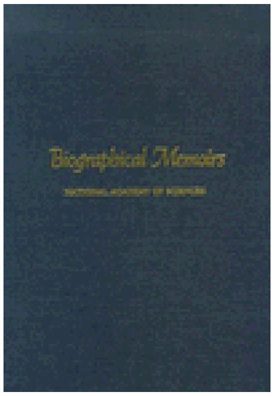

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Preface

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

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

PETER H. RAVEN

HOME SECRETARY

Biographical Memoirs

Volume 74

Albert Francis Birch

August 22, 1903-January 30, 1992

By Thomas J. Ahrens

Francis Birch was a founder of the science of solid earth geophysics. He demonstrated how study of the physics of minerals and rocks could lead, in conjunction with knowledge of seismic structure and geochemical abundances, to an understanding of the constitution of the Earth. Birch obtained, in his Harvard University laboratory, some of the first reliable data for the elasticity (especially at high pressures and temperatures), thermal expansion, and thermal conductivity of rocks and minerals. Based on his own and other experimental results and his analytical perception, he pioneered our understanding of the composition, structure, and temperature of the Earth's interior and developed models of the Earth's initial differentiation. Francis Birch was fascinated by the Earth and worked his whole life refining his analysis of it.

At Los Alamos National Laboratory during World War II, Birch played a crucial role in translating his considerable Harvard experience in the high pressure and temperature mechanics of materials to developing a gun-type device that evolved into the first of the two atomic bombs produced by the Manhattan Project. Although the decision for their deployment is still controversial, it is clear that these explosions decisively ended World War II in August 1945. Francis

Birch returned to Harvard and remained continuously on the faculty until his death in January 1992. Birch had a remarkable and extremely productive scientific career that spanned some sixty years.

LIFE IN WASHINGTON D.C. AND AS A HARVARD UNDERGRADUATE

Francis Birch was the eldest son of George Albert Birch and Mary Hemmick Birch, who resided in Chevy Chase, Maryland. His father hailed from Falls Church, Virginia, and was in the banking and real estate business. His mother was a homemaker and participated in choir singing, notably as soloist at St. Matthew's Cathedral in Washington, D.C. Young Francis attended D.C. public schools, and on entering Western High School in 1916, he enrolled in the High School Cadets, or what we call today the Junior Army Reserve Officer Training Corps.

The United States was just plunging into World War I. In high school Francis was an excellent student and participated in track, boxing, and gymnastics. Francis had three younger brothers who pursued varying careers. Next oldest David Birch, like his father, went into banking. John Birch was a diplomat with the U.S. Department of State, and Robert Birch, the youngest, became a composer of songs.

In 1920 Francis Birch entered Harvard supported by a scholarship and majored in electrical engineering. He also continued his military commitment and joined Harvard's Army ROTC Field Artillery Battalion. At that time artillery pieces were horse drawn. Francis spent several hours a week removing manure and grooming the horses maintained by the Harvard ROTC contingent in a large barn-like structure, which was later converted to the Gordon McKay Laboratory of Applied Physics.

Francis Birch received his bachelor of science degree in electrical engineering magna cum laude in 1924. This was followed by two years (1924-26) in the engineering department of the New York Telephone Company.

Francis became restive and applied for an American Field Service Fellowship. The fellowship supported Francis Birch's research visit from 1926 to 1928 to the Institut de Physique at the University of Strasbourg in France in the laboratory of Professor Pierre Weiss. Weiss was a pioneer in the modern sub-field of the magnetic properties of matter. At the time of Birch's visit, the laboratory contained one of the world's highest strength steady magnetic field devices (10 kilogauss in a volume of several cubic centimeters).

Birch rolled up his sleeves and managed to be involved with this group in his first four research papers-all first rate and written in French. He reported for the first time the paramagnetic properties of potassium cyanide, and in a paper first-authored by Weiss, Birch provided initial data for the magnetic saturation field value for metallic Co-Ni alloys. A separate paper on the magnetic moment of Cu^{++} ions was solely authored by Birch. Birch's appetite for research had been awakened by this interlude in Strasbourg, and he later wrote that the experience resulted in his decision to become a research scholar.[1]

FRANCIS BIRCH AND PERCY BRIDGMAN

Recognizing that some of the world's most exciting new research on materials was being conducted in the physics department at Harvard by the already well-known Hollis Professor of Mathematics and Natural Philosophy Percy Bridgman, Francis applied to the graduate school at Harvard. On acceptance as an assistant in physics, Francis immediately started to work in Bridgman's pioneering high-pressure laboratory. Work was supported in this laboratory by grants of only a few hundred dollars from the Sanford and Bache funds of the American Academy of Arts and Sciences and the National Academy of Sciences. This laboratory, which produced a 1946 Nobel Prize in physics for Bridgman, was old-fashioned even by the standards of the 1930s. Francis adapted Bridgman's pressure system by adding a small metal chamber fitted with tiny metal tubes and wires and instrumented with Wheatstone bridges, long slide-wire variable resisters, galvanometers, and dial gauges. Thousands of measurements were read by eye, recorded by hand in notebooks, plotted, and smoothed graphically.

On starting graduate school, Francis traveled to the office of Arthur L. Day, director of the Geophysical Laboratory of the Carnegie Institution of Washington. Although Day undoubtedly suggested that Birch should conduct research on geophysical problems at the Geophysical Laboratory, Birch instead followed the advice of his mentor Bridgman, who suggested that he should measure for the first time the liquid-vapor critical point of mercury. Unsuccessful previous measurements for the critical point indicated conditions above 500 bars and 1000° C were required to obtain such data for mercury. Pressure apparatus to reach 10 kilobars were available in the 1930s in Bridgman's laboratory, but the expected temperature requirements for achieving mercury's critical point was a formidable 1400° C, well above Bridgman's normal range. Birch obtained technical advice from the Geophysical Laboratory staffs pioneering researchers F. H. Smyth, Leason Adams, Ralph Gibson, and Roy Goranson. He very carefully machined his own high-pressure chambers in Jefferson Laboratory in the physics department. The advice proffered both by the Geophysical Laboratory and Bridgman was well taken, and Birch's fifth paper reported the results of his Ph.D. thesis project. This was a measurement of the critical point of mercury. He obtained the values 1460 ± 20° C and 1610 ± 50 bars and solely authored this work in *Physical Review* in 1932. This was a scientific home run! Some thirty years later several groups repeated these measurements and newer data agreed closely with Birch's original study.

In 1930, during the period that Birch was working on his Ph.D., the Committee for Experimental Geology and Geophysics was formed at Harvard. This effort was led by Reginald A. Daly, a leading Harvard geologist, and included physicist Percy Bridgman, astronomer Harlow Shapely, geologists L. C. Graton and D. H. McLaughlin, and chemist G. P. Baxter. This committee, empowered to raise funds and advance the field of experimental geology and geophysics, exists to this day. In 1931 geophysics at Harvard thus began with the appointment of Professor Don L. Leet as director of the Seismological Station at Harvard. A second appointment went to W. A. Zisman, another Bridgman student. Zisman was appointed to study the elastic properties of rocks. Their complexity, however, was not to Zisman's liking, and he resigned in 1932. At the depths of the American and European economic depression, the position of research assistant in geophysics was offered to Birch, who readily accepted the job. This occurred several weeks after another graduate of Harvard, Franklin D. Roosevelt, began twelve years of service as President of the United States.

In 1933, having accumulated laboratory experience under Percy Bridgman, one of America's premier experimentalists, Francis Birch was asked to lead the geophysics program at Harvard. In retrospect, Harvard's geology department at that time was one of the most forward looking in North America.

Francis Birch married Miss Barbara Channing of Cambridge, Massachusetts. She was a collateral descendant of the American theologian William Ellery Channing. She brought a sense of warmth, family, and intellectual and cultural affairs to the household. She was Birch's lifelong mate.

They lived in Cambridge. The Birches had three children, Anne Birch (now Mrs. Harry Hughes) and Mary Birch who both reside in Cambridge. Their son Francis (Frank) S. Birch is professor of geophysics at the University of New Hampshire.

GEOPHYSICS AT HARVARD

Birch's first geophysics papers coauthored with R. R. Law and R. B. Dow, dealt with the compressibility of rocks and glasses. They used the apparatus from Birch's Ph.D. thesis project on mercury and adapted Bridgman's slide-wire method to measure length changes to 10 kilobars and 400° C simultaneously.

Soon after, Birch broke new ground in his study of the shear velocities of rocks as a function of temperature and pressure. All previous shear moduli measurements at high pressure employed measurement of angular strain of samples wound into the shape of helical springs. This technique was clearly impossible to apply to rock.

Assisted by J. Ide and D. Bancroft, Birch[2] developed a new type of torsional-bar resonance experiment that, for the first time, resulted in the determination of the shear modulus and quality factor in a wide range of rocks and glasses within a 4-kilobar gas apparatus that could also be heated to 100° C. This was no trivial experiment to operate in the 1930s. It would be a difficult experiment even today, and was state of the art in the 1936-37 time frame, when pressure was measured with manganin coils insulated with silk fiber.

Birch then constructed the first theoretical mineral physics models of the lower crust and compared these with velocity models from crustal refraction seismology. Birch found that the lower crust's elastic properties were compatible with several granites and gabbros he had tested.[3] This work probably demonstrated to Birch a serious deficiency in definitive information on the actual temperature at crustal depths. It probably was obvious to Birch that the near surface thermal gradients, which Birch and Clark[4] measured before the war, but published after World War II, were too high (50° C/km). Such values would suggest implausibly high temperatures deep in the Earth.

The clouds of war were ominous and it turned out that Birch did not seriously grapple with the question of the thermal state of the Earth until much later. In 1937 Francis Birch was promoted to assistant professor of geophysics at Harvard.

Birch obviously was concerned about how one could extrapolate data taken to a few kilobars to the millions of bars in the Earth's interior. He discovered the then recent theory of finite strain compression developed by applied mathematician F. D. Murnaghan, which was published in the *American Journal of Mathematics* in 1937. This theory appeared to provide a route to extrapolating low-pressure compression data to high pressure. Much of these data had been produced by Bridgman in a long series of papers starting in 1909. In a brilliant paper published in the *Bulletin of the Seismological Society of America* in 1937, using the framework of Murnaghan's finite strain theory, Birch derived equations for the extrapolation of seismic velocity into the upper and lower mantle; he found such models agreed closely with seismically determined compressional and shear velocity versus depth data recently obtained by Bullen, Jeffreys, and Gutenberg. Although Birch assumed a primitive isothermal Earth model, this promising paper provided the first complete, self-consistent, elastic and density model of the Earth's mantle.

Before the United States became engaged in World War II, Birch and Clark[5] published the first careful, definitive study of the thermal conductivity of a wide range of igneous rocks. They found striking differences in the thermal behavior of the thermal conductivity of feldspar-bearing rocks, which increased in conductivity with temperature. This contrasted with the usual decreases in conductivity with temperature found in many other rock types. They then went on to calculate the heat flow from continental regions and obtained a value close to modern averages. All this work and virtually all of his measurements on the elastic properties of rocks, and all the other then existing physical properties of Earth materials were reported in a 21-chapter data collection, the first edition of the *Handbook of Physical Constants* published by the Geological Society of America in 1942.[6] Birch was the principal editor of this remarkable data collection, and he himself wrote contributions to eight of the twenty-one chapters. This handbook was the first collection of mineral physics data and it is still being used, although later versions have been published.

WORLD WAR II AND LITTLE BOY

In 1942 Francis Birch took a leave of absence to contribute his experimental talents to developing proximity fuses at the Radiation Laboratory at the Massachusetts Institute of Technology. He remained in Cambridge only a year. On the basis of prior ROTC training at Harvard and his promotion to associate professor in 1943, he accepted a commission in the U.S. Navy as a lieutenant commander. His first assignment was at the Bureau of Ships in Washington, D.C., where he spent only ten months. To his great surprise, and probably because of his research experience with Percy Bridgman and his friendship with J. Robert Oppenheimer, who knew Francis from his Harvard years, he was assigned to the Manhattan Project. This was an Army Corps of Engineers project formed in June 1942, and was designated as a special district with initial headquarters in New York City. The Corps of Engineers, Manhattan District, evolved into the Manhattan Project, which encompassed all aspects of the United States's secret effort to build atomic weapons: all research and development programs, processing of materials, and fabrication of components in localities all over the United States, as well as assembly and delivery of the military weapons to the Army Air Force.

Francis Birch was ordered to report to the Los Alamos Ranch School some 30 miles northwest of Santa Fe, New Mexico. In 1943 he packed his spouse Barbara, his three children, and himself into the family car and drove from Washington to Los Alamos.

Francis Birch was assigned to a gun development group in 1943. Initially, he worked under the leadership of Navy Captain W. S. Parsons to develop a fission device. This device conceptually would launch a projectile made of some tens of kilograms of ^{239}Pu into a ^{239}Pu target at the end of a 2-meter-long gun. The goal was to achieve projectile velocities in excess of 1 km/sec. This simple gun concept of mechanically and rapidly assembling a greater than critical mass of fissionable ^{239} Pu was considered more likely to succeed than the then untested concept of explosively imploding an assembly of ^{239}Pu to greater than critical densities. This later led to the atomic device named Fatman, first demonstrated at the New Mexico Trinity Test of July 16, 1945. Most of the research and development at Los Alamos was directed not toward gun assembly, but to design and building of implosive devices. The coterie of famous physicists, mathematicians, and chemists-many émigrés from Europe assembled from all over the United States-was concentrating on the implosive plutonium design. The gun concept was considered less difficult to design and construct.

The goal of the Ordinance Division Section to which Birch was assigned was the development of a gun that would weigh only 1 ton and have a muzzle energy that was comparable to then existing guns having five times more mass. Thus, special high-strength alloy steels were used to fabricate barrels to test ordinary uranium dummy projectiles and targets as mechanical proxies for enriched ^{239}Pu and ^{235}U.

In the period April-July 1944 the very high neutron emission rate of the first reactor-produced ^{239}Pu was discovered and the 1-km/sec gun method was discovered to be unworkable and was abandoned for use in developing a plutonium bomb in favor of the implosion Fatman design. At that point, all the gun development activities were redirected towards a ^{235}U-fission bomb. The revised uranium gun program (code name Little Boy), started in February 1944, was now led by newly promoted Commander Francis Birch. Testing began in March 1944.

Before the guns for the uranium bomb arrived in Los Alamos in October 1944, Birch continued to use unenriched uranium for tests, first on a subscale. In December 1944 high-strength alloy-steel gun tubes constructed by the Navy were delivered and test fired several times. After each gun test was fired at Los Alamos, the uranium gun required massive force for disassembly. A D-8 Caterpillar bulldozer was used to pull the gun apart. Birch actually used the Los Alamos gun device that went into the Hiroshima bomb in four previous tests at Los Alamos.

In February 1945 the amount of ^{235}U required to produce excess of a critical mass was determined and the actual first bomb began to be fabricated. It was composed of the entire supply of enriched ^{235}U in the United States. Thus, *all* the ^{235}U produced in the United States (64.1 kilograms) was used in the ^{235}U Little Boy bomb. There would be no test prior to combat!

The components were cast between June 15 and July 3, 1945, under Francis Birch's watchful eye. The Little Boy bomb case and projectile were shipped to Tinian Island in the Marianas on the U.S. Navy cruiser *Indianapolis.* Birch accompanied the ^{235}U target, which for safety reasons was flown in three parts from Kirtland Air Force Base, New Mexico, to Tinian; it arrived on July 28, 1945. On August 5 Francis Birch supervised the assembly of the Little Boy device and loaded it into the specially outfitted B-29 heavy bomber named Enola Gay. On August 6, 1945, this bomb was dropped on Hiroshima. It was hoped by many that Japan would immediately surrender. A blood bath was expected if the Allies were forced to invade the Japanese islands. Japan did not surrender. On August 9 Fatman, the ^{239}Pu bomb, was dropped on Nagasaki. The second explosion proved decisive to the military leadership in Tokyo and Japan surrendered within a week. World War II ended. After receiving the Legion of Merit from the U.S. Navy, Birch was mustered out of the Navy, and he returned to Harvard in late 1945. He was promoted to full professor in 1946.

HEAT FLOW AND RADIOACTIVITY OF THE EARTH

Birch's objective was to understand how the temperature versus depth in the Earth was controlled by radioactivity and thermal conductivity. In the post-World War II years, Francis Birch supervised a comprehensive program to understand the thermal regime of the continental crust of the Earth. Boreholes were drilled specifically for heat flow measurements. Measurements of the in-situ radioactivity of rocks, the Earth's thermal gradient as sampled within boreholes, and thermal conductivities in which a large number of rock samples in the laboratory (taken from these same boreholes) were undertaken. This campaign had its roots in the pre-war thermal conductivity technique development of Francis Birch and Harry Clark. Temperatures versus depth data were initially supplied to Birch by the Gulf Oil Company from its wells in the West Texas Permian Basin. Taken with conductivity data measured on samples, also supplied by Gulf, heat flow for that region was reported by Birch and Clark.[4]

In 1949 Birch was promoted to Sturgis Hooper professor of geology, a chair that was formerly held by Reginald A. Daly, the geologic mentor of high-pressure research at Harvard. The following year, 1950, Birch was elected to the National Academy of Sciences.

Later, with the help of Harvard postdoctoral fellows and students Robert F. Roy, Henry Pollack, David D. Blackwell, Edward R. Decker, W. H. Diment, Arthur Lachenbrook, and major National Science Foundation support, Birch and his coworkers discovered that various broad geological provinces (e.g., Eastern United States; Basin and Range; Northern Rocky Mountain - Columbia Plateau, Southern Rocky Mountain, Sierra Nevada; Colorado Plateau - Wyoming Basin - Middle Rocky Mountains; and the Pacific Coast) had heat flows that were uniquely described in a province by a relation that was simply given as

$$\mathbf{Q = a + bA}$$

Where Q is the surface heat flow, A is heat production per unit volume in the surface rocks, and a and b were parameters characteristic of the geological province. This simple empirical result implied a decreasing radioactivity in rocks downward into the crust. Because the radiogenic elements U and Th are also "incompatible," meaning that they have large ions that are not easily taken into major rock-forming silicate mineral lattices, the radioactivity of the crust is concentrated upward. This concept was accepted by the Earth science community. Moreover, Roy et al.,[7] in a publication celebrating Francis Birch's accomplishments upon his retirement from active teaching, showed that the measured radioactivity per unit volume correlated with the depth that crustal igneous rock masses had been eroded in a given region.

ELASTICITY AND CONSTITUTION OF THE EARTH'S INTERIOR

Francis Birch's most important paper (1952) appeared in a 57-page article in the *Journal of Geophysical Research* in 1952 and contained an enormous quantity of new material and analyses, as well as several predictions on the nature of the Earth's interior. Birch wrote easily and well, reflecting his undergraduate stint as a contributor to of the *Harvard Crimson.*

Birch first generalized the Williamson-Adams[8] equation, which relates compressional and shear velocity profiles to density for a uniform phase adiabatic region within a planet. Birch showed how this relation could account for differences between the actual thermal gradient (increase in temperature with depth) versus that expected upon adiabatic compression of rock in a self-gravitating field. He derived a new form of the finite-strain, pressure-density equation of state following the ideas of Murnaghan. This equation is now widely used in Earth and planetary science, as well as in material science, and is referred to as the Birch-Murnaghan equation of state. Birch showed that this equation described the compression behavior of a wide range of materials reported earlier by Bridgman. Moreover, for the first time, he used the Debye theory to estimate the thermal expansion coefficient of materials at high pressure, a quantity very difficult to actually measure, and then derived complete equations of states specifying density as a function of pressure and temperature, and the internal gravitational acceleration in the Earth from the seismic velocity profiles versus radius. New seismic profiles had been recently refined by K. Bullen, B. Gutenberg, and H. Jeffreys for both the mantle and the core. The paper came to a number of conclusions and inferences, all of which appear to have been basically correct. These were:

1. The Earth's mantle between the depths of 900 and 2900 km is uniform in phase and composition with elastic properties similar to that of the close-packed oxides. Except for the close-packed oxides $A1_2O_3$ (corundum) and MgO (periclase) at the time of Birch's paper, virtually nothing was specifically known about the silicate phases of the lower mantle. Moreover, Birch suggested that the incompatible elements (e.g., Th, K, and Ca budget of the Earth) were concentrated upward in the crust and in an upper mantle eclogite layer.
2. The liquid outer core of the Earth was less dense (by about 10%) than expected for pure liquid iron at the pressures and temperatures of the liquid core, and he suggested that the liquid core contained lighter alloying elements (S, C, Si, or O) in addition to molten iron.
3. The inner core was solid, nearly pure iron. The solidity of the inner core was confirmed by A. M. Dziewonski and J. F. Gilbert[9] and Anderson et al.,[10] using the free-oscillation frequencies of the Earth.

It is interesting in spite of the fact that most colleagues and students considered Birch to be a very serious man, he started the introduction of the seminal 1952 paper with a little joke describing the way he felt about the magnitude of unknowns with regard to the nature of the materials under high pressure and temperature in the Earth's interior:

> Unwary readers should take warning that ordinary language undergoes modification to a high-pressure form when applied to the interior of the Earth. A few examples of equivalents follow:

High Pressure Form	Ordinary Meaning
Certain	Dubious
Undoubtedly	Perhaps
Positive proof	Vague suggestion
Unanswerable argument	Trivial objection
Pure iron	Uncertain mixture of all the elements

A. E. Ringwood, later at Australian National University, spent several months of 1957 visiting Birch's laboratory. This visit motivated him to conduct his first experiments demonstrating that the transition from olivine to spinel structure occurred at relatively low pressure in Fe_2SiO_4.[11] Later, Ringwood spent much of his career working on the phase transitions of the mantle, first at lower pressures using analog germanates and subsequently exploring phase transitions in silicates.

Birch continued his research over the next twenty years and produced seminal papers on the energetics of core formation and the Earth's thermal state. He conducted laboratory and field work on thermal gradients and conductivity measurements in crustal rocks. He also performed elastic constant measurements on important rock-forming minerals and carried out an extensive series of measurements on rocks with the assistance of several graduate students, including Gene Simmons, who studied the systematics of shear-wave velocities using ultrasonic methods.[12]

Birch's two papers on compressional wave velocities in rocks, published in 1960 and 1961 in *the Journal of Geophysical Research,* are landmark works, and they resulted in a second linear relation now called Birch's law. This describes the variation of compressional wave velocity V_p of rocks and minerals of a constant average atomic weight with density *r* as:

$$V_p = a + b$$

The success of this simple formula was later explained theoretically by Birch's graduate student Thomas J. Shankland[13] and independently by Don L. Anderson.[14]

Geologist William Brace was another visitor who learned static high-pressure methods from Francis Birch in the early 1960s. Brace went on to study rock failure and other physical properties of rocks in a laboratory he later established at MIT. Another student, Peter Bell, established (with colleague David Mao) the world's premier diamond cell high-pressure laboratory at the Carnegie Institution of Washington in the late 1960s.

Birch became interested in the theory of Ramsey,[15] who suggested that the core-mantle boundary of the Earth was not due to compositional differences between a rocky mantle and a metallic core, but resulted from metalization *and melting* of mantle silicates *at the same pressure.* Birch found this implausible and was thus motivated to get involved in the explosive shock compression measurements then being conducted on a wide range of materials at Los Alamos National Laboratory. Birch supplied mineral and rock samples and sample analyses, as well as much encouragement, to Robert G. McQueen and his coworkers at Los Alamos in the 1960s in their application of shock studies to Earth materials. McQueen demonstrated decisively that all silicates underwent major shock-induced phase changes. Later, these were demonstrated to be accounted for by transition from tetrahedrally to octahedrally coordinated silicon with oxygen. Moreover, he showed that silicates emphatically *did not* transform from a density of approximately 5.8 to 10 g/cm^3 at a pressure of 133 GPa (which corresponds to the core-mantle boundary pressure) as Ramsey's theory required. The shock-wave data also supported the conclusions Birch had reached earlier in his 1952 paper about the nature of the lower mantle and the outer and inner cores of the Earth. Birch encouraged me to conduct shock-wave experiments, although, in spite of his experience developing the Hiroshima gun bomb, he did not immediately grasp how useful light-gas guns could become in shock-wave research.

As a member of the Harvard faculty and chairman of the Experimental Geology Committee, Birch sponsored and supported many graduate students, who later made important contributions to Earth science. Early in his Harvard career, Birch supported two students, David T. Griggs and George F. Kennedy, who were doing research as members of the Society of Harvard Fellows. Before World War II Griggs worked on the deformation of rocks under pressure in Bridgman's laboratory, and after the war he continued these studies at the University of California, Los Angeles. Just after the war, Kennedy worked on moderate pressure geochemical research, including the high-pressure and temperature properties of water and CO_2. Kennedy later joined Griggs and moved to UCLA.

After the war, Birch lectured in several courses in geophysics. He was an extremely effective teacher. One year at his last lecture on "Physics of the Earth," he summed up the topics covered so well that the class gave him a standing ovation.

During his career, Birch supervised the doctoral thesis of some fifty graduate students. As might be expected, the theses covered all aspects of the physical properties structure and processes in the Earth. Contributions to his retirement Festschrift and symposium "The Nature of the Solid Earth" included papers from his former graduate students D. D. Blackwell, S. P. Clark, E. C. Decker, W. H. Diment, E.

Herrin, R. F. Roy, and E. C. Robertson. Robertson was the principal editor of the Festschrift book.

PROFESSIONAL SERVICE AND RECOGNITION

Birch officially retired from Harvard in 1974, but he continued to conduct research. He spent more of his time with his family and took summer vacations at his farm in nearby Kensington, New Hampshire.

He received the Geological Society of America's Arthur L. Day Medal and Penrose Medal in 1950 and 1969, respectively, and served as president of the society in 1963-64. The American Geophysical Union honored him with its highest award, the William Bowie Medal, in 1960. President Lyndon Johnson presented him with the National Medal of Science in 1967. In 1968 he shared the Vetlesen Prize with Sir Edward Bullard. This prize was highly symbolic, as Bullard pioneered the measurement of Earth heat flow beneath the oceans, whereas Birch pioneered heat flow measurements on the continents. The University of Chicago and Harvard conferred honorary degrees to Birch in 1970 and 1971, respectively. The Royal Astronomical Society's Gold Medal was awarded to Birch in 1973. Birch received his last major award, ironically named the Bridgman Medal, from the International Association for the Advancement of High Pressure Research in 1983.

I thank Mrs. Francis Birch, Francis S. Birch, Edward R. Decker, Eugene C. Robertson, Hatten Yoder, Jr., Don L. Anderson, John Reynolds, David D. Blackwell, Gene Simmons, J. B. Thompsen, F. R. Boyd, and Susan Yamada for assistance in collecting materials on F. Birch's life and scientific achievements, as well as helpful comments on this memoir.

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G. Breit

Gregory Breit

July 14, 1899 - September 11, 1981

By Mcallister Hull

For nearly fifty year, Gregory Breit was a leading figure in the development of physics in the twentieth century. John Wheeler in *Some Men and Moments in Nuclear Physics* wrote, "Insufficiently appreciated in the 1930's, he is today the most unappreciated physicist in America." This was written in 1979, when Gregory was in physical decline, and he probably never saw it, but if public recognition was slight (but by no means absent), he was appreciated very well (in spite of a difficult personality) by his students, collaborators, and colleagues. The range of his interests and duration of his active career made this cadre a large one.

