scientific interests ran the usual course—mechanical gadgets, insect collecting, electricity, chemistry via a "chemistry set", microscopy via an inexpensive Sears microscope, astronomy—and I read everything scientific I could get my hands on.[1]

Bob entered the University of Rochester intending to major in engineering, it not having occurred to him that he might make a living as a physicist. He credits Lee A. DuBridge with attracting him to physics and Frederic Seitz at Rochester and E. U. Compton at Princeton University for brokering his transfer there as a junior. While at Princeton he published his first research paper, on a dynamical model of a globular star cluster as an ideal gas sphere.

Bob returned to the University of Rochester for graduate work in nuclear physics. There he courted Annie Currie; they were married in Rochester on June 6, 1942. Bob completed research for his Ph.D. degree at the University of Rochester in the spring of 1941. His topic, which he had selected, was one of the first experimental studies of inelastic scattering of protons. He recalled that "Professor DuBridge offered me a position as instructor in the department for the following fall (at the impressive salary of $1,800.00 for the academic year). I was happy to accept, but I didn't have a chance to serve. War rumblings were growing louder and Professor DuBridge had left to establish the Radiation Laboratory at MIT to develop microwave radar. A few months later he asked me to join the laboratory as soon as I could get my thesis finished. I arrived at MIT in September of 1941."[1]

A year later Annie joined Bob in Cambridge. She was not supposed to know about his classified research. Her first hint came from Bob's cousin Tom Kuenning, a pilot in the antisubmarine campaign off the New England coast. A storm during patrol forced Tom to land away from his base and, since the crew had no money, they had to stay with friends; Tom stayed with the Dickes in Cambridge. Over breakfast Tom remarked on the marvelous effect of the radar sets from the Radiation Laboratory.

The Radiation Laboratory also produced a brilliant crop of physicists, Bob notable among them for his imaginative and subtly effective approach to physics. Among the results was his microwave radiometer, which he took to Florida to demonstrate that humid air radiates strongly near 1-cm wavelength, and hence that humid air is a strong absorber at that wavelength. At the time this limited the push to shorter wavelength radar for better resolution. Bob found time for a little pure science, using his radiometer to measure the surface temperature of the moon and to show that the space between the stars could not be warmer than 20 degrees above absolute zero.[4]

After the war Bob returned to the Department of Physics at Princeton University. He brought his 1.25-cm radiometer, but he recalls that "as a very junior member of the physics department, I considered it rash to start doing astronomical research, and I could not develop any interest in the astronomy department. I realized only later that the physics department was tolerant and that it would have been proud to have the first radio astronomy in the country."[1] Instead of radio astronomy Bob spent the next decade on the rich physics of the quantum mechanical interaction of radiation and matter. The book on quantum mechanics by him and his former student James P. Wittke was published in 1960.[5] It was used in many graduate courses, and, we suspect, was consulted by a lot more teachers of quantum mechanics.

Beginning in 1955, Bob turned to gravity physics in a series of elegant and searching experiments and theoretical analyses that set the stage for today's active research community. Two of the authors (PJEP as a student and DTW as a postdoc) remember when his Gravity Group met on Friday evenings; we complained but attended because the physics was too fascinating to miss. He probably knew we called ourselves "Dicke birds"—it fit his quiet good humor, which kept us from taking ourselves too seriously, while always remembering that we had better take the physics very seriously.

Bob was among the most imaginative of physicists. One sensed this in personal interactions, by his close attention, and support for work on anything of substance in biology, geology, astronomy, physics, or any of the other sciences. Discussions with Bob tended to leave one feeling that science is a wonderful adventure that one could join.

Bob Dicke was elected to the National Academy of Sciences in 1967. Among his many prizes and awards were the National Medal of Science (1971), the Comstock Prize of the National Academy of Sciences (1973), and the NASA Medal for Exceptional Scientific Achievement (1973). He was a member of the National Science Board from 1970 to 1976. Bob was appointed to the Princeton University Department of Physics in 1946, served as chair from 1967 to 1970, moved to emeritus in 1984, and kept active in research until prevented by physical problems, including Parkinson's disease. He and Annie loved and supported each other, and Bob followed developments in science until his last moments. He is survived by Annie and their children: Nancy Dicke Rapoport, John Robert Dicke, and James Howard Dicke.

PROFESSIONAL HISTORY

At the Radiation Laboratory Bob was assigned to the Fundamental Developments Group under Harvard's Ed Purcell. As one of the young stars of the Radiation Laboratory, he invented chirped radar, coherent pulse radar, and monopulse radar, all of which came into widespread use after the end of World War II. He also invented the magic tee microwave junction and the microwave radiometer, devices at the heart of radio telescopes. The flavor of Dicke's elegant contributions to microwave radar comes through clearly in *Principles of Microwave Circuits*,[6] one of the classic volumes of the Radiation Laboratory Series. Characteristically, Bob was the first to make systematic and potent use of symmetry principles and scattering matrix ideas from nuclear physics to analyze waveguide junctions and other microwave devices.

Back at Princeton after the war Bob used the microwave skills he had acquired at the Radiation Laboratory to make fundamental measurements in physics. With excellent taste, he started to measure the fine structure of the $n = 2$ level of hydrogen, but on learning that Willis Lamb was already working hard on that problem with the resources of the Columbia Radiation Laboratory, Bob turned to other challenges. Unswayed by careless assumptions of others that because the g-value of free electrons could not be measured in an atomic beam machine there was some fundamental reason the g-value could not be measured at all, Bob began to generate free electrons by photoionization of sodium atoms with circularly polarized light. Unaware of Kastler's work in Paris, Bob and his student Bruce Hawkins[7] carried out one of the first optical pumping experiments —on a beam of sodium atoms.

Bob understood how important narrow spectral linewidths are to precision spectroscopic measurements. He soon realized that gas-phase collisions, often a source of line broadening, could be an advantage in the right circumstances, since sufficiently rapid randomization of the thermal velocity vector would eliminate the Doppler broadening of the line. [8] Bob and his students showed that this collisional narrowing is particularly effective for the 0-0 "clock" transitions of hydrogen and the alkali-metal atoms.[9] Further development of these ideas by Tom Carver and others led to fabulously stable atomic clocks. Bob wondered about applications of these narrowing ideas to other spectral regions, but it remained for R. Mössbauer to show that at sufficiently low temperatures the Doppler broadening of certain gamma-ray lines could be eliminated by the same physics.

Fascinated by coherent microwave radiation from pulse-excited ammonia molecules, Bob conceived of the phenomenon of superradiance, where properly phased atomic systems can radiate with great intensity in narrow pencil-shaped beams.[10] Characteristically, Bob made the concept of superradiance clear to a large audience with apt and quantitative analogies to the high-gain antennas he understood so well from his work at the Radiation Laboratory. Many years later a beautiful series of experiments in the infrared by Mike Feld and colleagues[11] at MIT confirmed the striking properties of superradiant systems that Bob had foreseen.

During sabbatical leave at Harvard in 1954–55 Bob turned to the experimental and theoretical basis for gravity physics. At the time the Eötvös experiment showed that test bodies of different composition have the same gravitational acceleration to a few parts in 10^9. That was a guide to Einstein's general relativity: A gravitational acceleration may be transformed away by going to an accelerating coordinate frame. There were three tests of Einstein's theory. First, it agreed with the measured rate of advance of the orbit of the planet Mercury, 42.56 ± 0.94 arc seconds per century faster than Newtonian theory.[12] This was an impressive success, but Bob was to emphasize that it depended on the mass model for the Sun. Second, the relativistic deflection of light by a mass concentration is twice the Newtonian value. The deflection of starlight by the Sun arguably was detected and consistent with relativity; the accuracy was at best 10%. Third, in a static mass distribution the fractional shift of the wavelength of light is proportional to the gravitational potential difference through which the light moves. The redshift was detected in spectral lines from surfaces of white dwarf stars, consistent with the theory to perhaps 30%.[13] The contrast to the present range and precision of the experimental basis for gravity physics is striking.

Among his gravity experiments Bob was most proud of the modern Eötvös experiment and the solar oblateness measurement as a probe of the solar interior. The Eötvös experiment monitored the difference of gravitational accelerations of test masses in a torsion balance. The balance is triangular, to suppress tidal torques, with two aluminum weights and one gold. The orientation of the balance is measured by a light beam reflected by an optical flat to intersect a wire vibrating at 3,000 Hz. A servo system electrostatically torques the balance to null the fundamental period in the light passing the wire. A difference of gravitational accelerations of aluminum and gold toward the Sun would cause the feedback voltage to the electrodes to vary with the orientation of the balance relative to the Sun. This elegant experiment showed the fractional difference of gravitational accelerations of aluminum and gold is (1.3 ± 1.0) × 10-11, an improvement of two orders of magnitude. It is no slight to Eötvös's magnificent achievement to say the modern error budget is more reliable.

The oblateness experiment is another memorable example of effective design of an experiment to test a bold hypothesis, that the test of general relativity theory from the rate of precession of the perihelion of the orbit of the planet Mercury may be compromised by the departure from a spherical mass distribution in the Sun.[14] This would be reflected in the shape of the solar surface. By the time of the first oblateness measurements the experimental tests of gravity theories were much improved, in large measure because of Bob's work and example, and they favored general relativity (as they still do). But it was characteristic that, having set out to make this important test, Bob pushed it to the limit for a ground-based observation. With his former students Jeffrey R. Kuhn and Kenneth G. Libbrecht the experiment was improved and moved from Princeton to Mount Wilson (above Pasadena). Observations there suggested the oblate-ness varies from year to year.[15] Now, measurements from the Solar and Heliospheric Observatory satellite, above the blurring of the atmosphere, show the oblateness is close to what would be expected from the mean rotation of the solar surface, [16] indicating the departure from a spherical mass distribution is not a serious factor in the precession of Mercury's perihelion. Bob's former colleagues Henry Hill, Kuhn, and Libbrecht are among those who have established that the solar interior indeed is a dynamic system but not in the way Bob imagined.

While Bob was involved in the demanding Eötvös and oblateness experiments he and his students were producing many other tests of gravity physics. Here are examples with dates of Ph.D. awards. James W. Brault (1962) showed that the gravitational redshift of the solar spectrum is 1.05 ± 0.05 times the predicted value. The Doppler shifts that compromised previous measurements were suppressed by the use of a strong line that originates high in the atmosphere. Kenneth C. Turner (1962) improved the Kennedy-Thorndike bound on the variation of an oscillator frequency with velocity relative to a preferred frame, perhaps one defined by distant matter. The fractional difference of frequencies of two oscillators with relative velocity $\mathbf{u}$ that are otherwise identical may be expressed as $f/f = 2\mathbf{u} \bullet \mathbf{u}/c^2$, where $\mathbf{u}$ is the velocity relative to the preferred frame. The Kennedy-Thorndike bound[17] is $u = 10 \pm 10$ km s^{-1}. The Mössbauer effect with the gamma-ray source and absorber on opposite sides of a centrifuge gave $u < 900$ cm s^{-1}. James E. Faller (1963) obtained an absolute measurement of the local acceleration of gravity, to an accuracy of 7 parts in 10^7, by using one element of an optical interferometer as the freely falling object. Lloyd Kreuzer (1966) tested the universality of the ratio r of active to passive gravitational masses for a solid floating in a liquid. At neutral buoyancy the passive mass distribution is independent of the position of the solid in the liquid. If the ratio r were different in the solid and liquid the gravitational field produced by the active mass distribution would depend on the position of the solid. Kreutzer's limit (for Teflon floating in a mixture of methyl bromide and trichloroethelene) is $|r_1 - r_2| < 4 \times 10^{-5}$ at 68% confidence. With the discovery of the thermal background radiation it became of great interest to improve the measurements of the helium abundance, to compare to the predicted production in the early universe. The helium abundance in a star affects its luminosity for given mass; the mass measurement requires improved orbital elements in older binary stars, which are likely to have closer to primeval abundances. Work on improving measurements of the angular separations of close binary stars commenced with David R. Curott (1965) and Dennis J. Hegyi (1968), and concluded with William Wickes's (1972) interferometer, which is capable of measuring separations as small as 0.2" with experimental error of about 0.008".

Bob's largest experimental collaboration grew in part from his remark (and later independently that of Kenneth L. Nordtvedt) that, if the strength of the gravitational interaction were a function of position, the gravitational acceleration of a body massive enough to have a significant gravitational self-energy would differ from that of a nearly massless test particle. Nordtvedt[18] analyzed the effect in an extension of the parameterized formulation of metric gravity theory. In 1960 Bob with William F. Hoffman and Robert Krotkov showed that an optical corner reflector offers a good way to get precision distances to an artificial satellite. In 1969, at the first moon landing, the astronauts set out a rack of 100 corner reflectors designed to reflect a pulsed laser beam from Earth. The time delay gives a precision distance that can be used to test the Nordtvedt effect, among other uses. The results, under the early leadership of Bob's former student Carroll Alley, now significantly restrict the spatial variation of the gravitational interaction.

Bob's role in the discovery of the thermal background radiation is legendary, and legends tend to beguile even those personally involved. Bob wrote[1]:

> There is one unfortunate and embarrassing aspect of our work on the fireball radiation. We failed to make an adequate literature search and missed the more important papers of Gamow, Alpher, and Herman. I must take the major blame for this, for the others in our group were too young to know these old papers. In ancient times I had heard Gamow talk at Princeton, but I had remembered his model universe as cold and initially filled only with neutrons.

Many have wondered how Bob could have forgotten Gamow's work. The last sentence in this quote agrees with all we know: Memory can fail. For example, when work began at Princeton on a Dicke radiometer to search for thermal cosmic radiation we had to remind Bob that he had measured a significant bound on its temperature two decades earlier. In the second sentence of this quote Bob may be referring to an unpublished paper by one of us on light element production in a hot Big Bang cosmology, written before we knew Gamow already had worked out the key physics. Our paper[19] interpreting the radiation as a fossil of the Big Bang referred to the " " paper. This was inappropriate; Gamow had not yet taken account of the effect of the mass density in thermal radiation on the rate of expansion of the universe through the epoch of light element production. We also referred to a later paper by Alpher, Follin, and Herman[20] that gives a close to modern treatment of the centrally important evolution of the neutron-proton ratio and notes that the predicted hydrogen-to-helium ratio is in the range 1:7 to 1:10 by number, in line with astronomical data. This certainly was one of the most important of the earlier papers. We did miss the most important of all, by Gamow,[21] which set forth the now standard picture for light element production. By 1971 we had the story straight.[22]

Bob arrived at the idea of a hot Big Bang by considering element destruction rather than formation. He favored an oscillating universe as a way to understand what the universe was doing before it was expanding. There has to be a provision for removal of stars and the heavy elements they produce from the last cycle. Bob noted that, if the bounce were deep enough to blueshift starlight from the last cycle to above MeV energies, the radiation would thermalize and could evaporate stars and heavy elements. He persuaded

P. G. Roll and one of us (DTW) to build a Dicke radiometer to look for the thermalized starlight, which would be adia-batically cooled by a large factor since elimination of the heavy elements. News of this experiment led Arno A. Penzias and Robert W. Wilson to realize that the excess noise temperature in a radio telescope at the Bell Laboratories might be extraterrestrial. And cosmology dramatically advanced.

WE ARE DEEPLY GRATEFUL to Annie Dicke for her guidance to Bob's early life in science and society.

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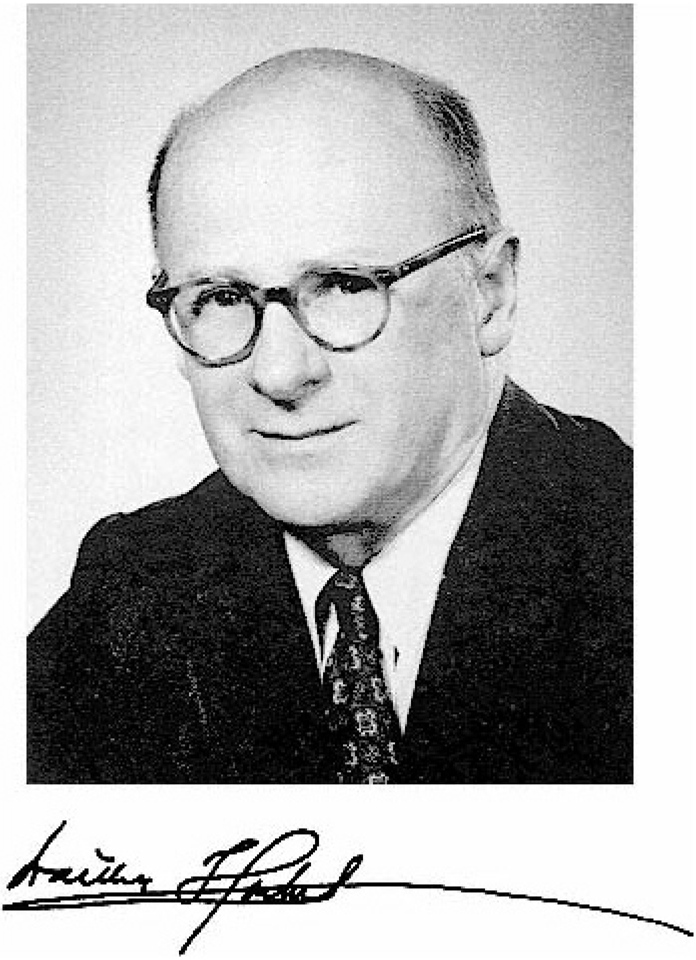

Photo by R. Carter

WALTHER FREDERICK GOEBEL

December 24, 1899–November 1, 1993

BY MACLYN MCCARTY

IN 1924, SOON after he completed his training as an organic chemist, Walther Goebel joined Oswald T. Avery and Michael Heidelberger at the Rockefeller Institute for Medical Research in their seminal work on the nature of the soluble specific substances of the pneumococcus. Their studies had recently established that these constituents of the capsule of the organism were polysaccharides and provided the first evidence that polysaccharides were capable of inducing the formation of specific antibodies. Over the next 20 years, Goebel made major contributions to the understanding of the basis for serological specificity of polysaccharides and their role in eliciting antibodies that protected against pneumococcal infection.

In the course of World War II, Goebel turned to work on the complex antigens of the dysentery bacilli, with the aim of providing more effective antigens for vaccination against infection with these organisms in the military. His work in this area continued during the postwar period and extended to studies on the bacterial viruses and colicines of these organisms before his retirement in 1970.

Walther Goebel was born in Palo Alto, California, on December 24, 1899, at a time when his father Julius Goebel was teaching in the Germanic language department at Stanford University. In this setting he experienced the great San Francisco earthquake of 1906, which destroyed the family home in Palo Alto. There were seven children in the family with an age range of 15 years, and under the influence of their parents, they had strong musical interests, with each child playing a musical instrument. In a pattern rather unusual even early in this century, Walther was tutored at home and did not attend school until he started high school in 1912 in Urbana, Illinois, where his father had moved to a professorship at the University of Illinois.

After completing high school, where an inspiring teacher, L. B. Howell, had stimulated his interest in chemistry, the remainder of Walther's formal education was at the University of Illinois. He received a B.A. in 1920, and then went on to an M.A. (1921) and a Ph.D. (1923) in the Department of Chemistry under the supervision of Professor W. A. Noyes. He added a year of postdoctoral work in Germany (1923–1924) with Professor Richard Willsätter at the University of Munich. This period coincided with the rise of Hitler, and he witnessed the infamous storm trooper attack on students on the Odeons Platz, during which he had the narrow escape of having his hat shot off.

On his return from Germany, Goebel found a position as an assistant in the hospital of the Rockefeller Institute for Medical Research, joining the immunochemical studies of Avery and Heidelberger. He remained at Rockefeller for the rest of his active research career, with promotion to associate member in 1934 and member in 1944. The title "member" was replaced by "professor" in 1957, after the institute initiated a predoctoral program and was renamed Rockefeller University. He became professor emeritus in 1970 on reaching the age of seventy.

Goebel married Cornelia van Renssalaer Robb in 1940.

They had two daughters, Cornelia van Renssalaer Bronson and Anne Kathryn Barkman. Cornelia Goebel died in 1974 after a long battle with rheumatic heart disease. Walther married Alice Lawrence Behn in 1976 and spent his remaining life in retirement in Greenwich, Connecticut. He died at age ninety-four on November 1, 1993.

I got to know Walther Goebel well soon after my arrival in the Avery laboratory in 1941, and we became good friends over the years. He was helpful to me in my early orientation in the laboratory and in general displayed a friendly demeanor. On occasion, however, he assumed a gruff and aggressive exterior that upset some people, but I found this had no depth whatever and could be turned off with a quip. The interest in the arts he had acquired in his early years was maintained, but I do not believe he continued to play a musical instrument. He was, however, responsible for organizing and recruiting the instructor for an evening art course at Rockefeller, which was appreciated by faculty and staff over a period of years.

He was elected to the National Academy of Sciences in 1958. His research contributions were also recognized by an honorary Sc.D. degree from Middlebury College, Vermont, in 1959. In May 1973 he sent me a copy of a letter he had just received and included a handwritten note that read:

> Dear Mac: I thought this would interest you. It was given me the other evening by hand by Westphal. He is here for only three days. Needless to say, I was thrilled, though not certain of my deserving it. We plan to go. There's great wine in Alsace!

The letter was from the German Gesellschaft für Immunologie announcing the creation of the Avery-Landsteiner Prize for pioneering research in immunology and Goebel's selection as the first recipient. Otto Westphal was a distinguished German immunologist, president of the society, and a sig natory of the letter. The prize was, of course, named after Goebel's mentor Oswald Avery and Karl Landsteiner, a Nobel laureate, who had also played a major role in laying the foundations of modern chemical immunology and had come to Rockefeller from Vienna in 1922, so that he was well known by Goebel during his early years with Avery. The prize was presented at the first European Congress of Immunology held in Strasburg in September 1973.

In 1978, at the annual convocation for the awarding of degrees at Rockefeller University, I had the privilege of presenting Goebel for the honorary doctor of science degree, another honor he treasured.

The research activity that Goebel joined on his arrival at the Rockefeller Institute provided new insights in both bacteriology and immunology. The pathogenic bacterium involved, called the pneumococcus (and today classified as *Streptococcus pneumoniae*), was the principal cause of lobar pneumonia, one of the leading causes of death at that time. Earlier work on this organism, much of it from the Avery laboratory at Rockefeller, had established that many serologically different types of pneumococci existed, and that immunity to infection was type-specific (i.e., immunization with one type provided protection against only that type). Avery and his coworkers had found that this type-specific immunity was referable to a "soluble specific substance" present in the capsule surrounding the pneumococcal cells. Since it seemed important to know more about this soluble substance, Avery had sought the aid of a biochemist in the hospital, Michael Heidelberger, in determining the nature of this capsular material. At the time of Goebel's arrival, they had recently reported that this material responsible for type specificity of pneumococci was composed of polysaccharides.

Polysaccharides are large molecules formed by the join ing together of many molecules of simple sugars. Common table sugar, sucrose, is a disaccharide composed of only two sugars: glucose and fructose. Starch, on the other hand, is a polysaccharide made up of numerous glucose molecules. There are a large number of different simple sugars, thus providing for a wide variety of diverse polysaccharides, some of which have four or more different simple sugar components. When the nature of the soluble specific substances was discovered by Avery and Heidelberger, polysaccharides were not believed to be antigenic to induce the formation of specific antibodies. Only proteins were considered to have these properties. The findings of Avery and Heidelberger were thus revolutionary and not immediately accepted, so that it was important to undertake studies to further verify them and to establish the basis for the antigenic specificity of the polysaccharides. Goebel was quickly immersed in this work, which he pursued for the next 20 years.

He joined the ongoing research of Heidelberger and Avery and participated in the studies that led to the third paper on the polysaccharide nature of the specific soluble substance of the pneumococcus. With Heidelberger he turned to more detailed analysis of the composition and structure of the type III polysaccharide. They showed that this was an acidic molecule made up of two sugars, glucose and glucuronic acid. Additional work revealed that the total molecule was composed of repeating units of a disaccharide of the two sugars, combined in a specific linkage referred to as aldobionic acid. Thus, the principal antigen of one of the most virulent of the known pneumococcal types was shown to have relatively simple, repetitive composition.

During this period, the group also showed that their findings were not limited to the pneumococci. They found that the specific soluble substances of another bacterium associated with pneumonia, termed "Friedländer's bacillus" for its discoverer, were also polysaccharide in nature. This was another organism that occurred in different specific types determined by the nature of the polysaccharides involved. In later years, other examples of the importance of the capsular polysaccharides in pathogenic bacteria were described elsewhere.

Goebel began his own studies directed at determining the relationship between the chemical composition of monosaccharides and disaccharides and their ability to induce the formation of specific antibodies. To accomplish this it was necessary to devise a way of obtaining specific antibodies to the simple sugars. This he accomplished by synthesizing p-aminophenyl derivatives of several common sugars and disaccharides, so that they could be coupled to proteins for use as antigens. Distinct differences were found in the antibodies formed by even closely related sugars. The most dramatic of these experiments was the demonstration that the anomeric forms of glucose could be distinguished from one another by the antibody response. These anomers (termed alpha and beta) are determined solely by the relative position of the-H and-OH substituents on the first carbon of the molecule. Thus, Goebel established that a minor change in the configuration of a sugar molecule could alter its antigenic specificity.

Using these procedures, Goebel returned to a study of the type III pneumococcal polysaccharide. He synthesized the glucose-glucuronic acid disaccharide and linked it with p-aminophenol, so that it could be coupled with proteins. Antigens formed in this manner with several different proteins were each able to induce the formation of antibodies that precipitated with the type III polysaccharide. In addition, these antibodies protected animals against infection with type III pneumococci but not types I or II. This established with a synthetic antigen that the disaccharide was the determinant of antigenic specificity and sufficient for eliciting neutralizing antibodies against the intact polysaccharide as expressed on living pneumococci.

In the latter phases of his work on pneumococci, Goebel and his colleagues turned up other findings of interest. Examples are studies on the type XIV pneumococcal polysaccharide and its relationship to the blood group A specific substance, and work on the nature of the group-specific or somatic polysaccharide of the pneumococcus, which is common to all types of the organism.

At the onset of World War II Goebel changed the subject of his research in order to participate in the war effort, selecting studies of the principal organisms involved in dysentery. He and his colleagues focused on the specific antigens of *Shigella dysenteriae* and the development of these substances as more effective immunogens for use as vaccines against the disease. His work with the various Shigellae and their dominant surface antigens, which were shown to be composed of a lipo-carbohydrate protein complex, continued in the postwar years. These studies were extended to the bacterial viruses, or bacteriophages, of these organisms, showing that the antigenic complex included the receptors required for infection with these agents. Subsequently, studies were carried out on the nature and properties of substances known as colicines, which were produced by the bacteria and also found to be associated with the surface antigenic complex.

These were productive and useful studies, but they lacked the flavor of novelty and originality associated with the earlier work on polysaccharides. Interestingly, in a brief assessment of his "discoveries" found in the biographical material on file at the National Academy of Sciences' membership office, Goebel cites only the studies on the polysaccharides and does not mention the later work. His summary of his discoveries was as follows:

> These investigations indicated beyond question that it is the precise chemical structure of the carbohydrate, be it simple or complex, that determines its immunological specificity, a fact hitherto unknown.

This personal statement of his contributions provides a concise and objective summary of his most important work.

Although he maintained some contact with laboratory work after leaving Rockefeller, this part-time effort was finally replaced by full retirement in Greenwich.

THE BIOGRAPHICAL MATERIAL from the files of the National Academy of Sciences, including the personal observations Goebel had supplied, provided a range of the most useful sources. A copy of the complete list of Goebel's collected papers was obtained from the Rockefeller Archives. In addition, my own personal files (e.g., those related to the presentation for the honorary Sc.D. in 1978) also proved useful.

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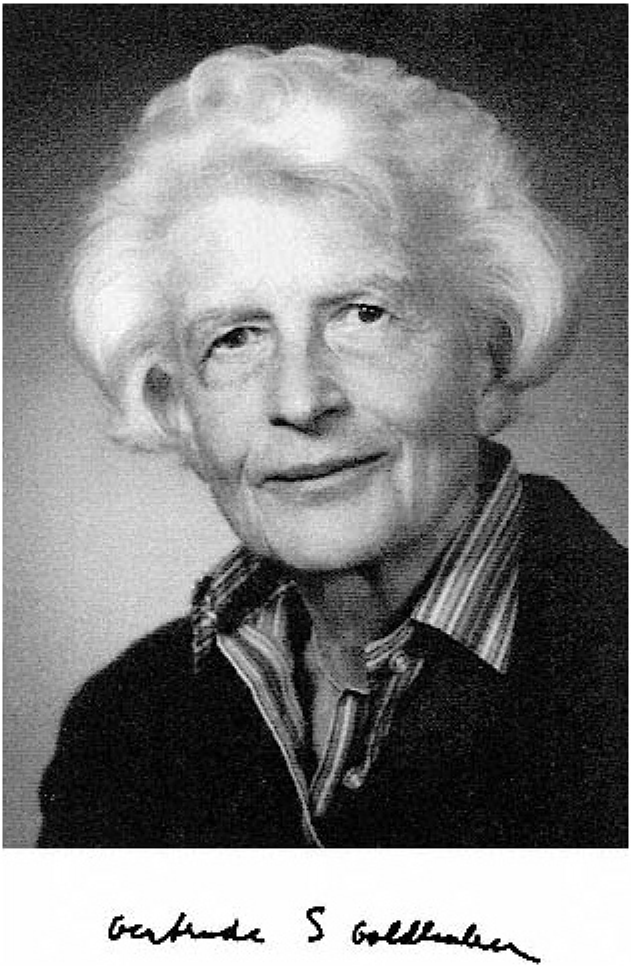

GERTRUDE SCHARFF GOLDHABER

July 14, 1911–February 2, 1998

BY PETER D. BOND AND ERNEST HENLEY

THROUGHOUT HER LIFE Gertrude Scharff Goldhaber had to struggle against tyranny and discrimination, as a child during World War I, as a Jew in Nazi Germany, as a woman in a scientific discipline when there were few such practitioners, and as the wife of another scientist at a time of strict nepotism rules. That she was so successful is a testament to her talent, drive, and will.

A person's life has peaks and valleys, sometimes so gentle that even the one living that life is barely aware of them, and sometimes so sharp that a mere glance is enough to bring them into high relief. Neither type is better or worse, but the dramatic form gives clearer insight into character, the most important thing one can learn about any person. Trude's life definitely belongs in the dramatic category.

We knew Trude, as she was called by her friends, above all as a dedicated physicist. She was often passing on news about a recent advance, whether hers or others. Despite all the hardships she endured, she was predictably cheerful. Probably, her happy marriage and family life helped her achieve an ever-cheerful disposition.

Gertrude Scharff was born in Mannheim, Germany, on July 14 (Bastille Day), 1911, just a short time before the relative serenity of the previous century was shattered by the outbreak of World War I. Many people know about the battlefield horrors of that war, but even well behind the lines things were tough. Trude recalled bread made partly of sawdust—good roughage but not very nutritious!

She attended public school in Mannheim, where she developed an interest in science, with the support of her parents (surprising for that time), perhaps because her father had to abandon a plan to go into chemistry at the age of sixteen when his father died and it became necessary to work in the family's food wholesaling business. Even with the end of the war, life in Germany was difficult, with hyper-inflation in the mid-1920s, followed by worldwide depression. Nevertheless, Trude went on with her studies, entering the University of Munich and fairly soon coming to focus on physics. Although her father preferred that she be a lawyer, she recalled saying, "I'm not interested in the law. I want to understand what the world is made of."[1]

A frequent pattern in those days was for students to spend semesters at various universities, and she did that three times, visiting Freiburg, Zurich, and Berlin before settling down to thesis research with Walther Gerlach at Munich. In Berlin she first met her future husband. We are unaware of any local female mentors or role models, but she had good relations with her professors.

A major problem arose with the accession of the Nazis to power in 1933. It became increasingly difficult for Jews working at a university or running a business to continue to do so, and next to impossible for one to find a position. Her father was jailed, and after release went with her mother for a short stay in Switzerland. After returning, they never succeeded in leaving Germany again; both perished in the Holocaust.

Meanwhile, Trude attained her Ph.D. in 1935; as work in

Germany was impossible, she headed for London. Her younger sister Liselotte had already left Germany and currently resides in France. Trude stated "I should have left earlier, but since I had started my thesis I felt I should finish."[2] Having a Ph.D. degree actually was a disadvantage, as there was more room for refugee students than refugee professionals. For six months Trude lived from the proceeds of selling a Leica camera and translating German manuscripts into English. Eventually she got a job in a lab run by G. P. Thomson, but she never obtained a real position. In 1939, prior to the beginning of World War II, she and Maurice Goldhaber married, and she moved to join him at the University of Illinois in Urbana.

An issue of women in science then came to the fore. Maurice already was on the faculty at Illinois, and so, according to strict interpretations of state anti-nepotism laws, she could not be hired as well. Beyond that she was not even allowed lab space, so her only options were to quit research or to accept her husband's invitation to join him in his lab, with no salary, of course. With her drive, there was no real choice. Not only was she unpaid, which meant that Maurice's salary had to cover all costs, including in due course child care for their two sons Alfred and Michael, but also she had to switch fields to make use of the resources in the lab, which was devoted to nuclear physics. Obviously, she made the transition with considerable flair.

Finally, some time after the war, she was placed on a soft-money line in the Physics Department at Illinois. We have heard that women faculty there in more recent times have expressed a debt to her for waging a struggle that at least weakened the resistance to hiring women that she experienced and made life easier for her successors. In 1950 the family moved from Illinois to Long Island, where Maurice and Trude both joined the scientific staff of Brookhaven

National Laboratory. It was fifteen years after obtaining her doctorate that Trude for the first time received a regular long-term, paid position. During almost all of the intervening time she had been engaged in experimental physics research, but without the recognition that would normally have been expected for her accomplishments. The only time she was not doing research was during part of the war, when she joined an engineer at Illinois in applied work.

Trude was always willing to help the profession by serving on committees. She was elected a fellow of the American Physical Society in 1947, and in 1972 became only the third female physicist to be elected to the National Academy of Sciences. She was a member of the Advisory Board for the Nuclear Data Tables and Sheets and served as chair of the National Research Council Panel on Nuclear Data Compilations. She was a member of the American Physical Society's (APS) Committee on the Status of Women in Physics and served on the National Science Foundation's Advisory Committee for the Physical Sciences and as a member of the Board of Trustees of Universities Research Association, Inc. She was an APS councilor and chair of its Panel on Pre-College Physics Education. She also served on many other committees. She received the Long Island Achiever's Award in Science in 1982 and the 1990 Outstanding Woman Scientist Award from the New York Chapter of the Association for Women Scientists. She initiated a training institute for Suffolk and Nassau County science and mathematics teachers and in 1960 she started the still justly famous monthly Brookhaven Lecture series. Trude served as a Phi Beta Kappa visiting scholar in 1984–85.

Now to her research. In Munich for her doctorate Trude studied the effects of stress on magnetization (1936) and completed her degree in 1935. She recalled her job hunt: "I wrote to 35 refugee scientists. They all wrote back and said, 'Don't come here. There are already too many refugees.'"[2] Actually one did write back with hope—Maurice Goldhaber, who was in Cambridge, thought something might turn up. Trude joined G. P. Thomson as a postdoc where she worked on electron diffraction. Once she got into nuclear physics she continued to focus on what today would be called low-energy aspects of the subject. Working with students who later had distinguished careers, she studied neutron-proton and neutron-nucleus reaction cross sections (1941) and then gamma radiation emitted or absorbed by nuclei (1942). In the early 1940s, Trude found that spontaneous nuclear fission, which might have been a gentler process than neutron-induced fission, is also accompanied by the emission of one or more neutrons. While such neutron emission was widely suspected to occur, she appears to have been the first to make a direct observation of this important phenomenon. The work was classified during the war, so it was not published until after the war was over (1946).

Shortly after the war, she and Maurice collaborated on an important experiment about the nature of elementary particles. The electron was the first explicitly identified elementary particle, and had been accepted as a basic constituent of atoms and therefore of all familiar matter. In certain rare processes, known as weak decays, so-called beta particles were produced that looked very much like electrons, and it was worth knowing whether they were identical to electrons. Many experiments had tried to find a difference by precisely measuring the charge-to-mass ratio for electrons and beta rays. What Trude and Maurice did in their experiment (1948) was to let beta particles impinge on lead, whose atoms have a large atomic number. If the betas were different from electrons, they could occupy the same locations as deeply bound atomic electrons, and therefore could fall in towards the nucleus of an atom, releasing a large amount of energy in the process. If they were identical, the Pauli exclusion principle would prevent the betas from doing so. The lack of X rays carrying the required energy showed that the beta particles must be indistinguishable from electrons. She also carried out important investigations of very long-lived excited states of nuclei, so-called isomers or isomeric states (1948, 1950, 1951).

A little later, Trude started to focus on her life work, determining the properties of nuclei when they are only "tickled" or excited a bit, a field where little was known when she started. In many cases, an excitation may be pictured as a slow rotational or vibrational motion of the nuclear medium as a whole, rather than just one nucleon jumping into an excited state like an electron in an atom (1952, 1953, 1954). Her early studies of such systems were an important component of the background for the collective theory of nuclear motion, which earned a Nobel Prize for Aage Bohr and Ben Mottelson.

From the early 1950s onward, Trude directed her scientific research to systematizing the properties of nuclear levels across the entire periodic table. Being methodical and purposeful she was ideally suited for these studies. It was a time of rapid development in both experimental techniques and nuclear structure theory. Both independent-particle and collective-motion models were proposed to describe nuclear excitations, but the delineation of the regions of applicability of each was not well defined. The then-recent Mayer-Jensen shell model appeared to contradict what was known about the short-range saturated nuclear forces, with their implied pronounced nucleon clustering: there was speculation that the forces that were known to act between free nucleons were somehow modified in dense nuclear matter and that shell structure existed only for ground states. Evidence for shell closures in heavier nuclei lacked experimental evi dence. Trude's research at this time was devoted to resolving these apparent contradictions. In 1953, Trude demonstrated the influence of shell structure on excited nuclear states through a detailed and comprehensive survey of the energy of the first excited states of even-even nuclei as a function of neutron number. Her conclusions included the important contribution that the excitation energy increased strongly at the shell closures.

A few years later she noted that the ratio of excitation energy of the second excited state to the first in nuclei in the $38<N<88$ region was much different than that in the adjacent region of $90<N<108$. The value in the lower mass region is near the ratio of 2 identified with phonon excitation in spherical nuclei, whereas in the upper mass region the value is near 10/3, signaling an abrupt change to the rotational states of deformed nuclei (1955). In 1957, she reported the remarkable isomerism in Hf-180 with its implied retardations of 10^{-16} and 10^{-9} for the El and E3 transitions respectively, dramatically verifying the prediction that in deformed nuclei there is a strong inhibition of electromagnetic transitions where the projection of spin along the symmetry axis changes.

Over many of the following years she developed the phenomenological variable moment of inertia (VMI) model (1969, 1970, 1976) and was able to smoothly parameterize the energy ratio of the first 4+ to 2+ states of the even-even nuclei over most of the periodic table with values ranging from about 2.2 to 10/3. In addition, this model gives a semi-empirical description for intrinsic and transition quadrupole moments and level energies. This also led to what may have been the first mother-son collaboration in physics. Famous precedents existed for all the other parent-child combinations, but not for this one. The joint work (1970, 1978) was based on the idea that if the nucleus has a dis torted shape, that fact should be seen both in its rotational behavior and in the electromagnetic radiation due to a related distortion in the distribution of electric charge. In these publications a case was made that there is a simple and instructive relation between the electrical and the mechanical deformation of the nucleus. Her research culminated with studies of high spin states, states that tend to be at the limit of high deformations (1973, 1983).

A tool that Trude developed to explain her findings to others, three-dimensional plots, has evolved into something so commonplace in this computer age that its origins may have been forgotten. In her case, the energy of the lowest nuclear excitation was plotted against the number of protons and the number of neutrons in the nucleus. To accomplish this, wooden bars with height proportional to the excitation energy were glued onto the appropriate isotope on the chart of the nuclides. In this way, one could see peaks and valleys, which gave an immediate intuition about the changes in behavior from one set of nuclides to another. Nowadays this kind of visualization is done with computer graphics and so has become much easier, if not as tangible.

Taken as a whole, Trude Goldhaber's work played an integral part in unfolding the story of nuclear structure, alerting experimentalists to regions of the periodic table of importance and confronting theorists with the realities of nature.

Throughout her life Trude faced adversity, and worked hard to overcome it. She was tenacious in her arguments and had a forceful personality. After starting in a regular position so late she was compelled to retire (in a way that couldn't happen now) at the age of sixty-six, as was the policy at Brookhaven National Laboratory. Her regular employment lasted only 27 years. For someone looking only to earn a living this might have been acceptable or even welcome, but for a dedicated researcher like her it was another blow. She continued for some years as a collaborator with researchers holding grants at other institutions, but the eventual end of that support left her angry. It would have been easy for the all-too-understandable anger to become bitterness, but that never developed.

Both long ago and since her death many people, men as well as women, have noted how she reached out to help and encourage them as they were starting out or at any stage when they were facing difficulties, whether professional or personal. Besides this kind of individual concern, she worked both at Brookhaven and on the national scene to develop educational initiatives and opportunities for school-children, university students, community members, and, of course, people already working in research. Her part in founding Brookhaven Women in Science is a characteristic example. Her interests were not confined to nuclear physics. She closely followed developments in biology and medicine, and many senior figures in those fields enjoyed and appreciated her involvement. Outside science she was interested in how mythology reflects a civilization, and she was an avid tennis player and mountain climber.

Even when her disabilities began to constrain her more and more, she continued each day as she had all her life to find fulfillment in what she still could do and experience. This readiness always to appreciate the positive and to maintain her dignity in the face of limitations and adversity was as admirable as it was amazing. With her intermittent hospitalizations, the last years were akin to a stone skipping on the water, rising again and again, usually not quite as high as the previous time, and finally disappearing into the waves.

Her contributions to science, to education, and to gaining recognition and equality for women will have a lasting impact.

In preparing this manuscript we received important help and contributions from Alfred Goldhaber and Chellis Chasman.

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CARL W. GOTTSCHALK

April 28, 1922–October 15, 1997

BY MAURICE B. BURG

CARL W. GOTTSCHALK made critical discoveries in renal physciology and pathophysiology with innovative techniques of micropuncture. One of his earliest findings was definitive proof of how urine is concentrated by countercurrent multiplication. That discovery catapulted him to the front ranks of renal physiologists early in his career. In the following years he made many more important observations about the mechanism of urea excretion by the kidneys, the role of renal nerves in salt and water excretion, urinary acidification, and pathophysiologic mechanisms of acute and chronic renal disease. Because of the brilliance and quality of his research he was widely recognized as a leader in renal research.

EARLY LIFE

Carl William Gottschalk was born on April 28, 1922, in Salem, Virginia, to Lula Helbig and Carl Gottschalk. His father had been born in Germany and emigrated to South Africa, where as a machinist he fabricated and repaired cigarette-making machines. Then, at age twenty-two he came to the United States. He lived most of his life in Salem, Virginia, where he owned a movie theater, automobile repair shop, and other downtown property. Carl William Gottschalk's older brother is a mathematician and was formerly head of that department in Wesleyan College in Middletown, Connecticut.

As a boy, Carl was studious, focused, and quiet. His early and ardent penchance for collecting stayed with him his whole life. His earliest collections were of stamps, coins, and most notably butterflies. At age fifteen he discovered a new butterfly in the hills of Virginia. The butterfly named for him, *Stryman cecrops gottschalki*, became the subject of his first scientific paper (1942). This precocious paper combined two of the great passions in his life, namely, science and collecting.

Carl graduated from Roanoke College in 1942 and the University of Virginia Medical School in 1945. His time in medical school was abbreviated because of the urgency of World War II. He took his medical internship at the Massachusetts General Hospital.

In Boston during his internship he met Helen Scott, a nursing student from Pennsylvania. They married in 1947 and had three children: Carl Scott Gottschalk, now an architect; Karen Gottschalk Strehlow, a pathologist; and Walter Parks Gottschalk, director of the Water Purification Plant of Chapel Hill, North Carolina.

Following his internship, Carl was drafted into the army as a medical officer and was stationed at the Army Medical Research Laboratory in Fort Knox, Kentucky, where he investigated physiological effects of cold exposure in humans. During his travels at that time he assembled a unique collection of Canadian arctic butterflies that now graces the Peabody Museum of Natural History at Yale.

REVIVING MICROPUNCTURE

Carl's interest in kidney micropuncture began during a postdoctoral fellowship with Eugene Landis at Harvard

Medical School between 1948 and 1952. He began by performing some kidney studies with Daniel Tosteson (1951), then a Harvard medical student. At the time they were surprised to observe increases in urine excretion when the kidney was partially infarcted by ligation of branches of the renal artery. Since, from the literature, the best explanation seemed to be changes in renal interstitial pressure, Carl, who was already interested in cardiology and hemodynamics, began a systematic study of renal interstitial pressure using small hypodermic needles and large glass pipettes. However, it became obvious to him that insertion of what he later (1990) called a "crow bar" into the renal parenchyma damaged many tubular and vascular structures and that a proper study would require micropuncture of individual tubules and micro vessels. It was not until a few years later, however, when he had finished his medical residency at Harvard and had started as a fellow in cardiology at the University of North Carolina in Chapel Hill in 1952 that he was able to assemble the necessary equipment to begin micropuncture.

Renal micropuncture had been developed at the University of Pennsylvania jointly by A. Newton Richards, who for many years was chairman of pharmacology, and Joseph T. Wearn, a young physician who had joined Dr. Richards's laboratory in 1921. Richards's group produced a series of famous reports, notable for their reliability, detail, and cautious interpretations, which established beyond doubt and in great detail the nature of renal glomerular filtration and of selective tubular reabsorption. The studies were confined to amphibian kidneys until 1941. Then, two persons in Dr. Richards's group, Arthur M. Walker and Phyllis A. Bott, working in collaboration with Jean Oliver (who was later a close associate of Carl's) and his able assistant, Muriel C. MacDowell, extended this work to mammalian species. In

1941 they published two landmark micropuncture studies in rodents. That same year when World War II intervened, the Richards laboratory was permanently disbanded as Richards and most of his coworkers entered national service. No other laboratories filled the gap, so that when Carl Gottschalk wanted to use the technique, there was no one to teach him.

Carl learned why the micropuncture technique had lapsed from Richards himself. He discovered that "Dr. Richards did not encourage the revival of the micropuncture technique after World War II and advised me, and I suspect others, against entering the field" (1969). Lest Richards be misunderstood, Carl added, "I am certain he had no selfish or proprietary motivations for doing this; rather he was concerned that a field to which he devoted so much of his life would be sullied by less competent workers." Richards and his colleagues took almost unbelievable care to assure the correctness of their results. Dr. Richards's reluctance arose from the fear that others less meticulous would publish misleading results. This fear was clearly misdirected in Carl Gottschalk's case, considering the greatness of Carl's eventual accomplishments with the technique.

Richards's advice aside, Carl set up a micropuncture laboratory at the University of North Carolina in Chapel Hill. He had moved to Chapel Hill as a cardiology fellow and instructor in 1952. When asked later why he chose that position, Carl said that it was the only job offer he had. He liked clinical medicine, but he recognized early in his career that it was science that tugged hardest at his heart and soul and that he could not combine the two if he wanted to succeed in either. So within a few years, he dedicated himself to bench research and began the challenging task of setting up the micropuncture laboratory with little space and less money. Carl's laboratory initially was very modest.

It had the dimensions of a chicken coop and a $2,000 grant from the Edgecomb County North Carolina Heart Association. In collaboration with Margaret Mylle, who became his long-term associate, he began working in earnest and soon published studies of the hydrostatic pressure in the tubules and small vessels of the rat kidney (1956). From 1957 to 1992 the American Heart Association supported Carl as an established investigator and then as a career investigator, which made it possible for him to discontinue clinical activity and devote his life to bench research.

A DECISIVE MICROPUNCTURE EXPERIMENT

Carl addressed what was the most important problem in renal physiology at the time. He later summarized the background in a commentary accompanying republication of his original landmark article (1959) as a "Milestone in Nephrology" in the *Journal of the American Society of Nephrology* . He recalled that in the 1950s renal physiologists understood reasonably well how the urine is diluted. In the absence of antidiuretic hormone some renal epithelial cells are impermeable to water, so that active reabsorption of NaCl lowers the concentration in the remaining fluid. The mechanisms responsible for high concentration of the urine remained perplexedly undefined, and investigators had little recourse but to invoke the active transport of water out of the nascent urine. However, nowhere in the animal kingdom was there a proven example of active transport of water, so this explanation was both unsatisfying and unproven.

An alternative hypothesis, the countercurrent theory, had been proposed by the Swiss physical chemist Werner Kuhn, but it was not generally accepted by renal physiologists of the day. Kuhn's hypothesis invoked passive diffusion of water out of the collecting ducts into the fluid surrounding them in the renal medulla and involved salt transport by the loop of Henle to concentrate the surrounding fluid. A striking feature of Kuhn's model was that transport between the countercurrents of fluid moving up and down in adjacent limbs of the hairpin-like loop of Henle in the renal medulla concentrates the fluids both in the loop and the surrounding tissue; these fluids become progressively more concentrated as the tip of the renal medulla is approached. In support of his theory Kuhn actually constructed and tested countercurrent model systems and demonstrated several arrangements by which concentrated solutions could be produced in compartments separated by semi-permeable membranes. The theory attracted little immediate notice because it was published in German in 1942 at the very height of World War II.

Simultaneous evidence for the theory came in a collaboration between Kuhn and Heinrich Wirz, who was in the Physiology Department of the University of Basel, where Kuhn chaired the Department of Physical Chemistry. Over a five-year period they demonstrated several of the predicted consequences of the countercurrent theory. They found by cryoscopic measurements of kidney slices that there is an osmotic gradient in the renal medullary tissue with the highest concentrations at or near the tip of the renal papilla. By micropuncture they found a corresponding gradient in the blood vessels (vasa recta) of the renal medulla. Also, by micropuncture they found that during hydropenia the concentration of solutes in distal tubular fluid at the surface of the cortex increases from that of a dilute solution at the start of the distal tubule to a concentration similar to systemic plasma (but never higher) near the end of the distal tubule during antidiuresis.

Despite Wirz's extensive experiments there was no experimental proof for one critically important prediction of the theory, namely, that fluid in the bend of the loops of Henle should be hyperosmotic not only in antidiuresis but also in water diuresis. Notwithstanding his efforts, Wirz had been unable to micropuncture the loops of Henle because he could not distinguish them from vasa recta on the surface of the renal papillae of living animals.

There was very serious skepticism about the countercurrent theory. When Homer Smith, then the acknowledged "dean" of renal physiology, visited Chapel Hill in 1953, Carl had the opportunity to discuss the countercurrent concept with him. Carl recalled (1997) that Smith said "the smart boys don't believe in it." Nevertheless Carl found the counter-current hypothesis attractive and credible, perhaps because he had no formal training in renal physiology and was not burdened by establishment biases. Smith did not change his opinion until much later after he had the opportunity to review the data in Carl's paper, and even then he confessed that he didn't like it.

Other highly respected authorities also remained skeptical of the countercurrent theory, which seemed unbelievably complicated. There was an ongoing search for other mechanisms. For example, Robert Berliner, then at the National Institutes of Health, speculated that the fluid in the bend of the loop of Henle might be hyposmotic, and he was developing indirect evidence to support his theory. Hypertonicity of the loop of Henle fluid clearly was the smoking gun. Finding it was the evidence needed to prove the counter-current hypothesis.

By this time Carl and Margaret Mylle had substantial experience in micropuncture of renal tubules exposed on the cortex of the rat kidney, as well as expertise in confirmation of the localization of the puncture sites with microdissection techniques. Further, J. A. Ramsay and R. H. J. Brown had described a method for measuring the osmolality of nanoliter volumes of fluid. Based on the published description, Gottschalk and Mylle constructed a similar instrument. They were soon able to confirm the 1941 work of Walker, Bott, Oliver, and MacDowell that fluid in proximal tubules at the surface of the renal cortex maintains the same osmolality as plasma and also the later work of H. Wirz that fluid in early distal tubules has a low osmolality, regardless of the urinary concentration. However, the hardest challenge remained: the sampling of fluid in the loop of Henle.

It was so difficult at the time to distinguish loops of Henle from vasa recta in vivo that Carl took special pains to describe his method for doing so in his preliminary report in *Science* (1958). As evidence that the collected fluid was from renal tubules and not blood vessels, Gottschalk and Mylle showed that it did not contain appreciable protein. As well, they injected dye followed by microdissection, showing that the punctured loop was contiguous with more proximal and distal parts of the nephron.

The classic report (1959) is remarkable for its thoroughness and the extraordinary care with which the experiments were conducted. They used four species of mammals with different kidney anatomy under three different conditions, namely, hydropenia, water diuresis, and osmotic diuresis. The results were unequivocal. The data from nine hamsters, one kangaroo rat, and one *Psammomys obesus* showed virtual equality of osmolality in collecting ducts and at the bend of the thin loops of Henle during antidiuresis. This was a technical triumph, providing final decisive evidence for the countercurrent hypothesis. Carl later wrote (1997), "Nothing I had ever done before or have done subsequently was as thrilling as obtaining these data."

LATER STUDIES

Over the next thirty years, Carl made many more advances in renal physiology, which he published in more than 100 papers. His influence extended to numerous scientists whom he trained and collaborated with. Prominent among these is Bill Lassiter, who initially joined Carl to add his expertise with the use of radioisotopes to the research. Bill remained a long-time collaborator and friend. One of their first collaborative efforts yielded, as was so often the case in that laboratory, a surprising and exciting discovery (1961). They found a large net addition of urea to the fluid in the loop of Henle, indicative of urea recycling in the renal medulla. That and additional urea recycling pathways discovered subsequently facilitate concentration of urea in the renal medulla and urine during antidiuresis.

Another important discovery was the direct role of renal innervation in the renal tubular handling of sodium chloride. It had been known for many years that denervation of a kidney increases the rate of salt and water excretion by that kidney. The burning question was whether the denervation acts by decreasing renal tubular transport or increasing glomerular filtration. A change in glomerular filtration too small to be measured directly by renal clearance methods could easily do this. The micropuncture experiments in Carl's laboratory settled the issue conclusively by showing directly that the renal nerves affect the rate of salt reabsorption by the tubules. With Elsa Bello-Reuss and others, Carl found by direct measurements that denervation decreases sodium reabsorption by the proximal tubule (1975) and that renal nerve stimulation increases the rate (1976). With Romulo Colindres the observations were later extended to conscious, unanesthetized animals (1986). As with the experiments on the concentrating mechanism, Carl designed careful and insightful experiments that proved to be decisive.

There were also pioneering studies in urinary acidification (1960), calcium excretion (1963), potassium depletion (1965), glomerular dynamics (1980) and much more, in cluding some very important observations on the pathophysiology of acute (1968, 1975) and chronic (1974, 1975) renal failure. Carl and his colleagues found that in diseased kidneys the rate of filtration in individual nephrons and the hydrostatic pressure in individual tubules varied over a large range. In each nephron, however, the rate of reabsorption by proximal tubules is in balance with the rate of filtration so that the fraction reabsorbed proximally varies no more in diseased kidneys than in normal ones. For an individual in balance at any salt intake, as filtration rate falls, the same amount of salt can be excreted only if the fraction of the filtered salt that is excreted is increased. That means that each distal tubule must reabsorb a smaller part of what is delivered to it. Carl named this process the adaptive nephron.

Associated with these later studies, were numerous trainees and collaborators whose lives Carl touched. When Carl first set up his laboratory, micropuncture was limited to it and a very few other newly established laboratories. Carl trained or invited to his laboratory many young scientists who wanted to be on the cutting edge of renal physiology. Many of them went on to prominent positions. They include Karl Ullrich, who studied tubule fluid composition (1963) and electrochemical potentials (1963); Michael Kashgarian, who investigated transtubular electrochemical potentials (1963); Thomas Biber, who studied acute tubular necrosis (1968); Francois Morel, who developed tracer microinjection measurement of tubule permeability (1965); Andrew Baines and Paul Leyssac, who studied proximal tubule function (1968); Klaus Thurau, who studied glomerulo-tubular feedback; Marjorie Allison, who studied chronic renal failure (1975); and Bill Finn, who studied acute renal failure. William Arendshorst, who shared in studies of glomerular ultrafiltration dynamics (1980) and acute renal fail ure (1975), succeeded Carl as head of the micropuncture laboratory.

SERVICE AND AWARDS

In addition to his research, Carl Gottschalk established an impressive record of service. One of his major contributions was serving as president of the American Society of Nephrology (1975–76).

Another stellar instance of his service was as chair of what came to be called the Gottschalk Committee. The actual name was the Special Committee on Kidney Disease, initiated by the Bureau of the Budget in 1966. At that time Belding Scribner's development of methods for continuing access to blood vessels had made it possible for chronic hemodialysis to keep alive and in reasonable condition patients with end stage renal disease. The demand for such treatment greatly exceeded the availability of equipment and trained people to supply it. The Gottschalk Committee was charged with determining the prevalence of chronic renal disease in the country, the percentage of such patients that could be treated with dialysis, and whether dialysis had in fact moved from an experimental to a therapeutic procedure. The committee contained individuals of widely varying backgrounds, specialties, and opinions. Robert Berliner, who sat in as an observer, attests to the remarkable job that Carl did to bring that ill-assorted group to a consensus. The conclusions and recommendations were far-reaching. The committee reported that there were a large number of patients with chronic renal disease who could be treated successfully with dialysis and they recommended that no one should be denied these forms of treatment for financial reasons. The recommendations served as effective ammunition in the hands of those who later successfully lobbied for the passage of the end stage renal disease amendment to

Public Law 92–603. That legislation has saved the lives of literally tens of thousands of patients. Carl also served on numerous other university and national advisory committees.

He was on the editorial boards of *Physiological Reviews, Circulation Research, American Journal of Physiology, Journal of Applied Physiology* , and *Kidney International*. He edited the third edition of *Diseases of the Kidney* with Lawrence Early and the fourth through sixth editions with Robert Schrier.

He delivered numerous distinguished lectures, including a Bowditch Lecture, and a Harvey Lecture. He received numerous awards including the Homer Smith Award in Renal Physiology, the David Hume Award of the National Kidney Foundation, the A. N. Richards Award of the International Society of Nephrology, and the first Berliner Award for Excellence in Renal Physiology. He received honorary degrees from Roanoke College and Universite de Mons-Hainaut. He was elected to the National Academy of Sciences in 1975.

Carl was professor of medicine and physiology at the University of North Carolina from 1961 to 1992 and distinguished research professor of medicine and physiology from 1992 until his death. He served on almost all important committees of the university and remained a major figure in university life.

COLLECTOR AND HISTORIAN

Carl was a close friend of Jean Oliver, a distinguished renal anatomist and pathologist. Oliver, who had no children, considered Carl a protégé, virtually a son. When Oliver became depressed after his wife's death, Carl and his wife Helen gave Oliver much support. Oliver collected rare books, Chinese and Japanese paintings, and other works of art, many of which he bequeathed to Carl. He also gave Carl all of his original lab notebooks and photomicrographs of his kidney dissections. Carl's widow Susan Fellner has donated this material to the Wilson Library of the University of North Carolina.

Carl had always been a collector. The association with Oliver resulted in Carl's extending his interest in collecting to rare scientific books and Asian art. Carl and Helen and two other couples spent a month in a vicarage in London every year for about eight years. It was there that he fine-tuned his collecting, becoming known to every rare book dealer in the city. They sent him catalogues regularly and called if they had a special volume he was seeking. So, over the years, and especially after the death of Helen, when he was terribly lonely, Carl systematically amassed an extraordinary collection of rare books in the fields of nephrology and in physiology, anatomy, and pathology, usually but not invariably related to the kidney. His house had to be enlarged to hold the collection, and then enlarged again. It was fortunate that by the time the garage succumbed under the tide of books there were almost no works bearing on the field of renal physiology and renal disease that Carl did not already possess. He was especially keen to possess first editions and in the end had them for all important works over several centuries, with only very few frustrating exceptions still remaining. He shared his passion for collecting with Leon Fine and J. Stewart Cameron. Cameron recalls that, as Carl's collection became more and more complete, the hunt for rare volumes in the excellent condition that he always demanded became ever more difficult and therefore ever more exciting. One of Carl's greatest joys was to have Leon, Stewart, and other scholars and book lovers visit the library in his home. He would beam with delight as he showed his treasures, touched the linen pages, and viewed the engravings. Each book was catalogued with its provenance, date of purchase, price, and recent catalog price.

The collection has been donated to the Rare Books Library of the University of North Carolina library system. A section of the rare books reading room of the Wilson Library has become a closed alcove to house his collection. Carl's kidney-shaped desk, lamp, chair, and Chinese scholars table, as well as the oriental rug from the great room of his house, furnish the Gottschalk collection space.

Not only did Carl collect medical books but many other books as well. He donated his nearly complete collection of first editions of Robert Louis Stevenson to the Rare Books Library of the University of North Carolina in 1997. Fine editions of most of the classics of English literature sat on the shelves around his home. He liked to touch beautiful books and took pleasure in browsing through them whenever the fancy struck. No lending library for Carl: he wanted them available around him.

He went about collecting Chinese and Japanese porcelains in the same scholarly manner he pursued his books. After reading extensively, he combed the antique shops in London for beautiful, old objects. Particularly after Helen died, he spent much of his retirement time seeking beautiful works of art, both contemporary and old, for his home. The family joked that Carl felt that the purpose of walls was as spots for pictures or old maps and of floors to be covered with Oriental carpets.

Carl's penchant for medical history, particularly that related to the kidney, was no less than or different from his approach to science. It led him to edit with Robert Berliner and Gerhard Giebisch a book on the history of renal physiology and a historical archives series in *Kidney International*. He formed and chaired for 15 years the Commission for the History of Nephrology of the International Society of Nephrology.

THE LAST YEARS

The death of Carl's beloved wife Helen in 1988 from amyloidosis was a low point in his life. Carl's prolonged sorrow after Helen's death came to a happy ending when he met and married Susan Fellner, a nephrologist. Considering their respective occupations, it is fitting that they met during a Homer Smith Symposium at that bastion of renal physiology, the Mount Dessert Island Biological Laboratory. Just four months before his death Carl and Susan attended a meeting in Sydney, Australia. At that time he was a happy newlywed, full of energy and enthusiasm. Unfortunately their marital bliss had lasted less than two years when Carl died suddenly and unexpectedly from a cardiac arrhythmia.

In summary, no one who knew Carl failed to use the word modest to describe him. He was just that: unassuming, unpretentious, confident in his abilities but never blowing his own horn. He was gentle. He never had a harsh word. His criticism was always in the most constructive of guises. He was gentle with his family, his beloved dog Zoe, his colleagues, with all who knew him. He was a Southern gentleman without treacle or artifice. He was even somewhat motherly. That is, he was nurturing, protective, and caring of friends, family, and colleagues alike. His sense of fairness was so well known that he was asked to chair important university, national, and international committees. His integrity and fairness insured against bias in the proceedings. He was widely trusted because he held firmly to what he knew was right and proper.

Carl's scientific contributions were monumental. He designed and conducted micropuncture experiments that were decisive for establishing principles behind urinary concentration, neural control of salt excretion, function of remnant nephrons in failing kidneys, and much more. Behind these scientific achievements was a warm and generous person. Carl's modest manner concealed a piercing intelligence. His quiet, unaffected manner, his superior knowledge about many things and his inclination to convince with understated logic and without bombast made him a congenial and much admired colleague. He is sorely missed by the many associates, students, and friends whose lives he touched.

DURING THE PREPARATION of this memoir, I made use of family information provided by Susan Fellner; of memorial remarks by Robert Berliner and J. Stewart Cameron; a memorial written by William Blythe (In Memoriam, Carl William Gottschalk (1922–1977), *Kidney Int.* (53[1998]:1–2); and "Carl W. Gottschalk's contributions to elucidating the urinary concentrating mechanism" by Heinz Valtin (*J. Am. Soc. Nephrol.* 10[1999]:620).

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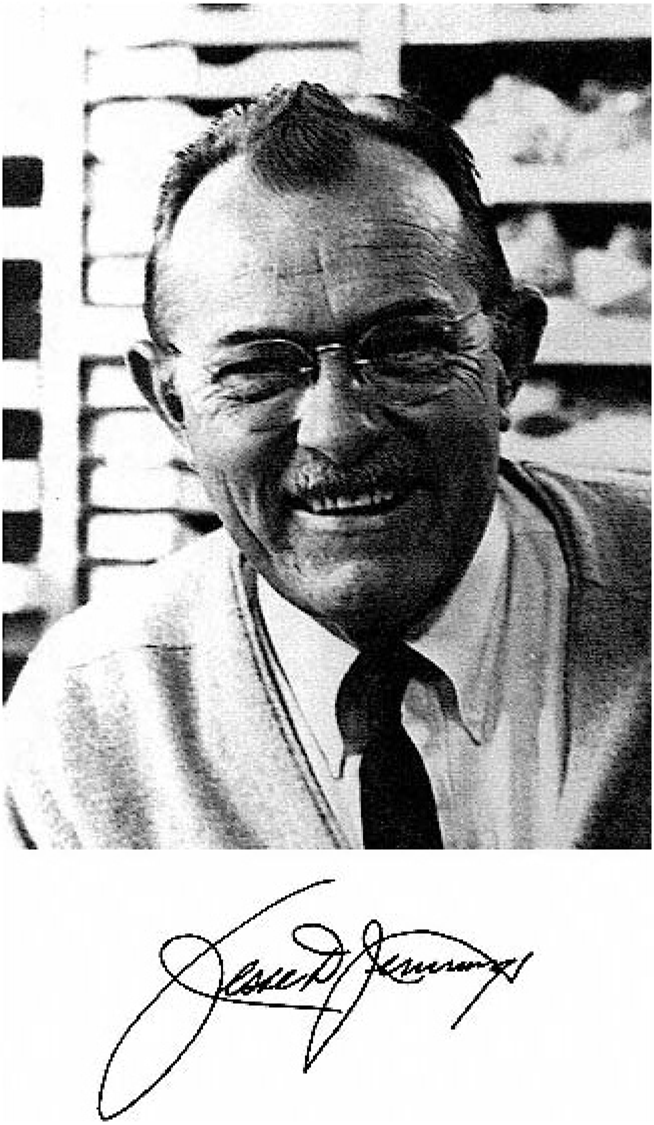

JESSE D. JENNINGS

July 7, 1909–August 13, 1997

BY C. MELVIN AIKENS

JESSE D. JENNINGS, prominent member of a highly productive generation of scholars that laid the empirical foundations of modern North American archaeology, died at his home in Siletz, Oregon, on August 13, 1997. Born in Oklahoma City on July 7, 1909, he was eighty-eight years old. Jane Chase Jennings, his partner since their 1935 marriage in Washington, D.C., was at his side. Jennings is survived by Jane, their two sons David and Herbert, three grandchildren, and many students who carry on his work and teachings in various ways.

In his personal memoir *Accidental Archaeologist* (1994) Jennings recounted his archaeological career, which began shortly after he arrived at the University of Chicago in 1929. After graduating from Montezuma College in Hot Springs, New Mexico, he caught a ride east with a faculty member who was returning to Chicago to further his own studies. Jennings found a job on a campus grounds crew and, after brief curricular explorations, found his way into the Department of Anthropology. There the faculty and prominent visitors who Jennings found memorable included Robert Redfield, H. R. Radcliffe-Brown, Fay-Cooper Cole, Edward Sapir, Paul Radin, Leslie Spier, Bronislaw Malinowski, and

Alfred Kroeber, among others. Student comrades of what became a famous generation included Donald Collier, Robert Braidwood, Fred Eggan, Sol Tax, Madeline Kneberg, James Griffin, Kalervo Oberg, Florence Hawley, Philleo Nash, and Alexander Spoehr, to mention a few prominent names.

Jennings initially was drawn to cultural anthropology, especially gaining from the mentorship of Robert Redfield and hoping to work with him in Mexico. But, in 1931 he was drafted to serve his obligatory term in the department's central Illinois field school in archaeology by department head Fay-Cooper Cole. Jennings found that, as a New Mexico farm boy who knew digging and dirt, he was better adapted to the work than his citified fellow students, and he progressed rapidly into a supervisory role. Pursuing opportunities that the Chicago field school and Redfield's advocacy opened to him put Jennings on the archaeological path he followed the rest of his life.

Jennings's first scholarly publication was "The Importance of Scientific Method in Excavation" (*Bulletin of the Archaeological Society of North Carolina*, 1934, 1 [1]:13–16). The piece already reflected themes that later Jennings students would recognize: a stress on order, cleanliness, and thoughtfulness in excavation, with serious attention to tracking and recording structural and contextual details. Its preamble also succinctly outlined the culture-historical paradigm in which he worked:

> The importance of scientific excavation can best be presented after a discussion of the scientific method itself. The chief desire motivating the archaeologist is for fuller historical knowledge. The archaeologist, both field and laboratory worker, attempts to reconstruct history through inferential reconstruction . . . Distribution of material culture traits is essential knowledge in determining distribution of cultural groups. Contact between cultural groups can and must be observed through artifactual evidence. To plot distributional groups or to ferret out cultural contact leaves the inves

> tigator in need of every fact. The record of excavation must be complete. Every possible effort must be made to see, record, and later interpret, every fact. The archaeologist is not altogether a grave robber, inasmuch as the actual digging is but the primary step in archaeology. The digging comes first, however, and unless it is done well nothing else can be done (p. 13).

Jennings's first major publication was a monograph on the late prehistoric and protohistoric Peachtree mound and village site in Cherokee County, North Carolina (1941). This was published with Jennings as junior author to Frank M. Setzler. Jennings had excavated the site in 1933–34, and subsequently analyzed the specimens at the U.S. National Museum. Setzler, a Smithsonian employee who was liaison officer for Peachtree and other joint Smithsonian-Civil Works Administration projects, in his foreword gives Jennings credit for writing the bulk of the report, but does not explain further. Jennings (1994) records that Setzler went down as senior author of the published report—which Jennings had written in its entirety—because Setzler had told him that Bureau of American Ethnology publications had to be signed by a Smithsonian author.

Leading into the Peachtree report, Jennings offers a classic statement of the interpretive paradigm he and others of the time followed, based on ethnographic analogy and a direct historical approach:

> No archaeological area, except perhaps the Pueblo region of our Southwest, is more blessed with direct ethnological and historical accounts pertaining to the organization and movements of Indian tribes than the general Southeast. For this reason every effort should be and is being made to interpret archaeological data from these early historical reports. This procedure is the only sound method for determining the ancestors of our historic Indian tribes and properly interpreting the few remaining indestructible fragments of their material culture (pp. 3–4).

Jennings interpreted his findings under the headings Secondary Mound, Primary Mound, Village Site, Architecture and House Life, Costume and Dress, Customs and Ceremonies, and Description of Manufactured Objects. Guided by the ethnohistorical clues, he stayed close to the descriptive characteristics and apparent functions of the observed archaeological traits. A final section on Archaeological Implications placed Peachtree in the emerging chronological sequence for the region. Jennings discussed briefly the site's relations to the Adena, Hopewell, and Mississippian patterns and identified its occupants as Cherokee on the grounds of Peachtree's late date and location in the heart of the ethnohistorical Cherokee range. The possibility that Peachtree was the Cherokee village of Guasili, visited by DeSoto in 1540, was entertained but not clearly resolved.

Another guiding concept was the taxonomically structured McKern system, which compared cultural traits from archaeological sites to define the foci, phases, aspects, and patterns of basic cultures. This became an important and widely used tool in the middle and late 1930s and lives on as an important underpinning of American archaeology. In Appendix B, Jennings compared 212 traits of excavated artifacts and cultural features from Peachtree across 7 other southeastern sites. Peachtree could not yet be placed in a McKern system framework, because regional data were still too few, but Jennings marshaled the relevant data in anticipation of future use for this purpose.

This early work is paid so much attention here because it clearly defines most—though not quite all—of the dominant emphases exemplified and advocated by Jennings in his long career: the indispensability of careful, attentive excavation and detailed reporting of same; an interpretive approach founded in ethnographic knowledge; a common-sense, dominantly functional rather than stylistic approach to artifact analysis; and placement of research findings in larger temporal and regional contexts.

Much additional fieldwork followed. Jennings's (1941) "Chickasaw and Earlier Indian Cultures of Northeast Mississippi" reported several years of work along the route of the National Park Service's Natchez Trace Parkway project, carried out by Jennings and others. The analysis and exposition continued in the vein already seen in the Peachtree study, the main interpretive effort being to link the archaeological manifestations to the historic Chickasaw encountered by DeSoto and trace them back into prehistoric time. In the beginning of his concluding section, Jennings expresses what much subsequent work shows to be a continuing and characteristic mistrust of theorizing and a conviction that conclusions must properly emerge from and be limited by the data in hand:

> Having waded through the minutiae necessary to a factual reporting of a series of excavation units, the student is usually ready to accept the challenge offered in a concluding section by indulging in the wildest of speculations and by parading his pet theories. In spite of the strong temptation to theorize and tie up loose ends in order to confuse future generations of students, it is probably more desirable to restrain this impulse, attempting instead to evaluate and weigh the meager artifactual data . . . On the basis of ethnological and archaeological data which modify each other, the information bearing on historic material culture of the Chickasaw tribe has been slightly expanded through the four historic sites dug (p. 213).

In 1938 Jennings seized an opportunity to dig with A. V. Kidder at Kaminal Juyu, Guatemala. This was a one-season job that came up on short notice when Kidder found himself short-handed, and Redfield recommended Jennings as being skilled in the kind of mound excavation it required. Jennings spent about five months excavating a complex series of superimposed earthen pyramids, and this work became the basis for his doctoral dissertation at Chicago, written during World War II, while he served as a Naval officer in Iceland (1946). After Guatemala, Jennings worked for a time as a National Park Service ranger at Montezuma Castle in Arizona, then was transferred to Ocmulgee National Monument in Georgia as its first superintendent. After time out for World War II he returned to work with the National Park Service, again in the southeast, but he shortly transferred to the plains region.

In the plains, Jennings functioned as a roving archaeologist, involved with early river basin surveys work. During this time he had an opportunity to visit sites up and down the plains, broadening his archaeological experience into a new and quite different area. Stemming from this interlude, Jennings was instrumental in establishing the *Plains Archaeological Conference Newsletter*, which later grew into the respected *Plains Anthropologist*.

In 1948 Jennings came to the University of Utah, beginning the professorial career that he continued to his official retirement from Utah in 1986, and extended until 1994 as an adjunct professor at the University of Oregon. Arriving at Utah, Jennings drew on his past experience to quickly initiate a statewide archaeological survey and set about filling in the map of a poorly known region through extensive surveys and test excavations.