Trained as an electrical engineer, Breit did his early work in radio, including the definition of the characteristics of early tubes and finite coils. The most important of this work, with Merle Tuve, was the use of radio to demonstrate the existence of the postulated ionosphere by receiving return signals of a pulsed radio beam sent from the earth's surface. Ranging with a pulsed signal is, of course, the principle of radar. He also inspired and worked with the production of high voltages to accelerate charged particles (protons) to use as probes of the nucleus—the first manmade probes in the United States (Cockroft and Walton at Cambridge, England, had a beam of protons and deuterons first, but at a lower voltage than the Carnegie team). With Tuve and Odd Dahl, he demonstrated the soundness of the betatron principle. Breit's work at Wisconsin included his organizing the infant theory of quantum electrodynamics (and studies of photon-photon interaction), early studies of the nucleon-nucleon interaction, and with Eugene Wigner the theory of nuclear resonances, which continues to be the basis for understanding many nuclear reactions.

During the second world war, Breit recognized very early that it would be sensible not to publish basic studies of nuclear properties, especially those of uranium and plutonium, for he among others understood the military (as well as energy) possibilities of a chain reaction in uranium. This caution resulted in the voluntary withholding from publication of many important papers until after the war. His own war work began with the organization of neutron studies that developed into the laboratory at Los Alamos—under Oppenheimer rather than Breit, who had gone off to the Naval Ordnance Laboratory to study degaussing of ships (for which he invented the magnetic extrapolator) as a defense against magnetic mines, and the Ballistic Research Laboratory of the Army to work on proximity fuses, exterior ballistics, and fire control. After the war, Gregory returned briefly to Wisconsin to take up his studies of nucleon properties and nuclear reactions. He transferred to Yale in 1947, with his advanced graduate students—and one undergraduate (me). At Yale, postdoctoral associates were added to the students who came from Wisconsin, and a few new graduate students joined what came to be called the "Breit group."

In addition to his prolific personal research, Breit led and participated in the work of members of the group. He and the members:

- Studied hyperfine structure of atomic levels; nuclear magnetic moments; the isotope shift; photo-disintegration of the deuteron; polarization of Bremsstrahlung radiation; Coulomb excitation; and semi-classical treatments of quantum mechanical calculations.
- Worked on Coulomb wave functions for calculating nuclear reactions (before high speed computers made the tables unnecessary);
- Initiated some of the first computer-aided calculations of the phenomenological nucleon-nucleon interaction, where it was shown that the strong force is charge independent, has a sharp repulsive core, and exhibits spin dependence;
- Showed that nucleon-nucleon scattering cross-sections could be expanded in phase shifts (and that the phenomenological force could be used to calculate those phase shifts); and
- Invented (theoretically) and studied heavy ion physics extensively.

PERSONAL HISTORY

Gregory Breit was born July 14, 1899, in Nikolayev, Russia, some 100 kilometers northeast of Odessa. His parents, Alfred and Alexandra Smirnova Breit Schneider, operated a textbook business until Alexandra died in 1911, and the business was sold. Alfred emigrated to the United States in 1912, leaving Gregory and his sister Lubov in the charge of a governess while Gregory attended the School of Emperor Alexander in Nikolayev. In 1915 Alfred instructed his children to come immediately to the United States, and with their governess they traveled by train to Archangel and then by ship to New York. They landed on July 30, 1915. Their father, now Alfred Breit, assisted them with entry formalities and took them to Baltimore, where he was living.

John Wheeler relates a story told to him by Lubov that she and Gregory were vacationing on the sea when the call to leave Russia came, and they 'came as they were.' For Gregory this meant dressed in a sailor suit with short pants; he was still wearing it when he enrolled in Johns Hopkins (at age sixteen!). Wheeler attributes some of Gregory's subsequent reticence in personal relationships to the ragging he took at the hands of his classmates for his dress—unlikely, perhaps, as the definitive reason, but a contributing influence? My wife Mary, who was one of the few women Gregory was comfortable with (she was the only person allowed to cut his hair when he became ill), attributes his personality to "middle European genes, especially prominent in Ukrainians" (she is first generation Czech).

There was an older brother, Leo, who had escaped the tsar's army through Turkey and then practiced medicine in Maryland. A deserter sought by the tsar's agents, he gave as little information about himself as possible, which, along with the dropping of "Schneider" from the family name, caused Gregory some difficulties years later when he needed clearance to do war work. Gregory, on the other hand, responded to Russian recruiters in 1918 and attempted to join the Russian Army. He was rejected because he couldn't pass the physical. He had a lifelong hatred of Communist Russia: much stronger than the intense distrust that most of us had.

Gregory was supported by scholarships as he continued his formal education and was awarded three degrees by Johns Hopkins: the A.B. in 1918, the A.M. in 1920, and the Ph.D. in 1921 (when he was twenty-two!). His publications began in 1920 (some with E. O. Hurlburt), and his dissertation (Joseph S. Ames was his advisor) on the distributed capacity of inductive coils was published in 1921. For those, like me initially, who wonder at this apparently trivial topic for a Ph.D. thesis, I suggest you look up the paper (*Phys.*

Rev. 17:649). It is a masterly piece of applied mathematics (a skill Breit demonstrated for the rest of his professional life). He was a National Research Council fellow at the University of Leiden in 1921-22 and at Harvard University in 1922-23.

Gregory married Marjory Elizabeth McDill on December 30, 1927, in Washington, D.C., and acquired a stepson, Ralph Wycoff, from Marjory's previous marriage. He had no biological children.

Breit knew and worked with the physicists in his field with a degree of comprehensiveness impossible today. At the symposium in his honor at Yale in 1968, over 200 of his colleagues and former students attended from around the world. Among the notable speakers were John Archibald Wheeler, Henry Margenau, Isidor I. Rabi, Victor F. Weisskopf, Hans A. Bethe, Eugene P. Wigner, D. Allan Bromley, Vernon W. Hughes (the latter two organized the symposium), Gerald E. Brown, Raymond G. Herb, and Merle A. Tuve. I also spoke, and had not been in so much fast company since Los Alamos!

Gregory Breit was elected to the National Academy of Sciences in 1939 and to the American Academy of Arts and Sciences in 1951. He was a fellow of the American Physical Society, Physical Society of London, Institute of Radio Engineers, and the American Association for the Advancement of Science. He was a member of the American Mathematical Society, American Geophysical Union, Washington Academy of Science, the Army Ordnance Association, Sigma Xi, and Phi Beta Kappa.

Gregory died in Oregon, where he had gone with Marjory in 1973, finally to retire. When Yale's mandatory retirement policy sent Breit from New Haven in 1968, I had gotten him an appointment as distinguished professor at Oregon State University, where I was chair of physics. However, one of his former postdoctoral associates, Moti Lal Rustgi, had, through the department and President Meyerson, beat me to it, and he went to State University of New York at Buffalo (as it happened, the same friend nominated me as chair at SUNY Buffalo shortly afterward, so I became Gregory's department chairman!). Marjory wished to spend her last years near her son and daughter-in-law, Ralph and Faith Wykoff, and her grand children, so they moved into a retirement home near Salem. Gregory, as vigorous a man as I had ever known, began to decline in Buffalo, and was not professionally active in Oregon. His death in 1981 called for a two-column obituary in *The New York Times.*

Noting that I am writing nearly eighty years after Gregory's first publication, the reader will understand that it has been difficult to get recollections from early colleagues. Anne Herb recalls that the late Ray Herb admired Gregory above most of his colleagues at Wisconsin, and that in tapes made before his death Ray had recalled with pleasure Gregory's interest in the work he was doing with accelerators. Norman Heydenburg has "fond memories" of Breit at New York University and Wisconsin where Norm was a postdoctoral fellow. Again, the subject (in part) was accelerators and nuclear physics.

Charles Kittel followed Gregory to Washington during the war, and his work on degaussing influenced his decision to spend his distinguished career in condensed matter physics. He tells as 'folklore' that Gregory and I. I. Rabi came from Russia on the same boat, and thus knew each other long before their collaboration. Glenn T. Seaborg remembers 'many important contacts' with Gregory in 1941 as the Uranium Committee was formed (more on this later). John Wheeler, who has already been quoted here, joined Breit as a National Research Council fellow at New York University when Norm Heydenburg was there. Wheeler especially remembers Breit's "kindness" in crediting his work with joint authorship on papers (I do not consider it kindness. Breit was more conscientious than most in giving due credit, and equally demanding that it be given by others) and in helping secure a fellowship renewal that made it possible for Wheeler to work with Niels Bohr. This also was typical of Breit. He was always interested in his younger associates, and regularly helped them get positions when they left him —as Norm Heydenburg also remembers. Wheeler, in this context, credits Gregory with "saving" Eugene Wigner for Princeton by getting him an appointment at Wisconsin when Princeton was moving too slowly (Willis Lamb tells me that Wigner always said that Princeton fired him!).

The collaboration between Breit and Wigner was significant for the development of nuclear physics (see below), and Wigner returned to Princeton when it "mustered the resolution and funds" to make him an appropriate offer. Willis Lamb recalls that Breit was a referee of an early paper of his and took the trouble to write directly to Willis with useful comments and encouragement—a rare instance of a referee abandoning the customary anonymity, and again demonstrating Breit's concern for the development of young physicists. For a later generation of associates, Arthur Broyles refers to Breit as "one of the most sincere and conscientious men I have known," and recalls that Gregory interrupted his work to help Arthur decide between opportunities for postdoctoral work. I do not recall a single student or postdoctoral fellow with the group who ever left without a suitable position.

Gregory had a dark side that was the source of legends even from the New York University days. He was formally polite in the European way (the result of his upbringing in the first decades of the century!), and usually apologized—

quite sincerely, I believe—after an outburst of temper. Wheeler recalls no personal incidents during his association with Breit, although he heard of them from others. I experienced only one, very early in our working relationship (I was still an undergraduate). He misinterpreted a comment of mine and when he blew up, I said something like, "I think I'll return when you are feeling better. Please call me." I walked out and back to my office. He duly called, apologized for the outburst (even more contritely when I showed him the source of his misinterpretation), and never raised his voice with me again, although we had our differences many times. We became very close friends over the succeeding years. Others of my colleagues were not so lucky. Gerry Brown, who remembers Breit as a second father, was regularly a target, and I was present when Gregory took the hide off a graduate student who had wished him 'a good talk' at a meeting: of course his talk would be good! There is no point in detailing more examples: they occurred regularly, and were simply a fact of life for his students (and on occasions his colleagues. Allan Bromley recalls mediating a heated 'discussion' between Gregory and Henry Margenau on the occasion of Gregory's seminar. Each thought the other's talk was the 'worst ever heard.' Having just reread them, I fear both were correct!). Wheeler thinks that this irascibility was at least a partial cause of Breit's lack of recognition commensurate with his accomplishments.

Gregory's devotion to his student's intellectual development and personal welfare was equally legendary. He was available at any time for consultation, and if a student was shy, he would be invited in for a chat. Weekly group lunches were remembered by Jack McIntosh as times of terror in anticipation of Gregory's asking someone a difficult question, but we learned a great deal, including how to think on our feet! Frequent "parties" at his home, set up by Marjory (who also entertained the wives of group members—separately) were opportunities to talk physics in general. Gregory was incredibly well informed. He received hundreds of preprints a month, read most of them, and shared them with the group members according to current interest. Bromley thinks he retained everything he read, including the location (journal reference, page number, location on the page!) of the source. The sessions at Gregory's home also were opportunities for us to meet the great physicists of the time. Jack McIntosh especially remembers Werner Heisenberg, for example (as I do; someone had told him I had been at Los Alamos during the war, so the head of the 'counter project', as we called him then, asked a number of questions, most of which I couldn't answer for security reasons!).

Gregory was equally solicitous of our health. Any ailment was occasion for concern and advice. Gary Herling, one of Breit's later students, recalls being advised that exercising "a few times a week for an hour is better than several hours every few weeks": sound advice according to current practice in the fitness world!

Gregory's recreations were exercise and reading (other than physics, I mean). He had a canoe on Lake Mendota in Madison, as Anne Herb recalls. Gerry Brown has even better reasons to remember that canoe. Gregory once took him and another student out for a paddle. Gregory seemed to wish to use the leverage of the stern position to turn the canoe away from the course, and the other student soon recalled an appointment, and was put ashore. Gerry then determined to keep the course against Breit's pull, and did so. Nothing was said at the end of the afternoon, but twenty-five years later Gregory recalled the trip to Gerry, and remarked "I saw then you had some stuff in you." The canoe didn't make it to New Haven, but, as McIntosh recalls, the group "was hiked" regularly, with "Mac or Bob (Gluckstern) leading with Breit, and the rest trying to keep up." On one of these occasions, Breit sat down, removed his shoes, and changed his socks, advising the rest of us to do likewise. As usual, he had our physical well being at heart! While Jack failed to take the advice, he recalls the faces at the windows of nearby houses staring out at this extraordinary sight.

Breit's recreational reading included historical fiction, as I know because when I was hospitalized in Buffalo once, he sent over several Hornblower novels. This matched my own tastes, and I have since read all of the series. But we never talked about such things. Physics was always on his mind, and like most driven and creative persons, his specialty provided most of his focus, and the line between work and pleasure was never a clear one.

His students, collaborators, and colleagues over nearly sixty years universally characterize him as a brilliant, informed, dedicated physicist, equally devoted to the subject and to developing and encouraging younger persons for the field. If he was sometimes difficult, the intellectual rewards for working with him were worth putting up with his moods. I, of course, am biased. A bond of affection grew between us that lasted all his life and is still fondly remembered by me.

PROFESSIONAL HISTORY

Breit's professional positions began with an appointment as assistant professor at the University of Minnesota (1923-24). He then began an extended association with the Carnegie Institution of Washington as a mathematical physicist (1924-29, with a residency at the Technische Hochschule, Zurich, in 1928) and as a research associate (1929-44). He was professor of physics at New York University (1929-34) and the University of Wisconsin, Madison (1934-47), with a visiting membership in the Institute for Advanced Study, Princeton (1935-36). During the war, he worked in the Naval Ordnance Laboratory (1940-41) and at the Metallurgical Laboratory, University of Chicago (1942). He was a member of the Applied Physics Laboratory, Johns Hopkins University (1942-43) and was head physicist at the Ballistic Laboratory, Aberdeen Proving Grounds (1943-45). His return to the Madison campus was short-lived, as he accepted a professorship at Yale in 1947. At Yale he was given the first Donner professorship in 1958, and he retired from that position in 1968 (at Yale's mandatory retirement age of sixty-eight!). He went to the State University of New York at Buffalo, from which he retired to private life in Oregon in 1973. His professional honors, in addition to membership and fellowships in a number of societies (already noted), included the award of an honorary doctorate of science by Wisconsin (1954), the Benjamin Franklin Medal in 1964, and the National Medal of Science in 1967. At various times in his career he was associate editor of *Physical Review, Proceedings of the National Academy of Sciences,* and *II Nuovo Cimento.*

A discussion of Gregory's research is made difficult by at least two circumstances. He worked in many areas and he returned to most of them over many years. He published, alone and with colleagues, some 320 papers. My list probably is not complete. I've added two references since I started this memoir! Since I may include only twenty-five items in an accompanying bibliography, you can sense the difficulty I have in doing justice to the work of this premier physicist. However, two review volumes deal with Breit's work. Volume 41/1 of the *Handbuch der Physik* (cited as HP and a number representing the order of the papers as they occur in the volume) was written by Gregory and some of his colleagues and was published in 1959, and a symposium in Breit's honor was held in New Haven on the occasion of his retirement from Yale. The proceedings of the symposium were published in 1970. The symposium volume *Facets of Physics* (cited as FP and a chapter number) has the best bibliography of Breit's work that I know. Between these two, one can find most of the items he published, as well as a hint of the breadth of his interests and the influence his work had on others.

The first research Breit published that had an impact beyond the community of physicists came out of his education as an electrical engineer. When he took a position in the Department of Terrestrial Magnetism of the Carnegie Foundation, he invited Merle Tuve to join him in attempting to demonstrate the existence of an ionized layer in the atmosphere, which had been postulated by Kennelly in the United States and Heaviside in the United Kingdom. It was typical of Breit's approach to physics that he worked on the theory of radio wave reflection from charged layers in general as he planned to measure them in the atmosphere. The idea was to measure the delay between the "ground" wave from a radio transmitter and the "sky" wave reflected from the ionized layer. Since the height of the expected layer was not known, the size of the delay could not be estimated. Breit thought that a parabolic sending antenna would be ideal for the attempt, but funding difficulties soon scotched that idea (cf. Tuve's contribution to the symposium, FP8). Station KDKA, Pittsburgh, the first licensed commercial radio station in the United States, was used in early trials with a special key Breit constructed. When the Naval Research Laboratory transmitter became available, the experimenters turned to it. It was only 13 miles away and had the schedule of a research installation! Evidence of a delay was indeed obtained, but interference made measurement difficult. Breit and Tuve thought of pulsing the transmission with a period so that the delay could be determined before the next train of waves arrived. Today we recognize that this is the central idea of radar. I was once told that Breit and Tuve noticed occasional spurious signals when they were testing their apparatus. These were finally attributed to planes flying from Washington's airport (of course, their 80m wavelength was useless in locating planes, but it could sense them), and the signals were ignored henceforth! In any event, they got their delay and were able both to prove the existence of the ionized layer and measure its height above the earth's surface. Breit and Tuve published their work in 1926 and with Odd Dahl in 1928. When Breit was given the Fellow Award of the Institute of Radio Engineers (now IEEE) in 1945, the citation read:

> For pioneering in the experimental probing of the ionosphere and giving to the world the first publication of the experimental proof of the existence of the ionosphere; and for having initiated at an early date the pulse method of probing by reflection which is the basis of modern radar.

Kittel considers this Breit's most significant work. Only in terms of public impact can one agree.

There were no more publications on the Kennelly-Heaviside layer. As early as 1923, Breit's interest had turned to the emerging field of quantum mechanics. He continued to publish interesting items on topics in radio until 1930, but increasingly his papers were devoted to problems in quantum mechanics. As the papers of Schroedinger, Heisenburg, and Dirac appeared, Breit wrote interpretive comments where he thought they could help and looked for problems with a classical analogue to study. The breadth of his interests began to appear, and it would never again be possible to discuss his work over a year or two as a package, except perhaps for his wartime activity.

The gap in Breit's publication record between 1940 and 1946 does not mean he was inactive! He served on committees on publications that persuaded American physicists to withhold any papers on properties of uranium, or the new elements beyond, that neutron bombardment could make. Breit early recognized the possibilities for both energy generation and explosive reactions with these isotopes. Papers published in 1946 with submission dates in the early 1940s are the results of this policy. He chaired the fast-neutron project at Chicago until his departure for Washington, when Oppenheimer took over and transferred the program to Los Alamos. Breit had contributed some five reports on isotope separation, neutron diffusion, and chain reactions while at Chicago. At Washington, he worked first on degaussing (to protect merchant ships from magnetic mines), as we have noted, and invented the magnetic extrapolator, allowing two men to accomplish in a day a task that had taken a month before. He wrote three reports on degaussing. At the Army Ordnance Laboratory, he provided fifteen reports on the proximity fuse (a major contribution to antiaircraft success), and thirteen on exterior ballistics and fire control. Both Navy and Army ordnance departments cited Gregory for exceptional and outstanding performance during the war. In 1967 President Johnson awarded him the National Medal of Science for his work in nuclear physics and his wartime work in ordnance.

Gregory's work on the question of ignition of the atmosphere and oceans by runaway reactions initiated by thermonuclear explosions may be considered an extension of his interest in supporting the military in its role of protecting his adopted country. Some work had been done (by Bethe) before the Trinity test of the first nuclear bomb, but the "super" was expected to be a thousand times as powerful—and to involve reactions closer to the constituents of the environment. Edward Teller, according to *The New York Times* obituary, considered Breit "the most conscientious, meticulous and painstaking of physicists," and hence the ideal person to undertake this task. Under Breit's direction, we geared up at Yale for the necessary calculations and created a "secret" room to house our work. A few of us were cleared, and Gluckstern and I were made responsible for security and supervision of the work of other participants. There was a vent in the room that Breit considered a possible security leak, so I designed and built a variable pitch audio oscillator to run in the vent when we had conferences (it sounded worse than the wobbling shriek of modern emergency vehicles—for the same reason: one can discriminate against a steady sound, but not a variable one). Our work was essentially applied astrophysics, and Gregory put it all together. Subject to the favorable outcome of some new experiments (which were successful), there would be no runaway reactions from super explosions. As it happens, he was right!

In general, however, we must trace Gregory's interests in a subject over many years. For example, he interested himself in accelerators for forty years. His participation in the invention of the betatron principle has already been noted. He was the first American physicist to realize that to induce nuclear reactions with artificial sources (accelerators) would be superior to the use of naturally radioactive sources pioneered by Rutherford. Breit, with his DTM colleagues, began to look at high voltage applications to particle acceleration in 1928. In 1929 he published his initial ideas on nuclear reactions produced by "artificial" sources of radiation. By 1936, the first proton-proton scattering experiments were published by the DTM group (Breit had moved by then, but he continued to encourage the work) using a voltage multiplier device Breit had begun. At Wisconsin, he inspired the work of Ray Herb and colleagues with Van de Graaff machines (FP7). In 1952, when the founding paper on heavy ion physics was published, the funding proposal that preceded it discussed the modifications in cyclotron operation that could accommodate the ions (the first experiments in the field were done at Oak Ridge using a cyclotron built on a wartime isotope separation magnet), provided a concept design for a heavy ion linear accelerator, HILAC, (we used matrices to follow the particles through the machine). Two HILACs were built, with design input from Bob Gluckstern, one of the authors of the heavy ion paper. Yale got one of them, and the other went to the Seaborg group at Berkeley for the study of trans-uranic elements. Trans-uranic elements are quite fascinating objects in nuclear physics, and we discussed the possible properties of super heavy ones that would be accessible to head-on heavy ion collisions with uranium, but the topic was introduced for completeness, and to attract the attention of the funding agency reviewers. It was not a central interest of Breit or the rest of us. Bromley (FP4) mentions it in his symposium article, and Breit notes the work of Seaborg's group and the Nobel institute at the end of his *Handbuch* paper (HP1). However we thought of it, the use of heavy ions in trans-uranic element studies has become an international effort. In addition to the Nobel institute, the Darmstadt group is in the field, and has made number 112! Breit's last direct concern with accelerators came in 1964. We proposed what we called a "meson factory," a linear accelerator designed to produce an intense beam of pi mesons for the study of nuclei. Legend has it that at the meeting with funding agencies, Louis Rosen stood up at the end of the Yale presentation and said, "I want a machine like Yale has proposed." Once again Gluckstern turned our conceptual design into a practical one, and the accelerator was built—at Los Alamos, not New Haven! Yale got priority in the use of the machine for a time, and Vernon Hughes made good use of it. By the time the accelerator was in operation, our theory group had begun to break up, and we never worked on the analysis of experiments. Our throwaway suggestion (I do not wish to imply that we were not serious, but only that the area was not central to our interests) this time was cancer therapy with pi mesons. The localization of energy deposition from particle ionization in matter is a function of particle mass, so that the heavier mesons will do less damage to healthy tissue as they pass through than lighter electrons or massless X rays. A Yale oncologist established the Cancer Research and Treatment Center at the University of New Mexico to pursue this idea, and it was clinically successful. It fell, however, to budget restrictions, but the excellent cancer center continues at UNM. Breit's final publication on accelerators was in 1954.

In 1928 Breit discussed the interpretation of the Dirac equation, and during the next few years developed the theory of two electron interactions, the separation of angles in the two electron problem, and calculated the fine structure of helium using the large-large component approximation to his 16 component equation. After the war, he returned to the two-fermion problem with Gerry Brown, and studied the effect of the finite size of the proton on the fine and hyperfine structure of hydrogen spectra. Over the next several years, this approach was used to obtain first order relativistic corrections to proton-proton scattering analyses. Hughes's review of the Breit interaction (FP5), as it is properly called, reports on measurements, modern extensions to higher orders of approximation, and on the study of the interaction in the fine structure of the positronium atom. The study of the Breit Interaction and its generalizations is "fundamental to atomic physics and modern quantum electrodynamics," says Hughes (FP5). As usual, Breit's pioneering work set the standard for the field.

Gregory began his study of dispersion relations in 1925 with two papers. These were followed by another in 1930 and major review articles in 1932-33. The latter papers essentially summarized the status of quantum electrodynamics then. The choice of dispersion relations was typically perceptive of Breit. Their analytic properties make it possible to obtain many useful results without having to specify details of the theory; optical level crossing, for example, was derived in these papers. It is a major aid in disentangling experimental observations. Analyticity properties were central to the use of dispersion relations in particle physics thirty years later. Breit himself applied them to nuclear processes in 1962.

In 1932 Breit started his work on the isotope effect. The shift of atomic spectra as a function of added neutrons tells us something of the behavior of the extra particle in the nucleus. As Brown notes in his symposium article (FP6), Breit's early work had settled on two effects. If the neutron is buried in the center of the nucleus, its presence pushes protons out because of the near incompressibility of nuclear matter. If the neutron is attached to the surface, it pulls protons out because of the nucleon-nucleon attractive force. Spectroscopists call these effects mass and field, respectively. Breit's work continued through the 1930s, and resumed in 1950, 1952, and 1953 with attempts to use our increasing knowledge of nuclear forces to understand the changes in the nucleus as neutrons are added. Brown's discussion of the problem in 1968 (FP6) summarized work that began when he raised the topic with Breit years earlier. It centered on an attempt to understand changes in the nucleus starting with a self-consistent description with a static potential between nucleons, hence a spiritual continuation of Breit's studies. Brown showed that there is no low-level "breathing mode" excitation in the nuclei studied (calcium and lead)—and hence none in intermediate mass nuclei.

He also showed that density dependence of the effective interaction is important for the isotope shift, and obtained believable information on the compressibility of finite nuclei—worthy extensions of Breit's earlier work.

Much of the work that occupied Breit during his remarkable career involved the scattering or reactions of charged particles—usually both positive (i.e., an interaction in which the repulsive Coulomb force operates). For nonrelativistic energies, the Schroedinger equation applies, and is separable. The problem, therefore, is to obtain solutions of the radial part. These solutions are called Coulomb wave functions (when only the Coulomb force acts). Breit's early interest in proton-proton scattering made it necessary to deal with these functions, and in 1936 he published the first of a long series of papers on the subject with F. L. Yost and Wheeler.

The mathematical difficulty arises because the values of the angular momentum appearing in the equation are integers (in terms of the reduced Planck's constant). The confluent hypergeometric functions defined by Whittaker and Watson (*Modern Analysis*), which are formal solutions to the Coulomb equation, have divergent expansions when the appropriate indices are integer. They are also not normalized properly for the physics applications. Before modern computers were available, tables of functions were needed, and they were given, even in the earliest papers. We organized the preparation of tables at Yale. For us a computer was a person using a desk calculator to obtain the values of the regular and irregular solutions of the Coulomb wave equation as defined in the original paper. When digital computers became available (large main frames at first), I found programming for them a direct translation of the steps we had set up for our human computers. We programmed in machine language then, with the only aid an assembly program, and I became proficient in octal arithmetic.

Some ten papers by Breit and colleagues written over the years from 1936 to 1959 were summarized, together with notational references to all known papers of other writers on the subject, in a *Handbuch* article (HP2). This extended effort was again typical of his approach. Whatever the physics problem needed, even some pure or applied mathematics, it was supplied. He was an excellent mathematician, and I became an adequate one working with him (it was typical of him that he never taught me any mathematics. It was only a tool, and we talked about physics).

In 1936 Breit also published his first paper on proton-proton scattering with E. U. Condon and R. D. Present (obviously there is a connection between the papers on Coulomb functions and the theory of p-p scattering). It is reliably reported that he was so anxious to get results that he would volunteer to help with the experimental runs if the experimentalists would just get on with it! Merle Tuve and Ray Herb tell of these early days at DTM and Wisconsin in papers for the symposium (FP7 and FP8). In the theoretical work, the shift in the phase of the outgoing wave with respect to the incoming one required some consideration, since the Coulomb part of the interaction has an infinite range. However, since the target protons are in a hydrogen atom, the proton charge is screened at sufficient distance. Let the screening distance go to infinity and one has a means of defining the phase shift—with respect to a Coulomb wave rather than a plane one.

In 1941 Breit showed that the most fundamental description of the collision process is the scattering matrix and the phase shifts that define it. This fact was used by Hans Bethe and colleagues in the study of nuclear matter with realistic nucleon-nucleon interactions in 1967 (FP2), as I shall note later. From the first, however, the delineation of the nucleon-nucleon force was a part of the study. Initially the distance dependence of the force was arbitrary. A square well, given its ease of treatment, was favored. Both phase shifts and the early models of the attractive nuclear forces suggested charge independence of the interaction. The singlet S phase shift for n-p and p-p scattering were nearly the same and the nuclear force models to give them were alike. This result supported the similarity of binding energies of mirror nuclei, already noted. The force was short ranged from the start.

When it became possible after the war to use more sophisticated mathematical forms for the potential, the Yukawa force was introduced (i.e., a descending exponential divided by the distance, with the exponent dependent on the mass of the meson presumed to cause the force). The analyses showed that the current value of the mass was too little to explain nuclear forces; the force was too short ranged. Although the phenomenological mass in these early studies turned out to be a bit large when the pi meson was discovered, our later work accommodated the experimental mass quite handily. As we introduced larger angular momenta, it became necessary to take into account the fact that the neutron-proton interaction contains a tensor part, and this affects both the scattering, matrix form, and the force potential description. The shell model contributed a spin-orbit force, and as the data came from higher and higher energies with more intense beams, experimentalists began to measure double and triple scattering. We, of course, kept up with our formulations. In particular, we looked at one-and two-pion exchanges as determinants of terms in the potential and introduced a hard core of short range to take account of higher order pion interactions we could not safely model (or, perhaps, of quark interactions that we didn't even think about). By 300 or so MeV, we needed to look at relativistic corrections, where Breit's two-fermion equations provided a basis.

The use of high-speed (for that day) computers was essential to our work; the AEC center at NYU and the IBM laboratory at Poughkeepsie gave us valuable time to run the analyses. Breit never learned to program, but he was instrumental in introducing the use of computers into the work. He understood my wiring of plug boards to run an IBM accounting machine in the Yale business office as a differential analyzer (Metropolis had pioneered this at Los Alamos during the war, and taught me), and even designed an analogue differential analyzer using Navy surplus ball-table integrators that worked, but he was happy to leave the making of stored programs, *à la* von Neumann, to his younger colleagues. Gradient searches in the multi-dimensional phase shift space produced excellent fits to as many as 2,000 pieces of data, n-p and p-p, by the early 1960s. Our formulation of the scattering matrix was verified. Yale finally got a computing center, and our graduate students became its first consultants. This made it possible for us to make a coup. I was scheduled to talk at one of the annual meetings of the American Physical Society, and an experimental group gave values of measured triple scattering parameters in an abstract for the same session. When I talked, I reported calculated values at their energy and angles that agreed within standard error with the measurements! All the work over more than thirty years by Breit and his colleagues was summarized in a presentation for the Breit symposium (FP1). As the nearly sixty papers reported with Breit's participation attests, and the care with which the work was done suggests, this effort was a major contribution to nuclear and particle physics in this century. It is, perhaps, remarkable that over the thirty years of this study, major characteristics of the interaction didn't change. The potential remained short ranged, essentially charge independent, and the phase shifts still provide the best representation of the physics. Bethe (FP2) used the phase shifts to derive phenomenological potentials to use in studying the properties of nuclear matter, with satisfactory results—an appropriate justification of Breit's expectation in undertaking the study in the first place.

Breit is, perhaps, remembered in the physics community for his work in nuclear reactions as much as for any other of his areas of interest. In 1936, he and Eugene Wigner published papers on resonance theory, and this work has been a major component of studies since. The obvious difficulty with treating the nucleus is that it is a many-body problem with the interaction between nucleons in a state of developing understanding. Thus, in the thirty years following the Breit-Wigner beginning, they and others have sought models that could capture properties of the nucleus (turning Wigner's black box into grey ones!) and other facets of the total picture that would be useful in organizing experimental data and revealing for the understanding of nuclear structure. Breit's papers, some fourteen between 1936 and 1963, usually dealt with specific problems rather than general formulations, but his treatments were intrinsically consistent with them, and Wigner attributes much of his own R-matrix theory to "Breit's exploratory work" (FP3). Wigner made use of the short range of the nuclear force by treating the interior of the nucleus as a "black box" and dealing analytically with the exterior—but for particles coming in at low angular momentum (i.e., more or less directly at the target nucleus). He defines a derivative matrix, the R matrix, which has obvious analytic properties. It can utilize the unitary and symmetric nature of the collision matrix readily to give sum rules and has poles that give a resonant structure.

The energies and reduced widths are parameters rather than physical entities; he wished to capture the general behavior rather than detailed levels. He summarized this beautifully in his symposium paper in 1968 (FP3).

Wigner's 1968 summary thus brought forward a few years the treatment that Breit had made a major part of his own summary of the theory of resonance reactions in his monumental *Handbuch* article (HP1) of 1959; his taste for treating as much of a problem as possible with mathematical elegance attracted him to his old collaborator's approach. He also treated the compound nucleus model, the shell model, stripping and pick-up, direct interactions, selection rules, threshold behavior, the optical model, reviewed then recent self-consistent field calculations with correlations between particle positions utilized, and heavy ion physics. He also dealt with radioactive states, showing as he had noted in 1940 that these states can also be treated by resonance reaction theory. With John S. (Jack) MacIntosh (HP3), he discussed the polarization of nucleons scattered by nuclei of arbitrary spin, which, among other interests, informs the angular distribution of nuclear reactions—particularly pick-up and stripping (Breit had published a paper on angular distribution of reaction products in 1947). Throughout, the central idea of the resonance reaction connects the various models and treatments.

By the 1950s the preparation of particle beams had progressed to the point that one could conceive of stripping all the atomic electrons from nuclei heavier that helium. In addition, octuply charged oxygen 16 has the same charge-to-mass ratio as the deuterium nucleus, so machines designed for accelerating deuterium could be used to produce the same velocity for oxygen. From such considerations came the initiation of heavy ion physics by Breit (the first publication was in 1952 with Gluckstern and me, but we had presented similar considerations in the "scientific justification" for a linear accelerator proposal a couple of years earlier, and I have no hesitation in claiming that the seminal ideas were Breit's). The principal types of experiment may be classified as (a) transfer reactions, where only the surface of the target nucleus is involved, and a nucleon is transferred from one nucleus to the other (obviously the older terminology of "pick-up" and "stripping" is subsumed by "transfer") and (b) Coulomb excitation, where closer approaches between target and projectile occur, but the possibility of treating the kinematics of the collisions classically is retained. A third type, head-on collisions, is the basis for trans-uranic element studies, and the suggestion was seminal for that field, but Breit never really pursued that idea after the initial proposal.

Coulomb excitation (an old idea that becomes powerful as a nuclear probe when the projectile is a heavy ion) became a major experimental field, and Breit with colleagues (especially Bob Gluckstern) discussed the theory in great detail over the fifteen years following the first paper—over twenty publications by my nonscientific count, the last occurring in 1967, when he looked at transfer in Coulomb excitation. As Gary Herling points out, this approach "foresaw the coupled-channel, distorted-wave method," and he notes the continuing activity in this most productive field. The rotational levels of the target nucleus are excited, and if one wishes to study a spin reorientation, one has a probe that is otherwise unavailable. Breit summarizes the field of heavy ion theory in HP1 and with Gluckstern treats Coulomb excitation extensively (including reorientation effects) in HP4. Allan Bromley's survey of the experimental data up to 1967 (FP4), with comments on the theoretical analyses, is especially illuminating. While, as I have noted, one of the linear accelerators built as a result of our 1950 proposal was at Yale, the most productive heavy-ion experiments there were done with the Emperor tandem Van de Graaff that Bromley got for the department and used to good effect.

I have by no means exhausted the areas, even by title, that Gregory initiated or advanced during his remarkable career. Perhaps the breadth and importance of his work has been suggested, however. In terms of the kind of recognition that John Wheeler felt Breit did not get, the fact that he addressed so many topics may have been a disadvantage. This review of his work may serve to redress the oversight that remark implies. Those who did not follow all of his work now have an introduction to it.

A number of Gregory's colleagues and former students responded to my request for recollections. When I quoted or paraphrased their comments, I named them without a footnote. In all such cases the reference is "private communication." Ralph and Faith Wyckoff, Breit's stepson and daughter-in-law, have provided some personal items of Gregory's life that I could have gotten nowhere else. John Wheeler was especially generous with his response, and D. Allan Bromley's recollection extended into Breit's later years with affection. Mrs. H. H. Barschall responded for Heinz, who unfortunately died before he could answer my inquiry; I am grateful for her help. Anne Herb wrote of Ray's regard for Breit's interest in his early work at Wisconsin. Norman Heydenburg gave a look at Breit's early years that no one else is left to give. Charles Kittel recalled the war years when Breit bundled up the graduate students and took them to Washington, and Glenn Seaborg recalls working with Breit on the neutron project. Willis Lamb talked at length over the telephone about his interaction with Breit, both early and later in his own career. My own contemporaries, Jack McIntosh, Gerry Brown, Bob Gluckstern, and Arthur Broyles revived many memories, and Gary Herling, a later student, gave a good account of Breit's work after the Breit group began to dissolve.

The Breit symposium was organized and the proceedings edited by Allan Bromley and Vernon Hughes; I am grateful for both activities. The work of surveying Breit's contributions would have been

much more difficult without the contents of the proceedings and the bibliography they included. I have referred to the papers in the proceedings by "FPx", where the x is the chapter number in *Facets of Physics* (Academic Press, Inc., New York, N.Y., 1970). The papers in volume 41/1 of *Handbuch der Physik* (Springer-Verlag, Berlin, 1959) were invaluable, and are referred to as "HPx," the x being the order of the papers as they occur in the volume.

Finally, I should note that the material supplied by the National Academy of Sciences was very helpful. I have never seen a complete vita written by Breit, but the fragments the Academy sent filled in some gaps, especially about reports during the war years. The obituary from *The New York Times* allowed a quote from Edward Teller that I otherwise would not have known.

Selected Bibliography

Breit published some 320 items in his career. I have not referred directly to any of them in the memoir text; the selection below is intended to note the ones that initiated a study or made a significant advance. I have, in consideration for Academy guidelines, included only the first of the many papers on Coulomb wave functions. They're all listed in the *Handbuch* article I did with Breit (HP2). The bibliography in the symposium proceedings is the best there is, but there were one or two papers after 1968, and I found two from 1936 that were missed.

1926 With M. A. Tuve. Radio evidence of the existence of the Kennelly-Heaviside layer. *J. Wash. Acad. Sci.* 16:98.

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1940 The interpretation of resonances in nuclear reactions. *Phys. Rev.* 58:506.

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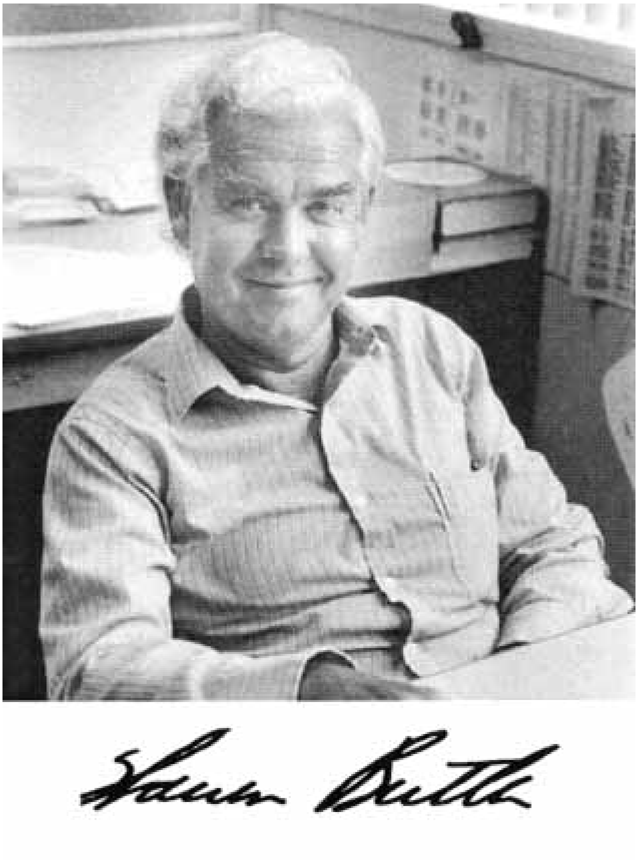

Warren Lee Butler

January 28, 1925 -June 21, 1984

By Andrew A. Benson

Understanding the mechanisms adopted by plants for detecting changes in day length and season was to become one of Warren Butler's crowning achievements. Last student of Nobel physicist James Franck, Butler chose to understand the photochemical adaptations and pigment systems of plants and developed important concepts and understanding of basic photometabolic processes.

Warren Butler, only son of Orval L. and Lois Jordan Butler, was born January 28, 1925, in Yakima, Washington. His mother was a daughter of a Methodist minister. The family moved to Portland, where Warren and his younger sister, Connie, graduated from high school and his father was successful in an auto parts business. Young Butler enrolled in the Army Specialized Training Corps for training in physics. During training in Texas he was conscripted into the infantry and was shipped overseas.

After one day in England, the eighteen-year-old Butler left for France before Christmas in 1944. On patrol searching for enemy soldiers said to have penetrated behind the lines over snow-covered terrain, the group of four or six men detonated a land mine. One was killed; Butler was seriously wounded. Because of inadequate medical support during the Battle of the Bulge, Butler ended up with an amputated left hand and left leg removed above the knee. Recovery in Utah and development of prostheses was time consuming. Because of his immense will power, he survived the severe wounds, determined to live, learn, and lead a normal life.

With the skill and ingenuity of an engineer, Warren went through his exceptionally productive life with a prosthetic left arm and leg. His sense of humor prevailed as he showed students he could handle hot flasks and equipment with his mechanical hand. Winslow Briggs recalls a macabre scene illuminated by a green light source in the laboratory: Butler with his arm up to the elbow in a huge dewar of liquid nitrogen, vapor swirling around him, one bushy eyebrow raised, and a Machiavellian expression on his face. In the words of Jon Singer, "As to Warren's personal boldness, again all of us who knew him have many examples to cite. His most remarkable expression of his boldness was the absolute indifference he seemed to show to his physical handicaps, and his simple refusal to let them interfere with his life and work. He scaled such obstacles in his life as would have defeated many of us."

Back in Portland, Warren entered the physics program at Reed College, where he met Lila Bowen; they were married in 1951. With a B.A. thesis entitled "An Investigation of the Use of Ultrasonics in an Optical Shutter Arrangement" in 1949, he sought graduate training in biophysics. Yale and the University of Chicago accepted him. He visited Yale, but he decided to return to Chicago, which admitted thirty-six applicants; after a year, however, only a third were accepted for the doctorate program.

Warren had not considered photosynthesis as an option until he met James Franck, and was accepted as his last graduate student. Butler often expressed admiration, warmth, affection, and great appreciation for his distinguished mentor.

With his 1955 doctoral thesis ("Measurements of Photosynthetic Rates and Gas Exchange Quotients During Induction Periods") finished, he sought a position at the University of Maryland, but decided against it and accepted a position with the U.S. Department of Agriculture at Beltsville, having been attracted by Sterling Hendricks. His work with Hendricks and Borthwick continued from 1956 to 1964.

As S. J. Singer pointed out at the memorial service for Butler:

> The interactions of radiation with matter were quite well understood in the '30s and it seemed likely that Franck, fascinated by the problem of photosynthesis and Delbrück by the effect of X-rays in producing genetic mutations, found that radiation provided the means to introduce the elegance of Quantum Physics into the major problems of biology. However, biology being a complex science is more directly related to chemistry than to physics. These pioneers treated Biology as a sub-discipline of Physics. It was left for the next generation of physicists, now known as biophysicists, to take the plunge into the chemical world of the cell and the organism. This required the elegance of the physicist to be coupled to the boldness of the biological chemist, and in Warren Butler this combination of qualities shone forth.

Understanding plants' recognition of the changes in day length and season was to become one of Warren Butler's crowning achievements. The problem, long recognized by plant physiologists, came into focus at the Beltsville laboratories of the U.S. Department of Agriculture with the studies of botanist Harry A. Borthwick and physiologist Marion W. Parker, which implicated a specific on-off photopigment system controlling flowering in a soybean plant. A single flash at night could trigger the process. With the foresight of F. G. Cottrell, they were joined in 1940 by chemical physicist Sterling B. Hendricks, head of the Mineral Nutrition Laboratory, who had actually worked on USDA projects since 1922, and was determined to study the "action spectra" for the process. Resuming their strategy after the war with the first large spectral illumination culture system in which whole plants grew in light of specific wavelengths, they discovered essentiality of red light for floral initiation. Their work with the more rapid lettuce-seed germination system of Vivian and Eben H. Toole led to recognition of photoreversibility of a "red" and "far-red" form of the pigment. Contemporary science, though, did not believe that plant physiological experiments could ever reveal molecular details of their pigmentation.

Biophysicist Warren Butler joined the group in 1956 and recognized their problems in demonstrating the reality of their "pigment of the imagination." He credited Hendricks with "probably the boldest and most brilliant single stroke in the history of plant physiology."

Butler had approached Alan Mehler for a job, and Mehler called Siegelman, who talked to Karl H. Norris in the Poultry Division. Norris had been to the University of Chicago and got along very well with Butler. Though he was never an official part of the USDA plant physiology division and worked under Norris in the Poultry Division, he adapted the novel and powerful dual monochromator spectrophotometer of Norris to examine small, highly scattering plant tissue samples. His outstanding capabilities as a photophysicist led to important contributions to both theory and technology. Hendricks and Siegelman, whom he and Borthwick had recruited from the pioneering Research Laboratory, brought their samples to Butler for examination. None revealed detectable red-far red pigment.

In the middle of June 1959 the breakthrough came. In Butler's words:

> Hendricks appeared in my lab one day with several Petri dishes of dark-grown turnip seedlings . . . We removed the cotyledons and pressed them loosely into the cuvette and measured the absorption spectrum of the sample after irradiation with red and far-red light. To our amazement and delight,

> mixed with skepticism, we found that the difference spectrum between the red and far-red irradiated sample was precisely that predicted for phytochrome by the physiological action spectra.

Lila recalls Warren's elation that evening, when he said, "Lila, I think we've hit on something big."

At last the Beltsville group had the stuff in a bottle and the stuff needed a name. According to Harry Borthwick, Warren Butler one day "half jokingly" suggested the term phytochrome, from the Greek words for plant and color. Upon purification, the red-absorbing form of phytochrome proved to be blue in color and the far-red absorbing form green.

Butler, Norris, Siegelman, and Hendricks published their landmark paper ("Detection, Assay, and Preliminary Purification of the Pigment Controlling Photoresponsive Development of Plants") in the December 1959 issue of *Proceedings of the National Academy of Sciences*. That paper reported: "This work supplies the three needed elements for further progress: A source of the pigment, a method of assay, and a system for separation." The paper ended with: "There would seem to be no essential barrier to finding the nature of the enzymatic action of the pigment P735, which constitutes the limited pacemaker or bottleneck of control evident in plant development, and to elaborating physiological and biochemical aspects of its action."

Butler found a simple and elegant way to carry out accurate light absorption studies on samples of living plants, despite their opacity due to light scattering. He used light as a simple non-destructive analytical tool to follow chemical events in the plant. To someone other than a physicist, the problem of making such measurements on opaque materials must appear to be intrinsically insoluble. The technique Warren Butler employed and the instrument he devised were highly successful. With agricultural engineer Karl

Norris, Butler had developed a single beam spectrophotometer to measure absorption spectra of fruit, vegetables, eggs, dry seeds, and even a two-by-four. By placing the sample directly on top of an end-on phototube, they obtained useful absorption spectra of dense light-scattering samples like intact tissues and thick homogenates. Butler published a review article on light scattering and its utility in studying biological systems. After a paper by Butler and Siegelman at an AIBS meeting at Stanford, James Bonner remarked, "It sounds like you guys have a new Erector Set."

With this single-beam methodology, Warren, together with Sterling Hendricks, was able to demonstrate the existence of a minute amount of a pigment in living etiolated turnip and maize seedlings that exhibited the predicted reversible light absorption behavior of the agents responsible for photoperiodism. These absorption characteristics provided the indispensable criteria that enabled the protein pigment to be isolated in a pure state. A more convenient dual-wavelength difference photometer was developed for assaying phytochrome. This made possible the purification and isolation of phytochrome.

The first public announcement of the detection of phytochrome was a legendary fiasco, but it led to an important discovery. Hendricks had been invited to speak at the Ninth International Botanical Congress in Montreal in August 1959. Norris had produced a simple, easily transportable photometer in which absorbance of short segments of corn seedlings could be measured following red and far-red light. The instrument had a large circular meter, easily seen by an audience. Borthwick, Hendricks, Siegelman, and Butler drove to Montreal with the instrument and several clear plastic boxes of corn seedlings in their car trunk. On several occasions when stopping for gas, someone would open the trunk to see how everything was riding.

I clearly remember helping Warren carry his box and papers from the living quarters of the university to the lecture hall. I had been totally unaware of his physical handicap and, at first, was surprised when a perfectly healthy young man asked me for help in carrying his materials. Only then did I notice that he had only one arm; later I realized he had only one leg.

Word had spread that this remarkable, even mystical pigment was finally to be demonstrated. The lecture hall was full. Hendricks talked for thirty minutes giving background material to prepare the audience for the demonstration. Butler described what the instrument was going to measure; irradiated their seedlings with red light; and obtained the first reading, setting the meter at "9 o'clock." Then he irradiated the plants with far-red light, expecting the meter to move to 3 o'clock as regularly observed. The meter never moved. He rapidly prepared another sample, but again the meter refused to move. The audience was kind, but the failure was a complete mystery.

On their return to Beltsville, Butler and Siegelman salvaged an important lesson from that failure. They found that the far-red form of phytochrome is not stable; once generated by exposure to red light, it is slowly destroyed. Every time the trunk was opened on the way to Montreal, the red-absorbing form was converted to the far-red form, some of which then disappeared. Discovery of the instability of the far-red form was the key to the subsequent purification of the pigment. Siegelman, however, felt that the "loss of signal is still not really understood."

The 1959 Montreal congress was the first opportunity for western scientists to meet their counterparts from the Soviet Union. An afternoon discussion group included A. A. Krasnovsky and E. V. Evstigneev, along with Hiroshi Tamiya (Japan), C. S. French, N. I. Bishop, Mary Belle Allen, M.

Gibbs, H. Gaffron, James C. Smith, A. A. Benson, and Warren Butler.

When computers arrived on the scene, Karl Norris immediately adopted the PDP-8 and helped Butler get into programming. Soon Butler recognized the mathematical simplicity of plotting the fourth derivatives of spectral curves. His "fourth derivative spectrometry" was put to good use in resolving spectrally vicinal components. His last two phytochrome papers published in 1980 and 1982 were concerned with subcellular distribution and localization of the two forms.

Butler cooperated with many colleagues and was interested in numerous aspects of photosynthesis, including chloroplast development, development of PS I and PS II in light, dependence of pigment absorption maxima on chloroplast structure, relationships of structure and energy transfer, changes and lifetimes of the long-wavelength chlorophyll fluorescence in vivo and in vitro, orientation of chlorophyll in vivo, the position of cytochromes b-559 and b-563 relative to the other components of the photosynthetic electron transport chain, and restoration of electron transport in tris-washed chloroplasts by electron donors to PS II. He and his coworkers subfractionated chloroplast membranes into functional pigment-protein complexes and sought for changes of P-680, of C-550 during irradiation at low temperature.

The primary photochemistry of photosystem II of photosynthesis was a major objective of Butler's later research. Of the two chlorophyll light-absorption systems, photosystem II, the oxygen-liberating system, has been a major challenge. Absorption of a quantum of light by an array of several hundred "light-harvesting" chlorophyll molecules cascades to a reaction center where P_{680} + and Q^- are produced. In the process, oxygen is liberated. Butler and Erixon developed a method for reducing Q^- and thereby isolating the photochemical process for spectrophotometric measurement.

Butler was able, with his fourth derivative spectrometry, to reveal the absorption changes of cytochrome b-559, which became reduced under extremely high light intensities, binding a proton strongly in the reduced form when photoinhibition of PS II would occur. Thus, the low potential form can accept electrons directly from the reduced primary acceptor, pheophytin, the reaction center of PS II, binding a proton strongly when in the reduced form. Cytochrome b-559 fascinated him for years. It possessed a potential difference of 300 mV between the reduced and oxidized forms. It was assumed and later confirmed that the high potential form was closely associated with photoinhibition. Observations were greatly simplified by making measurements at -196° where the PS I system did not interfere. Butler's classic review, "Fluorescence Yield in Photosynthetic Systems and Its Relation to Electron Transport" (1966) defined his interests and concerns during the last part of his career. From his measurements of fluorescence yields, Butler became interested in energy distribution and utilization in the photosynthetic systems. He used his tripartite model of the plant's photochemical apparatus first in 1974 to describe the energy partitioning among photosynthetic units, PS I, PS II, and a third system, LHC, light-harvesting Chl a/b complexes. He introduced terminology for measurements of energy transfer among the three. With Masao Kitajima, Butler enjoyed the excitement of their improved understanding of low temperature fluorescence. It made possible the measurement of distribution of energy absorbed by photosystems I and II and their light-harvesting complexes, which he described as a tripartite model system.

The blue light responses attracted Butler's imagination. Butler was interested in the blue light phenomenon, long recognized by plant photophysiologists. He analyzed the destructive effect of blue light on mitochondrial cytochromes, the Soret bands that serve as photoreceptors, and consequent inhibition of respiratory activity. Starting with the typical biophysicist's concept, namely that absorbed light is likely to induce a change in the photoreceptor molecules that might be detected spectrophotometrically, Butler determinedly searched for such blue light-induced absorbance changes (LIACs) in fungal cells, presumably expecting changes in the 440-480 nm and 560 (557) nm regions, after irradiating the cells with blue light that corresponded to a photoreduction of a non-mitochondrial b-type cytochrome. An action spectrum for this LIAC in *Neurospora* cells demonstrated that the photoreceptor was a flavin, which, upon irradiation, emitted an electron, reducing the b-type cytochrome. Thus, Butler speculated that the cytochrome b-557 might be a component in the signal transduction chain very close to the primary photoreceptor flavin.

After the hemoflavoprotein nitrate reductase had been proposed as the photoreceptor for light-stimulated conidiation of the fungus *Neurospora crassa*, Butler and his coworkers found in a partially purified enzyme preparation, which had been inactivated by the reduction of the internal molybdenum cofactor, that blue light could reactivate the nitrate reductase by reoxidizing the cofactor. His action spectrum revealed again a flavin as photoreceptor for this reaction. Interestingly, as we now know, a flavin coacts with a second pigment molecule; more recently, it was discovered that the molybdenum of the cofactor is bound to a special pterin. In one of his earliest studies (1973), they identified a photoreceptor for phototaxis in the slime mold *Dictyostelium.* The absorbance changes that Butler and his collaborators observed corresponded in the fungi to a photoreduction of a b-type cytochrome; the action spectrum for the response revealed a flavin as photoreceptor.