In 1949 Jennings began excavations in a series of dry desert caves on the northern Utah-Nevada border. Danger Cave was the richest of these, and ultimately gave its name to the monographic report on the work, published in 1957. This was pathbreaking research, now recognized as Jennings's classical work and greatest contribution to our understanding of North American prehistory. At Danger Cave, Jennings's established habits played out in a painstaking approach to excavation and a commonsense, ethnographic interpreta tion of the archaeological evidence. An important new element was presented, however, by the nature of the site itself. In the cool, dry grotto of Danger Cave lay some 11 feet of stratified deposits rich in well-preserved artifacts and biotic specimens, which radiocarbon dating showed to have accumulated slowly over more than 11,000 years. This uniquely detailed record offered an exceptional opportunity to study the environment and subsistence practices of the cave's inhabitants over a very long period of time, and in developing that opportunity Jennings documented a convincing millennial perspective on human ecology in the desert west.

Looking as always to ethnohistory and ethnography for bases on which to interpret the archaeology, Jennings found guidance in Stewart's (1941) "Culture Element Distributions: IV Northern Paiute" (*Anthropological Records* 4(3):361–466), and Steward's (1938) seminal treatise on subsistence and settlement patterns among a broader range of Great Basin aboriginal peoples, "Basin-Plateau Aboriginal Sociopolitical Groups" (*BAE Bulletin* 120). Matches between these ethnographies and what Jennings saw in the artifacts and biotic remains from Danger Cave were close and numerous. Detailing the evident similarities shared between archaeological inventories and ethnographic accounts, and essaying comparisons with other dry cave sites across the west, Jennings described a Great Basin desert culture that was widespread, ancient, and stable, lasting from about 10,000 years ago down to the nineteenth century. The evidence and argument were compelling, and he established a conception that will forever influence research into desert west and hunter-gatherer prehistory.

The desert culture idea attracted widespread attention and, along with approbation, it generated a storm of critical interest and competing interpretations from other far western archaeologists, including attacks on the stratigraphy and dating of the Danger Cave record itself. This discussion lasted the better part of two decades, critics mainly arguing that Jennings's broad generalizing description of a western desert culture was misleading because it glosses over obvious subregional and temporal variation. The debate has subsided, with the growth of a general recognition that both generalizing and particularizing views engage reality at differing levels of regional and temporal scale.

Following Danger Cave, Jennings continued to prosecute the work of the statewide archaeological survey, directing and supporting the work of his students all over Utah. His 1966 "Glen Canyon: A Summary" pulled together years of rescue archaeology under his direction in the canyon lands of southeastern Utah to give a first synthetic account of Anasazi agricultural life along its northern frontier. His 1978 "Prehistory of Utah and the Eastern Great Basin" is a still broader synthesis that pulls together results from several major desert culture sites that followed Danger Cave, from the Glen Canyon project, and from numerous investigations into Utah's distinctive horticulture-based Fremont culture. The bibliographies of these two summaries document the substantial breadth of this research, and Jennings's continuing role in educating and launching students into the professional arena.

Another major work for Jennings during this period was creation of the University of Utah Museum of Natural History. This was a long and large undertaking, spanning in all more than 20 years. It culminated in 1973 when, with the museum finally built and legislative funding assured, he heaved a sigh of relief and resigned as director, passing the job on to his long-time curator of exhibits. The story, detailed in Jennings's 1994 memoir, is a remarkable tale of vision and persistence, and the museum continues today as a major living contribution to public education, including but going much beyond the archaeological interests that were otherwise the focus of Jennings's career.

In addition to his field technical studies and his museum work, Jennings early entered into the writing and editing of broadly synthetic volumes directed to peers and students. In *Prehistoric Man in the New World* (1964), a compendium of regional summaries written by leading scholars and co-edited with Edward Norbeck, and in his *Prehistory of North America* (1968), Jennings gave students and teachers the first textbook syntheses of the continent's archaeology. These books lived on, each growing and changing shape through three editions, informing and influencing both younger and older students of American archaeology across three decades. *The Prehistory of Polynesia* (1979), stimulated by Jennings's 1970s foray into Pacific fieldwork (four seasons of excavation in Western Samoa), brought together under his editorship synthetic essays by more than a dozen prominent students of this vast area for a first-time summation of its prehistory. Although Jennings's interest in archaeological synthesis no doubt has additional intellectual roots, the taproot surely is the sheer breadth of the archaeological experiences he accumulated in his long career. As he worked his way around the continent from the southwest to the Midwest, the southeast, Guatemala, the plains, the Great Basin, and out into the Pacific islands, Jennings repeatedly found himself learning new regional contexts in order to understand the implications of his field data, and thus equipped himself better than perhaps anyone else for such broad undertakings.

In his 1994 memoir Jennings reflected on a question put to him late in his career by a University of Oregon graduate student about his thoughts on fieldwork. That question elicited his final chapter, "Archaeology Without Theory: An

Innocent at Work." In it, Jennings expressed his strong skepticism about much of the theorizing that had come to characterize modern archaeology, picking up again the thread previously quoted from his Chickasaw report of nearly 60 years ago that what typically passes for theory is more likely to be confusing than helpful:

> Certainly I profess no scientific goals, having wearied during the 1950s of attempting to follow the sterility or the speculative dead-end paths or the convoluted mazes that lie within the tangled forest of theory upon which "scientific" archaeology is based (p. 264).

Although Jennings never joined seriously in the published polemics over the "new archaeology" of the 1960s and 1970s, his skepticism was well known to colleagues and students. Jennings's friend and Pacific colleague Roger Green (*Archaeology in New Zealand* 40(4): 251) reminisced about the time both were visiting professors at the University of Hawaii:

> The late Chet Gorman and Donn Bayard were among our graduate students. The "new archaeologists" of the time certainly gleaned what Jesse thought of their theoretical stance through his direct assistance in facilitating Donn's publication of "Science, Theory, and Reality in the New Archaeology" in *American Antiquity* (34:376–84).

As this brief sketch of his intellectual history has shown, despite Jennings's disavowal in "Archaeology without Theory," he certainly was not innocent of guiding principles that most would place in the realm of theory, even though clearly he regarded them as simply common-sense investigative approaches. And he generated results—most notably his account of the desert culture—that are surely to be reckoned as having great theoretical importance, even though he did not consider the desert culture a theory, but merely the conclusions arrived at through his effort to consider as objectively as possible the archaeological and other data he was dealing with. To the end, as confirmed in his memoir, Jennings believed that archaeological data, and especially new kinds of data generated by careful fieldwork and ancillary physical-chemical analytical techniques, were far more important than theory in advancing archaeological understanding of the human condition.

The remainder of this biography offers a few impressions of Jesse D. Jennings the man. It has to be written in the first person, because what I have to say in this vein was learned at first hand as Jennings's student, employee, colleague, and friend over a period of 40 years. My association with Jennings began in 1958, when he hired me as an undergraduate field hand on the Glen Canyon project, and we remained friends until his death. Necessarily, considering the source, these impressions stem from a later phase of Jennings's career after he became a professor of anthropology at the University of Utah and after he was well established as a major figure in American archaeology. What follows is taken with permission from a foreword I wrote for Jennings's 1994 autobiography.

> Jennings is most prominently defined, especially among his students, by his characteristically direct and demanding approach to both teaching and research situations. Never unclear about his expectations, he is dependably insistent and —if need be—forceful in seeing to it that they are met, or their achievement at least vigorously attempted. Possessed of boundless energy himself, he expects to see it in others, too. In the classroom or in the field—the latter one of Jennings' most important teaching venues—things are not left to chance, and things are not let go. Responsibility is demanded of a seminar student scheduled to perform at a given time or of a field crew chief coping with the many necessities of that position. Though good work is never left unremarked, neither is a failure to perform up to standard. Nor are too facile statements left unprobed, and a student who doesn't keep up the pace in a seminar presentation will be told to "kick it along." Helping to relieve the tension this regime can generate is Jennings's

habit of lacing his interactions with wisecracks and asides ranging from groaners to the hilarious. Thus is engendered that certain blend of striving, nervous anticipation (for some verging on fear) and, ultimately, respectful affection for their mentor that is known to all Jennings students.

The early chapters of Jennings's memoir seem to illuminate the origin of this persona. Clearly, his boyhood was dominated by the certainties of his strong-willed mother's deeply held Baptist religion. Although Jennings records that his own Baptist fervor evaporated during his college years, something manifestly remained of the fundamentalist sense of good and bad, right and wrong, and willingness to make and act on such judgments. Jennings was also schooled early in responsibility by the obligations of helping to sustain house and home, which fell on him too heavily and too soon because of his father's frequent and prolonged absences and the extremely limited family income.

Jennings's students at Utah in the Glen Canyon project days of the late 1950s and early 1960s expressed that certain feeling of respect, affection, and dread in the brief fad of rendering his given name Jesse (with its obvious etymological connection to Jesus) as Yahweh, evoking the great and terrible desert god of the Old Testament. Similarly, he was referred to by a later generation of students as "the dark lord," after the powerful and implacable figure of J. R. R. Tolkien's *War of the Rings*. Jennings's often uttered expectation that we would cope appropriately with whatever exigencies the wild canyon lands field situation might present was memorialized in a little ditty sung to the tune of The Frozen Logger, accompanied by banjo and ukulele. The verses characterized our boss, not always flatteringly, in terms of various archaeological feats and incidents—some more or less real, some fabulous—and the song ended with the phrase, " . . . emblazoned on his forehead was the magic slogan, COPE!"

Those student exaggerations of Jennings's character and exploits seem to have reflected a sense of him as a kind of legendary figure, somehow larger than life. Manifestly, we at least occasionally thought of him as godlike, though not in any namby-pamby way. We knew about his previous work, of course, and certainly he was always a looming presence on the local scene. I know that in my own case I actually did think he was larger than life. I was greatly surprised to learn one day, in a conversation with his younger son Herb (Jennings regularly sent his sons Dave and Herb, then schoolboys, to the field on summer dig crews for what they could learn about work and life in general), that Jennings was about 5 feet 10 inches tall, and weighed about 175 pounds. I was surprised because, fitting those dimensions almost exactly myself, I had always perceived Jennings as a good bit larger, maybe something over 6 feet and closer to 200 pounds!

After "cope," another favorite Jennings expression was "making mistakes." This applied to an archaeologist's role in directing an excavation. Jennings insisted on clear stratigraphic and associational control, but, of course, he knew from much experience how hard it is to figure out the structure of an archaeological deposit while in the act of digging it away. A greeting to a neophyte crew chief, "so, Aikens, you're making the mistakes on this site," meant, "I see that you are in charge here," and was also a tip-off that this stern inspector could be understanding about an occasional error, if, of course, it devolved from a reasoned attempt to get the thing right and so long as the error was clearly described and properly labeled in the field notes. Although he was not one to overtly nurture a student, I do recall being comforted by a Jennings statement that a man who never made mistakes was a man who never did anything.

On campus, a feature of Jennings's behavior that I came to recognize as remarkable only long after leaving Utah, completing graduate work, and becoming a professor myself was his total availability to students. Unlike the latter-day professor who typically schedules but a few office hours each week for student conversation and consultation, Jennings was always there, and his door was always open. A student could depend on finding him interested and ready to act directly on the concern of the moment. A few snippets from a routine tenured faculty review done shortly before Jennings's "retirement" at Utah, describes similar relations with students some 20 years later:

> Unlike many university faculty, [Jennings] has faced the difficult task of providing direct and honest evaluation of his students so they all know where they and their work stand in relation to his judgment of quality . . . Students, past and present, stress the great amount of learning that goes on in his classes as compared to other classes . . . His involvement with students has been his outstanding characteristic. He is vitally concerned with their education, exceptionally active in finding them support during their studies and jobs when they get their degrees. His use of his many contacts for these ends has provided him with much vocal appreciation.

In conclusion, it should be noted that Jennings's long and valuable service to the profession is reflected in an exceptional list of major honors that came throughout his career. He was chosen editor of *American Antiquity* (1950–54), elected to the Executive Board of the American Anthropological Association (1953–56), selected as Viking medalist in archaeology (1958), elected president of the Society for American Archaeology (1959–60), and elected vice-president and Section H chairman of the American Association for the Advancement of Science in 1961 and again in 1971. His university named him a distinguished professor in 1974 and honored him with a doctor of science degree in 1980. He was elected to the National Academy of Sciences in 1977.

In 1982 he received one Distinguished Service Award from the Society for American Archaeology and another from the Society for Conservation Archaeology. He was a featured plenary session speaker at the 50th Anniversary Celebration of the Society for American Archaeology in 1985. In 1990 the Great Basin Anthropological Conference (which he founded in 1958) established the Jesse D. Jennings Prize for Excellence in his honor, and in 1995 he was awarded the A. V. Kidder Medal for Achievement in American Archaeology.

THIS BIOGRAPHY COMBINES text previously published by the author as an obituary in *American Anthropologist* (100[4]); as part of a foreword in *Accidental Archaeologist* (1994); and as part of an obituary in *SAA Bulletin* (15 [5]). I thank the editors of the *American Anthropologist* and *SAA Bulletin* and the director of the University of Utah Press for their consideration.

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John G. Kirkwood

JOHN GAMBLE KIRKWOOD

May 30, 1907–August 9, 1959

BY STUART A. RICE AND FRANK H. STILLINGER

IT IS A DISTRESSING fact that many of the most creative individuals suffer premature death, thereby robbing humanity of unrealized contributions. Examples abound in literature, music, and the graphic arts, as well as the fields honored by the National Academy of Sciences. John Gamble Kirkwood was one of these individuals; his remarkable career was compressed into just fifty-two years of life. During this shortened interval he managed to create a solid theoretical underpinning for many aspects of modern physical chemistry, with ramifications that still provide compelling directions for investigation forty years after his death. His legacy also includes a group of students and collaborators who developed into outstanding scientists, and whose research activities bear the imprint of the unmistakable Kirkwood style.

John Gamble Kirkwood ("Jack" to his family, friends, colleagues, and students) was the first child born to John Millard and Lillian Gamble Kirkwood in the small town of Gotebo, Oklahoma. His father had worked his way through college and law school in Chicago, and with a good business sense became a successful independent distributor for the Goodyear Corporation in the Midwest. Two sisters completed the immediate family: Caroline, born two years after Jack, and Margaret, who was fourteen years his junior.

The Kirkwood family moved to Wichita, Kansas, in 1909. Jack attended public school in Wichita through the third year of high school. He was recognized early as a remarkable student, excelling in science and mathematics, as would be expected from his later professional attainments. He also demonstrated considerable proficiency in foreign languages. In fact, he remained fluent in French throughout the remainder of his life and avidly displayed a continuing fascination with all aspects of French culture.

In the summer of 1923, while still a high school student, Jack visited California. As a consequence, he became attracted to the opportunity for a first-class scientific education at the California Institute of Technology. Cal Tech chemist A. A. Noyes (NAS, 1905) suggested that he skip the last year of high school, and so encouraged, Jack enrolled that fall at Cal Tech. This arrangement lasted two years, but it apparently caused some conflict with Jack's strong-willed father, who had a different concept of an ideal college education. To resolve the problem, Jack transferred from Cal Tech to the University of Chicago, from which he graduated with an S.B. degree in December 1926.

Jack entered the Massachusetts Institute of Technology as a graduate student in the chemistry department in February 1927. He received a Ph.D. in June 1929. His dissertation, under the direction of Frederick Keyes (NAS, 1930), involved measurement of the static dielectric constants of carbon dioxide and ammonia as functions of temperature and density. This research formed the basis for his first two published papers, coauthored with Keyes. Kirkwood's interest in the dielectric properties of matter persisted throughout his later career. In his classic 1939 paper "The Dielectric Polarization of Polar Liquids," he introduced for the first time the concept of orientational correlations for neigh boring molecules and showed how these control the dielectric behavior of liquids.

Upon completion of his Ph.D., Kirkwood became a National Research fellow for the period 1929–30. He remained in the Cambridge area to complete work with Keyes at MIT and to collaborate with John C. Slater (NAS, 1932) at Harvard. His research interests widened to include intermolecular forces and their influence on equations of state as expressed through theoretical analysis of gas-phase virial coefficients.

This early Cambridge period also included meeting the former Gladys Lillian Danielson, previously wife of the electrochemist Theodore Shedlovsky (NAS, 1953). They were married in 1930. A son, John Millard Kirkwood, was born in 1935. The couple was divorced in 1951.

In the 1920s and early 1930s it was logical for young American scientists to complete their professional training in Europe. An International Research Fellowship provided Kirkwood that opportunity, and the academic year 1931–32 was spent with Peter Debye (NAS, 1947) in Leipzig, with a visit to Arnold Sommerfeld in Munich. Four research papers born during this sojourn were written in German and published in *Zeitschrift für Physik*. The Debye-Hückel theory for strong electrolyte solutions was still less than a decade old and its significance and validity were sources of lively debate. Not surprisingly, Kirkwood initiated then what was to become a lifelong research interest in ionic solutions, ultimately producing studies of the structure of concentrated ionic solutions and their electrical double layers.

Jack Kirkwood returned to MIT as a research associate in the Physical Chemistry Research Laboratory, a position he held during the years 1932–34. His scientific interests during that period included quantum effects on equations of state, and he carried out seminal investigations of the general statistical mechanics of fluid mixtures and the rigorous theory of electrolytic solutions. The last of these theoretical developments was honored in 1936 by the American Chemical Society Award in Pure Chemistry, which at the time was called the Langmuir Prize, and he was one of its youngest recipients.

The next three years (1934–37) saw Kirkwood as assistant professor in the Cornell University chemistry department. This arrangement was interrupted for a year (1937–38) during which he became associate professor at his second undergraduate institution, the University of Chicago. He returned to Cornell as Todd Professor of Chemistry for the years 1938–47.

The liquid state theory that Kirkwood pioneered during the 1930s and early 1940s continues, sixty years after its introduction, to exert a major scientific influence. The recognition that calculation of the properties of liquids in terms of interactions between the molecules involves solution of a coupled hierarchy of equations laid the foundations for a variety of approaches that exploit intuitive approximations. The first and most famous of these, known as the Kirkwood superposition approximation, was invoked to render solvable the fundamental equations satisfied by molecular distribution functions. Although now replaced by better approximations, the superposition approximation captures the essence of many of the physical effects that dominate the structure and properties of liquids and it continues to resonate throughout many aspects of condensed matter chemistry and physics. But, and more important, the formalism of the theory of distribution functions developed by Kirkwood remains a key part of the theory of liquids.

Another seminal contribution from Kirkwood's Cornell years is the theory of fusion, published in three papers with Elizabeth Monroe in the period 1940–42. This set of papers ranks as one of the major classics in the theory of phase change and has generated a large number of subsequent variants and reinterpretations by other authors. These and other notable contributions to the molecular foundations of physical chemistry formed the basis of Kirkwood's election to the National Academy of Sciences in 1942.

The Second World War produced a major, albeit temporary, shift in Kirkwood's scientific direction. Military requirements of the time made it clear that basic understanding of explosives needed great improvement. Kirkwood took up the challenge and contributed to the war effort as a member of the National Defense Research Committee of the Office of Scientific Research and Development (1942–45) and as a member of the Basic Research Group, which was advisory to the chairman of the Defense Department's Research and Development Board. Kirkwood formulated quantitative theories of detonation and shock waves in air and water, some of which was accomplished in collaboration with H. A. Bethe (NAS, 1944), a colleague from Cornell and coauthor of a paper on order-disorder phenomena. A portion of the studies of explosions was published after the conclusion of the war. The U.S. Navy presented Kirkwood a Meritorious Civilian Service Award in 1945 as recognition for his contributions. In addition, he received a Presidential Certificate of Appreciation in 1947.

The phenomena that concerned most of Kirkwood's research attention prior to 1940 would be considered by chemists as involving low-molecular-weight substances. Subsequently, his attention began to turn to polymeric materials, both synthetic and biological. This trend began with collaborative work with R. M. Fuoss (NAS, 1951), explaining the dielectric loss mechanisms in polar polymers, and was a natural extension of his prior studies of the dielectric properties of polar fluids. Kirkwood's interest in polymers continued to grow and later it led to the development of theories for mechanical relaxation in polymers and hydrodynamic flow and theological behavior of polymer solutions. Furthermore, in 1941 he devised a new method for the fractionation (and thus separation) of proteins in solution, using electrophoresis-convection. Following World War II this method was applied, with modifications, to the isolation and purification of several significant proteins, including diphtheria antitoxin, and gamma globulin.

The year 1946 was especially notable for the appearance of the first paper in a long series devoted to the fundamental statistical mechanical theory of transport processes. This series of investigations was to remain a major theme in the thinking of Kirkwood and many of his students for the remainder of his life. A key element in this work was the concept of time-averaged molecular distribution functions and their dynamical equations; this was envisaged as a necessary component of any theory that was consistent with physical measurement protocols, and was inextricably connected to irreversibility. While subsequent studies have revealed the necessity of refinements to this point of view, it nevertheless provided a powerful technique for the deeper understanding of Brownian motion, of the Boltzmann and Enskog equations for gas-phase kinetics, and of the viscosity, thermal conductivity, and heat of transport coefficients in pure liquids. Furthermore, it led to derivation of the first autocorrelation function representation in transport theory, for the "friction coefficient," anticipating the later Green-Kubo-Mori-Zwanzig results of the same generic form.

Kirkwood moved from Cornell to Cal Tech in 1947 to become the Arthur A. Noyes professor of chemistry. He remained in that position until 1951. This period witnessed the appearance of the Kirkwood-Buff general theories of liquid solutions and of liquid surface tension. It was also the time when the Kirkwood-Riseman theory of macro-molecular motions in solution (determining viscosity, diffu sion, relaxation) was developed. The first of these theories is one of a tiny group of exact representations of the properties of mixtures in terms of molecular distribution functions and molecular interactions. It was only slowly appreciated, but now is widely used to help interpret experimental data. The last of these theories was, at the time of its introduction and despite the use of some approximations, the most realistic representation of the character of molecular motion of polymers, including both chain connectivity and the influence of the surrounding medium. It has influenced all subsequent developments in the field.

In 1951 Kirkwood accepted the position of Sterling Professor of Chemistry and department head at Yale, where he remained until his death in 1959. He became director of science at Yale in 1958. He also served as foreign secretary of the National Academy of Sciences from 1954 to 1958, a role that no doubt appealed to his early interest in diplomatic service and that must have benefited from his linguistic talent. As foreign secretary, he was involved in scientific contacts with the former Soviet Union at the height of the Cold War, and he monitored the cooperative arrangements required by the International Geophysical Year 1957–58.

Jack Kirkwood remarried in March 1958, to Platonia Kaldes. The couple had first met during July 1955 in Washington, D.C. During their few months together they occupied an old house built in 1762 in Guilford, Connecticut, not far from the Yale campus in New Haven.

Early in 1958 Kirkwood was diagnosed as a cancer victim with an estimated survival period of roughly one year. He resolved to make the most of his remaining days and in spite of increasing physical pain managed to maintain scientific activities at a high level. For his students and collaborators in this period, watching this confrontation between the physical disease and his intellectual resolve was both uncomfortable and inspiring. In this last year of his life he spent several weeks at the University of Chicago, and he was Lorentz visiting professor at Leiden in early 1959.

The struggle finally ended on August 9, 1959, in Grace-New Haven Hospital. John Gamble Kirkwood was buried in the Grove Street Cemetery next to the Yale campus, also the final resting site for two other giants of statistical mechanics, Lars Onsager (NAS, 1947) and Josiah Willard Gibbs (NAS, 1879).

Following his death, Jack Kirkwood was honored by a three-day memorial symposium, held September 12–14, 1960, in New York City in conjunction with the 138th national meeting of the American Chemical Society. Many of his former students and collaborators contributed papers to this symposium, with many others in nostalgic attendance. The proceedings of this Kirkwood Memorial Symposium appear in the November 1960 issue of the *Journal of Chemical Physics*, with an historical introduction by George Scatchard (NAS, 1946).

Since 1962, the Yale University chemistry department and the New Haven section of the American Chemical Society have administered the John G. Kirkwood Award for outstanding theoretical or experimental research in the physical sciences. It has been conferred approximately every two years. The first recipient was Kirkwood's former colleague at Yale, Lars Onsager.

An eight-volume *John Gamble Kirkwood Collected Works* under the senior editorship of I. Oppenheim was published by Gordon and Breach Scientific Publishers during 1965–68. These volumes reprinted the majority of his 181 published scientific papers, collected into topical subsets with prefatory comments by former collaborators. A complete chronological bibliography is available at the National Academy of Sciences archives.

We close this memoir with a few personal notes. One of the authors was a doctoral student and the other a postdoctoral student of Jack Kirkwood at Yale. During these relationships we knew him as a straightforward individual who would speak his mind directly, and sometimes brusquely, but who would never intentionally wound another. We also knew him as an individual who would counsel his coworkers and work to help them find positions and to advance their careers. We learned from him how to balance rigorous analysis with approximations that render a theory useful, and we learned some of the important elements that constitute taste in the creation, execution, and evaluation of science. Along with his other students and colleagues we profoundly regret the brevity of our association with Jack Kirkwood.

THE AUTHORS OF THIS biographical memoir thank Mrs. Margaret Philipsborn and Judge Platonia Kaldes for kindly sharing reminiscences of their brother and husband, respectively.

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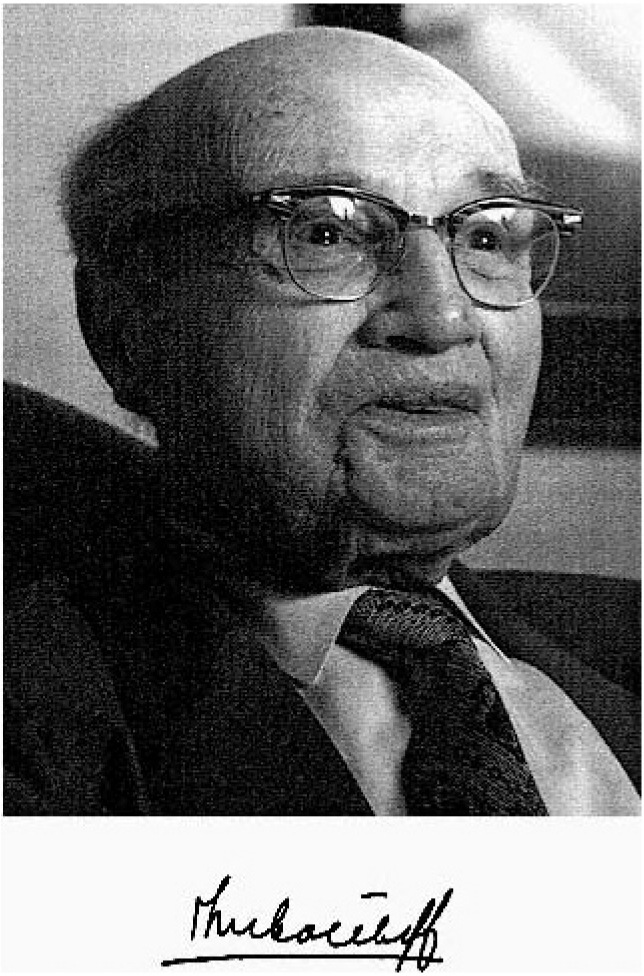

Photograph by Tom Foley, University of Minnesota

IZAAK MAURITS KOLTHOFF

February 11, 1894–March 4, 1993

BY JOHANNES F. COETZEE

IZAAK MAURITS KOLTHOFF is widely regarded as the father of modern analytical chemistry. His monumental research productivity (over 900 papers and numerous seminal textbooks and monographs) as well as his highly effective training of graduate students and postdoctoral associates were major factors in the elevation of analytical chemistry from a predominantly empirical art to a discipline based on sound fundamental principles.

It was September 1951. Professor Kolthoff leaned back in his chair and put a watch on his desk. "I am frightfully busy," he said, "but I want to talk to you two for half an hour." Bart van't Riet (from Holland) and I (from South Africa) had just arrived in Minneapolis as new graduate students planning on doing our doctoral research with Kolthoff as mentor. I was immediately struck by Kolthoff's unusually expressive face, especially his large, luminous, and intense eyes. For the next half-hour he mapped out our future activities and what he expected from us. At times he seemed to be lost in thought, looking up at the ceiling as he talked, except that now and then, at unexpected moments, he would fix us with a penetrating stare to determine, as we were to learn later in numerous research conferences, whether we were paying full attention. "An analytical chemist," he said, "must have a sound grounding in physical chemistry. Therefore, even though you are analytical majors, you must take the majority of the core courses taken by physical majors. Do not take courses designed for people not majoring in physical chemistry. MacDougall (F. H. MacDougall, author of a rigorous text on chemical thermodynamics) is retiring and will offer his three-term course on thermodynamics for the last time. It is a good, rigorous course; take advantage of that. Also take physical courses in quantum mechanics and kinetics, as well as courses in radiochemistry in the inorganic division. You should already know about the analytical courses and that you must take all of those. Finally, be sure to attend all weekly seminars in the analytical division and to study critically the current literature. These things will prepare you for research. I see that our time is up," he said, and then added his final directive: "Be sure to pick up keys for the building from the office so that you may begin to work nights and weekends." It was clear to us from the beginning that Professor Kolthoff ran a no-nonsense operation. I was fortunate to get to know Kolthoff well, both professionally and socially, over the next forty years.

Izaak Maurits Kolthoff, son of Moses and Rosetta (née Wysenbeck) Kolthoff, was born in Almelo, Netherlands, on February 11, 1894, the youngest of three children. His father was highly orthodox, his mother much less so. His brother and sister, and later Kolthoff himself, gradually became more and more liberal. During his kindergarten days he acquired the nickname "Piet," apparently for no particular reason, and he was called by this nickname by almost everyone. During his first chemistry course in high school he developed a keen interest in the subject and appropriated part of the kitchen for his laboratory. Some of his experiments involved hydrogen sulfide, to the dismay of his family. After graduating from high school in 1911 he entered the School of Pharmacy at the University of Utrecht. The reason he began his studies in pharmacy rather than chemistry was that he lacked Latin and Greek, which at that time were prerequisites for admission to the "pure" physical sciences. Nevertheless, he already was reasonably fluent in German, French, and English, in addition to his native Dutch. It is interesting to speculate about the direction Kolthoff's career would have taken if he had had the required competence in Latin and Greek. The pharmacy curriculum at Utrecht was thorough and involved a great deal of analytical chemistry. Furthermore, Kolthoff was greatly influenced by pharmacy professor Nicholas Schoorl, who emphasized a proper balance between descriptive chemistry and the fundamental principles of the field. At that time analytical chemistry tended to be largely empirical, and Schoorl's attention to the principles of chemistry was unusual. In his future career Kolthoff similarly emphasized fundamental principles, but he had an open mind about current hypotheses. He would often speculate about the probable outcome of experiments, but when unexpected results were obtained he would be entirely magnanimous in abandoning the assumptions on which his predictions had been based.

In 1915 Kolthoff received his "apotheker" diploma. He then took more courses at Utrecht, both in physical and colloid chemistry. He was impressed by the famous colloid chemist H. R. Kruyt and later did extensive research involving colloids. Also in 1915 Kolthoff published his first paper on the then-novel concept of pH introduced by S. P. L. Sørensen. In 1918 the requirement for Latin and Greek was abandoned by the University of Utrecht and Kolthoff received the Ph.D. degree in chemistry with a thesis titled "Fundamentals of Iodimetry." By then he had published 32 papers, all on subjects different from his Ph.D. research. He remained at the University of Utrecht, first as "conservator" and then, from 1923 until 1927, as "privaat docent" (lecturer) in electrochemistry. The significance of the pH concept was not generally recognized at that time, and Kolthoff gave many lectures on it to academic and industrial chemists, biochemists (especially bacteriologists), and pharmacists. At the same time his research productivity was astronomical. During the ten-year period from 1917 until 1927 he published 270 papers and 3 books, but it was the originality, insight, and timeliness rather than the mere bulk of these publications that created an enviable international reputation for Kolthoff at an early age. The majority of his early publications were in Dutch, German, or French and, after 1924, increasingly in English.

In 1924 Kolthoff was invited on a lecture tour in Canada and the United States, and in 1927 he was offered a one-year appointment as professor and chief of the Analytical Division of the School of Chemistry of the University of Minnesota (annual salary $ 4,500). In his letter of acceptance he promised, "I may assure you that [on] my side I will try to do my duty as well as possible and I hope that your expectations will not be disappointed." His one-year appointment became permanent and he remained at Minnesota until his nominal retirement in 1962 despite attempts by other institutions (including his alma mater, the University of Utrecht) to attract him. That Kolthoff fully lived up to his promise can be illustrated, in part, by the following statistics. At the time of his retirement he had published 809 research papers. During the next approximately 30 years, mainly in collaboration with his senior postdoctoral associate Miran K. Chantooni, Jr., he published another 136 papers. Over the period 1924–55 he authored or coauthored 8 textbooks and monographs, several in multiple volumes and editions, and from 1959 until 1980 he coedited 34 volumes of reference books. Finally, many of his 67 graduate students entered academia, with the result that by 1993 Kolthoff's academic descendants with Ph.D. degrees numbered almost 1,500.[1] All of this, however, is only part of the Kolthoff legacy, as will be elaborated below.

Kolthoff and analytical chemistry were fortunate, in a sense, that he appeared on the scene at an appropriate time for someone with the necessary ability to transform analytical chemistry. By 1915, when he began his research, analytical chemistry was essentially a highly developed art. However, key elements of the fundamentals of the field already existed in other disciplines, particularly physical chemistry, biochemistry, and pharmaceutical chemistry. One of Kolthoff's most significant accomplishments was that he recognized this fact and set out to further develop and apply these fundamentals to analytical processes. In doing this, he was always meticulous in crediting the work of pioneers in other fields. He particularly credited an early book by W. Ostwald (future Nobel laureate) on the principles of analytical chemistry,[2] even though the scope of the book was narrow with a number of puzzling omissions. It is amusing to note that Ostwald believed that analytical chemists should be the maidservants of other chemists, while Kolthoff (as he stated emphatically) did not want to be a maidservant of anyone. Nevertheless, Ostwald's little book proved to be an inspiration as Kolthoff systematically began to develop the fundamentals of analytical chemistry, an objective that he would pursue throughout his long scientific career. This fascinating process was described by Kolthoff in a number of publications and was summarized in a critical discussion in 1978.[3] Here he lists the fundamental contributions relevant to analytical chemistry of a number of luminaries from related fields, particularly J. W. Gibbs (thermodynamics, phase rule), J. H. van't Hoff (stereochemistry, kinetics), S. Arrhenius (electrolytic dissociation), W. Nernst (electrochemistry), and N. J. Bjerrum (electrochemistry, principles of acid-base reactions). The significance of these contributions to analytical chemistry was not generally recognized when Kolthoff began his research, and one of his major accomplishments was that he amalgamated such diverse contributions and built on this background to create a vast edifice of the interpretation of analytical procedures. This, in turn, led to the improvement of existing procedures as well as the introduction of new methods.

Great diversity and insight characterized Kolthoff's research, whose main subjects were the following, listed more or less chronologically. It is to be noted, however, that he often returned to a favorite topic after a lapse of a few years, if new insights justified renewed attention.

1. **Proton transfer reactions in analytical chemistry: the pH concept, titrations, indicators, and buffers**. Kolthoff's first paper dealt with the titration of phosphoric acid as a mono-and diprotic acid and appeared in 1915. This was followed by a number of papers dealing with both fundamental and applied aspects of proton transfer reactions, subjects taken for granted today but very incompletely understood at the time. In 1922 he published his first monograph, *Der Gebrauch von Farbenindikatoren* (Julius Springer, Berlin). This book went through several German editions, was translated into English by N. H. Furman of Princeton University, and finally appeared in an expanded version in 1937 with C. Rosenblum as coauthor, titled *Acid-Base Indicators* (Macmillan, New York).
2. **Electron transfer and precipitation reactions.** Kolthoff's thesis work on the fundamentals of iodimetry led to 19 papers in 1919 and 1920. In this thorough work he addressed

the variety of reactions occurring in iodimetry, the mechanisms of these reactions, side reactions, and titration errors. During this period he began to use conductometry (1918) and potentiometry (1920) extensively, eventually leading to monographs *Konduktometrische Titrationen* (Dresden, 1924) and *Potentiometric Titrations*, coauthored with N. H. Furman in 1926 and revised in 1931 (John Wiley, New York). Particularly the latter monograph proved to be highly influential, not only in analytical chemistry but also in other fields. At the same time he continued his fundamental studies of classical methods, leading to the publication in 1927–28 of two volumes of *Massanalyse* (Berlin). This monograph was translated and coauthored by N. H. Furman, appearing in 1928 as *Volumetric Analysis* and finally, during the period 1942–58, in an expanded three-volume edition (Interscience, New York) coauthored by V. A. Stenger, G. Matsuyama, and R. Belcher. These reference books had a major impact on analytical chemistry. Parenthetically, Kolthoff served as an important adviser to Marcel Dekker and Eric Proskauer in creating Interscience Publishers, noted for scientific publications and later incorporated with John Wiley.

3. **Formation and properties of precipitates**. Kolthoff devoted much attention to the thorough study of the formation and properties of precipitates. In 1920–21 he published a set of 9 papers on the significance of adsorption in analytical chemistry. After a lapse of 11 years he returned to this field with a vengeance, then at the University of Minnesota. Fresh crystalline precipitates tend to be highly imperfect, but above ambient temperatures "aging" occurs, whereby purification by recrystallization takes place. This process was studied with radiotracers, thorium B for lead and bromine activated by neutrons from a radon-beryllium source. Surface areas were measured by dye adsorption. During the period 1932–48 he published 37 papers on aging

of precipitates and coprecipitation. He continued with these studies, but on a smaller scale, until 1960. These investigations were fundamental, rather than applied, and attracted much attention (e.g., by Otto Hahn).

4. **Voltammetry.** Kolthoff became interested in voltammetry in 1933 when J. Heyrovsky, the inventor of polarography (voltammetry at the dropping mercury electrode) and future Nobel laureate, visited Minneapolis. Two of Kolthoff's top students, J. J. Lingane (Ph.D., 1938) and H. A. Laitinen (Ph.D., 1940) began working on voltammetry, Lingane on the fundamentals of the dropping mercury electrode, Laitinen on solid microelectrodes. In 1939 Kolthoff and Lingane published a 94-page paper in *Chemical Reviews*. This was followed in 1941 by an influential monograph with Lingane as coauthor, *Polarography* (Interscience, New York), expanded in 1952 into two volumes. Kolthoff with several of his students continued to study voltammetry, both in aqueous and nonaqueous solutions, into the 1960s.
5. **Emulsion polymerization.** In 1942 the Office of Rubber Reserve was set up to promote the production of synthetic rubber as a crucial part of the war effort. Kolthoff was one of several prominent professors, including physical chemist P. Debye, organic chemists M. Karasch and C. S. Marvel, and colloid chemists W. D. Harkins and J. W. McBain, invited to work with the major rubber companies. Kolthoff was asked to develop analytical methods so that the rates at which reactants were consumed could be determined. A key constituent turned out to be n-dodecyl mercaptan, referred to as "OEI," for "one essential ingredient."[6] Kolthoff quickly developed an effective method for the determination of OEI based on amperometric titration at the rotated platinum microelectrode with silver nitrate. This method found worldwide use after the war, when it was published (1946). In typical fashion, immediately following this im

portant applied research, Kolthoff launched a thorough fundamental investigation into factors influencing the rates of reaction of mercaptans, as well as the kinetics and mechanism of emulsion polymerization in general. These studies led to the development of novel initiating systems that worked at lower temperatures than usual and that produced so-called "cold rubber" with superior properties. In this field Kolthoff published, in addition to a number of significant papers, a monograph coauthored with F. A. Bovey, A. I. Medalia, and E. J. Meehan, *Emulsion Polymerization* (Interscience, New York, 1955).

6. **Induced reactions.** Kolthoff studied a number of these reactions; one example follows. Typical of numerous induced reactions is the iron (II)-hydrogen peroxide (Fenton) reaction. Kolthoff and Medalia (1949) showed that hydroxyl radicals produced in the first step can induce the oxidation of many organic compounds.
7. **Compounds containing sulfhydryl and disulfide groups.** Beginning in 1950 and continuing until 1980 Kolthoff carried out extensive studies of the reactivity of these groups in native and denatured albumin. These papers may be among the first in bioelectrochemistry, an active field at the present time.
8. **Chemistry of nonaqueous solutions.** Kolthoff did much to rectify the paradox that the chemistry of solutions as typically presented in textbooks and elsewhere had (and to some extent still has) what may be called a strong aquacentric bias, even though the majority of reactions in solution were carried out in nonaqueous media and, furthermore, water was (and is) the most atypical of solvents. His interest in the subject dated back to the early 1930s (1931, 1934), but it was not until the early 1950s that he began a long series of fundamental studies of how solvents influence the properties of solutes. Particularly noteworthy were five classical

papers in 1956–57 with Stanley Bruckenstein on acid-base equilibria in glacial acetic acid, in which the complex interactions occurring in this solvent were quantitatively interpreted. In particular, the contributions of proton transfer (ionization of Brønsted acids) followed by electrolytic dissociation were resolved. These studies were followed by a long series of investigations of a broad spectrum of solute-solvent interactions in various dipolar aprotic solvents, beginning with acetonitrile in 1957. This important solvent, which later became the workhorse of electrochemists, was studied in great detail well into the 1980s, particularly with M. K. Chantooni, Jr., as coworker. Parallel studies were carried out by a number of other research groups in several countries, but Kolthoff's contributions were among the most significant. During the course of this work Kolthoff became interested in the macrocyclic ligands (crown ethers and cryptates) introduced by J. Pedersen in 1967 and carried out extensive investigations of the reactions of these ligands in various solvents. In 1979 he wrote a critical and stimulating review of applications of these ligands in analytical chemistry, incorporating in his characteristic fashion many suggestions for future work. Kolthoff's fundamental studies in nonaqueous solvents occupied him until the end of his long and fruitful career in 1993 and produced a greater number of publications than any other topic studied by him.

Kolthoff generally produced a monograph on every subject on which he had done extensive research. These books had significant impact, and the majority was translated into several languages. His monographs on conductometric titrations, potentiometric titrations, indicators, classical volumetric analysis, polarography, and emulsion polymerization already have been mentioned.

In 1931 he published his first book intended primarily as a text, *The Colorimetric and Potentiometric Determination of pH*. A second edition coauthored by H. A. Laitinen, *pH and Electrotitrations* (John Wiley, New York), appeared in 1941. Particularly noteworthy was the publication of a second text, *Textbook of Quantitative Inorganic Analysis* (Macmillan, New York, 1936) by Kolthoff and E. B. Sandell (Kolthoff's first graduate student [Ph.D., 1932] and professor at the University of Minnesota). This text was destined to become a seminal influence in the teaching of undergraduate analytical chemistry. It presented an admirable balance between the fundamentals and the experimental features of the field and repeated Kolthoff's motto, which had first appeared in *Massanalyse:* "Theory guides, experiment decides." This text was a quantum jump ahead of existing books and served as a model for future texts over many years. In my own case, it was the major cause of a change in career plans. For the first time, it was clear that someone actually understood the reasons for the experimental details in analytical procedures. Particularly noteworthy was the inclusion of numerous references to the original literature, many to research by Kolthoff himself. After reading a number of these, I decided to do my doctoral research with Kolthoff, and so I became an analytical chemist rather than a synthetic organic chemist as planned until then.

The most monumental of Kolthoff's productions is his *Treatise on Analytical Chemistry* (John Wiley, New York) in three parts coedited with P. J. Elving (University of Michigan) and others in later volumes. Part I deals with the general fundamentals of the field and was published over the period 1959–76 in 11 volumes. These reference books were so well received that an expanded second edition soon followed, appearing in 14 volumes until 1986. Part II deals with the analytical chemistry of organic and inorganic compounds in more specific terms and appeared over the period 1961–80 in 16 volumes. Finally, Part III concerns analytical chemistry in industry, with four volumes appearing until 1977. This treatise is the principal reference source of analytical chemistry and it has had a huge impact.

The significance of Kolthoff's prodigious output of research papers, textbooks and reference books can be summarized by quoting Lingane.[4] "Analytical chemistry has never been served by a more original mind, nor a more prolific pen, than Kolthoff's."

Kolthoff received numerous awards and other honors, including three awards from the American Chemical Society (Nichols Award [1949], Fisher Award in Analytical Chemistry [1950], and the Willard Gibbs Medal [1964]), the Electrochemical Society Olin-Palladium Medal (1981), and the Pittsburgh Analytical Chemistry Award (1981), as well as honorary doctor's degrees from the University of Chicago, Brandeis University, University of Arizona, University of Groningen (Netherlands), and the Hebrew University of Jerusalem. He was the recipient of numerous other honors from chemical societies and universities abroad. In 1938 he was knighted to the Order of Oranje-Nassau of the Netherlands, and in 1947 he was elevated to a commander of the same order by the Dutch queen. He was elected to the National Academy of Sciences in 1958.

One would expect that Kolthoff's prodigious output could be accomplished only by running a large, efficient, and hard-driven operation. This indeed was the case in the latter two respects, but Kolthoff's program was never particularly large. Nor were his interests of a routine nature that could lead to a large output with little effort. Instead they were strongly focused on the elucidation of significant and complex problems. The efficiency of his program derived from his talent for finding the most direct route toward solution of a problem. While current analytical chemistry is strongly (arguably too strongly) instrumentation-oriented, Kolthoff's work was chemistry-oriented. Much of his research was done before the great influx of increasingly sophisticated instrumentation after World War II. For him, instrumentation was a means to an end, not an end in itself. Nevertheless, he used the complementary features of different types of instrumentation available to him to great advantage, e.g., conductometry, potentiometry, voltammetry, and ultraviolet-visible spectrophotometry in addressing the daunting problems of nonaqueous solution chemistry.

Kolthoff's personal work habits were unusual. He would begin his workday by spending a couple of hours reading abstracts, papers, and research reports in the seclusion of his apartment in the Faculty Club of the university. At the same time he would write directives to his coworkers for future work on notepaper printed at the top "From the desk of I. M. Kolthoff." All of us would find these notes on our desks later in the day. Kolthoff would not arrive in his office until 10:30 or 11:00. He would first dictate letters to his secretary, the highly competent Christa Elguther. He was a prolific correspondent and answered letters punctually. During the afternoons he would have individual research conferences with his graduate students and postdoctoral associates. The schedule in my own case was that I would turn in my weekly progress report on Wednesdays. This always would be returned to me on Thursdays, annotated in the margins and sometimes across the text with numerous comments, suggestions, and directives. During Friday afternoons, I would meet with Kolthoff for half an hour to discuss the report. He evaluated everything in a highly critical way, but the majority of us understood the need for that. Some of his suggestions were monumental in scope, requiring good fortune and several months of hard work, but they were presented with the clear expectation of a rapid solution. We all lamented such unrealistic expectations. Towards the end of my four-year stay Stanley Bruckenstein (cozily finished with his research) gave me some sage advice: "When Kolthoff mentions a particularly daunting task, keep it in mind but do not necessarily work on it. If he mentions it a second time, begin working on it, and if he refers to it a third time you better have results to report." I only wish Stanley had divulged this to me earlier in my career. Perhaps we worried too much about some of Kolthoff's apparently unrealistic expectations. One Friday afternoon he mapped out a new and wide-ranging investigation. As he was talking, I was thinking, "I hope I will have something significant to report a month from now." Kolthoff, however, concluded by saying, "I travel to Iowa State tomorrow morning at 9 o'clock. Come to the airport and report what you have found." After some soul searching I decided to ignore this directive. He never mentioned it again.

Kolthoff could be harsh with his coworkers. I believe he did not fully realize just how intimidating he could be. Quite often after research conferences some of his graduate students and postdoctoral associates appeared to be in a state of shock. Kolthoff, in turn, would grumble afterwards about "a tale of woe" and "babe in the woods." Nevertheless, the great majority of his coworkers became his devoted friends after they left. Kolthoff, in turn, expended great effort in promoting their careers, at least for those people who had satisfied him that they were serious professionals. I was fortunate in getting to know him well over a period of 40 years. He was a longtime friend of my parents-in-law, the Luytens, who were also natives of Holland. He would often visit to talk (in Dutch, mostly about politics and administrators of all kinds), to drink "Jenever" (Dutch gin), to eat such favorite dishes as "hutspot met boerenkool" (kale, potatoes and sausage), and to lament the slow progress of his research. On one such occasion he confided in me: "I could have accomplished much more if I had worked harder." I was at a total loss how to reply.

After I became a faculty member at the University of Pittsburgh my contacts with Kolthoff continued. My wife and I visited her parents in Minneapolis every year, and often Kolthoff would be in town. He would always say, "I will set aside a day to talk about our research." I would then spend several hours of stimulating discussions with Kolthoff and sometimes with his dedicated coworker Miran Chantooni. The last time we had such a discussion was on the occasion of his ninetieth birthday. I met him in the Campus Club. He was scowling at a reprint. Many other reprints were scattered on tables and even on the floor. Immediately after greeting me he lamented, in typical fashion, "I do not understand a word of this—I think everything is wrong."

Kolthoff's monumental professional contributions were accomplished in spite of a number of physical limitations. In his younger days he was quite a sportsman, enjoying swimming, tennis, skiing, and horseback riding. Then, in 1942, when he was forty-eight years old, he was injured in a skiing accident. This was aggravated when he was later thrown from a horse. He had spinal surgery and ended up partially paralyzed, but intensive rehabilitation aided by his indomitable willpower improved his condition until he could manage with just a brace on his leg, although he then walked with a pronounced and permanent limp. He also suffered from essential hypertension and frequent bouts with pneumonia that landed him in the hospital. His confinements did not deter him, however, from having daily research conferences without much regard for visiting hours. After his accidents he had to abandon some of his physical activities, but he continued swimming and even horseback riding. When he was seventy years old he gave a talk in Houston and then stopped for a week's respite at a ranch called "Whispering Winds." Here he made a big impression and became known as "Nature Boy" because of his strenuous program: 45 minutes of exercise before breakfast, followed by 4 to 5 hours in the saddle and finally a brisk swim. A newspaper reported: "Dr. Kolthopp (sic), Noted Chemist, Visits in Bandera Last Week." I have seen a clipping of this article on which Kolthoff had scribbled an addition to the headline: "Dr. Kolthopp, on a Horse."

Kolthoff never married but led an active social life. He had broad cultural and political interests. He particularly appreciated classical music and for many years regularly attended concerts of the Minneapolis Symphony Orchestra. He was a stimulating conversationalist and he was a friend of many prominent families in Minneapolis and St. Paul.

Kolthoff was concerned about social issues of all kinds, especially those that were of global significance. He was a freethinker, opposed to dogma of all kinds, and impressed this on his students. In his award address upon receiving the Gibbs Medal he discussed the duties of a mentor: "The teacher should impress upon his student the necessity to look on dogma as anathema and not to have unlimited faith in authority."

During the late 1930s Kolthoff and biochemist Ross Gortner were influential in relocating in the United States European scientists persecuted by the Nazis. Financial support came from the Rockefeller Foundation. Kolthoff abhorred all oppressive regimes. Immediately after World War II, on invitation of the respective academies of science, he traveled to the Soviet Union and Yugoslavia and wrote a long series of reports for the *Minneapolis Star*, in which he stressed the importance of reconstructing European universities and of cooperating with scientists in countries with which the United States had fundamental political disagreements. He was an outspoken proponent of freedom of thought and expression, and his reports on social issues in these countries were as candid and perceptive as his scientific publications.

Kolthoff corresponded with many notable scientists, including Peter Debye, Otto Hahn, Jaroslav Heyrovsky, Joel Hildebrand, Frederic Joliot-Curie, and Linus Pauling. Some of these letters are in the University of Minnesota archives. His correspondence concerned not only professional matters but, after World War II, also such issues as control of nuclear weapons. In the early 1950s his contacts with Joliot-Curie landed him in hot water with the House Un-American Activities Committee (HUAAC). Joliot-Curie was organizing an international meeting on nuclear weapons and asked Kolthoff to be a sponsor. At first Kolthoff agreed but subsequently withdrew when he learned that the meeting was to be communist-dominated, writing that he wished to speak as a world citizen, not as a communist. In a letter to Pauling, who was in even worse trouble with the HUAAC, he referred to the HUAAC as "that nuisance committee in Washington." At one stage Kolthoff was accused of belonging to 31 subversive organizations (!) but nothing came of this and eventually these witch-hunts came to an overdue end.

Kolthoff promoted analytical chemistry in every way possible. In addition to his scientific publications and numerous lectures in many countries he was responsible for the creation in 1951 of the Analytical Division of the International Union of Pure and Applied Chemistry. He subsequently served as president of the Analytical Division and as vice-president of the union as a whole.

In summary, Izaak Maurits Kolthoff led an unusually long and influential life. His contributions to chemistry in general and analytical chemistry in particular were monumental. He was *the* major mover in elevating analytical chemistry to a fundamentally sound discipline. He accomplished this through his extensive research papers and seminal text and reference books, as well as his decisive influence on graduate students, postdoctoral associates, and established scientists. At the same time he was an outspoken defender of social justice. He was a role model for all scientists.

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NOTES

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Photo by J. J. Scarpetti

EDWIN HERBERT LAND

May 7, 1909–March 1, 1991

BY VICTOR K. MCELHENY

LESS THAN TWO WEEKS after Edwin Land's death in 1991, members of the American Academy of Arts and Sciences (and the author) met to plan a day-long memorial conference. Swiftly, they decided on a title, "Light and Life." The agenda, however, was more difficult. Land was not just a scientist-industrialist. Speakers would have to encompass topics ranging from color vision to business innovation, from military intelligence to patronage of architecture. As the group talked about Land's character, Jerome Wiesner ex-claimed, "Din never had an ordinary reaction to anything!"

Wiesner was referring to the extraordinary versatility of Land's mind and conversation, which enabled him to concentrate intensely on solutions to problems, and to charm and win over the talented people to tackle them. Until late in his life, he took pleasure in leaping up stairs two at a time. Besides energy, the dominant impressions Land created were artistic sensibility, a sense of drama, delight in experiment, relentless optimism. Less evident was a remark-able ability to keep both work and people in compartments. Less than six feet tall, Land had intense eyes and a shock of black hair that riveted attention on him. Despite a soft voice and frequent use of half-sentences, Land was able to convert interior monologues into dramatic public presentations.

The watchword was: "If anything is worth doing, it's worth doing to excess." This principle applied also to behavior in the laboratory. He told a press interviewer, "My whole life has been spent trying to teach people that intense concentration for hour after hour can bring out in people resources they didn't know they had."

Land's attitudes from boyhood were those of a physicist, but he is best known as the inventor and re-inventor of instant photography from the mid-1940s to the early 1980s. Those innovations had a prehistory: twenty years' work on the first plastic-sheet light polarizers, the great invention of his youth. The polarizers in turn led him into work on the Vectograph process of three-dimensional photography, which found its first important application in reconnaissance during World War II. His desire for autonomy and the challenges of reliably manufacturing his inventions reinforced his determination not to be absorbed by large corporations with large research budgets. But as a pioneer of the science-based company from the early 1930s, he frequently formed alliances with big firms, such as Eastman Kodak, to manufacture components of the systems. He became a vigorous prophet of the efficacy of science-based companies in promoting innovation and providing all their workers a rewarding life on the job.

Research on color vision, which he described in lectures at the National Academy of Sciences in 1958 and 1983, brought him into conflict with many in psychology—but eventually led to collaborations with neurophysiologists. The crisis of the 1950s, when thermonuclear weapons were succeeding nuclear ones and an open United States confronted a closed Soviet Union, brought Land into a crucial role of energizing and supervising high-altitude airplane and satellite surveillance from the mid-1950s to the late 1970s. Such surveillance, by giving incontrovertible physical evidence of the size of opposing forces, helped limit U.S. spending on weapons systems and later provided a principal factual basis for a succession of arms limitation treaties. Land thought constantly about education and research and sought new institutional forms. In 1957, he challenged the Massachusetts Institute of Technology to provide direct experience of research to undergraduates, thus helping to spur MIT's eventual adoption of such a system. In the late 1960s, he helped formulate and advocate the program of federal assistance to public television. He endowed the new house of the American Academy of Arts and Sciences in Cambridge, Massachusetts, and founded the Rowland Institute for Science in the same city.

PERSONAL HISTORY

As refugees from the ever-tightening persecution of Jews during the reign of Russian Tsar Alexander III (1881–94), Land's grandfather Avram Salomonovitch, his grandmother Ella, Land's father Harry, and uncles Sam and Louis, sailed from Odessa and landed at Castle Garden in New York City. In an incident typical for immigrants to America, they acquired the name of Land and Avram's name was Americanized to Abraham. Once in America, Abraham and Ella Land had two more sons and three daughters. Abraham started a scrap metal business. Many of Abraham and Ella's children settled in Brooklyn, where two sons became lawyers and a third entered the secondhand machinery business. The three daughters married, respectively, a lawyer, an architect, and a retailer. Later in life, Land had few contacts with relatives, including eighteen first cousins. After his father's death, in 1965, Land told a nephew, "My work is my life." Harry Land's scrap metal business took him to Bridgeport, Connecticut, and then to Norwich, Connecticut. There he handled most of the scrap from Electric Boat, a major manu facturer of submarines, shrewdly evaluating the content and worth of the metal he was recycling. Harry and his wife Matha Goldfaden had a daughter Helen, and in 1909 (when Harry was twenty-six) a son, who was named Edwin Herbert Land. Helen found the name hard to pronounce, and called her little brother "Din," a nickname that stuck.

In 1929, Land married Helen (Terre) Maislen of Hartford, Connecticut. His wife and he had two daughters, Jennifer Land Dubois and Valerie Land Smallwood. Mrs. Land and they survived him at his death in Cambridge, Massachusetts, on March 1, 1991. He was elected to the National Academy of Sciences in 1953, during his service as president of the American Academy of Arts and Sciences.

POLARIZER

As a boy, Land acquired a fascination with the kaleidoscopes and stereopticons so notably studied in the nineteenth century by the English optical scientist David Brewster. He also came across the textbook *Physical Optics* written by Robert W. Wood of Johns Hopkins University. The first edition of Wood's book (1905) had become well enough known to be cited in the 1911 *Encyclopedia Britannica* article on polarized light. Wood's teacher had been Henry Rowland, who in turn had studied with Hermann Helmholtz. Land was fascinated and read Wood's second edition (1915), he said, like the Bible. At the age of thirteen, at a boys' camp not far from Norwich, Land's fascination with polarization deepened when the camp's leader used a piece of Iceland spar to extinguish glare from a table top. Also at the camp, a near-collision in a car at night with a farmer's wagon underlined the perils of nighttime driving. Headlights should be stronger, but how could they be prevented from blinding the drivers of oncoming cars? The boys discussed how glare might be controlled by polarization. In 1926, the year his sister Helen graduated from Wellesley College, Land entered Harvard College. In a hurry to do actual research on optics, particularly polarization, Land left a few months later and went to New York, where he spent long hours in the great reading room of the New York Public Library. His idealism was roused, as he often recounted, when he walked down a major avenue in New York. The procession of head-lights on the line of approaching cars embodied for him the primary reason for developing a thin and cheap polarizer.

Land began with experiments on reflection polarizers, but went on to repeat William B. Herapath's nineteenth century attempts at making giant thin crystals of iodosulphate of quinine in the hope of making simple polarizers for microscopes. Land had no more success than Herapath. Faced with this impasse, Land reversed course and envisaged a plastic material to be coated on sheets of film that would contain billions of tiny needle-like crystals in each square centimeter (in one dimension smaller than the wave-length of visible light). At first by electric or magnetic fields or later by stretching, the microcrystals were aligned to act as a polarizer. In 1929, with the invention perfected and the first patent applied for, Land returned to Harvard for three more years of study. His work on polarizers so intrigued Theodore Lyman, head of Harvard's physics laboratory, that undergraduate Land was given a separate lab.

In 1932, Land gave the first and so far the only Harvard physics department seminar by an undergraduate, "A New Polarizer for Light in the Form of an Extensive Synthetic Sheet." Instead of remaining to get a degree, however, Land pushed ahead with manufacture and commercialization of the polarizer, founding his own company in partnership with George Wheelwright III, a Harvard physics instructor. Because of the potential uses in controlling headlight glare—and in viewing three-dimensional movies—Land's invention received attention at the research laboratories of General Motors, General Electric, and Eastman Kodak. In 1934, Kodak became the first customer, buying polarizer sheet for camera filters. The next year, American Optical began buying polarizer-laminated sunglass lenses, opening the way to reorganization of Land's enterprise as Polaroid Corporation in 1937.

Polarizing spectacles, made of cardboard and plastic for viewing 3-D movies, were tried dramatically at the 1939–40 New York World's Fair. There, 5 million visitors to the Chrysler pavilion saw a 10-minute time-lapse 3-D film by John Norling, which showed the parts of a Plymouth car assembling them-selves. But commercialization only occurred during the short-lived 3-D movie craze of the early 1950s, a boom in polarizers followed quickly by a bust. In the headlight field, where Land was seeking universal adoption by all car makers, the technological barrier went ever higher as the illuminating power of lamps rose and generated more heat, and the car industry demanded lamination of the polarizers on the out-side of the lamps. To cope with wear from sun, dust, rain, and wind, as well as the higher temperatures, Land invented a new class of polarizers using dyes instead of microcrystals. At the same time, he worked along with Czech refugee Joseph Mahler on a new technology for 3-D still photos, which were named Vectographs.

INSTANT PHOTOGRAPHY

In World War II, the American military used Vectographs for aerial surveys of such major battlefields as Guadalcanal and Normandy. Land converted Polaroid Corporation entirely to war work in such fields as glare-controlling goggles, tank telescopes, gunsights, flight training machines, and heat-seeking bombs. In 1943, with the end of the war in sight, and uncertain about the commercial prospects for the polarizers, Land turned his thoughts to photography as a field ripe for innovation. His invention of instant photography, putting the chemistry of the darkroom between two sheets of film and producing a finished print in 60 seconds, was spurred during a vacation in Santa Fe by a question from his three-year-old elder daughter. Why couldn't she see right away the picture he had just taken of her? Setting off at once on a walk, "stimulated by the dangerously invigorating plateau air of Santa Fe," Land visualized the elements of an on-the-spot print system—in an hour. By chance, his patent attorney also was visiting Santa Fe, and Land could begin at once documenting his concept. Later, Land recalled, "You always start with a fantasy. Part of the fantasy technique is to visualize something as perfect. Then with the experiments you work back from the fantasy to reality, hacking away at the components." On another occasion, he said, "If you sense a deep human need, then you go back to all the basic science. If there is some missing, then you try to do more basic science and applied science until you get it. So you make the system to fulfill that need, rather than starting the other way around, where you have something and wonder what to do with it."

Experiments began at once, by Land and a small group of collaborators. The aim was a system for simultaneous development of the negative and positive. After exposure to light, the unexposed silver halides in the negative that developers had not reduced to metal were transferred to the positive, where special structures allowed the molecules to be anchored and then developed. The highly alkaline chemicals to set the process going were encased in metal-lined "pods," which were burst by the camera's rollers, thus spreading the processing fluid between positive and negative when the two were brought together after the exposure. To avoid interference, precise timing of many operations was required to achieve "the careful balancing of the simultaneous growth of the negative and positive." Equally vital was chemical stability before, during, and after the picture was made and the positive peeled apart from the negative. Land worked with relentless optimism: "An essential aspect of creativity is not being afraid to fail. Scientists made a great invention by calling their activities hypotheses and experiments. They made it permissible to fail repeatedly until in the end they got the results they wanted. In politics or government, if you made a hypothesis and it didn't work out, you had your head cut off." A colleague, the chemist Myron Simon, praised "the spirit, the joy, the excitement of working with Land. . . . He was a charismatic leader. . . . He [could] choose and train people to do the work just the way he wanted it done, [and could] select people to fill in the voids in his own scientific background."

After three years' work, Land demonstrated the film publicly at a meeting of the Optical Society of America in New York in February 1947. A commercial film and camera went on sale in November 1948. The first film produced sepia images, but in 1950 Polaroid began selling a black-and-white restatement of the technologies. A fading problem forced prompt redesign, including the use of a plastic coating, invented by Howard Haas, that had not been required with sepia. During the 1950s, Land and such collaborators as Meroë Morse developed faster versions of black-and-white films, positive-negative and high-contrast films for professional use, and transparencies.

Meanwhile, Land's co-worker Howard Rogers, began fifteen years' research on color instant pictures. Roger's key invention was a molecule of dye for each of three colors—yellow, magenta, and cyan—linked to developer. The dye developers could be distributed in separate layers that adjoined silver halide layers sensitive to blue, green, or red.

Just before the color film went on sale in 1963, Land led a crash effort to avoid a coating step, by creating a self-washing system. This involved a spacer and a layer of acid polymer that would trap alkali processing molecules and release water. As with sepia and black-and-white, Eastman Kodak produced the color negative, while Polaroid made the innovative positive and assembled the film.

In the 1960s, sales of instant color cameras and film soared even more steeply than the larger Eastman Kodak color business, which was driven chiefly by small Instamatics. Competition from Kodak seemed ever more likely. The mounting Polaroid sales around the world allowed the company to retain more and more earnings to finance the next stage of instant photography, including taking over the manufacture of negative. For this multi-hundred-million dollar effort, costing a large fraction of one year's sales, Land mobilized a larger array of research teams than before and made arrangements with a web of outside suppliers. He regarded the new system as removing many years of compromises with his goal of a highly immediate, intuitive mass photography. He said that "one of my main purposes was to have a camera that's part of you, that's always with you." He wanted most amateurs "to get as good as professionals because it would enlarge their horizons." Doing this, millions of photographers would gain "a feeling of personal identification with the world in the way that photography has always hoped to do."

The compact, motorized, electronically controlled, single-lens reflex SX-70 camera and its new film were introduced in 1972 and nationally marketed a year and a half later. For the camera, numerous inventions were made on demand, such as a compact, four-element lens designed by James Baker, a viewing light path involving several aspheric surfaces designed by William Plummer and colleagues, and a flat four-cell LeClanché battery concealed at the bottom of each 10-picture film pack.

The "integral" film of SX-70 posed new stabilization problems because it permanently held both positive and negative. Lloyd Taylor developed temperature-independent timing layers for the negative and polymeric interlayers for the positive. Land had specified a camera that could be carried in a pocket. Hence, the thin mylar-encased film units could not be processed inside the camera. The mechanized rollers ejected each picture into orders of magnitude more light than had been used to expose it. The required opacification system, developed under the leadership of Land's co-worker Stanley Bloom, combined a pair of phenol-phthaleine dyes found by Myron Simon's team with titanium dioxide particles, which formed much of the mass of the SX-70 processing fluid. The dyes were required to be completely opaque in the highly alkaline conditions of the first few seconds of processing and then to decolorize promptly to allow the photographer to judge the SX-70 image against the white backdrop of titania which sealed off the negative. The metallized-dye image, approximately 3 inches by 3 inches, "emerged" over several minutes, and a new acid polymer system regulated development and maintained stability thereafter.

Making the negative called for a large new factory, which drew on a new specialty chemical plant. Yet another new factory assembled the black-backed negative, the transparent positive, and pod of processing chemicals into integral film units, which were placed in 10-picture black plastic "packs." Now controlling all the key parts of film manufacture, Polaroid could and did introduce running changes, such as more brilliant colors, an anti-glare coating, and faster processing times. The films adapted easily to smaller and cheaper cameras. Under Land's continuing control, the SX-70 dyes were retrofitted to peel-apart color films, such as those used to make full-scale replicas of paintings with the help of yet another Baker lens.

Throughout, Land emphasized the need to identify basic needs and imagine a system to meet them: "There's a tremendous popular fallacy which holds that significant research can be carried out by trying things. Actually it is easy to show that in general no significant problem can be solved empirically, except for accidents so rare as to be statistically unimportant. One of my jests is to say that we work empirically—we use bull's eye empiricism. We try everything, but we try the *right* thing first!"

When he said this to employees and shareholders, Land was stepping down after forty-three years as chief executive of Polaroid, in part because of the commercial failure of an additive-color instant movie system, introduced in 1977 as Polavision. Minute stripes of color were deposited on one side of' the film, which was kept within a cassette. A very thin black-and-white negative was exposed to light in a camera similar to many used for home movies. As the exposed film was rewound for viewing on a television-like player, processing fluid covered the film to develop it, and bring metallic silver over into a positive layer. Instantly projected, the silver images were viewed through the color stripes.

A major factor in the failure of Polavision was the meteoric rise of electronic amateur photography with camcorders. Land was skeptical about the move from photographic to digital image making. The photographic emulsion, he said in 1982, was "that wonderful material, the first solid state recorder, done intuitively" in the 1850s through the 1870s. Although the light-sensitive silver halide grains are put on at random, the emulsion "records the whole image at once, just as your eye and brain record the whole image at once. It records it in graduated detail. It records it with just a few photons of light. Twenty photons on a grain. And then it is converted with the snap of a finger from . . . the recording medium to . . . the final image." He said that he was not the first to wonder "how far you could go if you knew how to put [the grains] down in orderly arrays."

SCIENCE-BASED COMPANY

The enterprise Land led for half a century was less a business than an institution focused on making significant inventions. In 1975, he told a press interviewer, "Every significant invention has several characteristics. By definition it must be startling, unexpected, and must come into a world that is *not* prepared for it. If the world *were* prepared for it, it would not be much of an invention."

Land argued that "neither the intuition of the sales manager nor even the first reaction of the public is a reliable measure of the value of a product to the consumer. Very often the best way to find out whether something is worth making is to make it, distribute it, and then to see, after the product has been around a few years, whether it was worth the trouble."

The world, Land understood, was not necessarily friendly to a scientist who wished to operate this way. "Most large industrial concerns," he lamented in 1945, "are limited by policy to special directions of expansion within the well-established field of the company. On the other hand, most small companies do not have the resources or the facilities to support 'scientific prospecting.' Thus the young man leaving the university with a proposal for a new kind of activity is frequently not able to find a matrix for the development of his ideas in any established industrial organization."

Land prophesied that "the small company of the future will be as much a research organization as it is a manufacturing company, and that this new company is the frontier for the next generation." In the "next and best phase of the Industrial Revolution," Land expected businesses to be "scientific, social, and economic" units on the periphery of big cities and in the countryside, which will be "vigorously creative in pure science" with contributions comparable to those of universities. The career of the pure scientist, he expected, would be "as much in the corporation laboratory as in the university." He said this at a forum on the future of industrial research' in 1944, just four years after he had been named, with Irving Langmuir, Edwin Armstrong, and others, as one of the most significant innovators of the previous twenty-five years. He was already working on instant photography.

In the small company Land had in mind "an industrial group of about fifty scientists," studying intensely the recent advances in "newly available polyamide molecules, the cyclotron, radar technics," color photography, and enzymology. If the industrial scientists were "inspired by curiosity" about such fields and determined "to make something new and useful," they could "invent and develop an important new field in about two years."

A small science-based enterprise depends vitally on patents, and Land eloquently defended the temporary monopolies created by the patent system from the charge, made particularly sharply during the New Deal, that it stifled innovation. Land asked, "Who can object to such monopolies? Who can object to a monopoly when there are several thousands of them? Who can object to a monopoly when every few years the company enjoying the monopoly revises, alters, perhaps even discards its product, in order to supply a superior one to the public? Who can object to a monopoly when any new company, if it is built around a scientific nucleus, can create a new monopoly of its own by creating a wholly new field?" Eventually, Land was awarded more than 500 patents, and other Polaroid researchers hundreds more. During 1976-85, Land and co-workers successfully defended a number of SX-70 patents. A federal court required Eastman Kodak to cease making and selling products involving infringements.

A corollary for such an enterprise, Land said, was a work force ready for constant reinvention of products and jobs. Polaroid workers, or "members," were protected by a growing array of benefits. He told employees, "You cannot rely on what you have been taught. All you have learned from history is old ways of making mistakes. There is nothing that history can tell you about what we must do tomorrow. Only what we must not do." To a remarkable extent, Polaroid operated in the fashion Land specified, and grew to nearly 20,000 employees by the time he left in the 1980s.

COLOR VISION RESEARCH

In 1951, years before a laboratory accident launched him on thirty years of study of color vision, Land described it as the "very beginning of vision in the human." He exclaimed, "How nebulous, how preliminary, our knowledge of the mechanism of vision is!"

In 1955, amid the difficult quest for a workable instant color system, Land decided to repeat Maxwell's experiments of a century earlier, which used projectors, one for each of the primary colors of blue, green, and red. Identical images taken through filters of those colors were projected onto the screen. To simulate the blue-poor light of sunrise, Land and his colleague, Meroë Morse, were experimenting with the red and green projectors only. Somehow, the green filter was removed, flooding the screen with white light. Morse noted that she still saw colors, although Land dismissed it as adaptation. Troubled by this glib explanation, Land later returned to the laboratory to repeat the experiment, this time with the intensity of the green projector turned down. With red light from one projector and white light from the other, the screen was filled with a gamut of colors.

In many years of research, Land found that such sensations would occur even if the eye had been exposed to the images for a millisecond, vastly shortening any interval for adaptation. He also found that the colors of objects reported by human subjects bore no relationship to the flux of light in particular wave bands from those objects. An apple was perceived as red early in the morning, and at noontime, even though the mixture of light frequencies was very different at the two times.

The beginning of the work was reported in 1955 to the Society of Photographic Scientists and Engineers and in 1957 and 1958 to the National Academy of Sciences in New York and Washington. Land developed what he called the retinex theory, which held that color sensation was the product of calculations, either in the retina of the eye or in brain structures, or both, in which lightnesses, not flux, in each of the three major wave bands were compared.

Psychologists tended to dismiss Land's color vision research as either trivial or not new. Neurophysiologists such as David Hubel of Harvard University and Semir Zeki of University College, London, however, conducted experiments on regions of monkey brains they had found to contain color-sensitive cells. They also collaborated with Land. Observations with a human patient, whose corpus callosum had been cut to moderate the number and intensity of epileptic seizures, demonstrated that color perception was located in the visual cortex and not the retina of the eye.

Land's studies of color vision led him to reject the notion of human beings as "advanced out of and away from the structure of the exterior world in which we have evolved, as if a separate product had been packaged, wrapped up, and delivered from a production line." In a 1977 address, he denied that the human spirit was condemned to "tragic separation and isolation" from the world. "Of what meaning is the world without mind? The question cannot exist." In a symphony, "the opening theme asks a question and the closing theme states that the question is itself the answer." Through science, the mind seemed to have been schooling itself in "reverence, insight, and appreciation of itself," so that it could pursue understanding "with all the techniques of' thoughtfulness that the mind has used for investigations away from itself."

OVERHEAD RECONNAISSANCE

Many members of the National Academy of Sciences have made contributions to American defense programs over many years. Land was no exception. His field of concentration was national means of reconnaissance of the size and location of an adversary's military forces. Cameras for reconnaissance developed rapidly during and after World War II, and Land's work in optics, including Vectographs, brought him close to designers of new equipment at Boston University and elsewhere.

In the early 1950s, he took part in the succession of MIT summer studies that helped to spur the formation of Lincoln Laboratory for air defense and to focus attention on the need for direct overhead surveys of the Soviet Union in a time of intense confrontation. The United States needed precise and sustained observations to help plan the pace and cost of its own armaments programs to keep them from growing too fast or too slowly. Land was on the steering committee of the Technological Capabilities Panel of 1954, led by James R. Killian of the Massachusetts Institute of Technology. The panel produced a timetable for U.S. development of intercontinental and intermediate-range missiles.