That concept developed into a concern for the function of a hemoflavoprotein nitrate reductase and Butler's action spectrum indicated that a flavin was a photoreceptor chromophore.

The achievements in blue light research of Warren Butler and his group included application of the biophysical approach to the search for blue light photoreceptors, the technical refinement that allowed detection of small absorbance changes in live, dense samples, and consequently the identification of flavin and cytochrome b-557 cooperating in the photoreception/signal transduction in several blue light-regulated responses.

Warren Butler's skills in teaching were superb; his seminars and his undergraduate teaching were models of clarity and enthusiasm. Jon Singer recalls asking Warren to give a lecture on phytochrome for his undergraduate chemistry class. "An unforgettable lecture, with several striking demonstrations that he had prepared, he proceeded to give a lecture that for its carefully chosen level of exposition, its brilliant clarity, its obvious significance, and its simple elegance was the best single undergraduate lecture I have ever heard. He had every one of the 250 or so students riveted to their seats throughout, and received an astonishing and spontaneous storm of applause at its end."

Jon Singer, who with Martin Kamen had recruited Warren Butler for the fledgling Department of Biology at the University of California, San Diego, spoke of it as "one of the brightest events of my term as chairman. Warren's work was matched to and clearly stemmed from his personal qualities. His elegance was evident in countless ways, in his handsome face, the striking shock of silver hair, and in his marvelous smile."

The Butler children—Alison, Hillary, Laird, and Leslie—enjoyed a busy schedule in La Jolla, spiced with frequent adventures in Baja California and the California mountains. With the capable strengths of their mother Lila, Warren's potential for accomplishment seemed unlimited. Engineering being his forte, Warren Butler enjoyed the challenges of navigating the impossible roads of Baja California. His counterpart at the Scripps Institution, Professor John D. Isaacs, provided experienced encouragement and appreciation of the problems and delights of such adventures. Resourcefulness and ingenuity were clearly major attributes displayed in all aspects of Warren Butler's life. A dominant characteristic of Warren Butler was his spirit of adventure. He felt at home in the palm oases of the desert, on sand dunes at the ocean, and often in the forests. In Baja California and other places, sleeping on the ground and driving over non-existent roads often included fearless attacks of technical and mechanical challenges. Warren Butler's response to challenges of nature and science ignited the enthusiasm of his students and colleagues, whose appreciation grows with time.

I am indebted to the many thoughtful statements by speakers at a memorial service honoring Warren Butler on June 28, 1984, the obituary memorials published by Professor Helga Ninnemann, and the tribute volume published by "The Japanese Students of Professor Warren Butler," Hideyuki Matsuda and Kimikyuki Satoh, editors, for much of the material presented here. Discussions with Lila Butler, Helga Ninnemann, H. W. Siegelman, Jonathan Singer, and Winslow Briggs provided personal reflections on many aspects of Butler's life and work.

CHRONOLOGY

1925	Born in Yakima, Washington, on January 28
1943-46	U.S. Army Infantry
1949	B.A., physics, Reed College, Portland, Oregon
1951	Married to Lila Brown in Portland, Oregon, on September 1
1955	Ph.D., biophysics, University of Chicago
1955-56	Research associate, University of Chicago
1956-64	Biophysicist, Instrumentation Research Laboratory, U.S. Department of Agriculture
1964-65	Visiting professor, Johnson Foundation, University of Pennsylvania
1964-84	Professor of biology, University of California, San Diego
1975-77	Professor and chairman, Department of Biology, University of California, San Diego
1984	Died of cancer in La Jolla, California, on June 21

AWARDS AND HONORS

1976	Elected to the National Academy of Sciences
1978	Elected to the American Academy of Arts and Sciences
1981	Elected a foreign associate of the French Academy of Sciences

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George B. Craig Jr.

George Brownlee Craig, Jr.

July 8, 1930-December 21, 1995

By Eddie W. Cupp

George B. Craig, Jr., Clark distinguished professor of biological sciences at the University of Notre Dame, died quietly in his sleep on December 21, 1995, at the age of sixty-five. He was attending a national meeting of the Entomological Society of America when he passed away. He is survived by his wife, Elizabeth (Pflum) Craig, two daughters, Patricia Craig and Sarah Craig Peterek, and his son, James Craig.

Craig had been a member of the National Academy of Sciences since 1983. He was born in Chicago, Illinois, on July 8, 1930, the son of George Brownlee Craig and Alice Madelaine McManus Craig. He attended the University of Chicago Laboratory School, graduated from the University of Indiana in 1951 with a B.A. in zoology and received both M. S. and Ph.D. degrees in entomology from the University of Illinois in 1952 and 1956, respectively.

Craig was an internationally recognized expert on the biology and control of mosquitoes, particularly species belonging to the genus *Aedes.* As a young scientist, he pioneered the study of mosquito genetics using *Aedes aegypti,* the yellow fever mosquito, as the subject of his investigations. His work resulted in the recognition that this species exhibited a wide variety of genetic traits that could be mapped to specific chromosome locations and also indicated that mosquitoes might be well suited for control measures using genetic means independent of the use of insecticides. During his career, he and colleagues from his laboratory participated in 485 journal articles, technical pamphlets, and abstracts devoted almost exclusively to the biology of aedine mosquitoes. Craig was author or co-author of 182 scientific publications.

His dissertation research was conducted at the University of Illinois in the laboratory of William R. Horsfall, a distinguished entomologist and eminent mosquito biologist. As a graduate student, Craig focused on the sculptured patterns of the chorion of *Aedes* eggs and utilized the uniqueness of these surface designs to develop taxonomic keys for identification of this particular stage in the mosquito's life cycle (1956). As a result, field research on a large group of mosquitoes previously considered intractable became possible by collecting eggs from soil samples and identifying them to the species level. These findings greatly assisted control programs aimed specifically at temporary pool mosquitoes and related species that plagued much of the Midwest and parts of Canada during spring and early summer.

Craig served in the military as a first lieutenant with the U.S. Army Preventive Medicine Detachment at Ford Meade, Maryland, in 1954 and as a research entomologist with the U.S. Army Chemical Center in Maryland from 1954 to 1957, splitting time between these duties and his work as a graduate student. He received his Ph.D. in entomology in 1956 and joined the biology faculty at the University of Notre Dame in 1957.

It was here that he accomplished so much as both researcher and teacher during his brilliant thirty-eight-year career in academia. He chose *Aedes aegypti* for study not only because of its historical prominence and medical importance but because it was also easy to rear and maintain in the laboratory. The result was the establishment of the Mosquito Genetics Project at Notre Dame, which quickly evolved into the Vector Biology Laboratory (VBL). From this scholarly haven he successfully directed over forty Ph.D. students and mentored thirty-nine postdoctoral fellows.

Following the premise that " . . . workers [do] not appreciate the value of genetic knowledge [of mosquito vectors] in studies of disease relationships and public health. . . ," Craig began to isolate mutants of the yellow fever mosquito and by 1962 he and his students had described nine inherited factors affecting color and thirty factors causing modification of body structures. In a 1967 landmark publication, Craig and his colleague, W. A. Hickey, expanded this list to eighty-seven mutants based largely on a systematic program at the VBL of inbreeding different populations of *Aedes aegypti* to uncover recessive alleles in the heterozygous condition. Approximately half of these mutants were useful as genetic markers. In addition, linkage maps placing about twenty-eight mutants on the three chromosome pairs of this mosquito were presented.

While this summary clearly demonstrated the richness of genetic variability in *Aedes aegypti,* it also had several other significant effects that greatly influenced Craig's career, as well as others in the field. *Aedes aegypti* began to rival *Drosophila melanogaster* as a laboratory animal in terms of knowledge of its genetics. Thus, this information also provided a detailed scientific background for non-mosquito specialists and stimulated several to investigate the biochemistry, physiology, and developmental biology of the yellow fever mosquito, thereby introducing to the field of vector biology a group of scientists with a very different research background and orientation (1967, 1968). This is a process that continues today and has helped energize the discipline of vector biology—the study of insects and related arthropods that transmit disease-causing organisms.

Recognition of both the size and scientific quality of his work on the formal genetics of *Aedes aegypti* also firmly placed George Craig in the pantheon of mosquito geneticists with other internationally recognized luminaries such as G. Frizzi, J. B. Kitzmiller, and H. Laven. Craig's influence as a scientist and thinker was expanded to the global playing field and, with this prominence, he began to shape the policy and operations of a variety of domestic and international institutions that touched virtually every aspect of his discipline. A further charge—the World Health Organization International Reference Center for *Aedes*—was added to the VBL in 1969, and he readily accepted this responsibility and opportunity to collect and house the many exotic strains of medically important *Aedes* mosquitoes from around the world.

Because of his deep concern that results from the VBL be translated into useful public health programs aimed at vector control, Craig and colleagues devoted a great deal of their research planning to developing genetic control schemes that could function with little or no use of insecticides (1967). As a result, he and his students identified genes from *Aedes* spp. that controlled susceptibility to parasites causing malaria as well as important eco-physiological traits such as autogeny, diapause, host choice, and sex ratio distortion (1968, 1969). If manipulated into a vector population, these genetic factors might compromise the ability of a mosquito to successfully transmit a pathogen or even cause the insect population to go out of existence. Concurrent with the genetic work, he and members of the VBL described the physiological basis for such critical pre-and post-mating processes as female receptivity (1968) and refractoriness, reinforcing his earlier description of matrone, a male-produced factor that induces monogamy in female mosquitoes following insemination. Not surprisingly, a great deal of the information discovered during those halcyon days remains valuable today as vector biologists attempt to construct transgenic mosquitoes using methods in molecular biology instead of laboratory crosses.

In 1969 Craig was selected as a scientific advisor and member of the research faculty of the International Center of Insect Physiology and Ecology (ICIPE) in Nairobi, Kenya. This appointment, which required a considerable time commitment away from the VBL, provided the first real opportunity to conduct field research on *Aedes aegypti* in the biological environment of its native origin. He directed the Mosquito Biology Unit (MBU, an acronym that means both "mararia" and "mosquito" in one East African dialect) for the next eight years and focused a great deal of his attention primarily on the population biology of *Aedes aegypti* and its control using genetic means. Working almost exclusively from a field laboratory in Mombasa, Craig and colleagues investigated the population genetics of this mosquito using electrophoretic surveys as a means to measure and compare genetic distances between resident and nonresident populations and to demonstrate sympatric subspecies in East Africa. Other studies measured vector competency for yellow fever virus and host-seeking behavior and movement of female mosquitoes both within and between villages. Attempts at area-wide control by using genes introduced via translocations were largely unsuccessful, however, leaving Craig dissatisfied with that aspect of the project. He left Africa with a much greater appreciation of the population genetics of this species and the belief that control of mosquitoes through genetic means alone would be an extremely daunting task.

The occurrence of La Crosse virus in the eastern United

States and the danger of encephalitis posed by this arbovirus led Craig and his colleagues to develop a multi-faceted program centered on the mosquito vector, *Aedes triseriatus.* This phase of his career, in many ways, mirrored his earlier efforts with *Ae. aegypti. Ae. triseriatus* has a similar biology to *Ae. aegypti,* using natural cavities such as rot holes in deciduous trees and other plants that temporarily hold water to lay its eggs; it is in this aqueous environment that the immature stages develop.

Spurred on by observations made primarily by scientists at the University of Wisconsin in the early 1970s, Craig and his colleagues at the VBL mapped the genetic differences in vector competence of *Ae. triseriatus* to La Crosse virus as a first step to understanding its endemic distribution. The results, published in 1977, demonstrated a high degree of variability for this trait in populations collected over most of the eastern United States and opened the way for a long-running series of laboratory and field studies. Over the next five years, the VBL's preoccupation with *Ae. triseriatus* (and its sibling *Ae. hendersoni*) led to fundamental knowledge of the population genetics of this species (1978, 1980), patterns of interspecific hybridization (for *Ae. triseriatus* and *Ae. hendersoni* and two other siblings found primarily in the western United States), linkage maps, cytogenetics, and formal genetics of diapause, an important phenological trait since La Crosse virus is transovarially transmitted (1980).

Other investigations produced in-depth biological knowledge of these two mosquitoes and were useful in understanding the sweeping epidemiology of La Crosse virus. These included the impact of larval nutrition in affecting the ability of the adult female mosquito to transmit the virus, the presence in *Ae. hendersoni* of a salivary gland barrier to virus transmission, the spatial distribution in wood lots of immature and adult *Ae. triseriatus* and average survivorship of the adult female, and the ability of *Ae. triseriatus* to move to the periphery of urban environments (including South Bend, Indiana) and readily establish itself in tree holes found in domestic settings and discarded tires. The latter study was significant and prescient since it foreshadowed another major chapter in Craig's career—his studies of the Asian tiger mosquito *Aedes albopictus.*

The emphasis by Craig and his group on studying all aspects of the vector biology of *Ae. triseriatus* also advanced a fundamental understanding of the tree-hole habitat in which the immature stages of this mosquito occurs. Over a ten-year period, a series of papers were published from the VBL characterizing the ecology of this niche and describing its influence on mosquito biology. In recognition of these contributions, a major workshop was held in 1984 and the resultant proceedings, published in 1985, were dedicated to Craig. Of the thirty-three papers presented at that workshop, over half specifically addressed the ecology of tree-hole or container-breeding mosquitoes.

It was also in 1985 that *Aedes albopictus* entered the United States through the port of Houston, Texas. This mosquito, given the common name of the Asian tiger mosquito, quickly became the center of attention at the VBL. Immediately sensing its potential medical importance as a vector of viruses, Craig and his colleagues at the VBL and elsewhere demonstrated that this invader species could transmit not only dengue viruses (if one or more strains of this exotic virus were introduced into the United States) but also medically important indigenous viruses such as La Crosse and eastern equine encephalitis (EEE). In a masterfully simple series of studies using photoperiodic sensitivity and cold-hardiness of the egg stage, members of the VBL demonstrated that the probable introduction of *Ae. albopictus* was in tire casings shipped to the United States from temper ate-zone Asia and not from a tropical location (1987). They predicted (quite accurately) that this species would be able to spread into the Midwest since the egg stage can diapause and withstand the rigors of winter. This has since come to pass.

Working with collaborators from the Centers for Disease Control, Craig later demonstrated that EEE virus occurred in naturally infected mosquitoes in Florida and that a geographic strain of the mosquito from that location was fully able to transmit the virus (1992). This report not only highlighted the broad host selection of blood feeding exhibited by *Ae. albopictus* (EEE typically circulates in avian populations) but it alerted the public health community to yet another threat—the potential hazard of a particularly virulent form of encephalitis associated with this introduced species.

As with *Ae. aegypti* and *Ae. triseriatus,* the members of the VBL characterized the Asian tiger mosquito from a variety of biological and genetic perspectives and over the ensuing years this species was the subject of dozens of journal and other technical articles, including a critical summary of information for the hemisphere (1995). Even in the months preceding his death, Craig refused to yield to the physical discomfort of heart disease and continued his usual exuberant pace. Having recognized the epidemiological significance of the diapause capability of *Ae. albopictus* for the eastern and mid-western United States, he and his colleagues published on the importance of this phenomenon (1995). He coordinated a field trip in the summer of 1995 along the Texas-Mexico border to assess the dengue situation and evaluate the relative roles of *Ae. aegypti* and *Ae. albopictus* as vectors. Ever the teacher and mentor, he and his VBL colleagues had just finished making six presentations at the national meeting of the Entomological Society of America in Las Vegas when he passed away.

George Craig was an exceptionally strong-willed person who took great pride in his occupation. He loved academia and the intellectual possibilities that it offered both society and his profession. He also loved the University of Notre Dame and once stated jovially that the only other job perhaps better than his was being a U.S. senator. Then, after a dramatic pause, he retorted, "No, being a full professor at Notre Dame is the absolute best job of all."

He placed immense demands on himself to excel as a scientist and required the same of his VBL colleagues. The result was a career filled with awards and numerous professional acknowledgments. Chief among them was the Walter Reed Medal of the American Society of Tropical Medicine and Hygiene, an award shared by a veritable elite of tropical public health. He was also posthumously awarded the Hoogstraal Medal by the American Committee for Medical Entomology, a subgroup of that society. Fittingly, an earlier recipient of this medal had been W. R. Horsfall.

Craig was an inspirational figure to many in the profession because of his willingness to use his international stature and position in science as a bully pulpit for support of vector biology. Indeed, his advocacy for this area of science became a major preoccupation during the five years prior to his death as he labored to increase funding opportunities for field-oriented, ecological, and epidemiological research to strike a balance with the burgeoning laboratory investigations emphasizing molecular biology. He was also deeply devoted to his students and postdoctoral fellows and often toiled persistently on their behalf after they had left his laboratory. He was a loyal friend to many and a fierce partisan to a few.

George Craig was also an outstanding teacher who worked tirelessly to be effective in the classroom, laboratory, and the field. Inspired throughout his professional life by the teaching example set by Professor Horsfall, he aspired to be as successful in the academic arena as in research. In the classroom, he had an effective, almost charming way of disarming students and creating a relaxed but challenging atmosphere, both in undergraduate colloquia and graduate seminars. One of my fondest recollections of Craig is watching him sit very casually in the VBL insectary, dissecting mosquitoes, and chatting in a light-hearted way with an undergraduate student. Over a period of an hour, the topics the two discussed ranged from the methodological approaches used in formal genetics to how to design a meaningful experiment. The student left, committed to becoming a biologist. He often said that his most treasured prize was the Distinguished Teaching Award given by the Entomological Society of America; he was selected as the first recipient in 1975.

Craig's service contributions were legion. He gave his time freely to an almost endless number of organizations, committees, and causes. Never content to remain in the ivory tower, he was particularly devoted to mosquito control. Having worked summers from 1951 to 1953 as a student in the Des Plaines Valley (Illinois) Mosquito Abatement District, he remained interested in this vocation his entire professional life, faithfully attending meetings for mosquito workers in the state, the region, and nationally. He was made a charter member of the Indiana Vector Control Association in 1976 and served as the director of the mosquito abatement program in his county from that year until his death. In 1988 he became president of the American Mosquito Control Association, one of the largest professional groups of its kind in the world. He also assisted other scientific societies, governmental agencies, and international organizations in a variety of committee and governing capacities. Conscious of his civic responsibilities and the importance of teaching nature to young minds, he was a merit badge counselor to both the Girl Scouts and the Boy Scouts of America. He advised numerous conservation organizations and was an ardent defender of the environment, retreating annually to his cabin in Michigan to vacation and reflect on his work.

His love for collegiate sports was renowned. Having been a wrestler at the University of Indiana, he followed that sport closely and often coordinated his spring speaking schedule to coincide with seminar invitations at institutions hosting NCAA wrestling finals or located within easy driving distance of that venue. As a member of the University of Notre Dame's athletic board, he reveled in the football team's success and the opportunities to attend post-season bowl games. At the same time, he was committed to maintaining the high academic achievements expected of Notre Dame's scholar athletes and expected only the very best from them both on and off the field of competition.

The contributions made by Craig to medical entomology are almost incalculable. During his prodigious career, his work and that of his VBL colleagues consistently joined laboratory and field research, thereby leading to the rapid development of promising leads that could be verified shortly after their discovery. This approach, with its commitment to deriving "real world" answers required by vector control professionals promulgated a much-needed trend in this area of entomology. The diversity and amount of biological information developed for the major aedine mosquito species studied by Craig and associates is enormous and, because of its fundamental nature, largely remains useful. As alluded to earlier, key aspects of the genetic information on *Ae. aegypti* are currently being used in an attempt to transform the genome of that and related species by introducing favorable traits into mosquitoes. The field ecology findings for the three major *Aedes* species studied by the VBL will also be extremely important as attempts are made to release transgenic mosquitoes into natural populations to insert and maintain genes that modulate vector efficiency.

During his tenure at Notre Dame, Craig recruited and trained an enormous number of students and postdoctoral fellows and he assisted dozens of scientists visiting the VBL for sabbaticals or short-term sojourns. His scientific accomplishments and engaging personality inspired students and professionals at other institutions to pursue vector biology. Thus, in the context of achievements during his career, this living legacy of scientific talent and commitment to vector biology is his greatest contribution and the one of which he would be most proud. When Craig's obituary appeared in *The New York Times* , it noted his passing by announcing that he was an entomologist "feared by mosquitoes." This pronouncement would also give him great satisfaction.

I thank Morton Fuchs, University of Notre Dame, for sharing biographical information and providing a photograph of Craig taken by the University of Notre Dame's Publications and Graphic Services in 1989. This photo appeared in the 1990 faculty directory. I particularly want to thank Leonard Munstermann, Yale School of Medicine, who contributed valuable bibliographical information denoting VBL publications from 1959 to 1996. Other sources of information included *The New York Times* (December 23, 1995), *The Washington Post* (December 24, 1995), and an obituary written by Bruce Eldridge, University of California, that appeared in the *Journal of Vector Ecology.*

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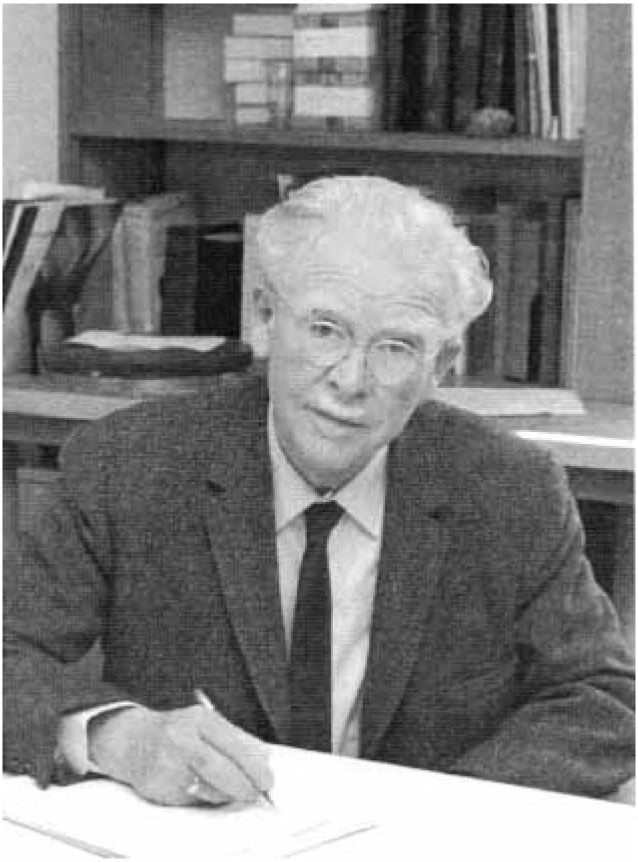

Scott E Forbush

Scott Ellsworth Forbush

April 10, 1904 - April 4, 1984

By James A. Van Allen

Scott Forbush laid the observational foundations for many of the central features of the now huge field of solarinterplanetary-terrestrial physics. The heart of his research was the patiently meticulous and statistically sophisticated analysis of the temporal variations of cosmic-ray intensity, as measured by ground-based detectors at various latitudes and altitudes, and the correlation of such variations with presumptively causative or at least related geophysical and solar phenomena. Among the latter were magnetic storms, solar activity, rotation of the Earth, and rotation of the Sun.

Working almost alone with only technical assistance, Forbush either discovered or put on a reliable basis for the first time the following fundamental cosmic-ray effects:

- The quasi-persistent 27-day variation of intensity;
- The diurnal variation of intensity;
- The absence of a detectable sidereal diurnal variation of intensity;
- The sporadic emission of very energetic (up to several GeV) charged particles by solar flares;
- Worldwide impulsive decreases (Forbush decreases) of intensity followed by gradual recovery;
- The 11-year cyclic variation of intensity and its

anticorrelation with the solar activity cycle as measured by sunspot numbers; and

- The 22-year cycle in the amplitude of the diurnal variation.

His pioneering work provided the empirical foundations and inspiration for an immense amount of subsequent research. For the most part, he made only tentative forays into the detailed theory of the relationships he established. Nonetheless he was thoroughly aware of the speculative interpretations of others and embraced them enthusiastically insofar as they were consistent with his perception of the facts. This characteristic of his work is well illustrated by his classical review article "Time-Variations of Cosmic Rays" in the *Handbuch der Physik* (1966).

EARLY YEARS

Forbush was born in 1904 on a farm near Hudson, Ohio. His boyhood was typical of the rural Midwest during that epoch, walking back and forth to a country school 2 miles from his home and taking over a progressively increasing share of the farm chores. His mother, a teacher, nurtured his curiosity and keen interest in learning and enrolled him in the nearby Western Reserve Academy, from which he graduated second in his class in 1920. A year later he entered the Case School of Applied Science in Cleveland and graduated in June 1925 with a major in physics. He then tried graduate study in physics at Ohio State University for a brief period, but he visualized observational geophysics as much more appealing than pure physics and sought employment in that field. He resumed formal graduate work later but with a fresh appreciation of its direct applicability to his research.

1926-32

After a year's employment by the National Bureau of Standards in Washington, D.C., Forbush joined the staff of the Department of Terrestrial Magnetism (DTM) of the Carnegie Institution of Washington (CIW) in September 1927. This appointment was the pivotal point in his professional life. His first job was as an observer at DTM's magnetic observatory at Huancayo, Peru, in the Andes about 100 miles east of Lima. Two years later he joined the staff of the famous nonmagnetic sailing ship *Carnegie*, a vessel built specifically for DTM's worldwide survey of the geomagnetic field. On November 29, 1929, an explosion and consequent fire destroyed the ship while she was at anchor about a mile offshore in the harbor of Apia, Western Samoa. Forbush was on board at the time, but by a quirk of fate, he escaped unscathed. He attributed his good fortune to having decided to take a daytime nap rather than return to work in the ship's photographic darkroom in a compartment near the explosion. He then returned to DTM, and ten months later was reassigned to Huancayo. In the autumn of 1931 he again returned to Washington, and was granted a year's leave of absence to take graduate courses in physics and mathematics at Johns Hopkins University.

In July 1932 Forbush married Clara Lundell, a concert pianist and former teacher of piano and organ at the University of Michigan. The couple had no children. Their marriage was a model of mutual support and harmony for thirty-five years before her death in 1967. Indeed, she inspired her husband to a new level of purposeful and productive professional work, as he later wrote.

1932-40

This period set the tone of Forbush's subsequent career. Back at DTM in Washington, he undertook the reduction and analysis of gravity observations by the *Carnegie* and the detailed study of the characteristics of magnetic instruments as related to data from observatories and field surveys. He developed a passion for sorting out significant effects in large bodies of geophysical data in the face of inevitable instrumental errors and irrelevant natural fluctuations. And he pursued a rigorous program of after-hours courses in physics, statistics, and applied mathematics at George Washington University, the U.S. Department of Agriculture, and the National Bureau of Standards.

In response to a 1932 recommendation by Robert A. Millikan and Arthur H. Compton, the CIW sponsored the development of a network of detectors for the continuous recording of cosmic-ray intensity. Such an undertaking was judged broadly supportive of DTM's mission of investigating all aspects of the Earth's magnetic field. Appropriate detectors were developed under Compton's direction and became known as Compton-Wollan-Bennett ionization chambers or simply as model C cosmic-ray meters. The central element of each of these devices was a spherical steel shell of 19.3 liters volume filled with highly purified argon at a pressure of 50 atmospheres. A carefully controlled electrical potential between the outer shell and a central electrode caused the collection of a current as penetrating cosmic rays ionized the fill gas. Ingenious auxiliary devices were used to balance out this collected current and record the departures from its mean value on a photographic strip chart with an electrometer.

These meters measured charged secondaries (principally μ-mesons, as was later realized) of the primary cosmic radiation after its traversal of the Earth's external magnetic field and its interaction with the overlying atmosphere. Forbush made a detailed study of the technical characteristics of the meters and the effects of temperature and barometric pressure. The latter effect was particularly important because of its diurnal, seasonal, and other variations. He became a master of the calibration and maintenance of these meters and of the necessary corrections. He was then responsible for establishing the initial network of the model C meters in collaboration with national research groups in the several countries in which they were located. The first meter was placed in operation in Cheltenham, Maryland (geomagnetic latitude 50.1º N, altitude 72 meters) in March 1935. The second and third meters of the network were placed in operation in June 1936 in Huancayo, Peru (0.6° S, 3,350 m) and Christchurch, New Zealand (48.6° S, 8 m). Others were added later.

The data from these meters were central to Forbush's subsequent research for many years. In scaling the photographic traces and converting them to numerical tables he had the dedicated technical assistance of Isabelle Lange until 1957 and then of Lisellote Beach until the latter's retirement in 1975. Otherwise Forbush led a largely solitary but never lonely professional life searching the cosmicray data for periodicities, trends, impulsive events, and associations with related geomagnetic and solar data. This was a challenging task in the face of statistical fluctuations and variability due to multiple and perhaps unrelated physical causes. Following in the footsteps of the German geophysicist Julius Bartels, he became a master of this process, often depending on Bartels' concept of the harmonic dial in searching for periodicities and assessing their validity. The harmonic dial consists of a vector whose length represents the amplitude of the periodic variation and whose direction corresponds to the phase as displayed, for example, as a hand on a 24-hour clock of Greenwich Mean Time. One test of validity was the carefully calculated statistical significance or insignificance of the relationships among harmonic dials for subdivisions of the data or for the data from different stations. Forbush benefited greatly from Bartels' visits to DTM and he enjoyed the durable encouragement and support of John A. Fleming, DTM's longtime director. Many of his preliminary findings appeared in the yearbooks of the CIW before formal publication.

His short (one figure plus one page of text) 1937 paper entitled "On the Effects in Cosmic-Ray Intensity Observed During a Recent Magnetic Storm" showed a convincing association between simultaneous impulsive decreases and subsequent recoveries of cosmic-ray intensity at both Cheltenham and Huancayo and the magnitude of the horizontal component of the geomagnetic field. Such impulsive decreases of cosmic-ray intensity in a few hours at the times of geomagnetic sudden commencements and the subsequent slow (typically a few days to a few weeks) recovery of intensity thereafter received the durable designation of "Forbush decreases." He modestly accepted this term, remarking occasionally that he preferred it to "Forbush declines." It was already widely accepted that sporadic bursts of solar corpuscular radiation (plasma) caused magnetic storms whose main phase was plausibly attributable to a westward-flowing equatorial ring current (Störmer ring current) whose radius was of about six Earth radii. The similar time signatures of the decreases of cosmic-ray intensity and of the main phases of magnetic storms led Forbush and others to suggest that the hypothetical ring current was also the direct cause of the cosmic-ray effect. This seductive although only qualitative explanation became known as the local or geocentric hypothesis.

In comprehensive follow-on papers in 1938 and 1939, Forbush used cosmic-ray and magnetic-storm data from Cheltenham (United States), Huancayo (Peru), Hafelekar (Germany), Christchurch (New Zealand), and Teoloyucan

(Mexico) to establish the occurrence of worldwide decreases in cosmic-ray intensity in association with magnetic storms. This impressive body of data further encouraged Forbush to continue to espouse a common geocentric cause for both effects although he gave a full description of one example to the contrary, namely a large magnetic storm during which there was no perceptible change in cosmic-ray intensity.

Over two decades elapsed before the physical causes of Forbush decreases were convincingly identified. Further remarks on this matter are included in the 1958-84 section of this memoir.

In two other important 1937 papers Forbush used the harmonic dial technique to report (a) the absence of a sidereal diurnal variation of cosmic-ray intensity and hence the isotropy of the radiation within the galaxy and the absence of detectable extragalactic contributions and (b) the clear presence of a 24-hour diurnal variation and its implication of a solar-interplanetary cause.

In a short 1939 paper he gave reasons for doubting the hypothesis that cosmic-ray decreases could be caused by variations in a general dipolar magnetic field of the Sun.

1940-45

As the early phases of World War II spread in other parts of the world and threatened to engulf the United States, Forbush temporarily laid aside his own beloved research. From October 1940 to December 1945, he was on leave from DTM heading a division on mathematical analysis at the Naval Ordnance Laboratory (NOL) and in 1944-45 a related section of the Office of Scientific Research and Development. His work at NOL contributed importantly to the development of degaussing techniques for ships and submarines (i.e., the use of systems of electrical current-carrying coils to annul or at least greatly reduce the external magnetic field of a seagoing vessel and thereby reduce its susceptibility to magnetic mines). On the complementary side of the problem, he guided the development of airborne magnetometers for the detection of submerged submarines.

1945-57

After return to DTM following World War II, Forbush quickly made another seminal discovery by retrospective study of ionization chamber data from Cheltenham, Godhavn (Greenland), Christchurch, and Huancayo. In the records of the three high- and mid-latitude stations, he found large impulsive increases in cosmic-ray intensity on February 28 and March 7, 1942, and on July 25, 1946, each following an exceptionally large solar flare. No increase was observed at the equatorial station, Huancayo. The brief (» hours) increases were precursory to large Forbush decreases at all four stations. He identified the increases as establishing the solar origin of impulsive emission of energetic particles having energies up to at least 3 GeV, but less than the geomagnetic cutoff at Huancayo, about 15 GeV. Study of the sporadic solar emission of energetic protons and heavier ions and of electrons has subsequently become a major field of research in solar and interplanetary physics as the energy and intensity thresholds for their detection have been progressively lowered, especially by instruments on spacecraft. Hundreds of such events are documented in the recent literature on the subject.

Forbush's geophysical program was again interrupted for about a year from July 1951 to August 1952 by the Korean War. During this period, he directed a mathematical analysis division of an operations research office based at Johns Hopkins University.

One of his most celebrated papers was completed and published in 1954 under the title "Worldwide Cosmic-Ray Variations, 1937-1952." In it he demonstrated that the intensity of galactic cosmic rays varied synchronously with the previously well known 11-year cycle of solar activity, being anti-correlated with sunspot numbers (i.e., greatest when solar activity was the least and vice versa). The following is quoted from the abstract of that paper:

> Annual means from continuous registration of cosmic-ray ionization at four stations from 1937 to 1952 show a variation of nearly four per cent, which is similar at all stations and which is negatively correlated with sunspot numbers. This variation in cosmic-ray intensity is quite similar for the annual means of all days, international magnetic quiet days, and international magnetic disturbed days, which indicates that it is not due to transient decreases accompanying some magnetic storms. . . .

Once again Forbush led the way for scores of others who, as of 1997, continue to investigate the solar modulation of cosmic-ray intensity.

Other related solar effects that Forbush first placed on a firm statistical foundation were the 27-day quasi-persistent variation of cosmic-ray intensity, identified with the synodic rotational period of the Sun, and the diurnal variation of intensity, earlier work on which was noted above.

1957-58

In 1957 Forbush was named chairman of a section on theoretical geophysics at DTM, then directed by Fleming's successor Merle A. Tuve. At about the same time he became chairman of the Panel on Cosmic Rays of the U.S. National Committee for the 1957-58 International Geophysical Year (IGY). As such, he played an important role in organizing and coordinating both national and international efforts in the observation of cosmic-ray intensity on a worldwide basis, especially by the use of neutron monitors developed by John A. Simpson. Networks of these monitors proved to be an important part of the IGY, and they continue to provide valuable data up to the present date and probably far into the future.

1958-84

During the IGY and thereafter Forbush fleshed out and extended his earlier seminal work on the relationships among solar activity, geomagnetic storms, and cosmic-ray intensity. Also he traveled extensively to lecture at international meetings and expanded his personal research style to include more collaboration with other investigators.

In several years encompassing the IGY the physical causes of Forbush decreases and the solar modulation of cosmicray intensity were finally placed on a convincing basis. The geocentric hypothesis for Forbush decreases has been described above. It was favored on phenomenological grounds by Forbush and was supported on theoretical considerations by Eugene N. Parker in 1956. An alternative hypothesis was proposed and argued persuasively by Philip Morrison, also in 1956. In it he visualized sporadic emission of clouds of magnetized plasma (or solar corpuscular streams) from active regions on the Sun. Such clouds would modulate the cosmic-ray intensity in interplanetary space *and* produce terrestrial magnetic storms. In Morrison's scenario, both effects had a common cause, but magnetic storms did *not* cause Forbush decreases.

In a series of theoretical papers, 1953-56, Ernest C. Ray discussed the quantitative effect of a ring current on geomagnetic cutoff energies for cosmic rays. He found that a ring current of appropriate magnitude and radius to produce the main phase of a large magnetic storm was inadequate to cause a significant change in mid- and low-latitude cutoffs. Moreover, the effect was of the opposite algebraic sign to that required to explain a Forbush decrease. In retrospect it is most remarkable that these papers did not come to the attention of the proponents of the geocentric hypothesis.

In early space experiments in 1959 and more convincingly in 1960, Simpson and his collaborators Charles Y. Fan and Peter Meyer observed a Forbush decrease in interplanetary space far from the Earth and Paul J. Coleman, Jr., et al. observed, also on the same 1960 spacecraft the simultaneous passage of a large increase (» factor of ten) in the interplanetary magnetic field. These observations were widely accepted as confirming the Morrison hypothesis and disposing of the geocentric hypothesis. Forbush, who had continued to be somewhat uneasy about the latter, was delighted to embrace those new findings, as was Parker, who soon became the foremost developer of the interplanetary hypothesis and its extension to encompass the 11-year cyclic variation as well.

As of 1997, scores of Forbush decreases have been observed by instruments on spacecraft near the Earth and by those very remote from the Earth. The most noteworthy Forbush decrease in the history of the subject was observed at the Earth on June 12, 1991, at Pioneer 11 on August 21 at 34 AU from the Sun, and at Pioneer 10 on September 30 at 53 AU, the progressively greater delay being attributed to the outward propagation of the burst of solar plasma.

The solar cycle variation of cosmic-ray intensity has also been massively confirmed and greatly illuminated by ground-based neutron monitors and space-based detectors, both much more sensitive to the lower-energy portion of the spectrum of primary cosmic rays than were the ionization chambers whose data Forbush used. There is little doubt that this effect is also encompassed by some form of the Morrison hypothesis. There remains the unsolved issue of the radial extent of the solar modulation, now known by direct observation to extend beyond 67 AU from the Sun.

In 1959 Forbush gave a series of lectures entitled "Geomagnetism, Cosmic Radiation and Statistical Procedures for Geophysicists" at the Peruvian National Universities of San Marcos (Lima), San Agustin (Arequipa), and Cuzco. In 196061, as a visiting professor in the department of physics and astronomy at the University of Iowa, he repeated these lectures and was the senior author of two valuable papers on the variability of the trapped particle population of the Earth's newly discovered radiation belts. Also in 1961 he was a visiting investigator at the Royal Institute of Technology in Stockholm, Sweden, and in 1968 at Imperial College, London.

His own version of much of his life's work and its relationship to the work of others is contained in an extended review article in *Handbuch der Physik* (1966) entitled "Time Variations of Cosmic Rays," the most valuable single reference of the appended bibliography.

During the last few years of his life, he enjoyed a fruitful collaboration with Doraswamy Venkatesan and with Martin A. Pomerantz and others of the Bartol Research Foundation. A year before his death he was senior author of a paper in *Solar Physics* as a result of the latter collaboration. This work refined criteria for the statistical significance of results from the superposed epoch analysis of geophysical and solar data.

GENERAL COMMENTS

Scott Forbush was profoundly influenced by Julius Bartels, who was a research associate at the Department of Terrestrial Magnetism of the Carnegie Institution from 1936 to 1940, and by Sydney Chapman, who occasionally visited there. During this pre-World War II period, the latter was collaborating with Bartels on completing their classical two-volume monograph *Geomagnetism* (Oxford, 1940). Forbush often acknowledged Bartels' personal guidance and his published papers on statistical methods for analyzing geophysical data and he extended Bartels's techniques and applied them with conspicuous success to a variety of problems, especially those involving the temporal variations of cosmic-ray intensity. In his research, Forbush was patient, persistent, and as objectively critical of his own work as he was of the often less careful and less rigorous work of others.

He traveled widely and was a standard contributor to international conferences on cosmic rays and related subjects. He typified a statement of a colleague: "Study cosmic rays and see the world." He carried a battered leather briefcase for many years and took a quiet pride in the dozens of stubs from airlines, shops, and hotels that he allowed to accumulate on its handle, a kind of archaeological record of his travels.

In his personal life, Forbush was shaken by the death of his wife Clara in 1967. The two had very different professional careers, he in science and she in music. But they were devoted to each other and delighted in each other's achievements. She once described their marriage as a two-person mutual admiration society. Their home was in Chevy Chase, Maryland, a suburb of Washington, D.C. He retired from the DTM staff in 1969, but he continued his research there for many years.

A visitor to Forbush's office at DTM in the late 1970s would have found him seated at a large work table carefully reading and annotating a preliminary manuscript. At that time he depended on flip-down magnifying lens attached to his usual spectacles for reading. Also, he often would have a roll-your-own cigarette, either lighted or unlighted, bobbing up and down between his lips as he greeted the visitor.

Forbush was a member of the Washington Academy of Science, the Philosophical Society of Washington (president, 1953), a fellow of the American Geophysical Union and of the American Association for the Advancement of Science, and a member of the Cosmos Club.

Among his honors were the title of Catedratico Honorario of the Republica del Peru, Universidad Nacional Mayor de San Marcos de Lima (1959); the Sir Charles Cree Medal and Prize, eleventh award of the United Kingdom's Institute of Physics and the Physical Society for "distinguished research in terrestrial magnetism, atmospheric electricity, and related subjects . . . cosmic radiation" (1961); an honorary doctor of science degree from Case Institute of Technology (1962); and the Waring Prize of Western Reserve Academy (1974).

He was elected to membership in the National Academy of Sciences in 1962, and in 1966 he received the especially appropriate John Adam Fleming Award of the American Geophysical Union "in recognition of outstanding contribution to the description and understanding of electricity and magnetism of the Earth and its atmosphere."

The writer of the present memoir, a friend and ardent admirer of Forbush for many years, edited his 1959-60 Peru/ Iowa lectures, much of them handwritten, and assembled these and a compilation of his original published papers into a monograph entitled *Cosmic Rays, the Sun and Geomagnetism: The Works of Scott E. Forbush*, published in 1993 by the American Geophysical Union, Washington, D.C. This volume also includes a tribute by Pomerantz from which the writer has drawn some material.

In June 1970 Forbush married Julie Daves, a science writer and watercolor artist, who, among other achievements, founded and edited the monthly publication *Space Science News* of the Smithsonian Institution's National Air and Space Museum. In 1982 the couple moved to Charlottesville, Virginia, where Scott died of pneumonia in 1984, being survived by Julie and his sister Louise Boyd of Hudson, Ohio.

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Ross Gunn

Ross Gunn

May 12, 1897-October 15, 1966

By Philip H. Abelson

Ross Gunn was one of the most versatile physicists of the early and mid-twentieth century. He made significant contributions to knowledge in many fields of science and technology. He created novel instrumentation, much of which was designed to facilitate studies of natural phenomena such as thunderstorms. In the course of his career he obtained more than forty patents.

From 1927 to 1947 Gunn was a research physicist on the staff of the U.S. Naval Research Laboratory. In 1934 he was appointed technical adviser for the entire laboratory. In that role he interacted with important naval personnel. In March 1939 he wrote a memorandum to Admiral H. G. Bowen, chief of the Navy's Bureau of Ships, outlining the tremendous advantages that could be expected from the use of atomic energy in submarine propulsion.

In the latter years of World War II Gunn was simultaneously superintendent of the Mechanics and Electricity Division, superintendent of the Aircraft Electrical Division, and technical director of the Army-Navy Precipitation Static Project, as well as technical adviser to the naval administration. He also fostered development of the liquid thermal diffusion method for separation of uranium isotopes. This led to large-scale use of the process by the U.S. Army's Manhattan District at Oak Ridge, Tennessee.

In February 1947 Gunn became director of the Weather Bureau's Physical Research Division, where for ten years he conducted and supervised important research related to severe weather phenomena. Until his death in 1966, he remained active in research and consultation while a professor of physics at American University.

Ross Gunn was born in Cleveland, Ohio, on May 12, 1897. His forebears were of Scotch and English descent. Three of his ancestors were soldiers in the American Revolution; two served as officers directly under George Washington. His father Ross D. A. Gunn was a graduate of Western Reserve Medical College and a practicing physician. As undergraduates, both his father and mother (Lora A. Conner) attended Waynesburg College in Pennsylvania.

In 1923 Ross married Gladys J. Rowley, an alumna of Oberlin College. Over the next fifteen years four sons were born—Ross, Jr., Andrew Leigh, Charles, and Robert Burns. All have had successful professional careers. Ross Gunn, Jr., who holds a degree in electrical engineering and an M.B.A., is in business in California. Rev. Andrew Leigh Gunn attended Yale Divinity School and is a minister. Charles Gunn is an aeronautical engineer and at one time was director of the NASA shuttle program. He is now engaged in private enterprise. Robert Burns Gunn is currently professor of physiology and chairman of the Department of Physiology at the Emory University School of Medicine.

While in high school in Oberlin, Ohio, Gunn became interested in amateur radio (then called wireless telegraphy). Without help, he built a successful wireless receiving apparatus and qualified for a commercial wireless operator's license. He also built one of the first long-range amateur wireless stations in northern Ohio and carried on conversations with amateur stations in most regions of the United States. These early activities in radio are reminiscent of the youthful interests of other physicists, including Ernest O. Lawrence and Merle A. Tuve.

Ross entered Oberlin College in 1915, but after two years he transferred to the University of Michigan. With the entry of the United States into World War I he enlisted as a private in the Signal Corps and was later called to active duty. He received his B.S. degree in electrical engineering in 1920 and an M.S. degree in physics in 1921 from the University of Michigan.

In the interval from 1921 to 1923 Gunn spent a year and a half with the U.S. Air Service as a radio research engineer. As part of his duties he did pioneer work in developing a radio range aircraft navigation system. In the course of this work he made a number of the first cross-country instrument flights. While employed in the U.S. Air Service he also developed devices for radio control of pilotless airplanes (drones). The Navy later used this technology as the master control mechanism for fifty of its first pilotless aircraft.

The years from 1923 to 1927 were spent at Yale University, where Gunn held an appointment as instructor in engineering physics and where he received a Ph.D. degree in physics in 1926. One of his mentors at Yale was Professor Leigh Page, a theoretical physicist. A consequence was a good grounding in classical physics.

In his later career Gunn combined excellent capabilities in identification of important problems with skill in developing innovative instrumentation, a zest for experimental work, and aptitude for theoretical analysis of practical problems.

In 1927 Gunn accepted an offer from the Naval Research Laboratory to become a research physicist in the Radio Division. He intended to spend only a few years at the laboratory, but he remained there until 1947. In the pre-war years the civilian staff was small and the naval officer management was willing to encourage pioneering basic research related to radios, the new electronics, and instrumentation employing vacuum tubes. Gunn was skilled in these areas, and he interacted well with naval personnel. Within a year he was promoted to assistant superintendent of the Heat and Light Division. He was allowed to choose his own agenda. During the period 1929-33 Gunn published twenty-eight articles in the open literature. Most of the items were theoretical treatments of natural phenomena, such as terrestrial and solar magnetism, cosmic rays, and other astrophysical phenomena. Thirteen of the articles were published in *Physical Review.* The remainder appeared in other standard journals. During this highly productive period Gunn invented and was subsequently granted seventeen patents on useful instrumentation. One device was an induction-type electrometer that could produce an induced alternating voltage from a small free charge. The basic principle was incorporated in a large number of instruments, including the vibrating reed electrometer. In addition to these activities, Gunn conducted classified research relevant to naval problems.

In 1934 Gunn was appointed technical adviser for the entire Naval Research Laboratory. He became responsible for the quality of the technical program and its coordination with the needs of the naval service. He took this top administrative and scientific job with the understanding that he would be given skilled assistance and that he would be allowed to continue his own research on problems of interest to the Navy.

In 1938 Gunn invented and subsequently patented another instrument that was widely used—a portable device that amplified thermocouple electromotive forces. The instrument was useful in detecting infrared radiation emitted by enemy ships and aircraft.

During the World War II years Gunn was assigned many administrative duties in addition to his role as technical adviser to the naval administration. One of them was to act as technical director of the Army-Navy Precipitation Static Project. This was a successful effort to identify and alleviate interference produced on aircraft flying through ice-crystal clouds or snow. A group headquartered in Minneapolis conducted the major part of the investigation.

Immediately after the announcement of the discovery of uranium fission in early 1939, Ross Gunn became a keen observer of and participant in developments relevant to nuclear power. He was particularly interested in its possible application to propulsion of submarines. Conventional submarines were propelled by batteries, which in turn were charged by electricity supplied by generators coupled to diesel engines. These required air. While near the surface of the ocean, the submarines were vulnerable to detection and attack.

By mid-1940 it had become evident that the rare ^{235}U was fissionable and that a chain reaction creating nuclear power was likely to be achieved. Gunn learned that I was conducting experiments on uranium isotope separation and arranged to provide me with financial support. I was then an employee of the Carnegie Institution of Washington. I obtained my first tiny isotope separation using equipment manufactured by me, but housed at the National Bureau of Standards. The method involved liquid thermal diffusion of uranium hexafluoride (UF_6). The simple apparatus consisted mainly of three concentric tubes 12 feet long. The inner tube was heated by steam. A second tube was maintained at 65° C. The third tube served to contain the 65° C cooling water. The UF_6 occupied the space between the walls of the inner and middle tubes. Runs on this column were made in April 1941, when a measurable isotope separation was obtained.

When Gunn learned that I had achieved a small separation of uranium isotopes, he invited me to join the staff of the Naval Research Laboratory, where enhanced supplies of high-pressure steam could be made available. In June 1941 the move was made. A series of experiments was conducted to determine the optimum spacing between the hot and cool walls. In June 1942 a column 36 feet long heated by 100 psi of steam produced an isotope separation factor of 1.11. This success led to an expanded effort that included authorization to build and operate fourteen columns 48 feet long. It also led to the procurement of a propane-fired boiler capable of delivering 1,000 lb/in^2 of steam. For a time, the facility at the Naval Research Laboratory was the world's most successful separator of uranium isotopes.

Ross Gunn, who was a member of the federal government's S-1 uranium committee, communicated results of the isotope experiments to committee chairman Lyman J. Briggs in August 1942. This led in October 1942 to a visit to the Naval Research Laboratory by General Leslie R. Groves and Admiral W. R. Purnell. Later, in January 1943, a special committee assembled by the Manhattan District inspected the installation. The committee was impressed by the simplicity of the equipment and commented favorably.

A Naval Research Laboratory report submitted to the Bureau of Ships by Gunn in January 1943 pointed to the advantages of using enriched uranium in nuclear reactors. It would be a necessary step in creating a nuclear-powered submarine. The report also stated, "A liquid thermal diffusion plant costing one to two million dollars could provide the necessary separated isotopes."

During the next six months, improvements were made in the construction of the separation columns. At the same time, the pilot plant produced 236 pounds of UF_6 possessing isotope separation. The quantity and the separation were greater than had been obtained by the gaseous diffusion method at that time.

Gunn decided that an expansion of production capabilities of the liquid thermal diffusion method was warranted. Doing so would provide an alternative if the Manhattan District's magnetic and gaseous diffusion methods failed. A survey of naval establishments showed that large-scale sources of high-pressure steam could be made available at the Naval Boiler and Turbine Laboratory at the Philadelphia naval base. Authorization to build a 306-unit plant at Philadelphia was obtained on November 27, 1943. Rear Admiral Earle Mills, assistant chief of the Bureau of Ships, signed the project order.

In June 1944 the Philadelphia plant was approaching completion. J. Robert Oppenheimer learned of the progress and recognized that a supply of partially separated uranium would increase the production of an electromagnetic plant at Oak Ridge. He communicated with General Groves, who sent a reviewing committee to the Philadelphia plant. Its report was favorable and led to the decision to build a 2,142-column plant at Oak Ridge. Construction there was rapid. The $20-million facility achieved production that shortened the duration of World War II by eight days.

Secretary of the Navy James Forrestal presented the Navy Distinguished Civilian Service Award to Ross Gunn on September 4, 1945. The citation included:

> For exceptionally distinguished service to the United States Navy in the field of scientific research and in particular by reason of his outstanding contribution to the development of the atomic bomb . . . For his untiring devotion to this most urgent project, Dr. Gunn has distinguished himself in a manner richly deserving of the Navy's highest civilian award.

Immediately after the end of the war Gunn returned to the concept of the nuclear submarine. Methods of detecting diesel-powered submarines had advanced greatly. In the latter part of World War II large numbers of German submarines had been destroyed. I was tasked with becoming familiar with the current state of nuclear reactors, particularly those using enriched uranium. I was provided with access to experimental programs at Oak Ridge and Argonne, and I participated in criticality experiments of enriched uranium assemblages.

Gunn also took part in obtaining blueprints of the most advanced German submarine. Analysis showed that the energy system of the submarine could be replaced by a shielded nuclear reactor. In September 1946 a report on the feasibility of a nuclear submarine was submitted to the Bureau of Ships. Later, Admiral Hyman Rickover directed a highly successful development and construction of nuclear submarines. However, some part of the credit for nuclear submarines belongs to Ross Gunn.

In the late autumn of 1946 Gunn decided he would not accept additional naval administration duties. Rather, he would return to more science-oriented activities. In February 1947 he transferred to the Weather Bureau to organize and direct a fundamental study of the basic physics of weather. A core objective was to investigate the processes responsible for precipitation under various physical conditions. His first task as director of the Weather Bureau's Physical Research Division was to organize a program to study the practicality of producing rain by cloud seeding. Results showed that while sometimes rain was produced, it was insufficient to be of much economic value. Subsequent events showed this early conclusion to be correct.

Physicists who are inclined to observe and study natural phenomena have been presented with great opportunities and puzzles. Among these are solar-terrestrial climatic effects, possible human-induced global warming, and violent weather phenomena. As early as 1935 Gunn became sufficiently interested in thunderstorms to begin studies on them. A paper titled "The Electricity of Rain and Thunderstorms" was published in *Terrestrial Magnetism and Atmospheric Electricity*. In 1944, after being named technical director of the Army-Navy Precipitation Static Project, Gunn participated actively in choosing instrumentation for it and in devising research and instrumentation aspects. Later he analyzed many of the experimental results. In the course of the program, airplanes flew through twenty-five thunderstorms collecting valuable data. These measurements provided what was then and later the best available cross section of thunderstorm electrification data. The airplanes were equipped with induction-type electric field meters placed on both the top and bottom of the main cabin. An apparatus capable of measuring the electric charge on snow and raindrops was installed under one wing. Simultaneous measurements could be made on the electric fields and on the charges on drops.