Land headed the TCP project on intelligence. In the summer and fall of 1954, Land worked with James Baker and others on the design of cameras for overhead photography of the Soviet Union, and was brought into contact with Lockheed Aircraft engineers who had developed the very-high-flying glider-like craft known as the U-2. The plane was designed with a bay behind the pilot to carry the cameras Land worked on or other packages for electronic monitoring. Land played an important role in two assignments to the Central Intelligence Agency: development of the U-2 by the CIA instead of the U.S. Air Force and firm control of the interpretation of its photographs. The program involved cooperation of Itek and many other firms. One of these, Eastman Kodak, provided a thin film for the U-2 cameras that allowed more pictures per flight, or more "bits per pound." The planes began flying over the Soviet Union in 1956 and soon discovered the limited size of both the Soviet bomber fleet and its intercontinental missile stock.

Because it was understood that it was only a matter of time before the secret of the U-2 was broken, development of U.S. spy satellites began soon after, going into high gear in 1958. The first successful Corona satellite returned pictures in a re-entry capsule in August 1960, less than four months after a U-2 was shot down over Sverdlovsk. Land headed a succession of panels supervising development of both spy planes to succeed U-2 and spy satellites. The panels gave particular attention to understanding the advancing art and helping designers and operational people clear away technical obstacles.

EDUCATION AND PHILANTHROPY

Education was the focus of many of Land's gifts from a fortune that reached $500 million in the late 1960s. His own experience made him impatient with the usual student's life. His chief complaints were lack of access, not only to first-rate minds but also to direct experience of research. The anonymous gift of $12.5 million in 1968 for a science center at Harvard was designed to give undergraduate science greater weight in a research-oriented university and to promote interaction among disciplines. His public gift in the 1970s of building funds and endowment for the new house of the American Academy of Arts and Sciences was also designed to intensify communication among scientists and humanists. Heading the building committee, Land sought what the architect called "a house of beautiful ideas. . . a large, comfortable house which would be a refuge from the unstructured intensity of the surrounding world." At the groundbreaking in 1979, Land noted that to add to knowledge individuals had to "limit themselves by excluding many other areas." To make sure that ideas moved from one field to another, the academy must provide "intimacy, informality, and friendliness, because the transfer is usually not a conscious process. Models for physics may come from music, for chemistry from physics, for art from cosmology." The speech is displayed on a wooden panel at the academy, not far from the often-used fireplace of a large public hall.

In a famous speech at MIT in 1957, "Generation of Greatness," Land said college education was destroying the dream each student had of "greatness," that is an original contribution. Group research and "community progress" must not take over. In a democracy, one must cooperate, but democracy's "peculiar gift is to develop each individual into everything he might be." If the dream of personal greatness died, he said, "democracy loses the real source of its future strength." He wanted arriving students to be assigned an "usher," an experienced researcher, and to be launched at once on research. Drawing from his life, Land said that education must produce people who, no matter how tightly they conformed to the innumerable commands of society, would find one domain where they would make a revolution. Students should go as rapidly as possible through all the intellectual accumulations of the past, to reach quickly the domain where they would have their own work to do. Lectures must be streamlined. Why not use movies to "can" a professor's best lectures "with the vitamins in"? The professors would be captured "at the moment when they are most excited about a new way of saying something or at the moment when they have just found something new." They would waste less time redoing their lectures. With the movies, students could view the lectures as many times as they needed. The proposal looked visionary in the 1950s, but Land soon launched his colleague Stewart Wilson on interactive lectures using such films, a process that appears more attainable in an era of ubiquitous networked computer keyboards and screens. Land's ideas on student research helped inspire MIT's undergraduate research program, created in the late 1960s. He also financed the start of Harvard's freshman seminars.

A concern with popular education made him an effective advocate in Congress for the 1967 recommendation of the Carnegie Commission on public television that federal support be increased sharply. He told a U.S. Senate committee that funds were needed for public television to aim at smaller audiences than the networks, to search for "ways to tell young people what we know as we grow older—the permanent and wonderful things about life." He added, "We are losing this generation. We all know that. We need a way to get them back."

Land was not satisfied with a university research system in which scientists were swamped with committee work and the endless search for grants. As a demonstration of another way to proceed, he endowed and led the Rowland

Institute for Science in Cambridge in the 1980s. A small group of scientists continues to work at the Rowland Institute on many topics related to light and matter, and light and life.

THE AUTHOR, A SCIENCE journalist since the 1950s, first met Edwin Land in the White House in 1968, when Land accepted the National Medal of Science. In 1972, he spent a year at Polaroid Corporation as a consultant on the description of the SX-70 film and camera system. At the *New York Times* between 1973 and 1978, he covered developments in the photographic industry. During 1982–91, while directing the Knight Science Journalism Fellowships at the Massachusetts Institute of Technology, he was a part-time public information consultant at the Rowland Institute for Science. This work brought McElheny into frequent contact with Land and his associates. His full-length biography of Land, *Insisting on the Impossible* (Perseus Books, 1998) contains a bibliography and footnotes. The book is based on more than twenty years of interviews and notes by the author, numerous unpublished sources in the archives of Polaroid Corporation, press reports over more than fifty years, and Land's published papers, including those listed below.

HONORS AND DISTINCTIONS

Cresson Medal, Franklin Institute, 1937

National Modern Pioneer Award, National Association of Manufacturers, 1940

American Academy of Arts and Sciences, 1943; president 1951–53

Rumford Medal, American Academy of Arts and Sciences, 1945

Holley Medal, American Society of Mechanical Engineers, 1948

Duddell Medal, Physical Society of Great Britain, 1949

National Academy of Sciences, 1953

Potts Medal, Franklin Institute, 1956

American Philosophical Society, 1957

Society of Photographic Scientists and Engineers, 1957

Doctor of science degree, Harvard University, 1957

Member, President's Science Advisory Committee, 1957–59; consultant-at-large 1960–73.

Member, President's Foreign Intelligence Advisory Board, 1961–77

Royal Photographic Society of Great Britain, Honorary Fellow, 1958

Presidential Medal of Freedom, 1963

National Academy of Engineering, 1965

Albert A. Michelson Award, 1966

William James Lecturer on Psychology, Harvard University, 1966–67

Frederic Ives Medal, Optical Society of America, 1967

National Medal of Science, 1967

Founders Medal, National Academy of Engineering, 1972

Optical Society of America, Honorary Member, 1972

The Royal Institution of Great Britain, 1975

National Inventors Hall of Fame, 1977

The Institute of Electrical and Electronics Engineers, Honorary Member, 1980

The Royal Society, foreign member, 1986

William O. Baker Medal of Achievement, Security Affairs Support Association, 1988

National Medal of Technology, 1988

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1961 With S. Wilson. Education and the need to know. Technol. Rev. 69:29–36. Reprinted in McCann, vol. II, pp. 61–67.

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Courtesy of Jack Liu

RICHARD MACY NOYES

April 6, 1919–November 25, 1997

BY RICHARD J. FIELD AND JOHN A. SCHELLMAN

RICHARD MACY NOYES WAS an exceptionally fine physical chemist who dedicated his abundant personal and intellectual abilities to making the world a better place for his having been a part of it. He directed his scientific work almost entirely toward understanding the details of how chemical reactions occur; making seminal contributions in isotopic-exchange processes, the theory of molecular diffusion in solution, and treatment of complex kinetics and reaction mechanisms; and most memorably, pioneering work on the mechanisms of oscillating chemical reactions and nonlinear dynamics in chemistry. He participated actively in public affairs, mainly through protection of the natural world he loved, promotion of international cooperation, and in administrative and leadership roles at the University of Oregon, where he spent many happy and productive years. His goodwill, integrity, and intelligence were highly valued and respected by all who met him.

Dick was born in Champaign, Illinois, on April 6, 1919, the first child of William Albert Noyes, Sr., and Katharine Haworth (Macy) Noyes. He was the third of four surviving children of his father, then nearly sixty-two years old and chairman of the Department of Chemistry at the University of Illinois. It is rumored that the senior Noyes near his sixty-fifth birthday responded to the dean's discreet inquiry concerning his retirement plans with an invitation to the christening of Dick's younger brother Pierre, born in 1923 and destined to become a prominent theoretical physicist. The boys grew up in a close but intense and formal Congregationalist family that received many prestigious visitors and emphasized all things intellectual, as well as active summers at the family's rural retreat near Frankfort, Michigan.

William A. Noyes, Sr., was a dominant figure in American chemistry from 1890 to 1930, and in 1977 Dick became at least the fourth descendent of the Massachusetts Puritan leader Nicholas Noyes (1615–1701) to be elected to the National Academy of Sciences. The other three, all professors of chemistry, being his father (Rose Polytechnic Institute, National Bureau of Standards, University of Illinois, elected 1910), half-brother W. Albert Noyes, Jr. (Brown University and the universities of Rochester and Texas, elected 1943), and distant cousin Arthur Amos Noyes (MIT and Caltech, elected 1905). Dick and his father also were elected to the American Academy of Arts and Sciences. The family tradition of scientific excellence and service is further exemplified by the three older Noyeses being president of the American Chemical Society, something Dick himself never aspired to, being somewhat shy personally and preferring to quietly "think globally-act locally." Dick did serve the American Chemical Society on the Nomination and Elections Committee and the Publications Committee, as well as the Committee on Committees and as chairman of the Division of Physical Chemistry.

W. A. Noyes, Sr., founded and was first editor of both *Chemical Abstracts* and *Chemical Reviews*. He and W. Albert Noyes, Jr., were editors of the *Journal of the American Chemical Society* and *Chemical Reviews* (the latter at one point was simultaneously editor of *JACS* and the *Journal of Physical Chemistry*). A. A. Noyes wrote a number of influential early chemistry textbooks, often based on his own research, and was known as a superb teacher and administrator. Dick served as associate editor of the *Journal of Physical Chemistry* in 1980–82 in order to bring nonlinear dynamics into the mainstream of physical chemistry, and he served on the editorial advisory boards of *Chemical Reviews, Journal of Physical Chemistry, Annual Review of Physical Chemistry, International Journal of Chemical Kinetics,* and *Physical Review A*. The development of departments of chemistry of international stature at the University of Illinois and at Caltech is credited largely to the efforts of W. A. Noyes, Sr., and A. A. Noyes, respectively. Dick himself served four staggered terms between 1960 and 1978 as head of the University of Oregon Department of Chemistry. Dick's maternal grandfather, Professor Jesse Macy of Grinnell College and a teacher of W. A. Noyes, Sr., was widely known in the field of political science, especially international peace and cooperation, an influence Dick felt throughout his life. Katharine Macy was teaching English at Grinnell College at the time of her marriage.

Steeped in these traditions and expectations, Dick entered Harvard College in 1935, shortly after his sixteenth birthday, where, as he occasionally quipped, "Having absolutely no imagination, I majored in chemistry." Chemistry classmates Frank Lambert and Dick Juday remember Dick for his polite demeanor, as well as personal and intellectual discipline, keen, precocious scientific insight, and the ability to express himself clearly. He and Pierre, who also entered Harvard at sixteen, both started their formal training in chemistry at the university level, because their father advised them not to take high school chemistry in Champaign but to wait for Harvard to do it. Dick was one of the top students of the class of 1939, graduating summa cum laude and a member of the Senior Sixteen elected to BK. He participated fully in undergraduate activities, including house wrestling and crew. The two wrestling victories of "Deacon" Noyes in the final event of the season gave Kirkland House the 1939 campus championship.

Dick chose to do his graduate work at California Institute of Technology, feeling that it had the strongest department in physical chemistry in the country at that time. He chose not to work with Linus Pauling, the dominant Caltech, and perhaps American figure in physical chemistry, because "I was interested in chemical reactions rather than structure." Independence was a mark of Dick's scientific and personal life. But he was correct! Despite the dramatic progress made since 1939, chemical reactions were and likely will remain the most mysterious part of physical chemistry. Dick instead did his Ph.D. with Roscoe Gilvey Dickinson, who had followed A. A. Noyes from MIT, had pioneered X-ray crystallography at Caltech, and had been Linus Pauling's Ph.D. mentor. Dickinson was then interested in chemical kinetics and mechanism and was in close contact with W. Albert Noyes, Jr., a pioneer in photochemical reactions.

Dick finished his Ph.D. in 1942 under the pressure of World War II, working mainly on the simultaneous *cis-trans* isomerization and iodine-exchange kinetics of diiodoethylene. He made some of the very first radioisotope exchange-kinetics measurements using very small samples of unknown isotopes of iodine obtained from a Berkeley cyclotron. Dick often commented that the only chemical reactions that are well understood are those that have not been investigated in detail. That is certainly true in this case, and even in his tenacious last paper on this process in 1967, he is not satisfied completely with explanations of why exchange is so much faster than isomerization.

The war kept Dick at Caltech until 1946 as a temporary instructor and research associate mainly involved with the analytical and physical chemistry of nitrocellulose smokeless powders. He systematically expanded personal and social interests during his seven years in Pasadena, developing an interest in folk dancing and focusing his intense devotion to the outdoors—mountains in particular. He joined with friends to buy and keep running an ancient Ford for trips to the mountains. Despite less than superb physical gifts, but with intensity, discipline, and enthusiasm, he made a number of climbs in the Sierra Nevada. He became deeply involved with the Sierra Club, in which he played an active role for over 50 years. After Roscoe Dickinson's untimely death in 1945, Dick loyally saw to it that Dickinson's major ongoing work was completed and published. Both at Caltech and later he carried on with some of the research ideas Dickinson was most anxious to see accomplished, especially photochemical space-intermittency, a method for determining the diffusion coefficients of reactive intermediate species.

Dick met and in 1946 married Winninette Arnold, a chemist and daughter of a prominent geologist. The newlyweds left California for New York City to begin his independent academic career as instructor of chemistry at Columbia University. His early work there was built on I_2-exchange kinetics, which could be carried out with good quantitative precision even in the 1940s, and which proved to be exquisitely sensitive to mechanistic detail. The juxtaposition of radical and polar mechanisms, as well as the characterization of free-radical kinetics and mechanism in these and a number of other systems, thermal and photochemical, were carefully and insightfully investigated. Dick continued I_2-exchange work until 1970, creating a characteristically detailed and thorough understanding of how such reactions occur.

This path led him to consideration of the fundamentals of molecular diffusion and diffusion-controlled reactions in solution. He and graduate student Fred Lampe noted in 1954 that the quantum yield of I_2 resulting from the photolysis of allyl iodide depends on solvent molecular weight, an indication that transport (diffusion) must be an important component of the mechanism. They concluded that the two geminate radicals ($I^\bullet$ and $CH_2{=}CHCH_2^\bullet$) produced in the primary photochemical event must be lodged initially in a "solvent cage" and that a competition must exist between their recombination and diffusion out of the cage in order to participate in secondary reactions leading to I_2. He then extended the cage concept profoundly by asking whether it exists if two radicals are close, even if not geminate. Using the exclamation mark, as Dick often did to display his passion for a remarkable observation: it does! The result is that in very rapid, diffusion-controlled reactions the equilibrium homogeneous spatial distribution of reactive species is destroyed in a manner equivalent to the disruption of the Boltzmann distribution of highly excited species if they are very reactive. The definition of a diffusive encounter becomes very important in this case, and Dick showed that a diffusion-controlled rate constant depends on time because the spatial distribution of very reactive species changes as the extent of reaction increases. The capstone of this work is his 1961 review of molecular diffusion in the first volume of *Progress in Reaction Kinetics*. It remains today a fundamental resource, receiving about 20 citations annually, 36 years after its appearance.

Dick returned in the mid 1950s to Roscoe Dickinson's photochemical space-intermittency suggestion, whose implementation caused him in 1957 to be among the first to solve numerically a reaction-diffusion equation on a digital computer, as well as to undertake prior to laser technology the very difficult experimental problem of projecting sharply focused leopard and tiger patterns of light into a reactive solution. This eventually successful effort was continued at the University of Oregon, and it was to work on this problem that I (R.J.F.) started in 1968 my long collaboration and friendship with Dick.

Dick and Win were warmly regarded at Columbia. In the department they were especially close with George and Alice Kimball. They were notable for their friendly social manner, close interactions with and concern for students, a broad array of scientific visitors, and an interest in travel and international customs and affairs. Dick had an abiding commitment to world peace, acquired from his father, who was intensely active in this area, especially in the period after World War I. He was a charter subscriber to the *Bulletin of the Atomic Scientists* and a regular participant at the annual meetings of the National Academy of Science's Committee on International Security and Arms Control. After being tenured at Columbia he and Win spent a sabbatical year on a Guggenheim Fellowship at Leeds in England.

New York City did not succeed in urbanizing the young couple. They took up backpacking in the Appalachian Mountains long before such a thing was fashionable, and thus it required Win's homemade equipment. They made a systematic effort to reach the highest geographical point of each of the contiguous 48 states, an RMN-like endeavor if there ever was one. They missed the influence and activities Of the Sierra Club, and since they could not live in California, they brought the Sierra Club to New York. Dick was instrumental in founding the Atlantic Chapter of the Sierra Club, now the New York Chapter. At the same time he contributed dedicated service to important conservation issues of the day, especially the proposed construction of dams on the Delaware and Colorado rivers. Thus he was a major figure in the conversion of the Sierra Club from a regional to a national organization. (Later he was chairman of the Northwest Chapter, now split into state chapters, and established Eugene, Oregon, as the first local subdivision of the Sierra Club. In the late 1960s and 1970s he and Win played a major role in the effort to preserve the valley of French Pete Creek, near Eugene. This campaign was successful in 1978 when Congress added French Pete, as well as the adjacent drainages of Rebel and Walker creeks, to the Three Sisters Wilderness, helping to change forever the ethic of the USDA Forest Service and its sensitivity to public concerns.)

Despite these efforts, life in the canyons of New York City, rather than the canyons of the western mountains, was confining for them. They were personally devastated by the loss of two infant sons, Win's bout with tuberculosis, and her long-standing diabetes, as well as some resulting personal and marital problems. Happily for all, the University of Oregon in Eugene at the same time was undertaking the construction of a world-class Department of Chemistry and had identified a group of outstanding younger people to build that department, four of whom went to Oregon and were eventually elected to the National Academy of Sciences. Dick was one of these, and in 1958 he eagerly accepted the position of professor of chemistry and the challenge and joy of helping to build an outstanding department. He and I (J.A.S.) arrived almost on the same day at the University of Oregon and thus began almost 40 years of friendship and scientific association.

In Eugene, Dick continued his work on various aspects of molecular diffusion and isotopic-exchange kinetics, as well as mechanistic studies in related systems. He began new fundamental work on the thermodynamics of ion formation, and he made a major thrust into the reactions of diatomic molecules (e.g., the formation of HI from H_2 and

I_2) presumed at that time to be a concerted reaction proceeding through a four-centered transition state, rather than the currently accepted $I^{\bullet}$-based mechanism. Much of his time and energy in the 1960s was spent in administration of a rapidly growing department. He was acting head of the department in 1960–61 and head in 1963–64 and 1966–68, with 1964–65 split between Victoria University of Wellington, New Zealand (Fulbright fellowship) and with Manfred Eigen at the Max Planck Institute für Physikalische Chemie, Göttingen. These two locations indicate the breadth of Dick's scientific as well as geographical interests.

By 1969 the combination of administrative work, steady deterioration of Win's health, development of fast direct methods in chemical kinetics, a global and local shift of interest to modern spectroscopic investigation of biochemical systems, some bad luck, and perhaps even his independent streak, left his research running down. The reactions of diatomic molecules thought to be concerted turned out to be mainly orbital-symmetry forbidden and probably radical or even heterogeneous in nature, making them of less theoretical interest. Photochemical space-intermittency is excruciatingly difficult to apply broadly, and he had pushed the diffusion work about as far as he could without taking into account the discontinuous nature of solvents. Dick and Win were increasingly directing their energies toward Sierra Club activities.

In 1969 a scientific challenge appeared that Dick's 30 years of work in complex reaction mechanisms and reactive diffusion had fitted him to meet probably better than any other person in the world: the Belousov-Zhabotinsky (BZ) Reaction. Starting with elucidation in 1971 of the BZ mechanism, he pioneered and solidified over the next 25 years an entirely new area of physical chemistry: oscillating chemical reactions. Over one-half of his 208 scientific publica tions are in this area and appeared after his fiftieth birthday.

The second law of thermodynamics requires that all spontaneous processes be accompanied by a decrease in Gibbs free energy; thus a reacting chemical system must move monotonically toward equilibrium. This means that the amounts of some species, referred to as reactants, must always decrease, and that the amounts of other species, referred to as products, must always increase in a spontaneous chemical reaction. Until the 1960s this absolute requirement of monotonic approach to equilibrium was widely thought to forbid oscillations in the concentrations of chemical species during such a reaction. However, the second law requires only that the amounts of reactants and final products change monotonically; the amounts of intermediate species, present in much lower concentrations than those of reactants, may indeed oscillate if the governing dynamic law contains suitable feedback loops. By 1968 Ilya Prigogine and coworkers had used a hypothetical chemical model known as the Brusselator to investigate the dynamic requirements for temporal oscillation, as well as for spontaneous spatial pattern formation, to occur in chemical systems. Both phenomena were dubbed dissipative structures by Prigogine because they are supported by the dissipation of free energy. The final approach to equilibrium however must be monotonic, and Prigogine's work showed that chemical oscillations are a far-from-equilibrium phenomenon. Similar advances were being made in other areas of physics, biology, mathematics, and engineering, partially spurred by the advent of digital computing. These ideas form the basis of what is now known as nonlinear dynamics and complexity theory.

There was in 1969 no unequivocal example of a real chemical reaction exhibiting oscillations based on a mechanism involving only homogenous component reactions to provide credibility to theory and to serve as a learning tool. The best known system, the IO_3^--catalyzed decomposition of H_2O_2, during which the concentrations of I_2 and I^- oscillate and O_2 is produced in pulses, is still not understood mechanistically, and the gas pulses allowed the oscillations to be attributed to supersaturation rather than to homogeneous chemical kinetics. Solving the BZ mechanism provided the unequivocal example that allowed an explosion of progress to be made. Beyond striking the spark, Dick contributed mightily to the blast that followed. Furthermore, he supplied a great deal of personal and intellectual leadership to the new area of research, working hard to assure communication and cooperation, and the opportunity for all, around the world and of all ages and stature, to contribute and to be respected for their contributions. The co-authorships of his papers are remarkable both for the range of his international collaborators and for the order of authors, which nearly always has Dick's younger or international coworkers first. Dick chaired the 1985 Gordon Conference on Oscillations and Dynamic Instabilities, and he was scientific and financial patron of this conference, as well as those in 1988, 1991, and 1994, working to assure participation of young people, especially from Eastern Europe and underdeveloped countries.

The BZ reaction had a shadowy history in Russia before arriving in Dick's hands. Boris Belousov was unable to publish his 1951 discovery of the oscillations because of the second law shibboleth. A. M. Zhabotinsky continued the work in the 1960s and managed to get word of its existence into the West. News of the BZ reaction reached Eugene in October 1969 with Bob Mazo, a University of Oregon chemical physicist just returned from a sabbatical year with Prigogine in Brussels. Upon hearing that essentially nothing was known of its mechanism, Dick and I (R.J.F.), soon to be joined by visitor Bandi Korös on sabbatical from the Eotvös Loránd University in Budapest, undertook to change that circumstance. The result in 1972 was the so-called Field-Korös-Noyes (FKN) mechanism.

The BZ reaction in its classic form is the metal-ion {e.g., Ce (IV) /Ce (III) or $Fe(phen)_3^{3+}/Fe(phen)_3^{2+}$} catalyzed oxidation of an organic substrate, e.g., $CH_2(COOH)_2$, by BrO_3^- in an acidic medium. Oscillation in the concentrations of intermediate species are driven by the exothermicity of the oxidation of $CH_2(COOH)_2$ by BrO_3^-.

$$3BrO_3^- + 5CH_2(COOH)_2 + 3H^+ \rightarrow 3BrCH(COOH)_2 + 2HCOOH + 4CO_2 + 5H_2O$$

The ratio of the oxidized and reduced forms of the metal-ion catalyst, as well as the concentrations of Br^- and a num

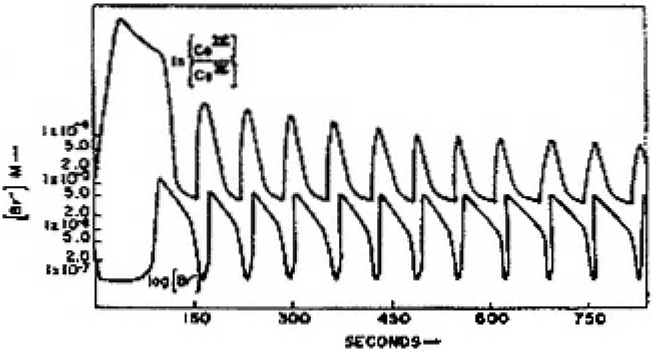

Figure 1.
Belousov-Zhabotinsky oscillations in Ce(IV)]/[Ce(III)] and [Br^-]. [$CH_2(COOH)_2]_0$ = 0.13 M (6.7 g), [$KBrO_3]_0$ = 0.063 M (5.3 g), and [$Ce(NH_4)_2(NO_3)_6$] = 0.005 M (1 g) in 500 mL of 0.8 M H_2SO_4. Reproduced from R. J. Field, E. Korös, and R. M. Noyes, *J. Am. Chem. Soc.* 94(1972):8649–64.

ber of other species, are easily observed to oscillate, sometimes through hundreds and even thousands of cycles as the overall oxidation reaction rushes toward equilibrium. A typical experiment is shown in Figure 1.

The FKN mechanism is shown below.

Process *A* (Polar)

A1 Reduction and exhaustion of Br

$$\text{(R3)}\quad Br^- + BrO_3^- + 2H^+ \rightleftarrows HBrO_2 + HOBr$$
$$\text{(R2)}\quad Br^- + HBrO_2 + H^+ \rightarrow 2HOBr$$
$$\text{(R1)}\quad 3 \times (Br^- + HOBr + H^+ \rightleftarrows Br_2 + H_2O)$$

$$\text{(NET: A1)}\quad 5Br^- + BrO_3^- + 6H^+ \rightarrow 3Br_2 + 3\,H_2O$$

A2 Bromination of organic substrate

$$\text{(A2)}\quad 3 \times (Br_2 + CH_2\,(COOH)_2 \rightarrow BrCH(COOH)_2 + Br^- + H^+)$$

$$\text{(NET: A = A1 + A2)}\quad 2Br^- + BrO_3^- + 3CH_2(COOH)_2 + 3H^+ \rightarrow 3BrCH(COOH)_2 + 3H_2O$$

Process *B* (Radical)

B1 Radical generation and oxidation of Ce(III)

$$\text{(R5)}\quad 2 \times (HBrO_2 + BrO_3^- + H^+ \rightleftarrows 2BrO_2^{\bullet} + H_2O)$$
$$\text{(R6)}\quad 4 \times (BrO_2^{\bullet} + Ce(III) + H^+ \rightleftarrows Ce(IV) + HBrO_2)$$

$$\text{(NET: B1)}\quad 2 \times (HBrO_2 + BrO_3^- + 2Ce(III) + 3H^+ \rightarrow 2HBrO_2 + 2Ce(IV) + H_2O)$$

B2 $HBrO_2$ disproportionation

$$\text{(R4)} \quad 2HBrO_2 \rightarrow HOBr + BrO_3^- + H^+$$

$$\text{(NET: B = B1 + B2)} \quad 4Ce(III) + BrO_3^- + 5\,H^+ \rightarrow 4Ce(IV) + HOBr + 2H_2O$$

Process *C* (Feedback, regeneration of Br^- and Ce(III))

$$\text{(C)} \quad Ce(IV) + {\sim}HOBr + {\sim}BrCH(COOH)_2 + {\sim}\,CH_2(COOH)_2 \rightarrow Ce(III) + f\,Br^- + {\sim}CO_2 + \ldots ?$$

~ indicates variable stoichiometry. *f* is a coefficient indicating the effectiveness of the major negative feedback loop that destabilizes the steady state and leads to oscillation.

Process *A* is a series of polar, two-electron oxidations carried out via oxygen-atom transfers. It is dominant at high [Br^-], but its net effect is only the removal of Br^-. None of the singlet oxybromine species in Process *A* carry out with any facility the single-electron oxidation of Ce (III) to Ce (IV). Thus high [Br^-] inhibits the oxidation of Ce(III) to Ce(IV). However, when [Br^-] falls below a critical value, [Br^-] is approximately less than K_{R5}/k_{R2} [BrO_3^-], the reaction of $HBrO_2$ with BrO_3^- to yield the radical species $BrO_2^{\bullet}$ (Reaction *R*5) becomes competitive with removal of $HBrO_2$ by Br^- (Reaction *R*2), and the autocatalytic oxidation of Ce(III) to Ce(IV), as well as simultaneous growth in [$HBrO_2$], explodes via the single-electron oxidant $BrO_2^{\bullet}$ in *R*6. Process *C* provides a negative feedback loop via which Process *B* inhibits itself by the production of Br^- from its products, Ce (IV) and HOBr. This brings Process *A* back into control, and the system reinitializes itself as Process *C* reduces Ce(IV) back to Ce(III) at high

[Br^-], preparing for the next cycle. The stoichiometric coefficient *f* in Process *C* is the number of Br^- produced per Ce (IV), which must be > 1, but less than some maximum value for the usual pseudo-steady state to be unstable, allowing oscillation to occur. Otherwise either Process *A* or *B* retains control indefinitely.

The FKN mechanism removed any doubt that homogeneous chemical oscillations can and do occur solely as the result of nonlinear dynamic structure. The source of the instability and oscillations in the BZ reaction is made clear as resulting from a negative feedback on an autocatalytic process. The FKN chemistry is reasonable and is supported by analysis of a large body of kinetic data, as well as deduction of a thermodynamically consistent set of rate constants for its principal component reactions. Numerical simulations based on the FKN mechanism reproduce nearly quantitatively the observed behavior of the BZ reaction. The chemical world now took chemical oscillations seriously, the shibboleth disappeared, and the search for other examples began in earnest. Many were found, particularly after the development of systematic search techniques by I. R. Epstein and colleagues.

This period of exhilarating scientific adventure and achievement sadly occurred under very difficult personal circumstances for Dick. He and Win had taken 1971–72 as a sabbatical year in Oxford, England, on the prayer that a period of rest would allow her some recovery from the ravages of diabetes. Indeed, the final form of the FKN mechanism emerged in an early-morning phone call between Dick in Oxford and me (R.J.F.) in Eugene. Dick was devastated by Win's death in Oxford in March 1972 after years of his attentive care. In January 1973 Dick married Patricia Harris, a well-known developmental biologist who shared his devotion to science and to environmental matters.

Meanwhile Dick's scientific work continued. His understanding of diffusion carried over to this work, because, when the BZ reagent is unstirred, the interaction of reaction and diffusion of the autocatalytic species $HBrO_2$ may lead to the formation of traveling waves of reaction. Initiation of Process B in a small area produces a wave front of metal-ion catalyst oxidation that propagates through an area under the control of Process A, much as a fire moves across a dry field. A wave front is followed by a transient refractory region under the control of Process C. Thus the interaction of several BZ waves produces elaborate patterns that may serve as models of such diverse biological phenomenon as Ca^{2+} waves in the cell and the complexity of heart muscle contraction. The reaction-diffusion equation resulting from the FKN mechanism rationalizes the BZ wave fronts.

The BZ reaction is experimentally reliable, easy to work with, and shows most of the behaviors typical of systems governed by nonlinear dynamic laws (e.g., simple and complex oscillations, multistability, excitability, traveling waves, and even deterministic chaos). The connection between the BZ reaction and the mathematics of nonlinear dynamics was made firm by Dick and R.J.F. in 1974 by their introduction of the Oregonator, a simple model derived from the FKN mechanism and similar to Prigogine's Brusselator. It is named after the State of Oregon, and its basic form and significance occurred to R.J.F. during an exceedingly dull sermon. The relationship of the Oregonator, shown below, to the FKN mechanism is made clear by the identifications: A ` $BrO_3^-$, X ` $HBrO_2$, Y ` $Br^-$, Z ` Ce (IV), P ` HOBr, Reactions 1 and 2 ` Process A, Reactions 3 and 4 ` Process *B*, and Reaction 5 ` Process *C*.

$$A + Y \longrightarrow X + P \qquad (1)$$

$$X + Y \longrightarrow P + P \qquad (2)$$

$$A + X \longrightarrow 2X + Z \qquad (3)$$

$$X + X \longrightarrow A + P \qquad (4)$$

$$Z \longrightarrow fY \qquad (5)$$

The Oregonator differential equations,

$$\mathrm{d}x/\mathrm{d}t = k_1\, ay - k_2\, xy + k_3\, ax - 2k_4\, x^2$$

$$\mathrm{d}y/\mathrm{d}t = -k_1\, ay - k_2\, xy + f\, k_5\, z$$

$$\mathrm{d}z/\mathrm{d}t = k_3\, ax - k_5\, z,$$

reproduce these behaviors, and feedback between this model and BZ experiments was and remains a work horse in the dramatic development of understanding of complex, nonlinear dynamical systems that has occurred in most areas of science since the 1970s.

The FKN mechanism and the Oregonator were pivotal to the development of an entirely new and broadly applicable area of science. Ilya Prigogine was awarded the 1977 Nobel Prize in chemistry for his theoretical work on dissipative structures. This likely would not have occurred without the BZ reaction and the FKN mechanism.

Dick underwent heart-valve replacement surgery in 1976, but it did not slow down his scientific work or interest in the world. He continued to work out details of the FKN mechanism and to generalize it to the class of catalyzed and uncatalyzed BrO_3^--driven oscillators. He investigated a large class of chemical oscillators in which gas supersaturation is important, as well as the cobalt-catalyzed air oxida tion of benzaldehyde, an important industrial process. He served a final long term as head of his department in 1975–78. He and Pat spent 1978–79 and 1982–83 (Alexander von Humboldt fellowship) at the Max Planck Institute für Biophysikalische Chemie, Göttingen, a center where they both could follow their scientific interests. He formally retired in 1984, but he and Pat continued to travel worldwide as Dick continued service as an international focus and leader of nonlinear dynamics. A series of strokes beginning in 1992 left him increasingly incapacitated, despite his heroic efforts to keep going. He passed away on November 25, 1997.

Dick Noyes was a classic progressive who practiced his conviction that human goodwill and intelligence will lead to a better world for all. His fundamental work on molecular diffusion, the cage effect, and especially the BZ reaction has found its place in textbooks of physical chemistry. Dick himself in his sixtieth year reflected on his life for the 1978–79 edition of *Who's Who in America:*

> When I was young, I wanted to be an "explorer." I am fortunate to have a job in which I can make discoveries as exciting as those of the explorers who first sailed uncharted seas. Then I can try to convey the excitement to another generation. As an avocation, I try to influence government policies toward our least developed lands. It is a gratifying mix of satisfying curiosity and serving society.

He will be warmly remembered by the many people whose lives he touched.

WE WOULD LIKE TO thank the following for supplying us with information and insights into Dick's early life and activities: Pat Harris Noyes, Pierre Noyes, Sandy Tepfer, Seymour Adler, Barbara Allred, John Amneus, Edward Anders, David Booth, John Bujake, David Curtin, Ben Dailey, Ted Eyring, Dick Juday, Alice Kimball, Frank Lambert, Fred Lampe, Wolfgang Panofsky, Prudence Kimball Phillips, Charlotte Schellman, and Cheves Walling.

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JAMES OLDS

May 30, 1922–August 21, 1976

BY RICHARD F. THOMPSON

JAMES OLDS WAS ONE of the most important psychologists of the twentieth century. Indeed, many of us feel that his discovery of the "reward" system in the brain is the most important single discovery yet made in the field concerned with brain substrates of behavior. In retrospect, this discovery led to a much-increased understanding of the brain bases and mechanisms of substance abuse and addiction. Jim also was a pioneer in the study of neural substrates of learning and memory and the first to show that neurons in the hippocampus become substantially engaged in basic associative learning.

James Olds was born in Chicago on May 30, 1922, and grew up in Nyack, New York, and Washington, D.C. Jim's father was an economist who had been appointed by Franklin D. Roosevelt to be chairman of the Federal Power Commission. Jim held various summer jobs and spent a year as a reporter for the International News Service. After three years of military service with the Persian Gulf Command in Teheran and Cairo, Jim returned to the United States and finished his B.A. at Amherst College in 1947. In 1946 he married Marianne N. Olds, nee Egier, a student at Smith College. They had one daughter, Nicole Jacqueline Olds, now a psychiatrist on the faculty of Harvard Medical School, and a son, James L. Olds, now himself a prominent neuroscientist.

Jim received his master of arts degree in 1951 and his Ph.D. in 1952 in psychology at Harvard University, where he remained for one additional year as a lecturer and research associate in the laboratory of social relations. He then received a U.S. Public Health Service fellowship to do postdoctoral research at McGill University from 1953 to 1955. Jim then spent an additional two years (1955–57) as an associate research psychobiologist in the anatomy department of the University of California, Los Angeles, following up on his discovery of the brain reward pathways in the exciting environment of the Brain Research Institute founded by Horace Magoun and Donald Lindsley. In 1957 Jim was appointed associate professor of psychology at the University of Michigan and was promoted to full professor in 1959. He remained at the University of Michigan until 1969, when he moved to the California Institute of Technology, where he held the position of Bing professor of behavioral biology. He remained at Cal Tech until his untimely death in 1976.

Jim's professional career is a fascinating story of the growth and development of an extraordinarily creative mind and talented experimental scientist. In his graduate work at Harvard, his mentor was the experimental psychologist Richard Solomon. He also came under the influence of Talcott Parsons, who hired him to edit one of his books. Jim's contribution was so extensive that Parsons made him co-author, and kept up a lifetime relationship to discuss theoretical problems. An example of Jim's theoretical interests is reflected in his early paper on "a neural model for sign-gestalt theory" (*Psychological Review* 61 (1954):59–72).