Repeated flights through active thunderstorms showed that the electric fields at levels close to ground were of the order of 1 to 10 volts/cm. These fields generally increased to a maximum in the vicinity of the freezing level. At this point the electric fields frequently exceeded 1,000 volts/ cm. The aircraft encountered both negative and positive fields of 2,000 volts/cm and more. The charges on snow and raindrops were largest when the electric fields were high. The potential differences between the top and bottom of a thundercloud were frequently greater than 10^8 volts.

In 1947, soon after Gunn joined the U.S. Weather Service, he began to devise experiments and new equipment aimed at obtaining better knowledge of the basic physics of weather phenomena. His flair for the development of new equipment was repeatedly evident. An early example was an unprecedentedly huge cloud chamber. A mining shaft in Arizona 700 feet long and 7 feet square was sealed and provided with means to humidify and compress the air within it. When the pressure was suddenly released a dense cloud formed throughout the chamber. If water drops of known size were released at the top of the shaft their growth as they passed through the cloud could be measured. In another set of experiments performed with different equipment the terminal velocity of various sizes of water drops was determined. The extensive data obtained from these studies were a unique contribution. The data continue to be widely used and quoted.

From the instrument development efforts emerged electric field meters capable of operating continuously in very heavy rain, whether on the ground or on aircraft. Instruments were also created to determine the sign and magnitude of free charges carried on falling rain. About these and other activities aimed at developing instrumentation Gunn could state, "As a direct result of efforts to develop new and better instruments, we have the largest store of coherent measurements yet made in the field of atmospheric electricity."

As a fellow of the Institute of Radio Engineers, Gunn was invited to write an article that appeared in 1957 describing recent developments in an important field of his choosing. He chose to present some of the knowledge that had been created about thunderstorms. I have selected a few items from the article to paraphrase and summarize:

- Cosmic rays and radioactivity produce at heights just above ground level about 10 positive and 10 negative ions/ sec. At an altitude of 15 km, the rate of production is about 45 pairs/sec.

  In a cloud that is not yielding rain the net overall charge is zero. However, droplets in the cloud become charged. About half are positive and about half are negative.
- Charges on raindrops may become enhanced when small

drops join together to make large drops. When there is turbulent motion in the cloud, the relative mobility of plus and minus ions results in differential charging and in separation of charged droplets. The electrification observed in thunderstorms implies a gross separation of free electrical charges with a consequent expenditure of large amounts of energy.

- An important index of thunderstorm activity is the electric field measured both at ground level and inside an active cloud high above. In fair weather, the surface field intensity is negative and of the order of-1.5 volts/cm. In a typical thunderstorm the electric field at the ground may increase to +/- 100 volts/cm and more. The field may be negative part of the time, but mostly it is positive. The field changes instantaneously during and immediately after a lightning strike and then recovers. Often the direction of the field overshoots during the strike.
- The typical summer thunderstorm is about 20 km in diameter. The cloud mass itself may be somewhat larger. It commonly extends vertically about 12 km, but occasionally can extend to 15 km.
- Normal lightning activity is not observed in clear air except in the vicinity of falling precipitation. The principal electrical effects accompanying a thunderstorm are closely related to the production and fall of precipitation, but the connection of lightning and precipitation is not a direct one.

From 1947 until his death in 1966 Gunn devoted most of his efforts to the study of atmospheric phenomena. He created improved measuring equipment on which he was granted about thirty patents. He directed research while also conducting measurements. A substantial part of his effort was devoted to theoretical analysis of results obtained by him and others. He also published about forty articles in the scientific literature. He was the sole author on most of them.

After he left the Weather Bureau in 1957 Gunn's rate of publishing diminished. Much of his time was spent as a consultant. However, in the last four years of his life he returned to active experimental work and the development of instrumentation. The core of his efforts related to aspects of the physical phenomena occurring in thunderstorms. His professional office was located at American University, where he was a research professor of physics. One of his last publications was an extensive invited article in *Science,* which described a new instrument for studying effects of collisions between simulated raindrops.

In his approach to research, Gunn followed a procedure that many of the most successful scientists follow. He identified an important phenomenon that involved potentially measurable reproducible effects. He devised or procured instrumentation that would measure the effects more reliably than they had been previously. He analyzed the data using tools of a classical theoretical physicist but with the attitude of a practicing engineer. Others have followed and will follow in his footsteps. Some of his data and analyses will be improved on, but in many instances it will be noted that he was the first explorer to view the new frontier.

After his death an issue of the *Monthly Weather Review,* published by the U.S. Department of Commerce, was assembled as a memorial to Ross Gunn. It contained twenty-three articles, most of which dealt with violent weather phenomena. An exception was a biographical sketch portraying some of Gunn's character traits. Prepared by F. W. Reichelderfer, a member of the National Academy of Sciences, the article was titled, "Ross Gunn, the Scientist and the Individual." Reichelderfer chose to quote a portion of a talk Gunn had given in 1938 in which he described the ideal research physicist. Reichelderfer states that the words quoted reflected the standards set by Gunn as a scientist:

> The scientist should be distinguished by intelligence and firm grounding in the fundamentals of physics, chemistry, and engineering. He should be especially keen in estimating situations and reaching sound decisions. His judgement and perspective should be such that he can give his talents systematic direction. He should be an original thinker . . . exceptional in his ability to plan, think, and do things without being told. He should have the courage of his convictions, yet not be blinded by them. He should *constantly seek the truth.* He should be especially successful in working harmoniously with others toward a common end.

In my dealings with Ross Gunn I noted that in a situation where he was certain of the facts, he did not avoid conflict, and he was resourceful when in a fight. Reichelderfer perceived a different side of Gunn's character. He stated:

> Any man whose work comes to public attention and who holds to his beliefs when the facts support them encounters opponents as well as supporters, especially when his work may incidentally affect the ambitions of others. So Gunn had his critics—this is rather well known. But he has strong support from associates who believe that most of the criticism directed at him was a result of misunderstanding, sometimes misrepresentation or ignorance of what he actually thought and did. Gunn's nature did not make him inclined to waste time in "explaining" to critics. He hoped the facts would speak for themselves and in such matters he preferred to remain silent.

During his life, in whatever role he found himself, Ross Gunn gave the best he could. As a result, his existence made the kind of difference to this world that only a few achieve.

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David Harker

October 19, 1906-February 27, 1991

By Herbert A. Hauptman

The focus of Dave Harker's life, around which all his thoughts and actions revolved, was the science of crystallography, which he dearly loved. To crystallography he gave everything—his time, his energy, his total devotion. So complete was his dedication to this science and so fundamental and many faceted were his contributions that he influenced forever the course of its development. To this day, the Harker section and the Harker construction play essential roles in the determination of the structures of very large molecules. The Harker-Kasper inequalities provided the inspiration for a new branch of X-ray crystallography, the so-called direct methods of phase determination.

PERSONAL HISTORY

Dave was born on October 19, 1906, into a scientific and medical family. He grew up on the side of Mount Tamalpais in Mill Valley near San Francisco within view of the bay end of the Golden Gate Bridge. His father, George Asa Harker, who died when Dave was five years old, was a medical doctor from the University of California at Berkeley. Dave's father introduced the concepts of shape, symmetry, and structure into Dave's life. His earliest memories of his father are of him sitting on the front porch and making plaster molds of his patients' feet, carefully hammering copper into precise forms of arch support.

His mother, Harriette Butler Harker, graduated from Vassar in 1898 and received her M.D. from the University of California at Berkeley. She boasted that she was the first woman in New Brunswick, New Jersey, to go to college, wear trousers, ride a bike, and smoke a cigar. Dave's mother personally took charge of his and his brother's education until the fourth grade. His mother, together with faculty members from Berkeley, taught classes at his high school in exchange for free tuition.

In 1928 Dave graduated with honors in chemistry from the University of California at Berkeley. His undergraduate years had brought him into contact with distinguished faculty that included Joel H. Hildebrand and Wendell M. Latimer.

In 1930 Dave married Katherine De Savich, who, as the daughter of the imperial prosecutor under the tzar, fled Russia in 1917. Katherine later aided Dave in translating scientific books into English. They also spent ten years working on the translation of a Soviet physics journal for crystallography, until her death in 1973. They had two daughters, Tatiana Harker Yates and Liudmilla Harker.

Following the death of his first wife, Dave married Deborah Maxwell in 1974. She died in 1997, six years after Dave's death.

PROFESSIONAL HISTORY

Atmospheric Nitrogen Corp. (1930-33)

After graduation from Berkeley, Dave continued on as a graduate student, but in 1930 he left to take a job as laboratory technician at the research laboratory of the Atmospheric Nitrogen Corp. in Solvay, N.Y. (near Syracuse). There he weighed samples, made mixtures, and occasionally read scientific journals. In one of these he read a paper on the crystal structure of sodium nitrate and its change as the nitrate groups rotate at elevated temperatures. This beautiful result so impressed him that he resolved to study crystal structures in greater depth at some future time.

Caltech: The Harker Section (1933-36)

In 1933 (the depth of the Depression) Dave lost his job. He returned to California with his wife and child, borrowed some money from an old friend of his parents, and entered the graduate school of the California Institute of Technology. There, under the supervision of Linus Pauling, he began to work on the determination of crystal structures using the technique of X-ray diffraction. After some preliminary studies of three or four simple structures, he undertook the solution of his dissertation problem: to determine the structures of the ruby silvers, proustite (Ag_3AsS_3) and pyrargyrite (Ag_3SbS_3), which are isomorphous (i.e., they have the same structure).

Although only six parameters were needed to describe these structures, the methods available at that time (essentially clever trial and error) were totally inadequate. Then, at one of the weekly seminars of Pauling's students, A. L. Patterson's famous 1934 paper on the Patterson function was presented. This function relates the experimentally observable X-ray diffraction intensities with the totality of interatomic vectors in the crystal. Owing to the large number of interatomic vectors, interpreting the Patterson function was, and still is, no easy task. A few nights after the seminar, in Dave's words, he "awoke in the dark, sat up in bed, and yelled, 'It's going to work.'" What he had seen was that the relationships between symmetrically related atoms would produce peaks in the Patterson function on certain planes or along certain lines determined by the known crystallo graphic symmetries. These "Harker" peaks often lead directly to the atomic position vectors and the crystal structure, particularly in those cases when the Patterson function itself is not readily interpretable. Thus was born the famous Harker section, which effectively made the Patterson function useful. In this way Dave quickly deduced the structures of proustite and pyrargyrite and earned his Ph.D. in 1936. The Harker section has withstood the test of time and even today is indispensable for the determination of macromolecular structures, particularly in those cases where the structure contains a small number of heavy atoms, when Patterson techniques are useful.

Johns Hopkins Years: The Donnay-Harker Law (1936-41)

Having become a physical chemist in 1936, Dave took an academic job in chemistry at the Johns Hopkins University, where he taught freshman chemistry, graduate courses in crystal structure, crystal chemistry, and quantum mechanics. He also inherited some X-ray diffraction equipment left over by his predecessor M. L. Huggins.

Since in those days research money was in very short supply, he and his students made their own equipment from secondhand materials. In this way they set up a continuously pumped X-ray tube and with its aid worked on several crystal structure problems. Of these, the structures of acetamide and hydrazinium difluoride were published.

During Dave's tenure at Johns Hopkins, Dorothy Wrinch came to visit the university for about a year. She and Irving Langmuir, who visited Johns Hopkins occasionally, engaged in extended discussions concerning her theories of protein structures. Dave was drawn into their conversations and soon became interested in the problem of protein structure determination. In addition, during this period, W. T. Ashbury of Leeds gave a colloquium on the structures of fibrous proteins. Dave, in his words, "became infected with the protein structure virus, but for many years it lay dormant."

During those years, Dave met Professor J. D. H. Donnay of Johns Hopkins and George Tunnell of the Geophysical Laboratory in Washington, D.C. From these prominent mineralogists Dave learned classical crystallography, some mineralogy, and the significance and measurement of crystal faces. It was Donnay's goal to correlate the internal structure and external face development of crystals. The earlier attempt to do this by Bravais resulted only in a rather poor approximation. Donnay and Harker discovered that the order of decreasing prominence of the faces of a crystal was the same as the order of decreasing interplanar lattice spacings, including the halvings, thirdings, and quarterings due to the space group symmetries. This correlation, while still not perfect, was an improvement over Bravais's earlier attempt. It is known in mineralogical circles as the Donnay-Harker law.

General Electric: The Harker-Kasper Inequalities (1941-50)

In 1941 Dave received an offer from W. D. Coolidge to work in the famous research laboratory of the General Electric Company and after some hesitation he accepted it. He became a member of the metallurgy division at General Electric and proceeded to learn properties of metals using X-ray diffraction and other crystallographic methods. Owing to the liberal policy of the General Electric research laboratory in those days, Dave was not compelled to work exclusively on metals, although he did publish several papers on solid state reactions characteristic of them, including a paper on grain shape and grain growth, another on order-disorder reaction, and several others.

Although Dave is known primarily for his contributions to X-ray crystallography, his metallurgical papers had a considerable impact on the physical metallurgical community. One of these, in particular, was primarily concerned with the microstructural subtleties associated with the ordering reaction in the alloy AuCu in which there is a change in unit cell from cubic to tetragonal. His theoretical analysis of the complex microstructures, which are to be expected as a means for the material to avoid long-range internal stresses, was far ahead of its time and had considerable influence on the research concerned with ordering reactions in alloys.

During his years at General Electric, Dave also developed an X-ray method for finding the orientation of quartz fragments, so that oscillator plates could be cut from them. In addition, he did several pieces of crystallographic work for other divisions of the laboratory. He also started work on the design of X-ray diffraction equipment with which the diffracted intensity would be measured with a Geiger-Müller or other particle counter.

It was during Dave's tenure at General Electric that he and his collaborator John S. Kasper produced their paper on the inequalities among the crystal structure factors, the famous Harker-Kasper inequalities. Because these inequalities constitute the first contribution to the direct methods of phase determination, which now (1997) has a fifty-year history and which continues to be a subject of intense interest, activity, and importance, it is appropriate to describe in some detail the circumstances surrounding their discovery. We are fortunate to have first-hand accounts by the authors. First, Dave's account:

> One problem in particular fascinated us—the determination of the crystal structure of decaborane, $B_{10}H_{14}$. This turned out to be surprisingly difficult. It was borne in upon Dr. John S. Kasper and me that a structure which could not easily be guessed at approximately from known stereochemical principles, could not be solved by the traditional trial and error

methods. Some twenty structures for the $B_{10}H_{14}$ molecule had been published, but none could be made to fit the X-ray diffraction data from the crystals.

One day John Kasper was sitting at his desk staring gloomily at a lot of algebra he had been writing down. I looked over his shoulder and said something like, "What on earth is that?" and he replied "Schwartz's Inequality for a structure factor, but it doesn't seem to help." He then kept on writing, while I looked on. I said, "Oh, well, let's expand those squares of cosines into functions of double angles." So we did. Then it hit us both, I think, at the same time. "Say! We can get the signs of some structure factors from this!" Then we went madly to work, and in a couple of weeks we had enough algebraic apparatus assembled "unitary" structure factors, sum and difference inequalities, etc.—to be useful. Kasper applied this schema to the decaborane data and came out with a preliminary model which explained the diffracted intensities from one zone, and, after another couple of months, the complete structure emerged. Thus was born the subject of "sign determination" from intensities. This was in 1947.

At my request John Kasper sent me his account, with a postscript by his wife Charlys:

Here is my version of the origin of the sign-determining inequalities. First, I would like to give you some background information that may be of interest to you.

At the 1946 meeting of ASXRED (American Society for X-ray and Electron Diffraction) at Lake George, N.Y., a method of attacking the phase problem was presented by A. Booth, namely, the method of steepest descent. While this did not turn out to be a viable method, considerable discussion of the phase problem ensued. Nothing useful resulted, however, and there was a consensus that nothing could be done about obtaining phases and that it was a waste of time to think about it. Among the minority were Dave Harker, Buerger, and Fankuchen, although no convincing evidence could be given to justify the optimistic viewpoint. For Dave and myself the phase problem was on our minds although we were quite busy with other problems at G. E.

I became intrigued with the fact that the straightforward squaring of a real structure factor, F_{hk1} (with cosine terms) contained, in part, the sum of modified cosine squared terms. These latter could be rewritten, by virtue of the relation 2 $\cos^2 A=1+\cos {}^2A$ as components of $F_{2h,2k,2l}$. A relation then exists between F^2hkl and $F_{2h,2k,21}$, but also with the summation of cross terms. I did not know what to do with the cross terms and so I put the thing aside. Some days later (in 1947) it occurred to me that Schwartz's inequality would deal only with the desirable summation of cosine^2 terms. Accordingly, one morning at work I wrote down the relationship between F^2hkl and $F_{2h,2k,2l}$ resulting from the application of Schwartz's inequality. No sooner had I written this down, when Dave walked in the office and looked over my shoulder. "What is that?" Dave asked. "That is the result of applying Schwartz's inequality to a structure factor," I replied. After satisfying himself that what I had written was allright, Dave became quite excited and remarked: "You can determine signs with that." "That's right," I replied.

I was unhappy, however, that the treatment so far was only for the case of one kind of atom. Dave said that could be fixed, and in short order he proposed using the unitary atomic structure factor, $\hat{f}$. This enabled treatment of more general situations.

For the next few weeks Dave was immersed in the applications to various symmetries and space groups, and other ramifications, such as sum and difference formulas. He also produced an elegant write up of the work. I concentrated on its application to the Decaborane problem which was uppermost in our minds.

I realize that my version is not exactly the same as one that Dave has given, but I stand by it. We were in communication in 1989, with the goal of achieving a version that was mutually agreeable, I regret deeply that Dave's illness prevented the completion of that project.

From what you say I wonder if you have the autobiography which was written in 1961, and which Dave sent to me in 1989. It is very interesting reading to anyone who knew Dave. I have little to add to it. I would mention what a good and influential teacher he was. I first knew Dave as a teacher of freshman chemistry at Johns Hopkins. He revolutionized the course with emphasis on basic principles. His approach was adopted by students who subsequently taught chemistry. He only mentions his work in metallurgy, but his contributions were fundamental in the areas of grain growth and recrystallization and in order-disorder phenomena. I would like to add that the single crystal orienter he developed was the first such device for use with a counter.

I hope this is useful to you. I am not able to do many things because I now am legally blind. That is why I am unable to attend the tribute to Dave.

I would appreciate a copy of the Biography when it is done.

Sincerely,

John S. Kasper

JSK:clk

P.S. I am typing this for John. I was working closely with both John and Dave on the decaborane problem at the time and clearly recall the sequence of events as John has described. I was also working in the office while John was busy working with the relationship of Schwartz's inequality and the structure factors to possibly help determine signs when Dave arrived in the office and became very excited at the possibilities of its use. It was an event one doesn't forget.

Charlys Lucht Kasper

It is appropriate to point out here the mathematical basis of the Harker-Kasper inequalities since this is not mentioned explicitly in their paper. This is simply the non-negativity property of the electron density function, a fact implicitly assumed in their analysis.

After a good deal of prodding on Dave's part, the X-ray department of General Electric was finally persuaded to build its first counter diffractometer for powder patterns, although not before the North American Philips Co. had already put a similar device on the market. Next, Dave set about adapting it to single crystal work. By 1949 he had built several models and had used them successfully, mostly on metallurgical problems.

During his time at General Electric, Dave served as president of the Society for X-ray and Electron Diffraction (1946). He also headed the American delegation to the London conference where the formation of the International Union of Crystallography was proposed and later was established, along with its adhering body in the United States, the U.S. National Committee for Crystallography.

Brooklyn Poly Years: The Harker Construction and Ribonuclease (1950-59)

The next phase in Dave's career was triggered in the fall of 1949 by Irving Langmuir, who asked him what he would do with a million dollars. To this seemingly rhetorical question Dave's offhand response was that he would take ten years off and determine the structure of a protein. To Dave's great surprise, within two weeks Langmuir came to his office and announced that he could raise the money. Dave suddenly realized that determining the structure of a protein was what he had wanted to do for some time. After months of interminable negotiations, the decision finally was made to establish the Protein Structure Project at the Polytechnic Institute of Brooklyn in July 1950. There Dave and his team built a good single-crystal X-ray diffractometer with counter detection of the diffracted beams. The central device in this unit was a sort of theodolite arrangement for orienting the crystal in any possible way. They called this device a "Eulerian cradle," because the angular motions it could give the crystal were Euler's angles. This instrument was eminently successful, and led to the commercial goniostat, which soon became increasingly popular. Much of the success of this instrument was due to its careful design, for which Thomas C. Furnas, Jr., was primarily responsible.

They chose ribonuclease as the protein on which to work, because it could be had relatively pure at a reasonable price, could be readily crystallized, and it had a quite small molecular weight. Murray King crystallized this substance in fourteen different modifications eventually. He also invented the method of attaching heavy atoms to specific sites in the protein crystals by "dyeing" the crystals with specially synthesized dyes, the molecules of which contained heavy atoms. Dave worked out the scheme of phase determination for protein structure factors, which involved using the intensities from three isomorphous crystals—one undyed, the other two dyed with heavy atoms in different arrangements, a scheme used by macromolecular crystallographers to this day. It turned out that Professor Bijvoet of Utrecht had found the same principle a few years earlier, but he had not emphasized it in his papers. This scheme, since called the method of multiple isomorphous replacement, led to the first successful structure determinations of crystalline proteins—those of myoglobin by Sir John C. Kendrew and of hemoglobin by Max F. Perutz, both of Cambridge University. For this work they received the Nobel Prize in chemistry for 1962.

Roswell Park Cancer Institute: The Structure of Ribonuclease (1959-76)

From 1950 to 1959 Dave and his team worked at the Brooklyn Polytechnic Institute on the crystal structure problem presented by the protein ribonuclease. In 1959 Dave moved the whole project to the Roswell Park Cancer Institute (then known as the Roswell Park Memorial Institute), where he accepted the position of head of the biophysics department. Due to the efforts of visiting crystallographers, a number of critical problems were solved during the Roswell Park years. M. V. King solved the problem of dyeing the protein molecules in crystals and he prepared ribonuclease in fourteen different crystal forms. F. H. C. Crick discovered the strong temperature dependence of the diffracted X rays from the protein crystals mounted in sealed capillaries and showed how to control it. V. Luzzati showed how the intensity statistics were related to the structure of the protein crystals and why the standard statistical methods could not be applied in these cases. A. Tulinsky worked out the exact structure of beryllium basic acetate and made it into a useful intensity standard. G. Kartha developed new ways of using the diffraction data from non-centrosymmetric crystals. A. de Vries showed how anomalous dispersion effects could help in determining the structures of crystalline proteins. J. Bello discovered new ways of labeling ribonuclease crystals with heavy atoms. T. C. Furnas, Jr., built their counter diffractometer, aided by that artist in instrument construction W. G. Weber.

The stage was set to begin to collect X-ray crystallographic data from which the structure of ribonuclease could be determined. This goal was finally reached in 1967 with the determination of the crystal and molecular structure of ribonuclease, the first protein structure to be determined in the United States.

In the years following the determination of the structure of ribonuclease, Dave was honored locally by three major awards. In 1967 he received the Sigma Xi Award for meritorious service to science from the State University of New York at Buffalo. The *Buffalo Evening News* awarded Dave its Outstanding Citizen Award in 1968, and the western New York section of the American Chemical Society awarded him the Schoellkopf Prize in 1969.

The Final Years: Hauptman-Woodward Medical Research Institute (1977-91)

In 1976 Dave retired from the Roswell Park Cancer Institute, but he continued his crystallographic studies as a research scientist emeritus at the Hauptman-Woodward Institute (then known as the Medical Foundation of Buffalo). He became interested in the more mathematical aspects of crystallography, in particular the theory of colored space groups and a description of several classes of infinite polyhedra.

During the next fifteen years, Dave was honored with several appointments and awards. The year 1977 marked Dave's election to the National Academy of Sciences and the American Academy of Arts and Sciences. Two years later, in 1979, he was nominated for a Nobel Prize. In 1980 the American Crystallographic Association honored him with the prestigious Fankuchen Award in recognition of his services to crystallography, in particular his research accomplishments and his role as a teacher of crystallography. In 1981 the State University of New York at Buffalo awarded Dave an honorary degree of doctor of science in recognition of his long and outstanding career in science, the first such award by this university. In 1984 Dave received the Gregory Aminoff Medal in Gold from the Royal Swedish Academy of Sciences in recognition of his fundamental contributions to the development of methods in X-ray crystallography and for his determination of the molecular structures of biologically important substances. On Dave's eighty-second birthday, in 1988, the David Harker Endowment Fund was established by an anonymous donor at the Hauptman-Woodward Institute in Buffalo. The fund is intended to support research and lectures in crystallography. In 1989 Dave prepared a paper announcing his discovery of four new types of polyhedra, which he named the "tortuously corrugated two dimensionally infinite polyhedra." This paper was published shortly before his death in the January 1991 issue of *Proceedings of the National Academy of Sciences.*

In conclusion, Dave was a warm and friendly man, courteous and unpretentious, concerned to be helpful, particularly to younger colleagues; and his teaching was unsurpassed. He was reserved, almost shy, an old-fashioned gentleman with old-fashioned values. He was one of the greatest crystallographers of this century, but he was never patronizing to others, young or old. He was kind and gentle and, at the same time, a man of uncompromising honesty and integrity. He was a tireless seeker of the truth, wherever he could find it, and in this quest he succeeded as few others have. On February 27, 1991, Dave died of complications due to heart disease and pneumonia.

I wish to make grateful acknowledgment to Ms. Tava Shanchuk for her help in writing an initial draft of this biography; this was of considerable assistance to me in the preparation of the final manuscript.

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Yandell Henderson

April 23, 1873-February 18, 1944

By John B. West

Yandell Henderson made important contributions to cardiorespiratory physiology over a broad area with a particular emphasis on practical applications such as resuscitation, air pollution, mine safety, and aviation medicine. Although his initial training was in biochemistry, he early turned to cardiovascular physiology, including the output of the heart, venous return, and shock. His interest in high-altitude physiology was sparked when, with J. S. Haldane of Oxford University, he helped to organize the Anglo-American Pikes Peak Expedition of 1911. His involvement with high-altitude physiology remained throughout his life and he subsequently studied the blood changes with acclimatization, work capacity at extreme altitude, and problems in aviation medicine. Issues of mine safety prompted his interest in carbon monoxide poisoning, resuscitation, and ventilation standards for long vehicular tunnels. He made an early plea for recognition of clinical physiology as a discipline and contributed to the physiology of anesthesia and asphyxia in the newborn. Henderson's emphasis on applied physiology has gone out of fashion (which may partly explain why this memoir is fifty years late), but his philosophy that one of science's main responsibilities is to the human condition will find a resonance in many quarters.

EARLY LIFE AND EDUCATION

Yandell Henderson was born in Louisville, Kentucky, oldest son of Isham Henderson and Sally Nielson Henderson, nee Yandell. His father was trained in law, but he disliked its practice and instead became owner and manager of the Louisville *Courier Journal* newspaper. In addition, he was a contractor and builder of canals and railroad, which made him a substantial fortune. Yandell's paternal grandfather Isham was an eminent judge in Kentucky. His maternal grandfather was Dr. Lundsford Pitts Yandell, the first dean of the first medical school west of the Allegheny Mountains. One of Dr. Yandell's sons practiced medicine in Louisville from an office above a drugstore in which the young Simon Flexner was apprenticed.

Yandell Henderson attended Chenault's school in Louisville and then entered Yale University in 1891, graduating with an A.B. in 1895. For the following four years he studied physiological chemistry at Yale under Russell Henry Chittenden, doyen of American biochemistry and one of the organizers of the American Physiological Society. Henderson received his Ph.D. in 1898. During his period as a student, Henderson spent most summer vacations traveling in Europe. An exception was in 1894, when with some friends from Yale he explored the region around Lake Louise in Alberta, Canada, surveying the lake and naming several of the peaks. It is interesting that the splendid Chateau Lake Louise, which was built by the Canadian Pacific Railroad, is now the site of international meetings on the physiology of hypoxia every two years; these would have been of great interest to Henderson. During these years, Henderson also joined the naval militia, and for one summer served as ensign in the U.S. Navy on the *U. S. S. Yale* in Cuban waters, with a later expedition to Puerto Rico.

Henderson's thesis work was published in 1899 in the new *American Journal of Physiology* (this was only the second volume) with Chittenden and Mendel under the title "A Chemico-Physiological Study of Certain Derivatives of the Proteids" (proteid was an early name for protein). By this time Henderson was at the University of Marburg with Albrecht Kossel, who was later awarded a Nobel Prize in physiology and medicine for his work on the biochemistry of proteins, especially nucleoproteins. A year later Henderson was in Munich with Carl Voit, whose main interests were nutrition and metabolism, especially in regard to proteins. Henderson returned to Yale in 1900.

PHYSIOLOGICAL LABORATORY, YALE MEDICAL SCHOOL

In 1900 Chittenden had become temporary head of the physiological laboratory in addition to his appointment in physiological chemistry. Chittenden turned the laboratory instruction and some of the lecturing over to Henderson, who was given an appointment as instructor. This marked the beginning of a remarkable career change for Henderson from biochemistry to physiology. Furthermore, he gradually abandoned research on isolated tissues and organs to work on the whole animal, including man. Henderson rose through the ranks to become professor of physiology in 1911, a position that he occupied until 1920.

It is not entirely clear what prompted Henderson to make the switch from the biochemistry of proteins to cardiorespiratory physiology. It may have been his conviction that the time was ripe for medical science to exploit the new advances in organ and clinical physiology. Certainly, he voiced that belief in his chairman's address to the Section of Pathology and Physiology at the annual meeting of the American Medical Association in 1911. For example, he stated, "Physiologists now and in the next few years will find their richest and most fruitful problems in the field of clinical, rather than in that of purely abstract, physiology . . . development of clinical physiology might well be the greatest event in the progress of medicine during the second decade of the twentieth century." In fact, Henderson's subsequent scientific career was dominated by problems posed by clinical and applied physiology, such as the physiology of shock, resuscitation, aviation medicine, carbon monoxide poisoning, atelectasis, and the effects of alcoholic beverages.

Henderson's first major investigation in cardiorespiratory physiology was a plethysmographic study of the filling of the ventricles of the heart, which was published in 1906. He noted that atrial systole played a minor role in the filling of the ventricles and argued that the atria should be "regarded as elastic reservoirs rather than as force pumps." This led him to become interested in the factors determining venous return to the heart and particularly the way positive pressure ventilation of the lungs interfered with venous return. This prompted a study of the effects of hyperventilation and the resulting reduction in the partial pressure of carbon dioxide on the heart and venous return; he concluded that a low PCO_2 (which he referred to as acapnia following the introduction of the term by Angelo Mosso) could be responsible for surgical shock. This proposal led to a spirited confrontation with Samuel J. Meltzer at one of the meetings of the American Physiological Society and brought out Henderson's combative propensity, which was to remain with him the rest of his career.

Henderson's interest in the filling pressures of the left ventricle has a modern ring to it because this remains a somewhat controversial area. An interesting sidelight to this work was that previous experiments on isolated cardiac muscle seemed to disagree with Henderson's results on the whole heart. This may have been a factor in directing his interest away from isolated tissue to whole animal preparations and eventually to human beings. At this time, Henderson also wrote one of the earliest papers on ballistocardiography, which he thought would be useful in studying the ejection of the left ventricle.

Henderson continued his interest in the output of the heart and wrote later articles on the topic. However, he was led astray by experiments that suggested that the stroke volume of the heart remained almost constant under a variety of physiological conditions when the heart rate was relatively slow, but at rapid heart rates the stroke volume was always substantially decreased. This result would be expected if venous return remained constant as in Henderson's preparation. However, his generalization to other situations brought him into conflict with German physiologists, such as Nathan Zuntz, who recognized that the large increase in cardiac output accompanying physical exercise was brought about both by an increase in heart rate and stroke volume. Because it was difficult to reconcile the known increase in oxygen consumption during exercise with the limited increase in cardiac output, Henderson concluded that there might be a change in "pulmonary oxidation during vigorous muscular work." Here he was apparently referring to the theory of oxygen secretion, which was championed by Christian Bohr and vigorously supported by Haldane.

It may have been Henderson's interest in the physiological effects of low levels of carbon dioxide that was responsible for his meeting with J. S. Haldane from Oxford, and Henderson's subsequent introduction to high-altitude physiology. Haldane had pioneered the role of carbon dioxide in the control of ventilation, but, as alluded to above, he also believed that the lungs secreted oxygen, and that the best place to investigate this was at high-altitude. Henderson met Haldane and his colleague C. G. Douglas for the first time at a cafe in Vienna during the International Congress of Physiology held there in 1910. About half an hour into their conversation, Haldane remarked that he was planning an expedition to high-altitude and that what he needed was "a nice comfortable mountain" for the expedition. Haldane and Douglas were familiar with the Capanna Margherita at an altitude of 4559 m on the Italian Monte Rosa, but to reach it required several hours of climbing over snow and ice, and the conditions in the hut were very Spartan. By contrast, Pikes Peak near Colorado Springs had a cog railway to the summit where there was a well-appointed "summit house" with several rooms. Henderson is reported to have said, "Come to America next summer and we will spend a month or two on the top of Pikes Peak." Thus began the famous Anglo-American Pikes Peak Expedition of 1911 (referred to by Henderson as the Yale-Oxford expedition).

The participants were J. S. Haldane and C. G. Douglas from Oxford University, Yandell Henderson, and Edward C. Schneider, formerly at Yale but at the time professor of biology at Colorado College in Colorado Springs. Pikes Peak had many advantages for a study of high-altitude physiology. Although the summit at an altitude of 4300 m (14,110 ft) was not quite as high as that of the Capanna Margherita, the fact that it could be reached by railway and that four rooms of the summit house were made available to the expedition made Pikes Peak very attractive. One of the rooms was fitted out as a laboratory. The investigators initially stayed for about five days in Colorado Springs and then took the railway to the summit, where they spent five continuous weeks.

Initially the whole party had some symptoms of acute mountain sickness, but most of these disappeared after two or three days. A wealth of information was obtained on the acclimatization process including the changes in alveolar

PCO_2 and blood hemoglobin concentration. Periodic breathing was observed particularly at night, and this was abolished by breathing oxygen. The only finding that could not subsequently be confirmed was the evidence for oxygen secretion (that is, that the partial pressure of oxygen in the arterial blood exceeded that in the alveolar gas). The blood measurement was made by an indirect technique based on the color of the blood when carbon monoxide was breathed, but it is still not clear where the investigators went wrong. Haldane who was a great champion of oxygen secretion actually believed in it until his death in 1936. The Pikes Peak expedition was described in generous detail in a 134-page paper in the *Transactions of the Royal Society of London, Series B.*

Mabel FitzGerald was also connected with the expedition, but for reasons that are not clear she did not stay on the summit with the four men. Instead, she toured mining camps in Colorado measuring alveolar PCO_2 and hemoglobin concentration in residents at moderate altitudes. When she was a student at Oxford, women were not granted degrees, and she had the distinction of being given an honorary M.A. at the age of 100! Henderson remained interested in high-altitude physiology the rest of his life and one of his last papers was on the problem of reaching the summit of Mt. Everest without supplementary oxygen. Along with several other physiologists of his generation, he believed this would be impossible, but the successful ascent by Reinhold Messner and Peter Habeler in 1978 proved them wrong.

Henderson also carried out a well-known study of the volume of the respiratory dead space. This was prompted by his interest in the carbon dioxide content of alveolar gas and the factors leading to acapnia. He made the important observation that some alveolar gas exchange can occur with tidal volumes that are much smaller than the dead space because of the axial flow of inspired gas through the airways. He pointed out that the size of the dead space is increased by lung inflation because of radial traction by the lung tissue on the bronchi. However, he overestimated the amount of carbon dioxide exchange that can occur in the airways of the lung, arguing that under some conditions such as in hyperemia of the bronchi, as much as one half of the total amount of carbon dioxide exhaled may come from the dead space. This conclusion has not stood the test of time.

Henderson's strong links with Europe, particularly Germany, led him to oppose America's entry into World War I. However, when the United States did enter the war, he took an active role in problems associated with chemical warfare. He became chief of the medical section of the U.S. War Gas Investigations, which became the research department of the Chemical Warfare Service. He was responsible for improvements in the gas masks used by the Allied armies in France. In addition, he was chairman of the Medical Board, Aviation Section, Signal Corps of the U.S. Army, and worked on problems of aviation physiology. Henderson was apparently the first person to suggest that decompression sickness ("bends") could occur at high-altitude, although he pointed out that the performance of aircraft at the time made this unlikely. When aircraft improved and particularly when cabins were pressurized, decompression sickness frequently occurred if there was a loss of pressure. Decompression sickness is currently a serious problem in space medicine.

After the war, Henderson was asked to study the problems of ventilation of vehicular tunnels including the Holland tunnel under the Hudson River. With H. W. Haggard he developed standards for appropriate ventilation based on levels of carbon monoxide and other toxic gases, and these were subsequently adopted all over the world and applied to tunnels in the United Kingdom, New Zealand, and Belgium. He worked extensively on carbon monoxide poisoning and the best methods of treatment, recommending that 8-10% of carbon dioxide be added to the inspired gas to stimulate ventilation. However, modern practice is to treat carbon monoxide poisoning with 100% oxygen, either at normal pressures or in a hyperbaric chamber. Henderson also advocated that the tail pipes of cars, buses, and trucks be placed at the tops of the vehicles and pointed vertically so that the hot exhaust gases would be vented upwards. He argued that this would reduce pollution by carbon monoxide and other toxic gases at street level.

PROFESSOR OF APPLIED PHYSIOLOGY, YALE UNIVERSITY

In 1920 there was a reorganization of Yale Medical School, and Henderson left with assignment to the graduate school, where he became professor and director of the laboratory of applied physiology. Over the next eighteen years or so, being free of teaching responsibilities, he worked in areas of research that interested him, and these were always characterized by some practical application. In 1922, together with Howard W. Haggard, he invented the H and H inhalator, which was used extensively by rescue crews, particularly for resuscitation of victims of carbon monoxide poisoning. He became very interested in the use of carbon dioxide after anesthesia to prevent collapse (atelectasis) of the lung. The effects of other industrial toxic gases, including hydrogen sulfide, were studied, and he wrote articles on appropriate forms of resuscitation.

Henderson maintained a lifelong interest in air pollution, particularly the effects of automobile exhaust gas on city streets, again being far ahead of his time. He continued his work on the physiology of anesthesia and chaired a commission that investigated anesthetic accidents caused by the explosion of anesthetic gases. Another interest was the maximal work capacity of elite athletes, and realizing that competitive rowing used a large proportion of the total skeletal muscle in the body, he studied the Yale University crew, which won the Olympic rowing championship in Paris in 1924. He took up the issue of neonatal mortality and, believing this was sometimes caused by inadequate expansion of the lungs, advocated inhalational therapy using carbon dioxide to stimulate ventilation.

Henderson became professor of physiology emeritus in 1938, but he continued his research interests. Some of these led him into more applied areas with political overtones. He became very concerned with the control of alcoholic beverages after the repeal of Prohibition and wrote a colorful paper on "Four Percent Beer and Its Place in Student Life" in the *Yale Alumni Weekly.* He worked on such diverse issues as the consumption of milk, fungus infection of the feet, and the use and abuse of barbiturates and other narcotics. He was particularly confrontational on the use of the inhalation machine known as the pulmotor, which was recommended as a resuscitator by some people rather than his own H and H inhalator. One of his last papers was a polemic in the journal *Science* with the title "The Return of the Pulmotor as a 'Resuscitator': A Backstep Toward the Death of Thousands." Henderson also had an interest in politics. He was a candidate for Congress on the Progressive ticket in 1912 and 1914, and was chairman of the Progressive party of New Haven.

In 1903 Henderson married Mary Gardner Colby of Newton Center, Mass. There was one son, Malcolm Colby Henderson, who was a professor of physics at Dartmouth College and later chairman of the department of physics at the Catholic University of America in Washington, and one daughter, Sylvia Yandell Henderson, who married G. McLean Harper, who became professor of Greek and Latin at Williams College. Henderson died in La Jolla, Calif., while visiting his son.

Henderson received many honors including election to the National Academy of Sciences in 1923. He was also a member of the American Philosophical Society. He was granted an honorary M.D. degree from the Connecticut Medical Society two years before his death. The charter of that society included this privilege and the only previous recipient was Russell Chittenden, Henderson's mentor. The M.D. degree gave Henderson a great deal of satisfaction, and in retrospect, with his strong interest in many aspects of clinical physiology, it may be that he regretted not having obtained an M.D. early in his career. In fact, in the lecture to the American Medical Association referred to earlier he stated, "I hold that in the future students who are being trained to be physiologists, whether in the field of physical and nervous or of chemical physiology, ought to have the M.D. degree . . . I do not regard the Ph.D. degree alone as ensuring a sufficiently broad training for a physiologist, either in the chemical or physical line."

As a man, Henderson apparently could be a very loyal friend, but equally a formidable enemy. Cecil K. Drinker, who knew him well and wrote Henderson's obituary in the *Journal of Industrial Hygiene and Toxicology*, stated, "By the death of Dr. Henderson many of us lost a warm friend. Many others lost an equally ardent enemy, for Dr. Henderson cherished both his friends and enemies and was so forthright a man, so impetuous in his regard or disdain, that all knew where they stood in his world . . . Friends and foes alike will miss Dr. Henderson. He was a valiant, yeasty man whose scientific life made for the growth of many subjects, of which industrial hygiene was one, and for the growth of many workers who followed his venturesome leadership."

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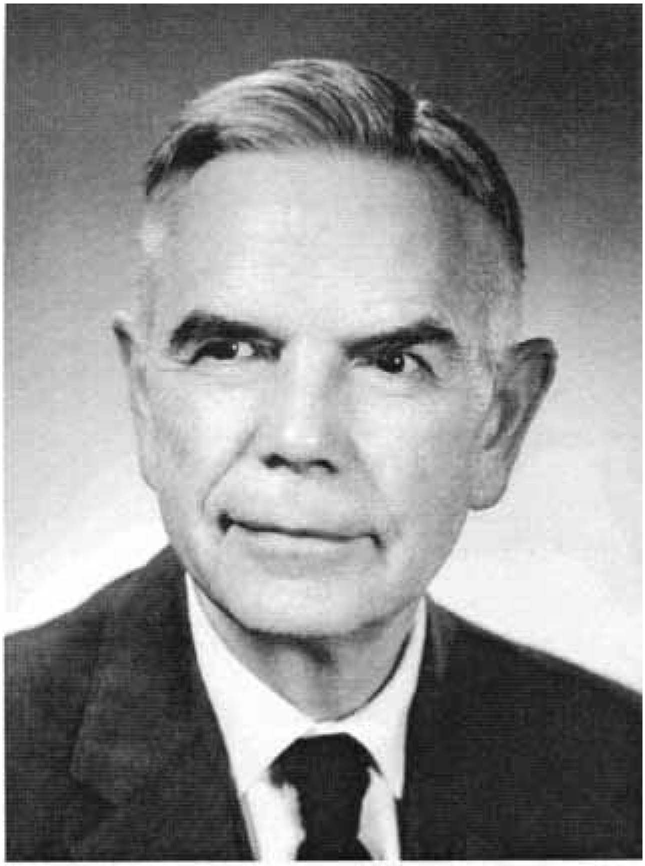

J. L. Hoard

James Lynn Hoard

December 28, 1905 - April 10, 1993

By Robert E. Hughes

James Lynn Hoard, or Lynn as he was known to his many friends and colleagues, was a central figure on the faculty of Cornell's chemistry department for thirty-five years before his retirement in 1971. In the ensuing years, he continued his distinguished career in structural crystallography, appearing daily at his office immersed in the painstaking scholarship characteristic of his entire career. In a half century of meticulous and thoughtful research he gained international recognition for seminal and classic work on the stereochemical principles underlying high-coordination discrete complexes; for founding the basis for the icosahedral crystal chemistry of elementary boron and binary borides; and, inter alia, for providing critical insights into the mechanism of the oxygenation of the heme in hemoglobin.

Hoard's father, Charles Ellsworth Hoard, from Monongalla County, West Virginia, was in his mid-twenties when he brought his young bride Bertha Terpening Hoard from Iowa to Republic County, Kansas, to raise a family. At the turn of the century, then six strong, they moved to a farm near Elk City in Beckham County in the Oklahoma Territory, where Lynn, the sixth of seven children was born. He was five when the family moved to Seattle, Washington, where he spent his formative years.

As a youth, Lynn was quietly competitive, determined to excel in every undertaking whether it was studies, a game of badminton, or playing the piano. This became a hallmark of his life and career—a self-driven, indeed stubborn, determination to do his best in every enterprise.

Throughout his life, Lynn was blessed with a remarkable memory that impartially delivered long discourses on any subject that interested him—phytography, meteorology, geography, Indian nations, musical scores, or the finest details of chemical structure. In his later years he could remember the names of his school teachers and the grades they gave him, a few of which he still contested. His versatility led him to consider undertaking a career as a classical pianist, and he interrupted his undergraduate program for a year of musical studies. He finally made the decision to turn to science, but the piano remained an integral part of his life.

Although he authored some 115 papers, Lynn was not a facile writer. He combined faultless syntax with precision and economy of expression. Each paper became a labor of love, written and rewritten, paragraph by paragraph, sentence by sentence, clause by clause. He devoted the same attention to papers from other authors sent to him for review, sometimes spending days reforming ideas, recalculating, and giving freely of his own contributions. Editors, recognizing this, sent him more than his fair share to review. He applied the same stringent criteria to his teaching and would spend hours trying to improve the clarity of a single important concept.

Scientific discussions with Lynn followed the cadences of his writing. Striving to get to the bright, hard core of an idea he would alternate reflective pauses with converging attempts to separate wheat and chaff. It was a phenomenon well recognized by all of his colleagues and students. I recall my first encounter with Lynn when, as a new graduate student, I paid a courtesy call at his office. Almost four hours later, I left, launched into a lifetime association with Lynn and his family.

In 1927 Hoard graduated from the University of Washington magna cum laude in chemical engineering. He was also awarded Phi Beta Kappa, the first chemical engineering student at the institution to be so honored. He continued at the University of Washington, earning a master's degree in chemistry in 1929. He then went on to graduate work at the California Institute of Technology, an institution rapidly rising as a major world center in science. It was there that he met and worked with Linus Pauling, forming a lifelong friendship. Reflecting on those early years with Hoard, Pauling wrote,

> One memory I have of him, from several occasions, is the following. He would have learned about something surprising that had been discovered in the field of science, perhaps just told to him by me. He would stand for some minutes with a look on his face that suggested strongly to me his feeling of surprise and pleasure about the new discovery—his mouth held somewhat open and his eyes seeming to flash with pleasure.

After completing his Ph.D. with Pauling in 1932, Hoard became one of the early pioneers in the application of X-ray diffraction techniques to the determination of crystal and molecular structures. In those days, the challenge of truly arduous and lengthy calculations required both determination and structural insight of investigators in this field. In the midst of the Great Depression, Hoard brought his newly acquired skills to Stanford University where he served as an instructor for three years. After a brief term at Ohio State University, he joined Cornell in 1936.

Lynn Hoard loved Cornell and Ithaca and was ever reluctant to travel, although he frequently did so in later years. He came to Ithaca with his bride, Florence Fahey Hoard of Seattle, and raised a family of three sons, David and the twins Thomas and Laurence. In the early years, although under constant pressure from Lynn's participation in the Manhattan Project and his intense dedication to research, they nevertheless completed a major family project. With a visionary architect, they helped design, manage, and participate in the construction of a unique home, which they were to share for forty-five years. Modeled along the lines of a Frank Lloyd Wright design, it has been an Ithaca landmark, graced by carefully planned and beautifully kept gardens. The family and the household revolved around Florence and she remained the invariant focal point of Lynn's entire life.

While working to establish a successful scientific career, raise a family, and put down roots in Ithaca, Lynn Hoard, in the early days of World War II undertook, as did so many young scientists at the time, to contribute to the national defense. He joined a group at Cornell, sponsored by the Office of Scientific Research and Development through the National Defense Research Committee. The project was concerned with the development of flashless propellants for large Navy guns; the Cornell role was to study the mechanism of the combustion process and to conduct the evaluation of nitrocellulose, plasticizers, and other ingredients. Within a year, the young crystallographer from Caltech was put in charge of the Cornell team among whose participants were listed such familiar names as F. W. Billmeyer, Jr., J. G. Kirkwood, Franklin A. Long, and Henry Taube. Working in parallel with the Explosives Research Laboratory at the Carnegie Institute of Technology, the project was considered very successful.

While this project required Hoard to accommodate to fields of science outside of his immediate expertise, he characteristically persevered in the task from 1942 to 1945, and was cited for his successes. He also found other uses for his structural knowledge; from 1943 to 1944 he participated in a program with the Manhattan Project and, as was acknowledged decades later, solved some important problems on the structure of uranium compounds.

In the early 1920s Roscoe Dickinson at Caltech made significant contributions to verifying the square planar, octahedral, and tetrahedral structures of a variety of complexes. This led Hoard to consider extending such work into the realm of high coordination complexes. Using Pauling's univalent radii and hard sphere packing principles, he made a priori predictions of seven-coordinate geometries for heptafluoro complexes of tantalum and niobium, as well as for the eight-coordinate octacyanomolybdate ion. In a letter to Hoard, Pauling suggested that the latter structure was much too difficult an undertaking. This was followed by another letter congratulating him for his success.

In Hoard's own words:

> As expected from a priori considerations, the anionic complexes were found to be seven- and eight-coordinate species. These analyses provided the first examples of discrete complexes in configurations that approximated closely to the geometries of the C_{2v} monocapped trigonal prism, the D_{4d} square antiprism, and—most notably—the D_{2d} dodecahedron having triangular faces, eight vertices, and eighteen edges. This dodecahedron emerged as the coordination polyhedron of the octacyanomolybdate (IV) ion.

These early successes, termed classic by some distinguished observers, presaged Hoard's lifelong interest in the structural chemistry of complexes, an interest he steadily advanced for the next four decades. Pauling was later to assert,

> Professor Hoard's work on the determination of the structure of coordination complexes has been a great contribution to knowledge. It is my impression that he has more contributions in this field than anyone else; he

> must surely be considered one of the world leaders in this field. His work on the determination of the structure of complexes in which metal atoms have ligancy seven or eight has been especially important.

Another distinguished inorganic chemist, Professor Earl L. Muetterties observed,

> Professor Hoard's work in the structural chemistry of discrete coordination compounds comprises a touchstone for other investigators. His analyses, from the first report of a seven-coordinate complex through pioneering studies of eight-, nine-, and ten-coordinate complexes, have been marked by a singularly comprehensive view of the field. This is particularly evident in his enunciation of stereochemical principles governing eight-coordination, an invariably cited standard.

In addition to pursuing the studies that led to complete elucidation of the discrete eight-coordination stereochemistry he had discovered, Hoard also undertook an extended, systematic study of high ligancy complexes of ethylenediaminetetraacetic acid (EDTA). Muetterties acknowledged the importance of this program in writing,

> Hoard's parallel interest in the structural properties of multidentate ligands has resulted in an extensive series of papers on coordination compounds of EDTA. These papers remain the outstanding source of carefully measured and critically evaluated structural data for EDTA complexes. A dramatic illustration of the value of the insights developed by Professor Hoard was his prediction and subsequent discovery of a seven-coordinate complex of iron(III), a complex which had been thought to be unrealizable by the scientific community.

Both the range and depth of Hoard's work on the stereochemistry of metal complexes were magnified by the almost unprecedented precision and detail with which the results were reported. No meaningful cross-comparison was overlooked. Over the years, in some seventy papers exploring the subject, he created and interrelated a dense body of knowledge that is still being mined by others. Importantly, he also educed structural principles that interlaced this panorama.

Commenting on this, Professor William N. Lipscomb, Jr., noted that

> Hoard's studies have become the important prototypes for both others and himself in the recent intensive interest in compounds of high coordination number. The work has influenced the areas of stereochemistry, rearrangements, transition states, and the theory of chemical bonding. The care with which his x-ray diffraction studies have been carried out has given them a high degree of permanence, and the cumulative effect has had a strong influence upon both the fundamental knowledge and the standards of research in inorganic chemistry.

Similarly, in another vein, Professor Henry Taube pointed out that

> Hoard has done work of outstanding importance to the development of structural inorganic chemistry. Its importance is not restricted to the crystalline solid state, but many of the results are so basic and of such far reaching significance as to affect profoundly the whole fabric of inorganic chemistry, including inorganic solution chemistry . . . Anomalous coordination numbers are as important to solution chemistry as they are to solid state chemistry (perhaps even more important because they are of direct significance to mechanisms of substitution reactions) . . . The principles that govern the structure in the solids are being delineated by Hoard—his 'instinct' in predicting unusual arrangements in the solid is, as far as I can tell, unerring—and these principles will apply also to structures in solution although, of course, with some modification.

It was characteristic of Lynn Hoard's approach to science that he did not—indeed, by nature, could not—turn away from difficult or seemingly intractable problems. His determination and self-confidence led him to pursue them relentlessly even if it took a decade of effort. Thus, quite independently of his ongoing programs on complexes, he chose to investigate the crystal chemistry of the element boron.

In the late 1940s and through the 1950s most crystallographic studies focused on structures with at least one moderately heavy element that would provide a reference point for determining the scattering phases and ultimately the structure. Molecular structures were largely confined to small molecules of known chemical structure. These constraints were driven by the primitive calculators available, the daunting computations required, and the absence of modern statistical analyses known as direct methods. It was in this context that he decided to undertake the structure determination of a crystal with fifty very light boron atoms per unit cell with unknown stereochemistry. It was my good fortune to join his group at this time and to share this adventure with him.

Lynn had earlier completed the landmark structure of boron carbide in which there appeared, for the first time, a structural element of twelve boron atoms at the vertices of a regular icosahedron. When we completed the structure of alpha-tetragonal boron, the icosahedron was established as the basic building block in the extraordinarily diverse array of boron polymorphs and higher borides. On seeing the results, Pauling wrote to say that it was one of the most beautiful structures he had seen.

Later work culminated in the structure of beta-rhombo-hedral boron, which is the thermodynamically stable polymorph of the element. More than a decade of effort in this field was brought together in 1965 in an authoritative treatise on the crystal chemistry of boron and higher borides that stands today as a primary reference in the field. Muetterties, the editor of the volume in which it appeared, later wrote:

> When I first received Professor Hoard's chapter, which was written in collaboration with Professor Hughes, I was struck by the extraordinary attention to detail and by the precision given to the description of the very

> complex solid phases associated with elemental boron and some of the other borides. This book has been reviewed in about ten different journals and magazines and, without exception, this chapter by Hoard and Hughes has been singled out as a major and outstanding feature of the book.