From his Harvard years and from the profound influence of D. O. Hebb's book *The Organization of Behavior* (New

York: Wiley, 1949) Jim developed a deep and abiding interest in motivation (see Jim's book *The Growth and Structure of Motivation,* Glencoe, Ill.: Free Press, 1956). By the time Jim received his Ph.D., "he was a convinced neuroscientist even if not an expert in all the techniques necessary to carry on research in the field. It was clear to him that psychological theory had to be derived from CNS function, and would constitute as such a realistic foundation for normative behavior. It was thus logical that after he obtained his Ph.D. he sought further training in physiological methods, and to do so in a setting (McGill) in which such an approach was an integral part of the work of Hebb, Jasper, and Penfield" (M. E. Olds, personal communication).

Jim arrived at McGill to work with Donald Hebb, who gave him free reign. The Hebb laboratory was on the second floor of the Doonner Building. Jim received a key to a storage area in the basement where pieces of wood and old equipment were kept. Jim had the impression Hebb would return a few months later to see what he might have discovered.

Jim was also given a McGill undergraduate, Ralph Morrison, as a helper. At that time everyone was interested in the reticular activating system (RAS), and thus Jim elected to record from that system. A simple electrode was made from standard insulated wire and a homemade connector was rigged up. There was much discussion at that time of the motivational bias impinging on the RAS activity; therefore, the first step in the project was to stimulate using the implanted electrode, to determine whether such stimulation was neutral or had positive or negative effects. The implantation of the electrode was done during the week, and on Sunday morning Jim decided to go to the lab to see whether everything was ready for the test to be given Monday morning by him and Morrison. The rat was placed in an open field, the electrode connector was attached, and a train of

60-Hz sine waves was applied for 0.25 seconds using a hand-held button to apply brief electrical shocks. The insight came when Jim noticed that the rat kept returning to the area in the open field where the last shock had been given. The shocks were repeated in that area but not elsewhere in the open field. The upshot was that, upon returning home, Jim announced that he had made a discovery, a real one, one that would not evaporate the next day. The phenomenon was demonstrated on Monday to the members of the laboratory, and in time was followed by testing for the positive effect of brain stimulation in a Skinner operant chamber. P. Milner was at that time a third-or fourth-year graduate student in Hebb's laboratory working on the neural basis of timing in the rat. His contribution to Jim's training was invaluable in terms of showing him the techniques of implantation, stimulation, and recording and, in general, contributing his knowledge of physiological techniques to the training of a postdoc more schooled in the theoretical than the experimental aspects of that field.

The course of this discovery is an extraordinary example of a creative mind seizing on an unexpected and serendipitous observation. In the words of Neal Miller, himself a leading scientist in the field, "His initial and greatest discovery resulted from having the wit to notice and exploit a totally unexpected outcome —an important aspect of science and inadequately understood by the general public or by those legislators who believe that it is efficient to concentrate most research on specific planned programs to attack targeted practical problems" (N. E. Miller. Forward in J. Olds. *Drives and Reinforcements: Behavioral Studies of Hypothalamic Functions*. New York: Raven Press, 1977). The circumstances of the discovery of the brain reward system are vividly described by Jim in his article in *Scientific American* (1956, p. 107–108):

With the help of Hess's technique for probing the brain and Skinner's for measuring motivation, we have been engaged in a series of experiments that began three years ago under the guidance of the psychologist D. O. Hebb at McGill University. At the beginning we planned to explore particularly the mid-brain reticular system—the sleep-control area that had been investigated by Magoun.

Just before we began our own work, H. R. Delgado, W. W. Roberts, and N. E. Miller at Yale University had undertaken a similar study. They had located an area in the lower part of the mid-line system where stimulation caused the animal to avoid the behavior that provoked the electrical stimulus. We wished to investigate positive as well as negative effects (that is, to learn whether stimulation of some areas might be sought rather than avoided by the animal).

We were not at first concerned to hit very specific points in the brain, and, in fact, in our early tests the electrodes did not always go to the particular areas in the mid-line system at which they were aimed. Our lack of aim turned out to be a fortunate happening for us. In one animal the electrode missed its target and landed not in the mid-brain reticular system but in a nerve pathway from the rhinencephalon. This led to an unexpected discovery.

In the test experiment we were using, the animal was placed in a large box with corners labeled A, B, C, and D. Whenever the animal went to corner A, its brain was given a mild electric shock by the experimenter. When the test was performed on the animal with the electrode in the rhinencephalic nerve, it kept returning to corner A. After several such returns on the first day, it finally went to a different place and fell asleep. The next day, however, it seemed even more interested in corner A.

At this point we assumed that the stimulus must provoke curiosity; we did not yet think of it as a reward. Further experimentation on the same animal soon indicated, to our surprise, that its response to the stimulus was more than curiosity. On the second day, after the animal had acquired the habit of returning to corner A to be stimulated, we began trying to draw it away to corner B, giving it an electric shock whenever it took a step in that direction. Within a matter of five minutes the animal was in corner B. After this the animal could be directed to almost any spot in the box at the will of the experimenter. Every step in the right direction was paid with a small shock; on arrival at the appointed place the animal received a longer series of shocks.

Next the animal was put on a T-shaped platform and stimulated if it

> turned right at the crossing of the T but not if it turned left. It soon learned to turn right every time. At this point we reversed the procedure, and the animal had to turn left in order to get a shock. With some guidance from the experimenter it eventually switched from the right to the left. We followed up with a test of the animal's response when it was hungry. Food was withheld for 24 hours. Then the animal was placed in a T, both arms of which were baited with mash. The animal would receive the electric stimulus at a point halfway down the right arm. It learned to go there, and it always stopped at this point, never going to the food at all!
>
> After confirming this powerful effect of stimulation of brain areas by experiments with a series of animals, we set out to map the places in the brain where such an effect could be obtained. We wanted to measure the strength of the effect in each place. Here Skinner's technique provided the means. By putting the animal in the "do-it-yourself" situation (i.e., pressing a lever to stimulate its own brain) we could translate the animal's strength of "desire" into response frequency, which can be seen and measured.
>
> The first animal in the Skinner box ended all doubts in our minds that electric stimulation applied to some parts of the brain could indeed provide a reward for behavior. The test displayed the phenomenon in bold relief where anyone who wanted to look could see it. Left to itself in the apparatus, the animal (after about two to five minutes of learning) stimulated its own brain regularly about once very five seconds, taking a stimulus of a second or so every time. After thirty minutes the experimenter turned off the current, so that the animal's pressing of the lever no longer stimulated the brain. Under these conditions the animal pressed it about seven times and went to sleep. We found that the test was repeatable as often as we cared to apply it. When the current was turned on and the animal was given one shock as an hors d'ouevre it would begin stimulating its brain again. When the electricity was turned off, it would try a few times and then go to sleep.

The discovery of the brain reward system led to an explosion of research in the field and for a period of years it was the most widely studied topic in physiological psychology. Other investigators attacked Olds's basic notion of a reward system on every conceivable ground, a not uncommon phenomenon in science when a major discovery has been made. The best work in the field continued to be done by Olds and associates.

In the initial observations by Olds and Milner (1954) the septal area appeared to be the region of greatest reward value. However, Olds (1962) completed a detailed mapping study of the reward value of various regions of the brain. Reward value could be determined by the rate at which the rat delivered shocks to its brain. In certain regions of the hypothalamus, for example, the animal would self-stimulate at a rate of 2,000 responses per hour (1958). The mapping study identified the general region of the medial forebrain bundle and lateral hypothalamus as the most reliable regions.

One objection to Olds's notion was that self-stimulation was simply a "forced motor" seizure. Olds (1956) showed that rats will learn mazes to obtain electrical brain stimulation in a manner essentially identical to hungry rats learning the same maze for food reward. Another objection was that the brain shock was simply activating a "feeding center" (i.e., that self-stimulation activated natural reward systems in the brain). In a most important study Olds, Allan, and Briese (1971) introduced the use of the microelectrode to stimulate very localized regions of brain tissue. They examined self-stimulation and electrical feeding and drinking behavior. Results indicated that these behaviors could in fact be differentially elicited. Stimulation of an anterior region of the hypothalamus elicited only drinking. Eating alone was elicited by stimulation of the more dorsal portion of the middle lateral region of the hypothalamus. Electrical self-stimulation alone was obtained from a fairly wide lateral region occupied by the medial forebrain bundle. Stimulation of the ventromedial nucleus (the "satiety" center) tended to inhibit or disrupt eating and did not elicit self-stimulation. However, stimulation in the middle lateral region of the hypothalamus produced mixed effects. In short a partially separable reward system did appear to exist in the brain.

When Jim moved to Cal Tech in 1969 a major focus of his work became brain substrates of learning and memory. He pioneered methods of single unit recording in the behaving animal (rat). At the time, movement artifact was a very serious problem in such studies. For Jim, one of the advantages of Cal Tech was the superb engineering talent. As John Disterhoft describes it:

> I recall his excitement when, in collaboration with one of the electrical engineers from the Jet Propulsion Laboratory, he designed what must have been one of the earliest telemetry systems for multiple single unit recording. The idea was to transmit signals from ten microwire electrodes simultaneously without danger of cable artifacts. The rat looked a little ungainly with the miniature transmitter on his head, but the system worked pretty well. Jim was always trying to come up with a better operational amplifier. . . . He also got involved in troubleshooting things like electronic waveform identifiers—he always wanted ours to work better, to be simpler and more state-of-the-art (J. F. Disterhoft, personal communication).

In addition to pioneering electronic methods to obtain movement artifact-free recordings, Jim also approached the problem from the other side with typical ingenuity. He arranged the training situation for the rats—he was using differential discriminations with auditory cues for food reward—such that the rat had to remain motionless when the conditioned stimuli were presented.

In his initial single neuron studies of learning, Olds and his associates recorded from a variety of brain regions, including the hippocampus, reticular formation, and midbrain (see, e.g., Mink, Best, and Olds, 1967; Phillips and Olds, 1969; Olds, Mink, and Best, 1969; Hirano, Best, and Olds, 1970; Olds, Disterhoft, Segal; Kornblith, and Hirsh, 1972; Segal, Disterhoft, and Olds, 1972; Segal and Olds, 1972; Segal and Olds, 1973; Kornblith and Olds, 1973). These were pioneering studies showing learning-related changes in neuronal activity in a number of brain regions. I believe these studies were the first to show clear learning-related changes in patterns of neuronal discharge in the hippocampus, as well as in other brain structures.

I will give an example of an extremely insightful analysis of unit activity in the midbrain (ventral tegmentum and reticular formation) during classical conditioning (Brauth and Olds, 1977). The procedure involved pairing one frequency of tone (CS) with rewarding brain stimulation (UCS). Results indicated that only neurons that responded to the CS before training showed learning-related changes in response patterns, a striking result. The authors concluded: "This implies that although the behavioral response of the animal arises de novo as a result of learning, only those midbrain units that possess connections to the CS pathway participate in conditioning process. This effect constitutes strong evidence in favor of a model of learning based on the intersection of CS and UCS pathways." This is a remarkably prescient conclusion, which has been strongly supported in recent years.

In this work, Olds and his colleagues wrestled with a fundamental problem, namely, how to distinguish between neurons whose discharge rates are influenced by nonspecific factors like arousal versus learning and how to distinguish between neurons that coded learning and neurons simply influenced by other neurons that coded learning. In brief, how can one localize the sites of memory formation? Olds took the approach of focusing on the shortest latency changes in patterns of neuronal discharge following CS onset (see Olds, Disterhoft, Segal, Kornblith, and Hirsh, 1972). This led to unit studies in the auditory system (e.g., Disterhoft and Olds, 1972).

Special note must be made of Jim's wife Marianne, who collaborated with him on the pharmacological properties of the sites where brain stimulation was rewarding. She had received training in neurophysiology from T. Bullock at UCLA, and had been a postdoc with Edward Domino, a professor of pharmacology at the University of Michigan Medical School working on the function of the acetylcholine transmitter. Their collaboration continued until Jim's untimely death.

Jim had a number of students and postdoctoral fellows who went on to become distinguished neuroscientists themselves. To name a few students: Aryeh Routtenberg, Menahem Segal, Bob Wurtz, Ralph Norgren; to name a few postdocs: Philip Best, John Disterhoft, Michael T. Phillips, T. Hirano, Paul Shinkman. He was a superb mentor.

Jim received a number of honors and awards in his career, beginning with the Newcomb Cleveland Prize from the American Association for the Advancement of Science in 1956. He was awarded the Hofheimer Award from the American Psychiatric Association in 1958; the Howard Crosby Warren Medal from the Society of Experimental Psychologists in 1962; and the Distinguished Scientist Award from the American Psychological Association in 1967. Jim was elected to the National Academy of Sciences at the young age of forty-five in 1967 and was elected president of Division 6 of the American Psychological Association in 1971. In my opinion Jim's discoveries are of such fundamental importance that he merited a Nobel Prize.

I close with personal recollections from people who worked with Jim. Paul Shinkman, now a distinguished professor of psychology at the University of North Carolina, spent a postdoctoral year (1965–66) in Jim's lab at the University of Michigan:

> Jim, as you know, was a small man with bright sparkling eyes and quick, agile gestures and movements. He was also possessed of a keen, finely developed sense of humor. One day in the lab he was telling a few of us about the way he had discovered rewarding brain stimulation 12 years ear

> lier. He delivered the brain stimulus with a hand-held button. On one particular occasion the (newly implanted) rat crept cautiously across the floor of the testing chamber. At this point in telling this story, Jim assumed the role of the rat, moving furtively across the room while continuing the narrative. When the first brief brain stimulation was delivered, the rat stopped abruptly, took two careful steps backwards, and peered up directly at Jim. (Here Jim looked up over his shoulder in a bemused position). "The rat," said Jim, "seemed to say, 'I don't know what I just did, but whatever it was, I want to do it again.' " Jim immediately stopped thinking about elicited behaviors and began on the spot to attempt informal shaping of emitted behaviors (P. G. Shinkman, personal communication).

Philip Best, now a distinguished professor of psychology at Miami University of Ohio, joined the Olds Brain Research Laboratory at the University of Michigan for a postdoctoral fellowship in 1965, following his Ph.D. at Princeton. He states:

> On a typical day, M. Olds would come to the laboratory early in the morning to set up her experiment, and following that, to discuss the status of the projects carried out by one or two technicians. Jim would typically work at home in the morning, and would come in for lunch. They would eat lunch in their office, usually without anyone else present. Occasionally they would ask someone to come in and discuss some particular issue, but they preferred to eat alone. It seemed to be a protected time together. After lunch, Marianne would either leave for the day or return to her work and Jim would do his rounds. His first stop was usually the unit room. He would then typically go to the machine shop or the electronics shop to discuss design changes, and then would visit with the graduate students and postdocs. Afterward he would handle business with the office manager, and then would come back to the unit room to discuss current problems.
>
> To me, he always seemed most intense and eager when discussing both technical and theoretical issues in the unit room. Early on we had many technical problems, and the most frequent topic of discussion was how to solve them. Often the discussions would become very heated. It was very easy to become frustrated by the technical difficulties or defensive if your solution did not work, or if the others rejected it. Yet, as intense as Jim could become, he was the least likely to get hooked into anger or defensiveness. While he was very eager to make progress, he was amazingly

patient and circumspect about the problem or "screw-up" of the day. He seemed to be able to treat each new problem as just another step in the process, and as something that soon would be solved. He showed persistence, and persistent optimism in the face of some pretty horrendous problems, and some pretty cantankerous junior colleagues.

Every week, on Saturday mornings at 10:00 A.M. we had a lab meeting. Usually one of the graduate students or postdocs would present a progress report on their experiments. Making the presentation could be rather stressful, but the mood was usually upbeat, because Jim set the tone of the meeting. He saw it as an opportunity to generate new ideas and to engage in group problem solving behavior. If you were afraid you did not have enough progress to report, the easiest way to get through the meeting was to raise a few hypothetical questions that would get Olds speculating. That was also the situation where he would shine the brightest. At times he would be Socratic, but he could do so without being pedantic or patronizing. The discussion was most fun when he would get off on a tangent, completely unrelated to the topic at hand. As I said before, he had such a fertile mind was so undefensive that everyone risked speculating and criticizing the ideas of others. At around noon, a few impatient wives would call to find out when we would come home.

He loved to play with ideas, and loved to argue over anything. I remember a few occasions when we would be discussing one of his ideas that I thought was particularly groundless. A few minutes later, it would occur to me that I was now defending his idea and he was attacking it. When he recognized my delayed realization, he would start laughing, and say something like "I just wanted to see if I could convince myself that it was as bad an idea as you originally thought," or "I just wanted to see if you could come up with better arguments than me to refute yourself." I never saw him become defensive about his ideas or impatient with the thinking of others, even if they were quite lame speculations. Frequently at the end of one of these arguments, I would marvel at how many good ideas I had, only to realize later that most of my best ideas were indeed his (P. J. Best, personal communication).

John Disterhoft, a distinguished professor of neuroscience at Northwestern University School of Medicine, spent two and a half years (1970–73) as a postdoctoral fellow in Jim's lab at Cal Tech:

Jim was immersed in his work. He loved science, especially as it concerned the brain and how it functioned. If he had one frustration, it was that he had not spent enough of his life immersed in learning facts about the brain. He felt that, the more facts he had stored with which to make associations, the more significant the insights that would be possible from his theoretical speculations. He spent a good bit of time thinking, talking, and writing about how the brain worked.

Jim was also fascinated with computers and electronics. Many of his ideas about brain function (e.g., his speculations about memory storage function in the hippocampus) used computers and their memories as analogies. Our laboratory was well equipped with computers, and a good bit of time was spent on developing and testing software and hardware. The burst of information we were able to gather in a relatively short period of time came from using a combined hardware-software system simultaneously to study a large number of brain regions in animals engaged in learning the same task. This was coordinated strategy, as Jim was well aware of the strengths of the system he had set up.

The portion of the laboratory where I worked was set up with four training stations. Jim always had one assigned to him and carried on a series of experiments separate from those of the postdoctoral fellows and graduate students. He spent a fair amount of time traveling, and so had a technician help him. But when he was in town, he came in every morning to check the rat that was being trained in his station and to check the setting of the waveform discriminators on the unit channels being used. Jim was very demanding about the quality of data he and the people in his group gathered. He was a firm believer that high quality findings came front high quality data. Our system had numerous checks for electronic noise and various other artifacts. He also took an intense interest in the experiments as they were being run. We all lived fairly close to the laboratory The experiments we were running with freely moving rats ran from the evening until early morning, the peak of the rats' diurnal cycle. I often came in during the evening to check on how things were going to discover that Jim had been there shortly before. Almost invariably when I came in early on Sunday (before going to the beach), Jim had already been in the lab and made some notes or adjustments on the computer or the printout. The experiments in which we were involved were truly a joint effort in which he took an active role.

Another thing that I remember vividly and very much enjoyed when I was in the laboratory was the almost daily data meetings. We were gather ing a lot of data and we were all trying to keep on top of it. So we got together in Jim's office every afternoon to discuss our data and what they meant. These meetings often included theoretical discussions that ranged far from the data at hand. They also included discussions of appropriate strategies to use in our ongoing experiments. Jim was intimately involved with me in designing and developing the software routines we used on the mainframe computer for summarizing the data for individual rats and for groups of' animals. He didn't just assign me to go over to the computer center and come back with data reduction routines. He went along during the discussions about how the routines should be set up, plotted, and what kinds of error checking we should incorporate into the programs. This was at a time when the minicomputers (DEC PDP/8s) that ran our experiments were not powerful enough to do the data reduction either during or after the experiment.

Some of my fondest memories of Jim Olds are the personal ones. He was a gracious, urbane person. He had a good sense of humor and often had a smile on his face. I never saw him use his position to intimidate or denigrate those working with or for him. He was pleasant not only to those he considered his academic or scientific peers and trainees, but also to the secretaries and staff at Cal Tech. I am the oldest in a relatively large family, and during my stay in his lab my parents and several of my siblings came to visit. I was always impressed by how Jim made a special effort to make my family members feel welcomed and at ease by taking time to say hello when they came to investigate what I spent my time doing. For example, he spent time one beautiful Pasadena afternoon explaining the appeal of neurophysiology to my father by guessing that my father was a fisherman and comparing hunting for cells to waiting for a fish to bite—not a bad analogy at all (J. F. Disterhoft, personal communication).

THE FOLLOWING RESOURCES were very helpful in writing this biographical memoir: Olds, J. *Drives and Reinforcements: Behavioral Studies of Hypothalamic Functions*. New York: Raven Press, 1977; Thompson, R. F. *Introduction to Physiological Psychology*. New York: Harper & Row, 1975; Thompson, R. F. James Olds: 1922–1976. *American Journal of Psychology*, 92 (1979):151–52; the biography in the files of the Home Secretary of the National Academy of Sciences; personal communications from Philip J. Best, John F. Disterhoft, Marianne E. Olds, and Paul G. Shinkman; and the many publications by James Olds and his associates.

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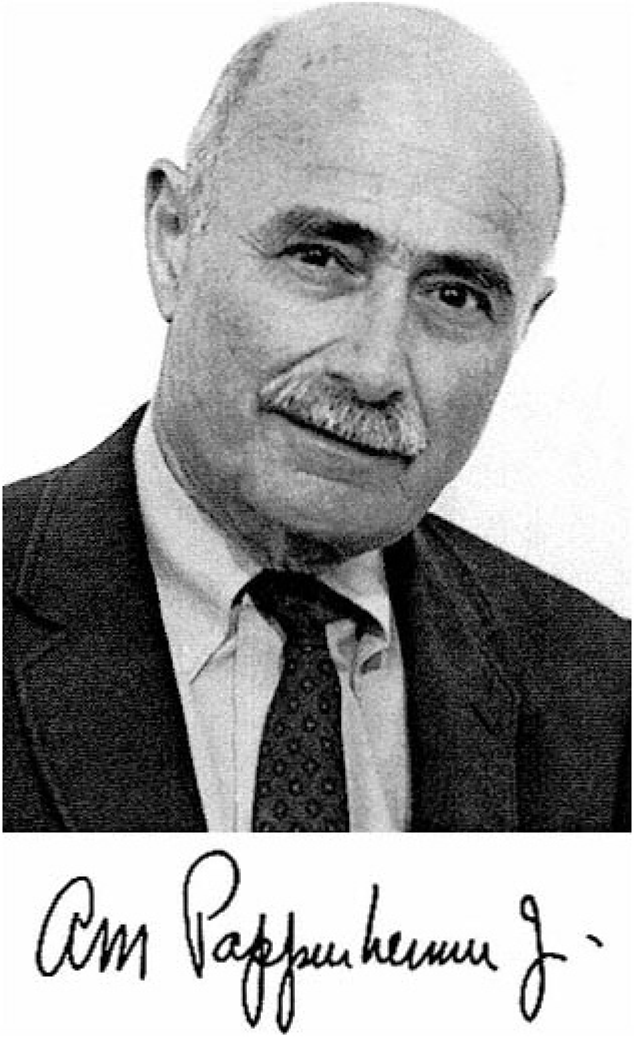
AM Pappenheimer Jr.

ALWIN MAX PAPPENHEIMER, JR.

November 25, 1908–March 21, 1995

BY H. SHERWOOD LAWRENCE

THERE IS ALWAYS A lingering sadness as we remember and sorely miss our dear friend Pap and, although we mourn our loss, it is also a time to celebrate his life and accomplishments. For he experienced an unusually gifted and full life and felt that he was the most fortunate man on Earth, as he often avowed. It was indeed a life studded with high achievement, surrounded by a proud loving family, devoted friends and colleagues, and successive generations of bright, eager, admiring, and appreciative students and fellows. In each he took the greatest pride and with each he maintained a strong bond over the years.

This is the saga of diphtheria toxin, which is the saga of Alwin M. Pappenheimer, Jr. The science itself has such a stunning and artistic symmetry it can only excite pleasure and admiration to behold. And even more so, since the whole pursuit of this idea encompassed Pap's greatest adventure and tells us so much about this unique man. As a young investigator Pap deliberately set himself the daunting task of unraveling the intricate mechanism of an infectious process in precise biochemical terms. Over the years, despite recurrent vicissitudes and occasional detours, he succeeded triumphantly in doing just that. Of the spate of innovative scientific contributions Pap made during his dis tinguished and highly productive career many are ranked as major advances that have illuminated the path to our current understanding of the pathogenesis of infections in general and the unique mode of action of microbial toxins at the molecular level in particular.

Pap, as he encouraged all his friends to call him, was born in Cedarhurst, New York, on November 25, 1908, the eldest child of Dr. Alwin Max Pappenheimer and Beatrice Leo Pappenheimer. His father was a distinguished pathologist on the faculty of the College of Physicians and Surgeons at Columbia University. Pap was raised with his sister Anne and brother John amidst a scholarly, academically oriented, and musically accomplished family environment. He admired his father greatly and sought his advice in many matters. As Pap has stated proudly elsewhere:[2]

> I cannot remember a time, since giving up my early ambition to drive a locomotive or a fire engine, when I did not plan a career in science. This was unquestionably because of the influence of my father and his academic friends . . . All three of my father's children pursued careers in science and all three became professors at Harvard University.
>
> Among my earliest memories are summers spent at Woods Hole before and during World War I. T. H. Morgan, the geneticist, lived nearby and I used to play with his children and later went to the same school with them in New York. I remember Jacques Loeb the physiologist; Gary Calkins, professor of biology at Columbia University; Alfred Redfield, who took us sailing and later became professor of biology at Harvard; and Michael Heidelberger, who lived next door, played the clarinet, and who years later stimulated my early interest in immunology and immunochemistry. There was a summer school at Woods Hole where I learned to catch and mount butterflies and moths and to raise them from caterpillars. Many years later this early interest led to my collaboration with C. M. Williams at Harvard University.

Pap had discovered joy in nature as a lad and it lasted a lifetime. He was happiest when regaling in its bounty—planting and reaping—and in its beauty and variety and even in the pursuit of its derangements to which we give names and call diseases.

When the family moved to Hartsdale in 1919 Pap went to the Lincoln School of Teachers College where he enjoyed learning calculus, physics, and chemistry. In 1925 he entered Harvard as a seven teen-year-old freshman and despite weighing only 99 pounds he coxed the freshman crew, as well as the crew that later defeated Yale in New London. From that time on he remained a dedicated sculler and engaged daily in the sport he loved in all weather until his early eighties, ultimately becoming a member of the Board of Directors of the Cambridge Boat Club.[2]

In 1926 Harvard inaugurated a new tutorial field of concentration (biochemical sciences), and Pap was the first student to enroll in this area, which was his main interest. Prophetically, he was later to return to Harvard in 1958 to succeed John Edsall as chairman of the Board of Tutors in the same biochemical sciences program.

Pap was faced with a dilemma during his senior year at Harvard: whether to go on to graduate school or to medical school, since the latter was the only place that biochemistry was taught at that time. He was intent on preparing for the future in biological research, where he was convinced an intimate knowledge of chemistry and physics were indispensable. For advice, he went to his father, who suggested that he see his friend the biochemist Hans Clarke, who in turn referred him to James B. Conant, professor of organic chemistry at Harvard. Conant readily accepted Pap as a graduate student despite his frank avowal that he was not interested in becoming a chemist but wished to acquire the training to enter biological research![1, 2]

Pap received his Ph.D. in organic chemistry in 1932 in the midst of the Great Depression and held a part-time appointment as instructor and tutor in biochemical sciences, which fortunately was extended for another year of postdoctoral study. For the latter he went to work in Hans Zinsser's laboratory with Hugh Ward and John F. Enders on pneumococcal polysaccharides. The choice of this problem was fortuitous in that, although the results remained unpublished, Pap's significant contribution was brought to the favorable attention of O. T. Avery at the Rockefeller Institute. As we shall see some years later, Avery, who thought very highly of Pap and his work, would recommend to Colin MacLeod that he recruit Pap to the new Department of Bacteriology that he was forming at New York University School of Medicine.

Following his work on the pneumococcal polysaccharides, at Zinsser's suggestion Pap applied for and was awarded a National Research Council Fellowship to work in Sir Henry Dale's laboratory at the National Institute of Medical Research in London. There he spent the next two years attempting to isolate a bacterial growth factor "sporogenes vitamin" in collaboration with B. C. J. G. Knight and Sir Paul Fildes. Although he was able to concentrate a gram of oil with specific activity, he was unable to crystallize it. He took this disappointment philosophically, knowing that he had learned a great deal and had made a lasting coterie of staunch friends.[1]

Pap returned to Cambridge, Massachusetts, in 1935 without a job but happy nonetheless that he had finally chosen a problem he wanted to solve, in his own words, simply "to isolate a pure potent bacterial toxin and to find out what made it so toxic."[1] So the pursuit of this idea was begun. He approached the isolation of diphtheria toxin reasoning that if he provided a simple medium for culture that contained only known low molecular weight nutrients, then the supernatant should contain only those proteins secreted by the bacteria. He discussed this strategy at length with

Howard Mueller of the Department of Bacteriology at Harvard Medical School, who thought it a good idea and helped Pap get a Bradford Fellowship at Harvard to provide him with living expenses, while Elliot Robinson, director of the Jamaica Plains Antitoxin and Vaccine Laboratory, provided space and the technical assistance of Sylvia Johnson to support the project.[1]

It was here that Pap showed that minute quantities of iron added to the cultures of diphtheria bacilli resulted in a significant increase in toxin production, and he was on the first step to his goal. The next step was taken when he produced large quantities of pure diphtheria toxin by this means. This feat was further improved by his subsequent finding that media that supported toxin production best did so after the excess iron present had been precipitated with calcium phosphate and removed from the culture. Of this period in his career Pap wrote:[2]

> As a graduate student and even as a postgraduate fellow, it worried me a great deal that I did not have an important problem in mind. However, with the discovery of the importance of iron in controlling the production of diphtheria toxin, this insecurity left me and I have never since had to worry about finding something to work on. One thing always leads to another.

The wisdom of this philosophy was amply illustrated in the next series of Pap's contributions, which resulted in the crystallization of the toxin; following that, he moved on to establish its purity by excluding contaminating proteins immunologically using the toxin-antitoxin flocculation reaction, which showed the toxin to be at least 95% pure. Pap then teamed up with Williams and Lundgren to determine the molecular weight of the toxin by sedimentation and diffusion in the ultracentrifuge and found it to be homogeneous.[1, 2]

What had all started innocently enough in the Antitoxin and Vaccine Laboratory in Jamaica Plains when Pap iso lated, crystallized, and characterized diphtheria toxin in the late 1930s became a tour de force for which he received the prestigious Eli Lilly award in 1942, along with international recognition for his achievements, since it was the first toxin to be obtained in pure crystalline form.

In 1938 Pap married Pauline Forbes, a gracious, gentle lady who was the light and lodestar of his life and with whom he was to live happily for fifty-seven years. They had three children, Ruth F. P. Brazier, Sarah, and John.

Pap remained at the Antitoxin and Vaccine Laboratory from 1935 to 1939, but, although his experimental work was most successful and rewarding, he wished to be at a university where he could teach as well. Hence, when an offer of an assistant professorship in the Department of Bacteriology at the University of Pennsylvania came, he accepted it; yet, after a two-year stint Pap found the restrictions of his teaching assignments and the limited future this position afforded were not encouraging.[1]

Fortune smiled on Pap once again at this critical time in his career. Colin MacLeod had been appointed chairman of the Department of Bacteriology at New York University School of Medicine in 1941, and was seeking to recruit a faculty with new and broader views of such a department's composition, scope, and mission. As noted earlier, O. T. Avery had come to know and admire Pap through his unpublished work on the pneumococcal polysaccharide, and he recommended that Colin recruit Pap as assistant professor in the department.[2] Needless to say, Pap accepted Colin's offer promptly and happily, because he and Colin shared the same ideas about what a new department of bacteriology should be like and because he was intrigued by the excitement of Colin's work on the pneumococcal transforming principle with Avery and McCarty at the Rockefeller Institute. The only stipulation that Pap had was that he bring his first fellow Alan Bernheimer along with him—an idea Colin was delighted to second.[1]

Pap joined Colin in the challenge of building the new Department of Microbiology. Soon new recruits were in place and new approaches to the boundaries and scope of the investigations, as well as the breadth and depth of the bacteriology curriculum, were enlarged and redefined by men such as Pap and Colin, Efraim Racker, Mark Adams, Royal Christianson, and Alan Bernheimer. Thus, a long and fruitful friendship and collaboration was launched with Colin, who became Pap's hero and his staunchest supporter.

In 1945, after service as an Army captain in the Pacific theater in World War II, Pap was delighted to return to the Department of Microbiology at New York University School of Medicine and to pick up where he had left off in his quest. It was in 1946 that I met Pap and had the privilege of becoming his first postwar fellow—studying the immunological responses of adults to immunization with diphtheria toxoid. Soon Pap's laboratory began to hum with fellows. Mel Cohn, Lane Barksdale, and then Jonathan Uhr, Matthew Scharff, Sam Salvin, and William Kuhns all were studying various phases of the immune response to diphtherial antigens, a task that resulted in a series of seminal contributions covering fundamental aspects of antigen-antibody reactions and mechanisms of delayed-type hypersensitivity. There was also a stream of visiting scientists, such as Ashley Miles, John Humphrey, Jacques Monod, Francois Jacob, and Andre Lwoff, who came to see Pap and discuss their individual scientific problems; on occasion, Monod and Pap would engage in heated debate.

During this period Pap was also back on the trail studying the effect of iron and the iron enzymes on diphtherial growth with Edelmira Hendee; phage-host relationships with Lane Barksdale and the metabolic effects of toxin on the

Cecropia silkworm with Carroll Williams at Harvard. Then in 1957–58 an exciting new approach in Pap's main pursuit to discover what makes diphtheria toxin toxic resulted from his urging Strauss and Hendee in his laboratory at NYU to test whether the toxin would inhibit protein synthesis in HeLa cells in culture. They found that the toxin indeed inhibited protein synthesis in the cells—a seminal finding that led to other findings of great significance that would soon unravel the mode of action of diphtheria toxin.

In the meantime Pap had returned to Harvard. This move came about gradually after Colin MacLeod had been recruited to become professor of research medicine at the University of Pennsylvania in 1954 and Pap was appointed chairman of the Department of Microbiology at NYU in 1956 to succeed him. In 1957 John Edsall invited Pap to accept an appointment as professor of biology and to succeed him as chairman of the Board of Tutors in Biochemical Sciences at Harvard. This was an offer he could not refuse, and yet with characteristic gallantry Pap felt that NYU had been very good to him, and he would not leave until a new chairman was found to replace him. It was not until the autumn of 1958 that Pap and his family moved back to Cambridge, where he succeeded Edsall as chairman of the Board of Tutors in Biochemical Sciences and was appointed professor of biology.[2]

In 1961 another appointment that also meant a great deal to Pap ensued, namely, that of master of Dunster House, a position he held until 1970. A skilled and devoted clarinetist and violist himself, Pap fostered chamber music concerts at Dunster House, an innovation for which it became noted and attracted many talented musicians who were enrolled at Harvard during his tenure.

It was also during this period that pivotal research findings in Pap's laboratory accelerated and the gathering mo mentum yielded results that led ultimately to the denouement of the problem.

In 1963–64 John Collier and Pap showed that the inhibition of protein synthesis caused by diphtheria toxin in intact HeLa cells also occurred in cell-free systems of HeLa cells. Collier then in 1966–67 showed that elongation factor 2 (EF-2) was inactivated by diphtheria toxin in the presence of NAD, and in 1968–69 Michael Gill discovered the ADP-ribosylation reaction by which diphtheria toxin inactivated EF-2. Jack Murphy working with Pap in 1974–75 began studies on the transcriptional regulation of the tox gene, and Patrice Boquet studied the interaction of diphtheria toxin with detergents and cell membranes in 1975–76.

Later in 1978–80 John Collier and his student Gary Gilliand crafted the first generation immuno-toxin from diphtheria toxin independently—at the same time as did Vitetta and Uhr in Dallas and Uchida in Japan. Jack Murphy, as well as Vitetta and Uhr, then went on to design new generations of targeted toxins from diphtheria toxin, some of which are now in clinical trials. Also in 1980 Collier obtained X-ray grade crystals of diphtheria toxin and began a collaboration with David Eisenberg at UCLA that ultimately led to the solution of the 3D structure of the toxin in 1992 by Eisenberg and students in collaboration with Collier's laboratory. In 1983 Collier cloned and sequenced the gene for diphtheria toxin and subsequently in 1984 showed that glutamic acid is a key residue that is conserved in all ADP ribosylating toxins like those of cholera, pseudomonas, and pertussis. In 1992 the receptor for diphtheria toxin was determined by Eidel and coworkers in Jonathan Uhr's department in Dallas.

As I note in the dedication of his commemorative issue of *Cellular Immunology*:[3]

> There is more to Pap's saga than just pure science and the poetic justice of bringing a daunting task to an incisive and clear solution. Along the way Pap was a powerful magnet that attracted generations of students and fellows at NYU and later at Harvard to a career in science. Pap's generosity was boundless; he gave of his talents, his vitality, his ideas, and his inspiration unstintingly. And he had a genius for bringing out the best in a long succession of students and fellows who came under his pervasive spell.

This resulted in the development of such first-rate scientists as Alan Bernheimer, Mel Cohn, Jonathan Uhr, and Matthew Scharff at NYU and John Collier, Ronald Goor, Mike Gill, Jack Murphy, and Patrice Boquet at Harvard. It also sparked in all of us a lifelong admiration and imitation of Pap's broad, imaginative, and rigorous approach to science and his chivalrous knack of gently nudging young investigators into the limelight while launching their careers. A reasonable assessment of Pap's success in this enterprise is that not only was he elected to the National Academy of Sciences (in 1973) but so have four of his former fellows and most recently the student of a former fellow as well, insuring the continuation of Pap's inheritance.

All of us try in our own ways to pass on to our own fellows the best of what Pap passed on to us. This is the hope for the future of Science and its rarest treasure. At Jonathan Uhr's initiative we formed a Society of Pappenheimer Fellows, which met on regular occasions, the most notable of which resulted in the publication of a symposium composed of papers given by Pap's former fellows, which was dedicated as a "Commemorative Issue in Honor of Alwin M. Pappenheimer, Jr.," and was published in *Cellular Immunology*.[3] As the late Lewis Thomas aptly remarked in an appreciation of Pap at that symposium, "Pap had a powerful influence on a great many younger people, setting in place the very course of the whole scientific careers of many of them. But it needs remarking that he had an equally powerful influence on the people in his own generation, faculty colleagues like myself."[4]

An unpublished excerpt of the reply that Pap had intended to deliver at that symposium, which he later sent to me and to Jon Uhr, provides a touching insight to the man and his characteristic modesty, candor, and frankness:

> Nothing like this has ever happened to me before, and I find it impossible to avoid feeling emotional and sentimental. When Jon Uhr asked me to send him a list of students and postdoctoral fellows who had worked in my laboratory over the years, I had no idea that the list would be so long and distinguished. I can honestly say that I have never felt that my own personal contributions as a scientist have been exceptional. It seems to me that my greatest talent has been to have recognized the quality of others and to have had the good fortune to have interested a number of exceptional and very nice people in what I, myself, was interested in. They were able, over and over again, to show me how wrong my own ideas were and to steer me back on the right track.

Numerous other honors and awards have graced Pap's pioneering scientific discoveries, among which are the Eli Lilly Award, the presidency of the American Association of Immunologists, and election to the American Academy of Arts and Sciences and to the National Academy of Sciences. Most recently, together with his former student John Collier, he received the Paul Ehrlich Prize and Gold Medal.

We have some additional insights into Pap's view of his life and work from the following excerpts of his envoi:[1]

> On looking back over my scientific and academic career, I realize how much I owe to good fortune and to chance. I was fortunate from the very beginning in my choice of parents, who not only provided me with an excellent education but encouraged me to be interested in things for their own sake, rather than for what I might gain from them, and finally who instilled in me a sacred regard for the truth. I was fortunate, too, to have received my education and to have become established in my field at a time when the competition was not as severe or as frenzied as it appears to be today.

> I have always enjoyed working in the laboratory and when administrative duties kept me away from the bench for any considerable length of time, I would begin to worry that I might have lost the ability to work with my own hands. Not only is lab work fun (especially when things go well) but it keeps one in closer and more intimate contact with one's students and one's coworkers. It also serves to remind one constantly of how much easier it is to sit in a chair and suggest experiments for others to carry out than it is to go out and do them oneself. It has always been my feeling that research should be fun, and I like to think that even in this day and age important and innovative contributions can still be made by individuals working in small groups. In hindsight, I believe that I have received as much satisfaction from the friendship and contributions made by my students as from any of the honors that may have come my way, and I like to think that I may have had something to do with starting them off on the road to success. If I had to do it all over again, I do not think I would wish my scientific life to be very different.

So, we salute our hero, Pap, illustrious son of a distinguished father, a born leader, a brilliant thinker, and an innovative and resourceful scientist who proved a wellspring for the good of science and humanity.

We all miss the warm presence and sparkling ideas of this unique gentleman of science who won the grateful admiration of us all and who has left so rich a legacy "to ages of children yet unborn who will speak in accents yet unknown."

IN PREPARING THIS MEMOIR I have been greatly assisted by John R. Pappenheimer and by R. John Collier. Additionally, I am indebted to Cambridge University Press for permission to quote from A. M. Pappenheimer, Jr.'s autobiographical paper, *The Story of a Toxic Protein 1888–1992*.[1] I am also most indebted to A. M. Pappenheimer, Jr.'s children Ruth F. P. Brazier, Sarah Pappenheimer, and John Pappenheimer for their permission to quote from unpublished portions of the original manuscript *Autobiographic Memoirs of a Scientific Career* by A. M. Pappenheimer, Jr., which had been submitted for publication to *Protein Science*, but which did not appear in the edited publication, and for permission to quote from a portion of Pappenheimer's unpublished remarks titled "Some Reminiscences

of Happy Bygone Days," which he had intended to deliver at the conclusion of the commemorative symposium.

NOTES

1. A. M. Pappenheimer, Jr. Recollections—The story of a toxic protein, 1888–1992. *Protein Sci.* 2(1993):292–98.

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FREDERICK DOMINIC ROSSINI

July 18, 1899–October 12, 1990

BY ERNEST L. ELIEL

FREDERICK D. ROSSINI was one of the preeminent thermodynamicists of the twentieth century. The thermochemical data he produced and catalogued continue to be invaluable not only in fundamental science (for example in conformational analysis and molecular mechanics) but also in applied science, notably in the petroleum industry, which, through the American Petroleum Institute, funded much of Rossini's research in this area.

Fred Rossini was born in Monongahela, Pennsylvania, in 1899, the oldest of six children of Martino Rossini and Constanza Carrera, both immigrants from Italy. His father died when Fred was eight, and the need to help support his family forced Fred to drop out of high school at sixteen to go to work as a hardware clerk, work he continued when his mother died three years later. Fortunately the principal of the high school (H. W. Crane, whom he had previously got to know as a football coach) provided him with a special program to finish high school with honors. At twenty-one Rossini entered Carnegie Institute of Technology (now Carnegie-Mellon University) in Pittsburgh, and soon was awarded a full-time teaching scholarship. He graduated with a B.S. in chemical engineering in 1925, followed by an M.S. degree in science (physical chemistry) in 1926. As a result of reading Lewis and Randall's classical text *Thermodynamics and the Free Energy of Chemical Substances* he wrote to Gilbert Lewis; an offer of a teaching fellowship at the University of California at Berkeley resulted. Among his teachers were Gilbert Lewis and William Giauque (both NAS members; Giauque received the Nobel Prize in chemistry in 1932). Rossini's doctoral dissertation on the heat capacities of strong electrolytes in aqueous solution was supervised by Professor Merle Randall. His Ph.D. degree was awarded in 1928, after only 21 months of graduate work, even though he continued to serve as a teaching fellow throughout this entire period.

Through his teaching supervisor W. C. Blasdale, Rossini was recommended to Frederick G. Cottrell (NAS member famed for the Cottrell precipitator and for endowing Research Corporation), who in turn passed on the recommendation to Edward W. Washburn (also an NAS member), who offered a civil service appointment to Rossini in the National Bureau of Standards (now National Institute of Science and Technology). He began work at the National Bureau of Standards in September 1928 in the field of thermochemistry under Washburn (at a salary of $2,600 per year). Rossini stayed at the bureau for 22 years, and a substantial part of his most important research was undertaken there.

AT THE NATIONAL BUREAU OF STANDARDS (1928–50)

In the mid-1920s Washburn, as editor-in-chief of the *International Critical Tables*, had become aware of a lack of reliable enthalpies of formation of many simple chemical compounds. Rossini stepped into the breach; his more than 20 publications between 1928 and 1935, all sole-authored, deal with such fundamental quantities as the heat of formation and of ionization of water, the heat of combustion of methane, carbon monoxide, methanol, ethanol, ethane, propane, butane, and pentane, as well as the heat of formation of hydrogen chloride and the thermodynamic properties of certain electrolytes in aqueous solution. All of this work was published in the *Journal of Research of the National Bureau of Standards* and was reviewed periodically by Rossini in *Annual Survey of American Chemistry*. That this work has stood the test of time and is still fundamental to present-day thermochemistry is a token of the extreme care Rossini took not only in the calorimetric measurements but most especially in the separation and purification of the compounds in question. (In a lecture delivered in 1975, he gives credit for part of the calorimetric work to both the Heat Division and the Electrical Division of the National Bureau of Standards, which had a number of excellent scientists knowledgeable in the areas of calorimetry, thermometry, and maintenance of standard cells and standard resistances for the accurate measurement of electrical power.) In this period Rossini, in collaboration with M. Frandsen and Washburn, also developed the basis for the "Washburn" correction for bomb-calorimetric results, invaluable in later calorimetry work. In 1936, in coauthorship with F. R. Bichowski, Rossini published an extensive selfconsistent table of "best" values for the heats of formation of many of the substances by then studied.

About his early years at the National Bureau of Standards Rossini said, "I shared Washburn's liking for thermodynamics and thermochemistry, for experimental measurements of high precision and accuracy, and for a logical order of arrangement of things." The precision and accuracy of the values he placed in the literature is certainly legendary; some specific examples will be given later. His work reflected his personality; he was thorough, painstaking, hardworking, purposeful, and focused.

In 1932 Fred Rossini married Anne Kathryn Landgraff.

Their son Frederick Anthony was born in 1939.

Edward Washburn died in 1934 and Rossini was asked to succeed him as director of the American Petroleum Institute Research Project 6 on hydrocarbons in petroleum. He held this position at the National Bureau of Standards for 16 years, while he perfected calorimetric measurement and, with the help of a growing group of dedicated coworkers, determined the heats of combustion of a total of 118 compounds, including all alkane isomers through C_8, as well as some nonanes, a number of alkylbenzenes, alkylcyclohexanes and alkylcyclopentanes, ethene, propene, cyclopropane, cyclooctatetraene, carbon, benzoic acid, and deuterium. (The methods used for the extensive purification of these compounds are described in his 1953 book with Mair and Streiff.) In 1936 Rossini also took on the position as chief of the Section on Thermochemistry and Hydrocarbons at the National Bureau of Standards.

In the late 1930s Kenneth Pitzer (later an NAS member) published seminal work on the statistical calculation of entropies of hydrocarbons (which gave rise to his renowned paper on the rotational barrier in ethane: J. D. Kemp and K. S. Pitzer, *J. Chem. Phys.* 1936, 4, 749). Rossini established contact with Pitzer around 1940 realizing that the combination of his thermochemical with Pitzer's entropy data would allow a determination of the standard Gibbs free energy of hydrocarbons and their isomerization equilibrium constants over a wide temperature range. The first paper on the free energies and isomerization equilibrium constants of the butanes, pentanes, hexanes, and heptanes appeared in 1941, and in 1947 Rossini and Pitzer jointly published a seminal paper on the configuration and thermodynamic properties of the dimethylcyclohexanes based in part on Rossini's amazingly accurate thermochemical data. (The heats of combustion of the 1,3-dimethylcyclohexanes are of the order of

5,200 kJ/mol. The difference between the *cis* and *trans* isomers is 7.2 kJ/mol. The presently accepted conformational enthalpy [i.e., axial-equatorial enthalpy difference] of a methyl group in methylcyclohexane based on numerous studies, which should in principle be equal to the above difference, is 7.1 ± 0.1 kJ/mol. Rossini's experiment has been likened to the accurate determination of the weight of a captain of a boat by first weighing the boat with the captain, then weighing it without the captain and taking the difference!)

The ability to determine complete thermodynamic properties of hydrocarbons (and other compounds) prompted Rossini to launch the American Petroleum Institute Research Project 44 on properties of hydrocarbons and related compounds in 1942. A unit of that project was also established at the University of California at Berkeley under the associate directorship of K. S. Pitzer. The initial results of this project, involving 483 data sheets relating to physical and thermodynamic properties, were summarized in 1947 in National Bureau of Standards Circular 461, *Selected Values of Properties of Hydrocarbons and Related Compounds* . A later, more extensive version was published in 1953.

During World War II the efforts of Rossini's group were directed to the analysis of aviation fuels. Beginning in 1944, the American Petroleum Institute Project 6 began to make available highly pure hydrocarbons (of certified purity) for spectral and physical and thermodynamic measurements outside of the National Bureau of Standards. Rossini served on the U.S. Petroleum War Council, in the U.S. Office of Rubber Reserve, in the U.S. Office of Scientific Research and Development, and on the U.S. Atomic Energy Program.

Rossini had been interested in teaching ever since holding a teaching assistantship at Carnegie Tech and later at Berkeley. Shortly after arriving at the National Bureau of

Standards, he began teaching a graduate course in chemical thermodynamics in the bureau's after-hours graduate school. In addition he taught thermodynamics at Howard University in 1937–38 and lectured on petroleum chemistry at the Catholic University of America in 1942. These endeavors eventually gave rise to his 1950 text *Chemical Thermodynamics*. The course at the bureau also gave Rossini an opportunity to see the younger members of the staff there in action; several of his coworkers were recruited from this group. It is obvious that the thermochemical work required a high degree of discipline. A former collaborator of Rossini's has characterized him as "kind, strict, and meticulous." He expected the work to be done by the high standards he set, but he was generous to his staff and treated them well. He was clearly a very effective organizer.

During his sojourn at the National Bureau of Standards Rossini received the Hillebrand Award from the Chemical Society of Washington in 1934 and the Gold Medal Exceptional Service Award of the U.S. Department of Commerce in 1950. In 1946 he was elected president of the Standing Committee on Thermochemistry of the International Union of Pure and Applied Chemistry, a position he held for 15 years although the name was changed in 1953 to the Commission on Chemical Thermodynamics and in 1961 to the Commission on Thermodynamics and Thermochemistry, of which Rossini continued as a member until 1973. In 1948 Rossini received an honorary D.Sc. degree from his alma mater, the Carnegie Institute of Technology, and in 1951, shortly after his departure from the National Bureau of Standards, he was elected to the National Academy of Sciences.

AT THE CARNEGIE INSTITUTE OF TECHNOLOGY (1950–60)

In 1950 Rossini was offered the position of Silliman professor and head of the Department of Chemistry at Carnegie

Institute of Technology in Pittsburgh by President John E. ("Jake") Warner (later an NAS member), who knew Rossini well both personally and professionally. Simultaneously Rossini became director of the Chemical and Petroleum Research Laboratory at Carnegie Tech and moved American Petroleum Institute Projects 6 and 44 to Pittsburgh. Beveridge J. Mair became associate director of Project 6. At this time Rossini received additional support from the newly formed National Science Foundation (NSF), and both Assistant Professor C. C. Browne and a total of seven graduate students and one postdoctoral associate became involved in Projects 6 and 44, in addition to the professional staff. Project 6 reached its twenty-fifth anniversary in 1952, and in 1953 Rossini, B. J. Mair, and A. J. Streiff published *Hydrocarbons from Petroleum: The Fractionation, Analysis, Isolation, Purification and Properties of Petroleum Hydrocarbons: An Account of the Work of American Petroleum Research Project 6*, which constitutes an extensive account of the preparation of pure samples of petroleum constituents by a wide variety of techniques. In the 10 years at Carnegie, a total of 87 additional compounds were studied, including a number of alkenes, cycloalkanes and-alkenes, naphthalenes and decalins, alkanols, and several perdeuterated hydrocarbons. From these measurements much fundamental information was derived: the enthalpy of *cis-trans* isomerization in non-terminal alkenes, the effect of substituents on the heat content of 1-, 1,1- and 1,2-substituted alkenes, the difference in enthalpy between alkylcycloalkenes and alkylidenecycloalkanes, and the difference in enthalpy between *cis*-and *trans*-decahydronaphthalene (decalin) and between *cis*-and *trans*-hydrindane. These measurements (and the earlier ones on the dimethylcyclohexanes) were of great interest to organic chemists and continue to be cited in standard textbooks of stereochemistry.

During this period Rossini became chairman of the Divi sion of Chemistry and Chemical Technology of the National Research Council (1955–58); he was a member of the Committee on Physical Chemistry of that division from 1948 until 1962.

When Rossini left Carnegie Tech in 1960, he entrusted American Petroleum Institute Research Project 44 to Bruno J. Zwolinski, who had been assistant director of the project since 1957. In 1961 Zwolinski moved to Texas A&M University, taking the project with him.

AT THE UNIVERSITY OF NOTRE DAME (1960-71)

Rossini was a devout Catholic all his life, and this was probably an important factor in his move to the University of Notre Dame in 1960 as dean of the College of Science there. The head of the chemistry department G. F. D'Alelio had resigned the previous year and the department was rudderless. Realizing that chemistry was one of the better departments in the college but needed help, Rossini additionally assumed the position of acting head, which he filled for over three years. (I followed him as head in 1964.) The combination of these administrative responsibilities obligated Rossini to suspend his beloved thermochemical research for the 11 years he stayed at Notre Dame.

In his new positions, Rossini proceeded to tackle several lingering problems involving budgets, inadequate salaries, and lack of organization. Having just taken on the deanship, being the only member of the National Academy of Sciences at Notre Dame, and arriving just when Notre Dame's President, the Reverend Theodore Hesburgh, was appointed to his second term on the National Science Board, Rossini clearly had the persuasiveness that comes with a professional honeymoon. The status of the department and its faculty improved markedly. In 1966 the college received a Center of Excellence grant from the National Science

Foundation, which allowed addition of faculty and some facilities. (No funding was found for the much-needed new chemistry building, which was to be the University's contribution to the grant. It was only built in 1980–82 according to the general plans that my colleague Rudolph Bottei and I had developed in the mid-1960s.) On the other hand, the Radiation Laboratory under Professor Milton Burton as director, which had existed since 1946, first with funding from the Office of Naval Research and later with generous support of the Atomic Energy Commission, did move into a major new building in 1966.

In 1965 Rossini received the Laetare Medal, Notre Dame's highest honor, awarded annually to an outstanding American Catholic lay person; Rossini was the first Notre Dame faculty member to receive the award in the 83 years of the medal's existence. The award of the medal reflected not just Rossini's scientific and organizational ability but also his deep moral and religious values.

I must mention here that the qualities that had been so essential in Rossini's scientific work—attention to detail and firm control of all operations—were not always appreciated by some of the heads of the science departments, who were used to a substantial degree of independence in their operations. (One of the indications of Rossini's desire to keep control of details were the very frequent memoranda he sent to all the department heads and sometimes to the entire college faculty, usually signed "FDR.") My own relations with Rossini, though rather formal, were generally cordial, and he left me to run the chemistry department without interference, except that the budgets of the science departments, including salaries, were almost entirely set by the dean in those days. He was, however, a strong-willed individual and once he had made up his mind on something, he was not to be contradicted—as I found out to my detri ment on one occasion when I tried to bring the problem of the lagging new chemistry building directly to the attention of the vice-president for academic affairs.

Despite his responsibilities as dean and his many other professional involvements, Rossini was anxious for me to give him a teaching assignment. I would have been happy to have him teach a graduate course, but he insisted on taking on undergraduate physical chemistry. (As mentioned earlier, he had written an extensive thermodynamics text and this presumably was to be the basis of his course.) I knew he was frequently away from the campus, giving lectures or participating in national committee meetings, and I was concerned about the course being taught on schedule. We finally solved the problem by my assigning another faculty member as a backup.

During his stay at Notre Dame, Rossini was asked by the National Academy of Sciences to chair a scientific mission to Romania, at that time probably the most dictatorial Iron Curtain country save the Soviet Union itself. Nonetheless, Rossini managed to negotiate a bilateral agreement of the type the Academy has with many foreign academies; he subsequently became a member (1966–71) and chairman (1970–71) of the Academy's Advisory Committee, U.S.S.R. and Eastern Europe. One of the advantages the Notre Dame chemistry department derived from this activity was that we were able to invite Romania's most renowned chemist Costin D. Nenitzescu and his wife to spend several days at Notre Dame.

At about yearly intervals Rossini held receptions at his home for the faculty of the college, which gave us an opportunity to meet his wife. Anne Rossini was a very gentle and amiable person, but even then, especially toward the end of the Rossinis' stay at Notre Dame, she struck us as rather frail. Fred was always very solicitous of her; despite his formal demeanor he was fundamentally a kind and considerate individual. Perhaps one example is in order here: While I was head of the chemistry department, one of the full professors in the department suffered a suicidal depression, had to be hospitalized, and was clearly unable to fulfill his course assignment. I proposed to the dean that we put him on paid sick leave for the statutory maximum of six months. Rossini pointed out that, if our sick colleague were not ready to return to teaching at the end of that period, I would have to put him on an indefinite unpaid leave, which he clearly could not afford. Instead, Rossini made it possible to keep our colleague on the regular payroll but not to give him a teaching assignment. He did this by providing funds from the dean's budget to compensate the Radiation Laboratory for releasing one of their senior research associates to teach the course.

In connection with his service in the Division of Chemistry and Chemical Technology of the National Research Council, Rossini became a member of the Executive Committee of the Office of Critical Tables in 1957. He continued on the committee until 1969 (when the office ceased functioning) and from 1965 to 1969 was its chairman. It may have been the intent at that time to revise (or redo) the *International Critical Tables*, but this job would have been beyond the available resources. Instead, in 1966, the activity led to the organization of a Committee on Data for Science and Technology (CODATA), of the International Council of Scientific Unions. Rossini was the first president of CODATA (1966–70); the original organizing groups came from France, Germany, Japan, the United Kingdom, the U.S.S.R., and the United States. Rossini's international contacts, his vast knowledge of the data problem and his organizational ability must have helped in the establishment of CODATA, as did the interest and assistance of the foreign secretary of the Academy, Harrison Brown. The CODATA office moved from Washington to Frankfurt, Germany, in 1968 and later to Paris, where the organization is headquartered today.

In 1967 Rossini left the deanship of the college to become vice-president of research at Notre Dame, a newly created position that he held until 1971, when he reached the mandatory retirement age of seventy-two. That same year (1971) he received the Priestley Medal, the highest distinction conferred by the American Chemical Society. In his Priestley address "Chemical Thermodynamics in the Real Word" he made a clever comparison of the counterplay of enthalpy and entropy in thermodynamics with that of security vis-à-vis freedom in the world at large.

AT RICE UNIVERSITY (1971–78)

After 11 years as an administrator, Rossini was anxious to resume the research he had to stop in 1960. Fortunately, Norman Hackerman (NAS), president of Rice University, offered Rossini a part-time appointment, which he held until 1975; he stayed on at Rice as professor emeritus until 1978. He taught a course in physical chemistry for biology majors and, in laboratory space provided by Professor John L. Margrave and with a grant from the Robert A. Welch Foundation, Rossini resumed his thermochemical work in collaboration with undergraduate scholars and postdoctoral associates. In this period he determined the heats of combustion of several C_{12} and C_{14} hydrocarbons as well as several azo compounds. He was also a supportive elder statesman and consultant to the younger faculty. In addition, in 1973 he was Strosacker visiting professor of science at Baldwin-Wallace College in Berea, Ohio. In the same year the Commission on Thermodynamics and Thermochemistry—in recognition of Rossini's service on the commission and its precursors for nearly 40 years—established a Rossini lec ture, and Rossini gave the first lecture at the International Conference on Chemical Thermodynamics in Montpellier, France, in 1975. In 1977, while still at Rice, he received the National Medal of Science for his "contributions to basic reference knowledge in chemical thermodynamics."

THE LATE YEARS (1978–90)

Anne Rossini's infirmity worsened in the late 1970s, and in 1978 the Rossinis moved to Fort Lauderdale, Florida, to be close to Anne's brother and sister-in-law. They had planned to proceed to a retirement community in Juno Beach, but before this move was realized, Fred damaged his spine trying to help Anne, and he realized she had to be cared for in a nursing home, where she died on December 18, 1981. Fred did move to Juno Beach in late 1982. There he met Dorothy Thompson Purcell, a retired AT&T executive, who became his second wife in 1983. Rossini kept up his scientific interests and introduced his new wife to his colleagues at the National Academy of Sciences meeting in 1985. In 1986 he had a bad fall that seriously impaired his health, as did a developing case of glaucoma. He became legally blind in 1988. In 1990 he contracted pneumonia and died peacefully on October 12. He will long be remembered for the enormous volume of broadly useful thermochemical data that he assembled, put on permanent record, and, in important part, generated.

HONORS AND AWARDS

A few of Rossini's honors and awards, including the Laetare and Priestley medals and National Medal of Science, have already been mentioned. In 1965 he received the John Price Wetherill Medal of the Franklin Institute and a year later the William H. Nichols Medal of the New York Section of the American Chemical Society. The United Kingdom's In stitute of Petroleum honored him with the Redwood Medal in 1972, and the Deutsche Gesellschaft für Mineralölwissenschaft und Kohlechemie conferred the Carl Engler Medal on him in 1976. In addition to the honorary D.Sc. from Carnegie Tech he was awarded honorary degrees by Duquesne University (1955), University of Notre Dame (1959), Loyola University of Chicago (1960), St. Francis College, Loretto, Pa. (1962), University of Portland (1965), and the University of Lund (Sweden) in 1974.

Besides being a member of the National Academy of Sciences Rossini was a member of the American Academy of Arts and Sciences and of the Philosophical Society of Washington and a fellow of the American Association for the Advancement of Science, American Institute of Chemists, American Physical Society, Franklin Institute, and the Washington Academy of Sciences—in addition to holding membership in a number of other organizations, including honorary membership in Phi Lambda Upsilon.

PROFESSIONAL SERVICE

The amount of professional service provided by Rossini was so prodigious that it must be enumerated here, at least in part. His diligence, thoroughness, and strength of personality made him a leader; he was not only a member of many organizations but frequently was called on to chair them. In some instances he did this to further the cause of thermochemistry and thermodynamics—subjects not generally fashionable during much of his lifetime—but in other instances he seems to have been motivated by a pure sense of duty.

He was a member of Council of the American Association for the Advancement of Science (1963–66); and chairman of the Washington Section (1950), of the Petroleum Chemistry Division (1954) and member of the Council

(1941–42, 1947–50, and 1957–60) of the American Chemical Society; in the latter capacity he also served as chairman of the Committee on Constitution and Bylaws (1949–50). He was also a member of the American Chemical Society's Petroleum Research Fund Advisory Board (1966–68); *Chemical & Engineering News* Advisory Board (1966–68); Editorial Board of the *Journal of the American Chemical Society* (1946–56), and of the Gibbs Medal Award Jury, Chicago Section (1965–73). He was president of Sigma Xi (1963–64), Washington Academy of Sciences (1948), Catholic Commission on Intellectual and Cultural Affairs (1958–59), and the Albertus Magnus Guild (1961–65).

While at Carnegie Tech Rossini also served as director (1955–60) of the Manufacturing Chemists Association's (now Chemical Manufacturers Association) Research Project "Data on Chemical Compounds." He chaired the Gordon Conference on Petroleum Chemistry in 1944 and co-chaired the "Conference on Geochemistry, Origin of Petroleum" in 1963. He was chairman of the Division of Chemistry and Chemical Technology of the National Research Council (1955–58) and chairman of its Advisory Board for Numerical Data (1969–70). He chaired the U.S. National Committee for the International Council of Scientific Union's Committee on Data for Science and Technology (1966–70) and the NRC's Committee on Climatic Impact (1974–75), in addition to being a member of other NRC committees. His service with the International Union of Pure and Applied Chemistry has already been mentioned. He was president of Associated Midwest Universities (1967–68), vice-president of the Argonne Universities Association and chairman of its Committee on Environmental Studies (1968–71), president of the World Petroleum Congress (1967–75), and chairman of the Advisory Board of the *Journal of Thermodynamics*, which he helped found (1968–75).

Other organizations on whose councils, committees, panels, or missions he served were the U.S. Army Ordnance Corps, the United States-Japan Cooperative Science Program, the U.S. Department of State, the Inter-University Committee on Travel Grants, the National Science Foundation, Argonne National Laboratory, St. Bonaventure University (trustee, 1968–71), State of Indiana Educational Services Foundation, State of Illinois Board of Higher Education, International Research and Exchanges Board, the Houston (Texas) Chamber of Commerce, and the Environmental Protection Agency.

MUCH OF THE INFORMATION in this memoir comes from Rossini's own writings: His Rossini lecture in *J. Chem. Thermodynamics*, 1976, 8, 803–34 and the preface thereto by S. Sunner, L. McGlashan, and E. F. Westrum, Jr.; his description of the CODATA history in CODATA *Newsletter* No. 38, 1986, 2–3 and his autobiography dated June 1978 in the files of the National Academy of Sciences. Obituaries in *J. Chem. Thermodynamics* , 1991, 23, 521–22 and the *CODATA Newsletter* were also helpful, as were entries in standard reference compendia.

Professor John L. Margrave (NAS) kindly made available press releases from Rice University; Dean Francis J. Castellino arranged for press releases to be forwarded from the University of Notre Dame; and Dr. Johanna Levelt-Sengers (NAS) provided information from the archives of the National Bureau of Standards.

I am greatly indebted to the late Professor Kenneth S. Pitzer (NAS) for providing much helpful detail, including correspondence with Rossini from 1940 and the late 1980s, and I regret only that Pitzer's death precluded his coauthoring this memoir.

Very useful telephonic and written information was obtained from Dr. David Lide (National Bureau of Standards, retired), Dr. Frederick Anthony Rossini, and Dean Francis J. Castellino (University of Notre Dame). Personal or telephonic conversations with Professor Robert Parr, Dorothy Rossini, Dr. John Jost, Professor Arnold Ross, Professor Robert B. Carlin, Dr. Alphonse Forziati, and the late Anton Streiff also helped to enliven this report.

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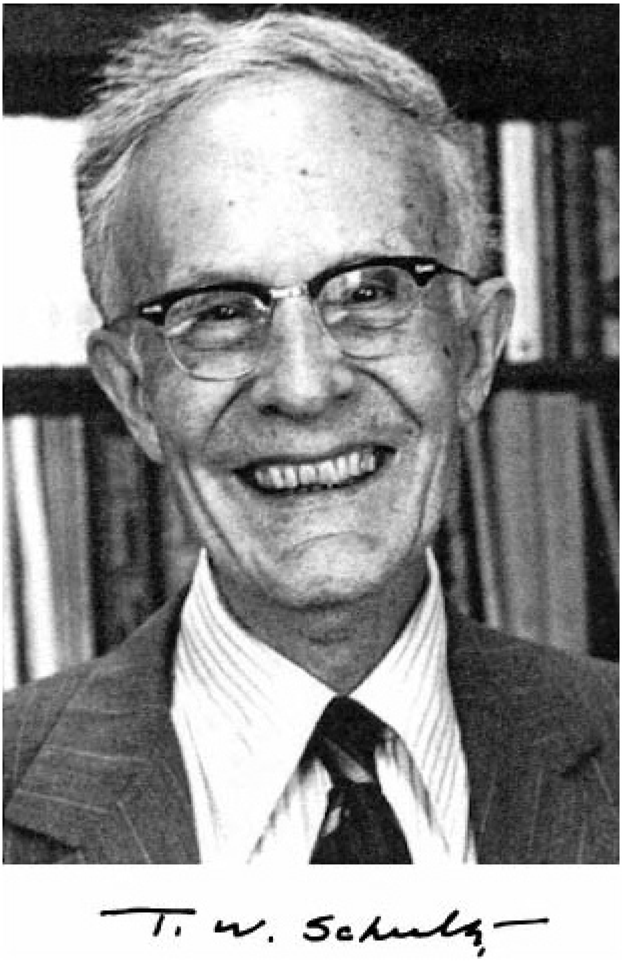

Photograph by Patricia Evans

THEODORE WILLIAM SCHULTZ

April 30, 1902–February 25, 1998

BY D. GALE JOHNSON

THEODORE WILLIAM SCHULTZ was an outstanding innovator in the development of economics, a teacher who had a remarkable impact on hundreds of students, a highly successful academic administrator, and a keen observer of the world in which he lived. I know of no one who learned more from direct observation than he did. Whenever he had the opportunity, he went to the field, so to speak, to see how real people addressed their problems. While he always cherished the structure of economic analysis as it existed, he wanted that structure to help him understand what went on in the world. If it didn't, he thought that the structure or the implications that were commonly attributed to it should be revised. As will be noted, he was responsible for a number of important innovations in the way economics helps us view reality.

He was born on a farm near Arlington, South Dakota, on April 30, 1902; he died on February 26, 1998, at the age of ninety-five. He was one of eight children, with four brothers and three sisters. He was unable to attend high school because he was needed on the farm. In 1921 he attended a short course at South Dakota State College. Someone at the college recognized that he was an obviously unusual individual, and in 1924 he was admitted as a regular student. He completed the undergraduate program in three years and received his B.S. degree in 1927. He immediately entered the graduate program at the University of Wisconsin, where he was awarded his Ph.D. in 1930. He became an assistant professor at Iowa State College and remained at the college until 1943, when he moved to the University of Chicago.

In this memoir I shall give major emphasis to why he had such a positive influence on the lives of others, provide an example of his strong dedication to academic freedom, and draw attention to some of his major administrative accomplishments. I will give less attention to his major contributions to economics because two excellent and authoritative reviews exist. One was prepared by Mary Jean Bowman (1980) at the time of his receipt of the Nobel Prize in economics and the other very recently by Marc Nerlove (in press). I strongly recommend each of them.

His influence on the lives of people—students, colleagues and others—was very great indeed. He was very open, always ready to intellectually engage anyone who approached him in a serious manner. He carried out an enormous correspondence, responding to all serious inquiries or comments that came to him, not mattering whether from complete strangers, fellow economists, or important political figures. As many testify, his impact on students was enormous, both in the classroom and as a thesis adviser. But he was accessible to more than just his students, colleagues and persons of importance. Let me illustrate by recounting how I first met him.

My first contact with him was sixty-six years ago. The nature of that contact and my first meeting with him tells a lot about why he had such a positive effect on the lives of so many people. I was a junior in high school and had entered a statewide speech contest. I had decided that the subject of my speech would be international trade and agriculture. Access to material on that topic was very limited in my small Iowa town. In preparing for the speech, I wrote a letter to Professor Theodore W. Schultz, then head of the Department of Economics and Sociology at Iowa State College, asking for his help and advice. I not only got a prompt reply, but he sent me two very relevant books as gifts—at least I assumed they were gifts, since he never asked for them back, and I still have them. I prepared my speech and went to Ames, where Iowa State College was located, to give the talk. Much to my surprise he was in the audience. He introduced himself to me and that was the beginning of a relationship that lasted for more than six decades. His taking the time to respond to a request for help from a high school student he did not know, and had no reason to believe he would ever meet, was indicative of his willingness to assist—to work with, to counsel—anyone who came to him with a reasonable request for intellectual assistance.

Anne O. Krueger, who was never a student of his, related a somewhat similar experience at his memorial service. She sent him a paper on the role of human capital differentials in explaining income differences among nations. He read the paper with care and, as he so often did, he sent her a response that included high praise for her work and suggestions for improvement. He invited her to give a paper at a conference he was organizing. Somewhat to his surprise, she turned down the invitation because the topic was outside her area of research competence. He indicated that he was somewhat bemused that someone so junior would turn down such an invitation. When he reissued the invitation to attend, she did. Thus began a relationship that spanned nearly three decades.

He was a remarkably successful academic administrator. As an assistant professor, he was made head of the Depart ment of Economics and Sociology at Iowa State College in 1935 at the age of thirty-two and only five years after the receipt of his Ph.D. Iowa State was then, and now, one of the premier Land Grant colleges, but his appointment came in the midst of the Great Depression. Raymond R. Beneke (1998) notes that at the time Iowa State College did not have the financial resources to bring an established economist and administrator from outside so they turned to Schultz. Perhaps financial exigency has never had such a positive outcome. By some means or other, he acquired over the next several years the resources to attract a large number of young economists, who later were recognized as outstanding. He was able to accomplish this in part because there were few academic openings anywhere in the United States in those years, and with a combination of his personal persuasiveness and limited money he built a department of the first rank, one that produced four presidents of the American Economic Association, four members of the National Academy of Sciences, and one Nobel laureate other than himself. I was a beneficiary of that outcome, since I was an undergraduate from 1934 through 1938 and a graduate student and faculty member from 1941 to 1943.

He left Iowa State and went to the University of Chicago in the fall of 1943. His reason for leaving Iowa State illustrates another aspect of his personality, namely his absolute support of the principle of academic freedom in our colleges and universities. He left because the president of the college, in response to pressure from a group that purported to speak for farmers, repudiated a publication authored by a member of the department. The pamphlet was the fifth in a series titled *Wartime Farm and Food Policy*. The main objective of the series was to analyze how agriculture and policies related to it might be modified to more effectively support the war effort. The subject of the offending pam phlet may seem arcane today—the pamphlet argued, among other things, that oleomargarine was nutritionally equivalent to butter. And since oleomargarine required far fewer resources than butter, the pamphlet suggested that the war effort could be furthered if various taxes and regulations restricting its production and consumption were removed.

The capitulation of the college president to the protests of the dairy interests resulted in the withdrawal of Pamphlet Number 5, *Putting Dairying on a Wartime Footing*. At Schultz's insistence and against considerable opposition, both inside and outside the college, the pamphlet was revised by the original author, Oswald H. Brownlee, and was published by the college in 1944, a year after the original edition. The revision made an even fuller and stronger case for the main conclusions of the original pamphlet, in particular for the nutritional equivalence of margarine and butter but also for the resource savings. While other examples of administrative interference with academic freedom had arisen, the precipitating factor was the margarine issue. Schultz resigned from Iowa State and accepted a position in the Department of Economics at the University of Chicago. Following his resignation, fifteen additional members of the faculty left for other positions, including several who went to the University of Chicago for periods of varying lengths (Beneke, 1998).

He became chairman of the Department of Economics at the University of Chicago in 1946, a position he held until 1961. The department was a premier one when he became chairman, and it was as strong or stronger when he concluded his chairmanship.

A great monument to his administrative strengths was his role in the creation of the relationship between the Catholic University of Chile and the Department of Economics that he chaired. While this relationship later became subject to a great deal of controversy, I believe that it can be said that no university ever had such a positive impact on a country as the University of Chicago had on Chile. First, Schultz displayed great insight in insisting on certain features of the relationship, which involved the United States International Cooperation Agency, the Catholic University of Chile, and the University of Chicago. He was concerned that the anticipated high level of economics education at Catholic University at the time the contract ended would not be maintained. At the time most of the universities in Latin America depended primarily on part-time faculty members, and few departments of economics had more than one or two full-time faculty members. He convinced the Catholic University that by the close of the contract it should have four full-time faculty members in economics. The Catholic University more than fulfilled this condition by hiring a dozen full-time professors and set a pattern that has been widely adopted throughout Latin America.

A major aspect of the agreement was that students from Chile would be provided the necessary financial support to undertake graduate studies in economics at Chicago. These students on their return helped to transform the teaching of economics in both the Catholic University and its main rival, the University of Chile. Gradually they also moved into important policy positions in the government. Starting with the presidency of Alessandri (1958–64) they have had significant roles in every Chilean government except that of Allende (1970–73). When a military coup overthrew the Allende government in 1973, a group that came to be known as the Chicago Boys was given the daunting task of rebuilding Chile's shattered economy. This they did, combining modern market-oriented policies with concentrated attacks on extreme poverty. Infant mortality, for example, fell by more than 70% in one decade.

When General Pinochet left the presidency, and democratic elections were held, the coalition party that won the election committed itself to follow the economic policies instituted during the Pinochet regime. That coalition party, which later won a second election, kept its campaign pledge. Chile today stands out as one of the few economic success stories in Latin America over the past decade or so. President Eduardo Frei came to the University of Chicago in 1997, and in a public speech thanked the University of Chicago for its major contributions to Chile.

Schultz made major and lasting contributions to the understanding of the economics of agriculture in developed countries in three important books: *Agriculture in an Unstable Economy* (1945), *Production and Welfare of Agriculture* (1949), and *The Economic Organization of Agriculture* (1953) and more than two-score important articles in professional journals. Among his publications related to agriculture, he is probably most remembered and acclaimed for his *Transforming Traditional Agriculture* (1964). During the 1950s developing countries, and most of the economists who advised them, accepted the view that agriculture could contribute little or nothing to economic growth. Economic growth, it was argued, depended on developing industry and transferring resources out of agriculture. The view that the marginal product of labor in agriculture was zero was widely accepted. It was believed that labor could be withdrawn and transferred to cities with no adverse effects on agricultural production, even if no other resources were added. Many also accepted the conclusion that farmers in developing countries did not respond to economic incentives but were guided by tradition or culture. He showed that these views were erroneous. Not only were they erroneous, but their acceptance by policymakers caused great harm to nations as a whole and to farm people in particular.

He provides a guide to what he hoped to accomplish:

> The purpose of this study is to show that there is a logical economic basis why traditional agriculture employing only the factors of production at its disposal is incapable of growth except at high cost, and why the rate of return to investment in modern agricultural factors can be high by past growth standards. It really does matter what is done in developing agriculture in countries that want to achieve economic growth as cheaply as possible (Schultz, 1964, p. 5).

If only this lesson had been learned much earlier, the people in most developing countries would be far better off than they now are.

On its publication, *Transforming Traditional Agriculture* encountered opposition from some quarters that lasted for many years. One example was a review in the *Economic Journal*:

> No transforming of Chicago: this is an ill-informed and potentially mischievous book on a subject [that] is among the most vital and most urgent in the world. It is ill-informed because Professor Schultz ignores literature essential if a balanced judgment on the problem of the transformation of primitive peasant agriculture production is to be arrived at, and the basis for effective policy is to be found in the largest and most populous parts of the world (Balogh, 1964, p. 996).

As an antidote to the criticism in the quoted review, and others as well, the following statement made at his memorial service by Anne Krueger serves very well:

> It is almost impossible, with hindsight, to understand how great Ted's contribution to understanding economic development was. Development was seen to be "different" because "normal economics" didn't apply. It was said to be that cultural obstacles, structural rigidities, dependence on primary commodities and other phenomena made developing economies different. At bottom, people (most of whom were then in agriculture) were thought to be set in their traditional ways, either too content or too ignorant to be willing to change or to respond to incentives.
>
> So, it was thought, there was a free lunch—zero marginal product of labor. All you had to do was to add capital (according to a plan) and you could extract savings and labor from traditional agriculture to industrial

> ize. Savings could come free because there would be no response to producer prices, and labor was free by assumption.
>
> Ted challenged all that frontally. *Transforming Traditional Agriculture* was central because it said peasants were well adapted to their circumstances, knew their environment, and could not do better until given the means (capability) for transformation to more productive agriculture.
>
> In saying this, Ted committed many heresies: he said peasants were rational and would respond to incentives; he said labor was not a free good, using data from Indian regions on declines in farm output after the flu epidemic; he said large, farms weren't necessarily more efficient; and much more.

The test of time and experience has confirmed his conclusions. In those areas of the world where governments have provided reasonable incentives to farmers and where new methods of production that were more profitable than those they superseded have been made available, the people are much better fed today than ever before, and the farmers have substantially higher real incomes. Life expectancy has increased in the developing world, due in considerable part to improved nutrition, from thirty or thirty-five years in 1950 to more than sixty years today. In those areas of the world where governments have exploited agriculture and relatively little has been achieved in terms of improved methods of production, the countries have stagnated in terms of food supply and income per capita.

In the document that accompanied the Nobel Prize in 1979 it was *Transforming Traditional Agriculture* and his other work on the same subject that was given as one of the two main reasons he merited the award.

The other major phase of his work given emphasis in the Nobel award was his work on human capital. The view that it was appropriate to invest in people was anathema to many. In his presidential address to the American Economic Association, "Investment in Human Capital," he notes this opposition:

> The mere thought of investment in human beings is offensive to some among us. Our values and beliefs inhibit us from looking upon human beings as capital goods, except in slavery, and this we abhor . . . To treat human beings as wealth that can be augmented by investment runs counter to deeply held values. It seems to reduce man once again to a mere material component, something akin to property. And for man to look upon himself as a capital good, even if it did not impair his freedom, may seem to debase him . . . (But) by investing in themselves, people can enlarge the range of choice available to them. It is one way free men can enhance their welfare (Schultz, 1961, p.2).

He understood what many others overlooked in their opposition to the analysis of human capital or investment in people. The investment is made in considerable part by those one is dependent upon, namely, one's family, or by oneself. Its outcome, in turn, depends to a very large degree on the effort each individual makes to take advantage of the opportunities it opens up.

His interest in human capital was, to some degree, due to his efforts to understand the sources of economic growth. He studied the efforts to explain economic growth by analyzing changes in the factors of production: land, labor, and capital. Each of numerous statistical analyses found that about half of the growth in output was unexplained. In the analyses, the unexplained part was attributed to productivity change. Schultz found this attribution unacceptable and argued instead that the unexplained residual was not a measure of productivity change but instead was, at least in some degree, a measure of our ignorance. He argued that one reason the residual was so large was that an important input, namely human capital, was ignored when labor was included in the analysis simply as the number of workers. It was his belief that once the value of the human capital was included in the analysis the size of the residual would be reduced.

It was surely no accident that his Nobel lecture was "The

Economics of Being Poor." I do not know if the poverty that he suffered in his youth influenced him in his interest in poor people. In the decades that I had close contact with him, he never mentioned the difficulties of life that prevented him from going to high school. There was never any doubt about his interest in and concern for the poor farmers of the world. He took advantage of opportunities to visit rural areas in Latin America and Asia. He was critical of our agricultural policies that adversely affected farmers in developing countries. For example, he called attention to the negative impact of our disposal of large quantities of our surplus grain on the prices received by farmers in the recipient countries. The topic of his Nobel lecture reflected his concern for the hundreds of millions of farm families in developing countries. Given the scope of his scholarly work, he could have used the lecture to express his views on a considerable number of topics, but he chose to emphasize his concern for the poor.

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JOHN CHARLES WALKER

July 6, 1893–November 25, 1994

BY DONALD J. HAGEDORN

FOR YEARS MANY PROFESSIONAL plant scientists considered John Charles Walker to be one of the world's greatest plant pathologists. Walker earned this reputation because of his high intellect, work ethic, and an unusual ability to scientifically assess plant disease problems and develop methods for their control. These control procedures almost always involved the practical application of his knowledge of and appreciation for related sciences, such as plant genetics, plant physiology, and biochemistry, which greatly benefited the vegetable growers and processors of our nation. They involved pioneering scientific achievements that scientists all over the world embraced and tried to emulate. His fundamental discoveries of plant disease resistance made a lasting impact on world agriculture.

John Charles Walker was born on July 6, 1893, in Racine, Wisconsin. His father was a cabbage grower and seedsman. Young Walker attended the rural district school and then Racine High School, graduating in 1909. In 1910 he enrolled in the University of Wisconsin, and in 1914 his B.S. thesis on onion smut disease won the university's Science Medal as the most outstanding thesis. For this honor his fellow graduates gave him a rousing cheer at commencement. He earned an M.S. in 1915, and then attended Cornell University for the spring semester of 1916. He was granted a Ph.D. from the University of Wisconsin in 1918.

Walker was preceded in death by his first wife Edna Dixon Walker in 1966, his second wife Marian Dixon Walker in 1982, and his son John William Walker in 1938. He is survived by 10 nieces and nephews, 23 grandnieces and nephews, and 26 great-grandnieces and nephews.

When a very young student, Walker showed a preference for biological subjects. As a farm boy he had early interests in plant disease problems. Inspiration for fundamental research very likely came from boyhood association with his uncle, Dr. D. J. Davis, who was later dean of the University of Illinois College of Medicine.

Walker was influenced professionally from the beginning of his University of Wisconsin matriculation by L. R. Jones, who was chairman of the department of plant pathology and was concerned about the plant disease problems of the vegetable growers of southeastern Wisconsin. Jones saw in Walker the potential for a first class scientist and encouraged him in every possible way.

Walker possessed a unique personality with a demeanor suggesting a person who was very serious and all business to the point where one wondered whether he was approachable. Indeed he was often in deep thought, but if students or colleagues needed his counsel he was always willing to listen to the problem, give it thoughtful consideration and discussion, and provide valuable guidance. His judgment regarding subjects personal or professional was of real help to those who sought it. However, Walker did not have much time for small talk, such as the weather or athletic scores. Even though plant pathology was his consuming interest, he went fishing occasionally and participated weekly in the faculty bowling league. He also enjoyed bridge parties, departmental picnics, and Christmas parties, and was a gracious host to students and friends in his home. He valued these occasions to become better acquainted with members of his research team and their spouses.

Good professional relationships with professors in other science departments at the University of Wisconsin were very important to Walker. Because he thought that many of his graduate students should be well trained in botanical and other subjects he kept in close contact with professors in the departments of botany, genetics, agronomy, and biochemistry. These people cooperated in various research efforts and often served as so-called "minor professors" to help select the most appropriate courses to be taken by Walker's students and to be members of the student's preliminary and final examination committee.

Walker received his B.S., M.S., and Ph.D. degrees from the University of Wisconsin-Madison. His first contribution to science was a paper presented in 1916 at the meeting of the American Association for the Advancement of Science in Pittsburgh, Pa. An abstract of this research, entitled "Association of plant pigments with disease resistance in onion," was published in *Phytopathology* in 1919. From 1917 to 1919 he was a scientific assistant in the U. S. Department of Agriculture; then until 1964 he was an assistant, associate, and full professor of plant pathology at his alma mater. From 1919 to 1944 he was also employed by the U. S. Department of Agriculture as a plant pathologist. In 1952 he was a visiting professor at the Instituto Biologico in Sao Paulo, Brazil.

When Walker first joined the faculty at Wisconsin his major research efforts concerned the severe disease problems of fresh and kraut cabbage, which were major vegetable crops in Wisconsin. His pioneering research on genetic resistance to the yellows disease saved the cabbage industry; but more importantly it showed the scientific community that disease control through genetic resistance could be an effective and relatively inexpensive approach to solving plant disease problems. He continued to research disease problems in cabbage. He found both Type A and Type B resistance to cabbage yellows, Type A being the most stable, thus the most preferred. Walker and his students also developed cabbage varieties resistant to other troublesome diseases, including clubroot, mosaic, and tip burn. Wisconsin cabbage growers and kraut packers were so impressed with Walker's cabbage researches that they provided unsolicited funds to build new greenhouses at the University of Wisconsin to support this research.

Field and garden beans in the United States were being severely attacked by the common bean mosaic virus; so Walker and one of his students, W. H. Pierce, successfully sought resistance and proceeded with bean breeding research that resulted in the development and release of the first virus-resistant field and garden beans. Later, similar successful research efforts resulted in the development of the first beans resistant to the troublesome bacterial disease known as halo blight.

Walker was also the nation's leader on the researches of diseases of canning peas. The Fusarium wilt and near-wilt diseases were important, especially in the Midwestern and eastern states, where canning and later freezing peas were widely grown. These wilt diseases also became important in western states, where peas for seed and later peas for processing was a critical industry. Walker and his students developed and released sorely needed peas resistant to both wilt and near-wilt. Walker was the first scientist to demonstrate the chemical nature of disease resistance in plants. He found that the resistance of onion varieties with colored bulbs to onion smudge and three Botrytis neck rots was associated with the pigments in the onion's outer scales, which con tain the phenolic compounds protocate-chuic acid and catechol. In the 1940s Walker saved the cucumber industry in Wisconsin by discovering resistance to the devastating spot rot disease and later developed cucumbers resistant to scab and mosaic. Also in the 1940s he restored the state's canning-beet industry by developing an inexpensive fertilization treatment to cure the troublesome internal black spot disease, which he found to be caused by soil boron deficiency.

Walker conducted pioneering studies on environmental factors that could have important influences on vegetable disease severity. Before plant growth rooms were in common use he devised ways to use greenhouses and water tanks to study the effects of temperature and plant nutrition on plants growing in quartz sand and inoculated with disease pathogens. These classic experiments proved for the first time that improper plant nutrition was an important factor for the initiation and eventual severity of important vegetable diseases. They led to new approaches to reducing the effects of specific vegetable diseases and sometimes to complete disease control. His innovative studies on the physiology of disease resistance often resulted in new and important research findings.

Walker recognized the need for a Wisconsin potato seed certification program and was the guiding force behind its instigation and development. The result was that many of Wisconsin's potato disease problems were brought under control.

Eighty-three graduate students in plant pathology were fortunate to have Professor Walker as their mentor. Many of them went on to prominent careers, applying his methods around the world. Walker authored or co-authored 450 publications and wrote two textbooks, *Diseases of Vegetable Crops*

(1935, 1952) and *Plant Pathology*. Both books are key texts in the plant pathology field.

Throughout his career Walker met his teaching responsibilities by being involved in the "backbone" courses in plant pathology and by conducting his course "Diseases of Vegetable Crops." His lectures, while not enthusiastic performances, were interesting, remarkably well organized, complete, and up-to-date, and thus were much appreciated by those of us fortunate enough to attend his classes. In 1952 he was a guest lecturer at the Agriculture Institute of Sao Paulo, Brazil. The Racine, Wisconsin, Chamber of Commerce awarded a grant to the Wisconsin Alumni Association to establish the J. C. Walker lectureship in plant pathology at the University of Wisconsin. Earnings from this grant are used to bring distinguished guest lecturers to Wisconsin in honor of Walker's development of disease-resistant crops, which has saved the multimillion-dollar vegetable growing and processing industry in Wisconsin.

As Walker's national and international reputation as one of the world's outstanding plant pathologists grew, potential graduate students and postdoctoral scientists sought out his research laboratory. This situation, while a real credit to Walker, was sometimes a problem, because most of these people needed financial assistance and only limited monetary resources were available at Wisconsin. In addition, research facilities were often crowded and could not easily accommodate additional personnel. Even so, he did a remarkable job as a major professor and a senior advisor to visiting scientists.

During the period 1923–64 Walker guided the research and graduate studies of 56 Ph.D.-degree and 18 M.S.-degree students of plant pathology. Fifteen of these people were from foreign countries. From Walker they all received professional guidance of the highest caliber, and also valuable personal advice if they asked for it. In this way the professionalism of a great scientist and the philosophy of a wise person was spread virtually around the world. Walker valued and enjoyed his relationships with these persons from foreign countries, whether they were his graduate students or scientists who came to undertake a postdoctoral research project. He learned about the governmental and societal as well as agricultural problems that they encountered. In this way he could assign research projects that would be valuable scientifically and also reduce disease losses in their home countries.

Walker's research activities and accomplishments were also unusually well received and greatly appreciated by the vegetable growers, processors, and seedsmen who served these businesses. These people looked forward to the development and release of new disease-resistant vegetables that had been developed by University of Wisconsin research programs under his supervision. In fact, some of the funds needed to undertake these researches were sometimes provided directly by commercial organizations. Without such support, which was sometimes unsolicited, some of this research could not have been undertaken or would have been substantially delayed. The vegetable growers and processors also supported his research programs by urging both state and federal granting agencies to provide research funds for his use. These moneys were so effectively and efficiently used that government and commercial research support organizations repeatedly looked with favor on Walker's research needs and supported them financially.

The vegetable seed industry greatly appreciated Walker's plant breeding efforts and was quick to accept and put to use the new vegetables he developed. In most cases he was given full credit by the seedsmen for these new vegetable cultivars. They really were giant contributions.

Walker's scientific accomplishments were well recognized around the world. He was elected to the National Academy of Sciences in 1945. The University of Göttingen in Germany granted him an honorary doctor of science in 1960. In 1961 he was honored by a gift of new research greenhouses from the National Kraut Packers Association in recognition and appreciation of his contributions to cabbage growers and packers. The Botanical Society of America gave him the Merit Award in 1963. He was elected a fellow in 1965 and given the prestigious Award of Distinction by the American Phytopathological Society in 1970. He was honored by the British Association of Applied Biologists, Vegetable Growers of America, American Seed Trade Association, National Manufacturers of Processing Equipment, National Pea Improvement Association, National Pea Packers Association, and the U. S. Food Processors through the Forty-Niners organization. The University of Minnesota Department of Plant Pathology presented Walker its prestigious E. C. Stakeman Award in 1972. In 1978 he received the $50,000 Wolf Foundation Prize in Agriculture in Israel for making "significant and lasting contributions to the advance of world agriculture." The prize committee judged him "among history's greatest three or four plant pathologists."

To summarize, the innovativeness, thoroughness, and number of Professor John Charles Walker's scientific accomplishments and publications were, and are, truly remarkable. He was the first scientist to demonstrate the chemical nature of disease resistance in plants. Furthermore he repeatedly and effectively used plant breeding as a pioneering approach for controlling important vegetable diseases. He developed disease-resistant cabbage, cucumbers, peas, beans, tomatoes, and onions. Seeds of these new vegetables were made readily available to seedsmen and growers and were promptly and widely accepted and used to solve pro duction problems that had been present for many years. Many of today's vegetable cultivars still carry the disease-resistant genes from his vegetable releases. He was truly a fine person and a great scientist!

ALTHOUGH THE WORDING has been changed here, some of the facts and thoughts presented were originally published in *Phytopathology* (vol. 85) and authored by C. R. Grau, D. J. Hagedorn, and P. H. Williams and in an article by G. S. Pound.

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GIAN-CARLO WICK

October 15, 1909–April 20, 1992

BY MAURICE JACOB

GIAN-CARLO WICK WAS A renowned theoretical physicist of this century. His name is explicitly attached to a very important theorem, the Wick theorem, which played a substantial role in the early perturbative use of quantum field theory. It quickly made its way to textbooks on particles and fields and found later a great use in nuclear and condensed matter physics. Gian-Carlo Wick's name is also associated with the Wick rotation, a theoretical technique using imaginary time, which had an notable impact on the development of fruitful relations between field theory and statistical mechanics.

Gian-Carlo Wick is also well known for the insight and clarity that he brought to several questions at a key time in their development, in particular in meson theory and in the many applications of symmetry principles in particle physics. There are also many properties that are not associated explicitly with his name simply because they have since become part of the common knowledge of physicists. Yet, they were first due to the clarity of his mind and to his sharp insight for physical phenomena. One may mention, for instance, the extension of the then new Fermi theory of beta decay to positron emission and also the relation be tween the range of a force and the mass of the exchanged particle, which is at the origin of that force. There are also the famous papers by Wick, Wightman, and Wigner on the intrinsic parity of elementary particles and on the question of superselection rules. There is the Lee-Wick approach to spontaneously broken symmetry, which paved the way to present developments on the transition between hadronic matter and the quark gluon plasma occurring at very high temperature and/or very high density. I had the privilege to be associated with his name in the Jacob-Wick expansion, which has long provided a handy formalism for the description of relativistic collisions between particles of arbitrary spins and in the spin-parity determination of the many particles discovered in the sixties.

This list of important and original achievements is long. To find a common thread, one may say that Wick was always fascinated by the mathematical structure of physical theory. In particular, he was a great expert in the application of group theory, something that he extensively used in his works on symmetries. But, together with this great mathematical expertise, he had a deep physical intuition and a strong desire for his contributions to be of direct use in topical matters and thus be expressed with clarity and rigor but still with the minimum amount of technicalities. He never lost sight of the physics. Three eminent qualities—mathematical expertise, physics insight, and a deep concern for practical use by others—appear over and over in his different and important contributions to physics.

Altogether, Gian-Carlo Wick made many fundamental contributions to nuclear and particle physics from the 1930s, when he was a close associate of Enrico Fermi in Rome, to the 1970s, when he worked with Tsung Dao Lee at Columbia. Indeed, in an address about Gian-Carlo Wick to the Accademia dei Lincei in 1994, Luigi Radicati, who was di rector of Scuola Normale when Wick was there, said, "To speak about the life and science of Gian-Carlo Wick is to recall the evolution of physics over forty years, a period that starts in the thirties and ends in the seventies."

Gian-Carlo Wick died of cancer in 1992 at eighty-two years of age in his hometown of Turin, Italy. He had returned there after a long and productive career that began in Rome before and during World War II, continued in the United States at Notre Dame, Berkeley, Carnegie Tech, Brookhaven, and Columbia, and concluded at the Scuola Normale in Pisa, which he had joined after retiring from Columbia.

In his talk on the occasion of the symposium marking the retirement of Gian-Carlo Wick from Scuola Normale in 1984, Tsung Dao Lee recalled how he first heard about Wick in 1947 in Fermi's class at the University of Chicago. Fermi was discussing some problem connected with the slowing down of neutrons, when he said, "This was solved by Wick," and then he immediately added, "Wick is a very good physicist." Conversely, Wick considered Fermi his "principale maestro" and, as he once said, "Fermi's advice over many years, but first of all the example set by Fermi were my essential guidances as a young researcher." In his 1984 talk, T. D. Lee presented several of Wick's achievements in physics and also said with emphasis, "Gian-Carlo Wick is a gentleman." He was a great physicist. He was a wonderful man.

Gian-Carlo Wick was born in 1909 in Turin, where he would spend his boyhood and attend the university. His name does not sound typically Italian. His great-grandfather had come to Italy from Switzerland (the St. Gall region). His father was a chemical engineer. His mother was the well-known Italian writer Barbara Allason, who took a very courageous antifascist stand in the 1930s. She brought a very strong intellectual slant to family life.

Physics attracted him early in life and, besides the intellectual inspiration he found in his family, he later paid tribute to one of his college teachers, in mathematics and physics, Professor Artom. He earned a Laurea degree in physics from the University of Turin in 1930, with a thesis on the electronic theory of metals, for which his main mentor was G. Wataghin, who was the first teacher of quantum mechanics in Turin. He then left Italy and worked one year in Germany, sharing the time between Göttingen and Leipzig in the institute of Heisenberg, working on atomic and molecular theory. He met there and became friends with many young physicists, who he later encountered in Copenhagen and the United States.

Wick then returned to Italy and, after one year in Turin, became Fermi's assistant in Rome in 1932. Fermi, the great leader of the Rome group, quickly recognized the talents of his young assistant. The position in Rome was to last until 1937, when Wick was awarded a professorship in theoretical physics, first in Palermo and then in Padova. He came back to Rome in 1940 to take the prestigious chair of theoretical physics, which had been declared vacant after Fermi's departure for the United States. The university actually invited him following the very suggestion of Fermi. He spent the war years in Rome, but he followed Fermi's advice again in deciding to move to the United State in 1946. He first went to Notre Dame to be close to Fermi in Chicago, and then moved to Berkeley.

Before continuing with his career, let us first stop at his Rome period. The achievements of the Rome group in the mid and late 1930s under Fermi's magnificent leadership are world-renowned. This was a fascinating time for physics. Nuclear physics was becoming understood with results that would soon unlock nuclear energy. Gian-Carlo Wick's contributions were important and numerous:

- He showed how to calculate the magnetic moment of the hydrogen molecule, a result that was later used by Stern in his measurement of the magnetic moment of the proton. This was a masterly application of group theory, a tool used by few physicists at that time but the book of Hermann Weyl held no secret for Wick.
- He extended Fermi's theory of beta-decay to positron emission and to electron K-capture by the nucleus. He was thus a precursor for our now familiar substitution of an outgoing particle for an incoming antiparticle or vice versa. I remember Wick recalling Roman café discussions with E. Majorana, who was a great pioneer in the description of particles in terms of quantum fields, with the resulting symmetry between particle and antiparticle.
- He worked on the slowing down of neutrons in materials, the basis of the remark by Fermi mentioned earlier. He developed on his own the basic properties of neutron scattering by crystals, which later was going to find so many important uses in neutron pile research.
- It is also to Wick that we owe the simple relation between range and mass seen as a direct consequence of the Heisenberg uncertainty principle: the heavier the mass of the exchanged particle that is the origin of the force, the shorter the range of that force. As we know today this explains the apparent weakness of the weak interactions. Wick first thought of it as a rather obvious effect, as it indeed looks to many physicists today. It was however not obvious to many at that time. When this caught the interest of Niels Bohr, Wick was very pleased.
- In order to explain the anomalous magnetic moment of the proton, Wick proposed that the physical proton was actually a superposition of a bare proton and of a neutron surrounded by a positron and a neutrino. At that time there was some understanding of weak interactions, but the na

> ture of the nuclear forces with meson exchange was still not known and there were attempts to link them with the weak force. Wick was wrong about the relevance of the weak process, but he was right on the superposition idea, which was later vindicated by meson theory.

In Rome, Gian-Carlo Wick was making his mark as a first class theorist. Perhaps following again the example set by Fermi, Wick also took part in experimental work as a phenomenologist, as one would say today, but also directly. This was in nuclear physics in Rome and later, when he came back to Rome, in cosmic ray research in the Italian Alps. The latter had the advantage of keeping him for weeks in the spectacular mountain settings he loved. Mountaineering was indeed long a strong hobby. He could be easily enticed later to give a long series of lectures on symmetries at the Les Houches summer school in the French Alps, when I was running a session there in 1965. In the 1930s and 1940s, living conditions in the Alps were sometimes still rather rough. I remember him telling that he once complained to an innkeeper about the lack of running water in his room, and was informed, "You know, this is a fashion that will fade away."

Wick worked with the group led by Gilberto Bernardini in the laboratory of Testa Grigia near the Matterhorn. The group, which included many physicists who became well known, obtained in particular the first measurement of the mean life of the muon. Also known from his cosmic ray work are the Wick's curves, which have been much used to study the penetrating component of cosmic rays.

The brilliant young Rome theorist also quickly became well known for the quality of his lecturing, which combined rigor with clarity in the most perfect way. His gift, patience, and talent for clarifying complicated matters for the ben efit of his fellow physicists has been indeed a constant element of Gian-Carlo Wick's style of work and led to some of his important contributions. He is the author of several well-known and long-used review articles and lecture notes, in particular on meson theory and symmetries. His great talent as a lecturer remained intact throughout his entire life. Near the end of his professional life at Scuola Normale in the late 1970s and early 1980s, he taught a course on general relativity. The students found his lectures inspiring, because he always stressed the important points without getting involved in lengthy calculations. This was not for lack of technical mathematical expertise! He had an impressive mathematical knowledge that covered both classical and modern mathematics, but he had, most of all, the sharp insight to know where the key physics questions were.

In connection with that I must mention an episode reported to me by Luigi Radicati. Once at the European Laboratory for Particle Physics (CERN), two well-known American colleagues came to lunch with a problem of Euclidean geometry they had failed to solve despite much effort the previous night. They even offered a bet to anyone who could solve it. Gian-Carlo sat quietly through the presentation of the problem. As they later took coffee, he offered a simple solution based on projective geometry. The two well-known colleagues had probably never heard of projective geometry, and the reported story does not say whether they paid their bet. Recalling the event, Gian-Carlo Wick said, "When I was in college, Artom had made projective geometry look so beautiful to me that I remembered it perfectly all through my life."

Gian-Carlo Wick's interest for mathematics came back strongly late in life. The librarian of the Scuola Normale still remembers the old gentleman sitting quietly for hours in the library, surrounded by piles of mathematics books.

We now come back where we first stopped, to 1946, when Wick moved to America, first to Notre Dame to benefit from the vicinity of Fermi. Arriving in the United States, Wick found a stimulating research atmosphere and was fascinated by all the activities originating from the results obtained with accelerators that could produce mesons. His interests in field theory and in the origin of nuclear forces were quickly and strongly revived and he soon accepted the offer from Berkeley to take the chair left by Oppenheimer. He offered his assistant position to Jack Steinberger, who came from the Institute for Advanced Study. They wrote a paper together on neutron polarization, but, when Steinberger became enticed by the experimental possibilities offered by the machine there, Wick let him go to do what he liked best and thus to start his brilliant career as an experimentalist.

The Berkeley period is that of the famous Wick theorem. This landmark paper, entitled "Evaluation of the collision matrix" (1950), shows how to conduct explicit practical calculations starting from the formal relations of relativistic quantum field theory through expression of the chronological product of quantum fields in terms of a sum of normal products. This leads to a direct derivation of the Feynman rules in the calculation of a reaction amplitude.

Wick also worked with Geoffrey Chew on the impulse approximation. His important work on meson theory came out explicitly only later, in 1955, in his famous review article "Introduction to some recent work in meson theory." This very informative paper is a masterpiece of rigor and clarity.

His association with Berkeley was, however, to be rather short. Wick's deep attachment to freedom led him to resign rather than sign the loyalty oath that the regents of the University of California had imposed. Yet one cannot say that he had any strong allegiance in politics. He cared deeply about the status of the world, and he would observe with accuracy and comment with clarity and precision on political events. However, he would repeat that "anyone who claims to predict the future is a fool." He was first of all very deeply attached to freedom and his opposition to fascism in Italy, which he shared with his mother and many friends in Turin, had warned him of the great dangers of witch hunting. In the same way that he was critical of fascist ideas in Italy in the 1930s and 1940s—when many around him accepted the state of affairs not so much with enthusiasm but as an inescapable condition at that particular time—Wick could not accept the limitation to the liberty of opinion that McCarthy was trying to impose. Wick was not at all a Communist (he considered Communists fanatical). Yet he refused to swear in that loyalty oath that he had never been a member of the Communist party, considering that the question brought an intolerable limitation to the liberty of thought. That he put his profound conviction above the benefit of obvious working conditions and career advantages still commands respect. As he once said to Radicati, "I had once to take such an oath in Italy for mere survival reasons and I always regretted it. I did not want to repeat an act that repelled me as being so much against my liberal principles." Wick left Berkeley and moved to Carnegie Tech in Pittsburgh in 1951.

During his tenure at Carnegie Tech, which was to last until 1957, Wick spent a year at the Institute for Advanced Study in Princeton, collaborating with Wightman and Wigner on the paper on the intrinsic parity of elementary particles. It was to be followed by another collaborative effort, in 1970 this time, on "Superselection rule for charge." He also spent a year at CERN in Geneva, a place he visited many times. He was there for another year in the early 1970s during his tenure at Columbia, and after he moved to Scuola Normale, he became a regular summer visitor for one month. Of particular importance during his time at Carnegie Tech was his work on the Bethe-Salpeter equation, where the Wick rotation appears. The analytical properties of the scattering amplitude are studied using imaginary time, with an eventual continuation to real time. We already noted that this technique finds many applications in the fruitful interplay between field theory and statistical mechanics.

Gian-Carlo Wick moved to Brookhaven National Laboratory in the fall of 1957 to head the Theory Division. An important contribution at this time was the development of the helicity formalism for the description of collisions between particles of arbitrary spin. It soon became a basic tool for the analysis of the many particles discovered in the 1960s, and the paper I wrote with him quickly became a citation classic. I had come to Brookhaven at the same time. I was a very green young physicist at that time and I did not yet have a Ph.D. As a student at Ecole Normale in Paris, I had benefited from the teaching at Saclay and at the Les Houches summer school, but I was merely a beginner. Feeling at first lost at Brookhaven and alone in an office where I was supposed to do research on my own, I turned to Saclay for advice. The answer came loud and clear: "Hang on to Wick; he is highly worth it." Gian-Carlo was kind enough to tutor me as his student. The research on the helicity formalism eventually became my thesis work. My association with him was to mark me for life.

In 1967, Gian-Carlo Wick was awarded the Dannie Heineman Prize for Mathematical Physics by the American Physical Society "for contributions to quantum field theory, for the investigation of the theory of scattering of particles with spin, and his deep analysis of the symmetry principles in Physics."

Wick became a tenured professor at Columbia in 1965, having had an association with the university since 1958. His research at Columbia was done mainly in collaboration with T. D. Lee. Among its highlights was their work on discrete symmetries, the study of indefinite metric and unitarity, and their introduction in 1971 of the idea that, in quantum field theory, the vacuum could show a special structure that disappeared at high temperature. The title of the paper is "Vacuum stability and vacuum excitation in a spin 0 field theory." This new resulting form of matter was proposed long before the same general idea emerged in describing the structure of matter at the quark level, with an expected transition between hadronic matter and a quark gluon plasma. Since the 1980s, this has been an important field of research, bringing together nuclear and particle physicists. There is already good evidence that the transition takes place, but we are still struggling to study its properties. Early work at CERN and Brookhaven, which already has yielded a very interesting crop of results, will soon continue in a dedicated way at Brookhaven with RHIC and at CERN on the Alice detector at the LHC.

As T. D. Lee has said of Wick, "Gian-Carlo was a person of gentle disposition and deep thoughts. Throughout his career, he immersed himself in the fundamental problems of physics, invariably motivated by their challenge and importance. His solutions were always characterized by a special clarity of mathematical analysis." This summarizes perfectly the memory that he leaves with those of us who had the privilege to know him.

The last part of Wick's career was spent at Scuola Normale after he retired from Columbia. This was a quiet and serene period for him, with much reading in the library. But people recall his beautiful lectures, his pertinent interventions in seminars, and many wonderful conversations.

His work in physics was rewarded many times. In addition to the Heineman Prize of 1967, Wick was the first recipient of the Ettore Majorana Prize in 1968. He received the Medaglia Matteucci of the Academia del XL in 1981. He was a member of the National Academy of Sciences, Academia Nazionale dei Lincei in Rome, Academy of Sciences of Torino, and the Ligurian Academy.

When he turned seventy-five, Wick summarized his life as a physicist in a talk titled "Ricordi di una fisica diversa" at the symposium that marked his retirement in Pisa. In his research he could be abstract in a subtle and deep manner, but he always remained close to the physics in a very concrete way. Never imposing himself, he was at the same time very generous of his time and effort for those who approached him for advice and guidance. As he once said, "I cannot boast of having created a school, but I derive much satisfaction in having known as my students younger physicists who later, developing further their talents, have become well known for their own contributions." I would add that the teacher here shows far too much modesty. His impact was certainly profound and quickly felt. Everyone who approached him was impressed by his knowledge of physics, by the clarity of his ideas, and by the depth of his understanding of any phenomenon. He carried this rigor and clarity of ideas well outside the domain of physics. He assessed political problems with the same uncompromising attitude as he did physics. He was a man of extreme intellectual honesty, whose ethical judgment came before material advantages, qualities demonstrated by some hard choices he had to make. He shared with his mother a very wide culture well above any particular ambition or national pride. Fluent in four languages, Wick was a humanist as well as a physicist. He had a great span of intellectual interests. As a young man he took inspiration from the breadth of interest of Niels

Bohr and Arnold Sommerfeld, who both much impressed him.

Gian-Carlo Wick was deeply concerned about the special responsibility of physicists in society and the more so through-out the long Cold War years. He once allegorically compared the physics community to adults who left their children in a log cabin during their absence, giving them a box of matches so that they could have fun with them, but later deeply regretting having done such a thing! He did not think that physicists could be better than others at solving the problems of the world, but he repeatedly stressed their strong responsibility to inform and advise. Being to the left in the Italy of the late 1930s and a liberal in the America of the late 1940s, Wick was often and almost systematically against the prevailing current, and often anxious about the future of the world. Yet it is people like him who give hope for humanity.

Near the end of his life Wick was honored by the "la Stampa" club of Turin, which awarded him its Silver Plate for 1991. This is a prestigious prize, which every year since 1980 honors three Piedmontese for achievements that have contributed to the international renown of Piemonte, the province of Turin. He shared the 1991 prize with actress Caterina Boratto and publisher Giulio Einaudi. He felt strongly this sign of gratitude from his own city at a time when his illness was already well advanced.

Wick is survived by his wife Vanna of Turin and two sons from a first marriage, Lionel of Forest Hills, New York, and Julian, of Tokyo. Gian-Carlo Wick has left us. However some of the works of the young Rome theorist—the Wick theorem, the Wick rotation, the Jacob-Wick expansion, the Lee-Wick approach to the vacuum, and his important contributions to meson theory and to symmetry properties—will all long remain in physics textbooks. And, for some time, there will also be the wonderful memory that is kept preciously by all those who had the privilege to know him.

THIS MEMOIR IS A MUCH enlarged and edited version of a note that I wrote in 1992 as an obituary for *Physics Today*. Bringing it to its present form, I benefited from information and editorial help of L. Radicati from Pisa and J. D. Jackson from Berkeley.

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