Professor Lipscomb in commenting on Hoard's studies on "compounds involving the important icosahedral B_{12} unit in boron itself and in borides" further stated that:

> His most sustained, and most penetrating studies of complex systems have been the studies of beta-rhombohedral boron and tetragonal boron. From these studies he has developed, in collaboration with R. E. Hughes, a general set of principles for icosahedral units in boron and the very large number of borides having high boron content.

It is clear that Lynn Hoard, from his earliest works, earned the respect and ultimately the admiration of distinguished scientists in many fields. While to many this might be somewhat gratifying, as all of his colleagues would attest, it was totally irrelevant to Lynn's approach to or conduct of science. He answered to an inner judge. Nowhere was this more evident than in his choice of research areas.

Well past mid-career he began a new program on the stereochemistry of porphyrin derivatives. His objective was to examine model systems that might shed light on the mechanism of hemoprotein function. This was a major venture into an important and very active biological field—his first. It occupied his attention for the rest of his career.

Hoard reported his first structure of a porphyrin derivative in 1963, the year when there first appeared meaningful determinations of crystal structures and molecular stereochemistry for porphyrins. The principal obstacle to progress in this field was the great difficulty in producing single crystals suitable for X-ray diffraction analyses, especially for materials of biological importance. Crystalline disorder often precluded obtaining meaningful data; in most cases that were tractable, heroic effort was required to account for the effects of disorder.

By 1965 Hoard had fully immersed himself in the field and characteristically had produced some of the highest resolution structures of iron-porphyrin derivatives. Combining his own results with a critical assessment of the available literature, he formulated a hypothesis about the effect of the spin state of iron on the stereochemistry of its interactions with the porphine skeleton, a proposal that was to be central to the understanding of the mechanism of oxygen binding in hemoglobin.

Each of the four possible combinations of ferrous (FeII) or ferric (FeIII) iron in a high- or low-spin ground state is realized in one or more of the hemes as these occur in the several families of the hemoproteins. From the low resolution maps of the hemoglobin structure available at that time it was assumed that the iron atom was centered in the plane of a rigid protoheme, bonded to the four nitrogen atoms in the framework, and directly attached to the globin framework through an axial complexing bond from the iron atom to an imidazole nitrogen atom of the proximal histidine residue. Molecular oxygen occupies the sixth position in the coordination group of the iron atom in the low spin oxyhemoglobin (oxy-H_b) molecule; there is, of course, no sixth ligand in the high-spin deoxyhemoglobin (deoxy-H_b) species.

Hoard, relying heavily on his deep understanding of the stereochemical principles governing metal complexes, firmly postulated that "the porphine moiety, as compared with an aromatic hydrocarbon of the benzene series, is quite susceptible to significant folding, ruffling, or doming when subjected to moderate stressing." He further asserted that "a substantial displacement (`.40Å) of the iron atom from the plane of the four nitrogens to which it is bonded is a normal structural property of all high-spin iron porphyrins."

These two postulates formed the basis for a new understanding of the trigger mechanism that precipitated the allosteric transition accompanying the oxygenation and deoxygenation of hemoglobin. Max Perutz at Cambridge remarking that "Hoard predicted it all," used these concepts to develop a detailed model of the transition that starts with the movement of the iron atom pressuring the histidine ligand and generating a cooperative shift of the two halves of hemoglobin through an angle of 15 degrees.

Hoard later commented that "Perutz has made full and generally perceptive use of our precise stereochemical data and our interpretations thereof for the iron porphyrins . . . He has put forward a boldly conceived and rather detailed structural model for the overall mechanism of oxygenation in which the conformational change attending the oxygenation of a single heme suffices to initiate cooperative interactions in the hemoglobin molecule."

Professor Paul B. Sigler, reviewing the history of these developments observed:

> The cooperative binding and release of oxygen by hemoglobin is critical to its function as a carrier of oxygen from the lungs to the tissues. The challenge to explain this remarkable phenomenon in stereochemical terms motivated Perutz and Kendrew to begin their now legendary crystallographic studies. Hoard's seminal contribution to our understanding the triggering stereochemical event; i.e., the displacements of the heme iron from the plane of the porphyrin, illustrates an important principle in the field of structural studies; namely, that achieving high resolution and visualizing the stereochemical details is not just an exercise in crystallographic virtuosity but rather, the "details" of a high resolution image often provide a paradigm-shifting observation that is the key to a chemical mechanism. There is a natural tendency for all chemists and biologists to fill in the missing details of a low-resolution structure with their preconceived chemical bias; but more often than not they are wrong. Visualizing the event clearly

> with precise data, even from carefully chosen model systems, provides the unexpected clue that moves the field forward.

Lynn continued his exacting work on porphyrin structures for another decade and, in addition to being called on to address national and international conferences, he contributed two dozen finely wrought papers to the field.

As the years went by, the Hoard household was never without visiting children and grandchildren. Larry and Patti would arrive periodically with daughter Laura; Tom and Deborah lived nearby, so young Sarah and Cameron were daily itinerants, while David and Donna's Elisa and Daniel in California were more likely to be summer visitors. And always, the Hoards were gracious hosts, welcoming visits, long and short, from former students, postdoctorals, and visiting dignitaries from home and abroad.

At a celebration of his sixty-fifth birthday I tried to express the warmth and wonder with which he was viewed with the following words:

> After twenty years of close association with Lynn, I know better than to cite anecdotes that I will have to explain away for the next twenty years. After all, anecdotes tend to have an apocryphal flavor that does not stand up very well against the knitted brows of total recall. However, I am pleased to report that nothing has really changed in Ithaca. Interrupted conversations are still resumed in mid-sentence days later and the ebb and flow of ideas surges out into the hallways, across the laboratory and back into the office again. Each day, on into the evening, the creative process is renewed in an eternal struggle with syntax as sentences and paragraphs are forged, hammered and tempered until they pivot with delicate precision on the final semicolon. In recent years, Lynn has adjusted the tempo of his life to respond to increasing demands for his presence as a lecturer in such places as Rome, Cambridge, Zurich, Austria and Australia; such journeys are frequent now and his friends have sensed a growing wanderlust that was hitherto unsuspected. Nevertheless, even as he strides into Baker Lab freshly tanned from the Foro Romano, Lynn Hoard remains one of Ithaca's most familiar and important institutions.

James Lynn Hoard's scientific contributions were widely recognized throughout his career. In 1946 he was awarded a Guggenheim fellowship, which he pursued at the California Institute of Technology. He received a second Guggenheim award in 1960, which he used to consolidate his thinking about ongoing and future programs. He then received a very rarely awarded third fellowship in 1966. He traveled to Cambridge, England, and focused on the relationships between metalloporphyrin structures and biological mechanisms in hemoglobin.

In recognition of his great body of work in three important areas of structural chemistry, he was elected to the National Academy of Sciences in 1972. In 1977 the American Chemical Society presented him with the Award for Distinguished Service in the Advancement of Inorganic Chemistry.

Perhaps the most significant recognition he received was the warm esteem in which he was held by his Cornell colleagues, students, and friends for more than half a century.

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Joseph Kaplan

September 8, 1902-October 3, 1991

By William W. Kellogg and Charles A. Barth

Joseph Kaplan's research was largely concerned with the spectra of diatomic molecules and, more specifically, in afterglows of nitrogen and oxygen and their mixtures. These spectra are important in understanding the photochemistry of the upper atmosphere of the Earth and other planets.

Kaplan will be more widely remembered, however, for his leadership in the geophysics community. He was one of the creators of the new science of aeronomy. For ten years he served as chairman of the U.S. National Committee for the International Geophysical year and for five years he was a member of the Executive Committee of the International Committee of Scientific Unions. He played a leading role in establishing such significant programs as the International Hydrological Decade and the Global Atmospheric Research Program.

In spite of his involvement in such public arenas, he remained a popular and inspiring teacher at the University of California at Los Angeles, not only of many science graduate students, but also of undergraduates. Because of his warmth and charm, he was in constant demand by the public and the media as well as the scientific community, and he always seemed to welcome the opportunity to explain the scientific enterprise to non-scientists.

EARLY LIFE AND EDUCATION

As is common in a scientific career, Joe Kaplan's early life was largely devoted to his studies and the pursuit of his research. In his later years his energies also went into furthering his interests in public education and promoting international scientific programs.

Kaplan was born on September 8, 1902, in Tapolcza, Austria-Hungary. When he was eight years old, his parents and eleven brothers and sisters moved to Baltimore. After graduating from the Baltimore Polytechnic Institute in 1921, he attended Johns Hopkins University, where he earned a bachelor's degree in chemistry in 1924 and a doctorate in physics in 1927. Among his professors at Hopkins were Joseph S. Ames and Francis D. Murnaghan, authors of the classic textbook on theoretical mechanics. He did his thesis work with R. W. Wood, an expert experimental spectroscopist.

On graduating, he became a National Research Council fellow at Princeton University, where he continued his research under Karl T. Compton. From there he accepted an assistant professorship at the University of California at Los Angeles (UCLA) in 1928. He was destined to remain on the faculty of UCLA for the rest of his professional life, becoming an associate professor in 1935 and a professor in 1940. He was appointed chairman of the Department of Physics (1939-44) and director of the Institute of Geophysics (1946-47).

Kaplan took great satisfaction in his many graduate students, including the authors of this memoir. He also took pleasure in teaching an introduction to physics (Physics 10) for undergraduates. Kaplan was an ardent booster of UCLA sports, and it is doubtful whether he ever failed any good athlete who was reasonably attentive in class. In an interview with the *Los Angeles Times* he is quoted as saying, "I see no difference in a boy preparing to become a football or basketball player or a boy learning to be a violinist, a social worker, or a surgeon." His advice to a defensive halfback was, "Never collide with anyone. Just intercept those passes and run out of bounds. That's elementary physics."

LABORATORY STUDIES

During the years of his graduate studies at Johns Hopkins University (1924-27), Kaplan learned how to produce atoms in laboratory vacuum tubes. Kaplan's advisor, R. W. Wood, was the master of laboratory research on atomic hydrogen. What Kaplan contributed to these experiments was the addition of a large (for the 1920s) evacuated bulb sealed to Wood's atomic hydrogen tube. Kaplan found that the hydrogen atoms entering the large bulb from a Wood's tube remained there for tens of seconds while recombining. Kaplan correctly deduced that the atoms recombined in three-body collisions with the energy of recombination sometimes appearing in the excitation of spectral emissions. This was the beginning of a career of laboratory experiments that simulated atomic and molecular reactions occurring in the upper atmospheres of the planets.

In 1928, as a National Research Council fellow, Kaplan went to work in Karl Compton's Princeton University laboratory, where he began his laboratory studies of active nitrogen. Active nitrogen is produced when a small amount of nitrogen gas is placed in an evacuated tube at low pressure and subjected to a high voltage discharge. When the discharge is turned off, the nitrogen gas continues to glow with the spectral emissions of atomic and molecular nitrogen. This is the phenomenon of the nitrogen afterglow. The source of energy for this afterglow is called active nitrogen. Working with Gunter Cario, Kaplan proposed the idea that both metastable nitrogen atoms and metastable nitrogen molecules were present in active nitrogen.

While experimenting with active nitrogen, Kaplan made a discovery that set the course of his research for the rest of his career. He was able to produce in the laboratory the auroral green line of atomic oxygen. This is the spectroscopic emission that gives the polar aurora its green color. Kaplan made this discovery by adding a small amount of oxygen to the active nitrogen he produced in his laboratory discharge tube. Today we know the aurora is produced by bombardment of the upper atmosphere by energetic electrons from the radiation belts during geomagnetic storms. The emission originates from the upper atmosphere at an altitude of about 100 km, where the maximum density in atomic oxygen occurs. The auroral green line is a forbidden transition and the excited state has a lifetime of a little less than one second. The bombarding auroral electrons ionize and excite the nitrogen molecules and oxygen atoms present at 100 km. Current thinking about the source of excitation of the oxygen green line in the aurora is that the oxygen atom is excited by a collision with a metastable nitrogen molecule. This is the type of process that Kaplan was studying in the 1930s in his laboratory experiments on active nitrogen.

Kaplan then began directing his laboratory research to reproducing the spectrum of the aurora in the laboratory. In 1932 he produced a laboratory afterglow, which he felt did reproduce the auroral conditions and from his experiments concluded "that the auroral display is really an electrical discharge in a nitrogen-oxygen mixture in which metastable molecules abound." The innovation of this particular experiment was that he ran the discharge in the tube for a long period of time, and this conditioned the walls so that collisions with the walls did not deactivate the metastable atoms and molecules produced in the afterglow tube.

Using this experimental apparatus, Kaplan discovered the

Vegard-Kaplan bands of molecular nitrogen. These bands had been observed earlier by Vegard in the spectrum of the aurora. Vegard did some laboratory experiments in which he bombarded solid nitrogen with electrons. The resulting luminescence included bands with approximately the same wavelength as the emission bands in the aurora. This result led Vegard to the speculation that solid nitrogen may be present in the earth's upper atmosphere. What Kaplan accomplished was to produce the bands in gaseous nitrogen, which, of course, is a known constituent of the upper atmosphere. Kaplan's measurements of the wavelength of the Vegard-Kaplan bands in the laboratory were of sufficient accuracy to permit the identification of the upper state of these bands as the A^3 ., a metastable state with a lifetime of about one second. Kaplan's laboratory apparatus truly did reproduce the conditions in the aurora.

Following the identification of the Vegard-Kaplan bands as the A^3 - X^1 transition in molecular nitrogen, Kaplan showed that in laboratory afterglows that produced the Vegard-Kaplan bands, the atomic oxygen green line at 5577 Å is also produced when a small amount of oxygen is added. This led Kaplan in 1935 to propose that the auroral green line is produced in the aurora by a collision of an oxygen atom and a metastable nitrogen molecule in the A^3 state. This idea is the current explanation for excitation of the 5577 Å line in the aurora.

During the same period, Kaplan produced the Goldstein-Kaplan bands in the laboratory and proposed that they were present in the "light of the night sky." That these bands are present in the ultraviolet aurora has been shown by rocket experiments that fly 100 km high into an aurora with an ultraviolet spectrometer.

Kaplan continued his laboratory experiments on the nitrogen afterglow, varying the physical conditions to enhance the intensity of the Vegard-Kaplan bands. When he increased the pressure of nitrogen in a well-conditioned tube, he found another emission that appears in the aurora. The forbidden transition ^2P - ^4S in atomic nitrogen at 3467 Å appeared in the laboratory apparatus. Atomic nitrogen in the excited ^2P state has a lifetime of about twenty seconds, a long time for a metastable atom in a small laboratory tube. When he repeated these experiments using a spectrograph sensitive to radiation in the visible part of the spectrum, he observed another forbidden line of atomic nitrogen at 5200 Å . The upper state of this transition, the ^2D, has a lifetime of twenty-four hours. It would be expected that this excited metastable atom would have many opportunities to be deactivated by collisions with the walls before radiating. It is a demonstration of Kaplan's experimental skill that these excited atoms were not deactivated and that the "tube behaves effectively as if it had no walls." Today, we know that the $N(^2D)$ excited atom plays an important role in the production of nitric oxide in the upper atmosphere.

In 1947 Kaplan revamped one of his afterglow tubes and studied the photographic infrared portion of the spectrum. When he added a small amount of oxygen to the afterglow, he discovered the atmospheric bands of molecular oxygen in emission. These are the bands that were first measured in absorption by Fraunhofer in the solar spectrum over 100 years earlier. Kaplan's discovery was the first observation of these bands in emission. Since he was studying the laboratory source to simulate conditions in the upper atmosphere, he recognized that these bands were potential contributors to the light of the night sky. These atmospheric bands were discovered subsequently in the night airglow infrared spectrum. This was the first time that Kaplan discovered an upper atmosphere emission in the laboratory before it was identified in the night sky spectrum. In all of his previous discoveries in the laboratory, the spectral emissions were identified first in the auroral spectrum, and he subsequently produced them in the laboratory.

In the 1950s Kaplan began thinking about the exploration of the upper atmosphere with rockets. He was particularly interested in the chemical reactions in the upper atmosphere, especially those that led to the production of the airglow. He suggested that the region of the atmosphere where the chemical reactions take place be called the chemosphere in analog to the ionosphere, where reactions between ions occur. As chairman of the U.S. National Committee for the International Geophysical Year, he provided leadership for the U.S. part of this international scientific program to explore space. He was an advocate of the plan to launch satellites to explore the upper atmosphere of the Earth. It was during the IGY that the first satellites were launched by the Soviet Union and the United States.

SERVICE TO GEOPHYSICS

Kaplan took pleasure in interacting with people, and in the later part of his life a great deal of his energy went into the promotion of some of his favorite scientific enterprises. His outgoing personality and ability to influence people made him a natural leader. He enjoyed taking part in the public arena, as well as in scientific research.

His research, as we have pointed out, led him to studies of the upper atmosphere, which emerged as a very fertile and important branch of geophysics in the 1940s. At that time, as radio developed, knowledge of the ionosphere and effects of solar activity was crucial. The rapidly improving technology of rocketry permitted sounding rockets to directly explore the upper atmosphere for the first time. Of course, eventually, the launching of earth satellites allowed direct, prolonged observation of atmospheric phenomena.

Kaplan established geophysics at UCLA, where he played a leading role in setting up the university's Institute of Geophysics in 1944 and served as its director from 1946 to 1947. This institute now has five branches in the California state system.

During the war years, Kaplan and Karl Gustav Rossby of the University of Chicago were instrumental in helping the U.S. Air Force organize a major program to train weather officers at several universities with meteorology curricula. In the postwar era, the field of meteorology in the United States seems to have been well populated by the many graduates of this program.

Kaplan moved easily among the top brass of the Air Force and was a personal friend of Chief of Staff General Hap Arnold and the commander of the Systems Command General Bernard Shriever. In 1943 Kaplan was named chief operations analyst for the Second Air Force and the Air Weather Service, a post he held for two years. Starting in 1948 he became a long-time member of the Air Force Scientific Advisory Board and the Advisory Committee of the Air Force's Office of Aerospace Research.

Despite his continuing involvement in national affairs as consultant to the White House's Office of Science and Technology, the National Science Foundation, and several California agencies, his real love was the world of international scientific organizations. In the early 1950s he became active in the International Union of Geodesy and Geophysics (IUGG) and with Sidney Chapman and Lloyd Berkner introduced the term "aeronomy" to describe the physics of the upper atmosphere, now taken to include studies of planetary atmospheres. After much debate, the IUGG Association of Geomagnetism changed its name to the Association of Geomagnetism and Aeronomy. At the time of this name change, officially adopted at the IUGG annual meeting in Toronto in 1954, Kaplan was elected vice-president; he became president in 1957.

Incidentally, there were some influential meteorologists who strongly resented the implication that upper atmospheric research (aeronomy) was being baptized as a separate science, and their response was to rename the IUGG Association of Meteorology the Association of Meteorology and Atmospheric Physics. We recall Kaplan's quiet amusement in Toronto at all the furor over a name.

Kaplan now moved to the center stage of the continuing evolution of international scientific organizations. In a very real sense, he lived at the right time in the history of science. In 1950 Lloyd Berkner suggested that the period 1957-58 be devoted to the Third International Polar Year, a suggestion later modified to include all of geophysics in the program. Berkner was joined by the distinguished British scientist Sidney Chapman in persuading the International Council of Scientific Unions to officially approve the International Geophysical Year (IGY) Programme.

As plans were being laid for the IGY, Kaplan became chairman of the U.S. National Committee for the IGY, established by the National Academy of Sciences. He retained this post from 1953 to 1963. Among other things, the IGY will be remembered for the launch of Sputnik 1 on October 4, 1957.

The IGY turned out to be an enormous undertaking, both in the United States and abroad, and Kaplan was greatly aided in organizing this program by Hugh Odishaw, executive director of the U.S. IGY Committee. The two men complemented each other in many ways and were an effective team. As Homer Newell recalls, "Kaplan . . . was noted for an inexhaustible supply of pleasant anecdotes. His genial personality was ideally suited to working with the difficult, dark, moody, sometimes abrasive Hugh Odishaw."

There were, of course, many organizational meetings leading up to the IGY, most of which Kaplan attended. At the meeting of the Committee Speciale de l'Annee Geophysique Internationale in Brussels in July 1955, Kaplan announced (after obtaining White House approval) that the United States would launch an earth satellite as a contribution to the IGY. Kaplan jokingly referred to such a satellite as a "long-playing rocket." As anticipated, this announcement attracted wide public attention.

In 1960, after the IGY ended, he assumed the vice-presidency of the International Union of Geodesy and Geophysics and the presidency in 1963. He was a member of the Executive Committee on Space Research from 1958 to 1967. From 1962 to 1967 he was a member of the Executive Committee of the parent organization of both of the above, the International Committee of Scientific Unions (ICSU). He continued to serve the ICSU from 1967 to 1968 as a member of its Standing Committee of Admissions and Organization.

In spite of all his international involvement, he was able to continue his duties as a UCLA professor and to serve on the Committee for the International Years of the Quiet Sun (1964-67) and the Committee for the International Hydrological Decade (1965-67). He will be remembered in California for his membership on the Advisory Council on Atomic Energy Development and Radiation Protection and also on the California Advisory Commission on Marine and Coastal Resources.

As he approached emeritus status at UCLA he had more time for other community service. He was a long-time member of the Rotary Club and was in demand as a speaker on many subjects. He and his first wife Katherine were invited on several occasions to take part in goodwill missions to developing countries on behalf of the U.S. Department of State. He derived great satisfaction from these kinds of activities.

Of course, he retained his membership in many scientific and educational organizations. Perhaps he was proudest of his membership in the National Academy of Sciences, to which he was elected in 1957. Other memberships included the Institute of Aeronautical Sciences (fellow), American Geophysical Union (fellow), American Meteorological Society (honorary member), American Physical Society (fellow), National Association of Science Writers (honorary member), and the International Academy of Astronautics (founding member). He was also an honorary member of the Board of Governors of Hebrew University of Jerusalem and a member of the Board of Governors of the Weizman Institute of Science in Rehovot. He was made an honorary chairman of the American Histadrut Cultural Institute.

In the course of his long career Kaplan received many awards and honorary degrees. Among the awards were the John Adams Fleming Award of the American Geophysical Union (1970), the Commemorative Medal for the 50th Anniversary of the American Meteorological Society (1969), the Hodgkins Medal and Prize from the Smithsonian Institution, and the Astronautical Award from the American Rocket Society. He was given the War Department's Decoration for Exceptional Civilian Service (1947) and the Air Force's Exceptional Civilian Service Award (1960 and 1969). He received the degree of doctor of science from Carleton College and the University of Notre Dame and a L.H.D. degree from Yeshiva University, Hebrew Union College, Jewish Institute of Religion, and the University of Judaism.

Kaplan married Katherine E. Feraud on June 24, 1933. She was a psychologist and was active in community service; she often accompanied her husband on his frequent trips abroad. She died in January 1977. On February 26, 1984, Kaplan married Frances I. Baum, who remained with him until his death. Kaplan died in Santa Monica, California, of a heart attack on October 3, 1991. He was eighty-nine.

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Stephen W. Kuffler

August 24, 1913 - October 11, 1980

By John G. Nicholls

From the beginning of Stephen Kuffler's career in neurobiology until his last experiments, each paper he produced was distinctive for its clarity, elegance, and originality. Time after time he provided fresh insights into mechanisms by which nerve cells generate electrical impulses, transfer information at synapses, and integrate signals. A hallmark of his work was that after formulating a key question, Stephen Kuffler would seek and find just the right animal species and the appropriate techniques for obtaining a decisive answer. Although he tackled a wide range of fundamental problems, a continuous thread ran through his work: the desire to understand how neurons that make up the brain carry out their functions. To this end he made electrical recordings, often requiring hours of skilled dissection, to study the functional properties of individual nerve cells and muscle fibers in invertebrates, frogs, and mammals.

A characteristic feature of his experiments was the use of whatever electrical, biochemical, or morphological techniques were necessary for solving the problem. This approach produced a major change in the study of the nervous system. By virtue of his superb research, his personality, and the generations of students that he inspired and influenced, he was an undisputed leader and dominant figure in neurobiology. To all his friends, colleagues, and students he was known as Steve.

EDUCATION AND EARLY LIFE

Stephen Kuffler was born on August 24, 1913, in Tap, a village in Hungary. His father, Wilhelm Kuffler, was a landowner living on a large estate. After his mother died when he was five years old, Steve was brought up by governesses at home until he went to a Jesuit boarding school in Austria at the age of ten, where he stayed until 1932. Steve often spoke to me about his childhood and youth. He was particularly happy at home where he had the free run of the estate; he greatly enjoyed horseback riding.

At school Steve studied humanities, Latin, and Greek, but virtually no science. In 1932 he went to medical school in Vienna; while he was a student his circumstances changed drastically when the family fortune was lost. The suddenness of this change from affluence to financial hardship had a profound effect on his view of life. During his training in medicine (which he was able to continue, albeit under straitened circumstances), he visited Egypt and England, which he enjoyed, except for a brief stint at the German Hospital in London. Steve found the atmosphere there to be authoritarian, repugnant, and reminiscent of the political atmosphere in Vienna with the growing intolerance and brutality that accompanied Nazism. After finishing his medical examinations in 1937, he worked in the Department of Pathology. Steve's distress at the situation after the *Anschluss* came to a head when he found that he had to do a postmortem on a colleague of his who had been murdered by the Nazis. After spending a few months in England he went to Australia, and it was there that his life as an experimental scientist began, through a meeting with Jack Eccles (Sir John Eccles, future Nobel Prize winner) on a tennis court.

PROFESSIONAL CAREER

The key catalytic event in Steve's scientific development was the arrival of Bernard Katz (later Sir Bernard Katz, Nobel Prize winner) who came to Eccles's lab in late 1939. From the very beginning Steve formed a close and long lasting friendship with Bernard Katz. Although Steve was to develop his own highly characteristic style of experimental research, Bernard Katz remained as the neuroscientist who most influenced his standards for the conduct of scientific research. In Eccles's lab Steve on his own made his first experiments on isolated nerve muscle junctions, which Bernard Katz described as "a brilliant technical feat . . . [that] . . . immediately and deservedly put him on the map." After the war Ralph Gerard offered Steve a position at the University of Chicago, where he worked for fifteen months before moving to the Wilmer Institute of Ophthalmology at the Johns Hopkins University Medical School as an associate professor and later professor. In addition to doing his own research he recruited a group of brilliant, independent young scientists, including David Hubel, Torsten Wiesel, Edwin Furshpan, and David Potter, together with an outstanding electronics engineer, Robert Bosler, with whom he was to work closely for the rest of his life. Steve also began to spend summers at the Marine Biological Laboratory at Woods Hole with his family and co-workers and started the first experimental lab courses devoted to the nervous system (the "Nerve-Muscle Program," later to become the neurobiology course). These intense lab and discussion courses had immense influence on generations of young graduate students and postdoctoral fellows coming from a variety of disciplines.

In 1959 the entire laboratory moved to the Department of Pharmacology at Harvard Medical School at the invitation of Prof. Otto Krayer, who offered generous space and facilities. At Harvard, Steve recruited a young biochemist, Edward Kravitz. A major contribution to the study of the nervous system was Steve's innovative idea of combining physiology, biochemistry, histology, neuroanatomy, and electron microscopy in one single group. In this way he shifted the focus of research from techniques that had been located in separate departments in universities throughout the world to neurobiology, a concept that Steve invented. From the time that the Department of Neurobiology was created in 1966 with Steve as chairman, he continued until his death to work in the lab with one or two postdoctoral fellows. Summers were spent at Woods Hole, except for the years 1967 to 1971, which were spent at the Salk Institute in La Jolla. Throughout his career, Steve provided the impetus for much of the research by his co-workers and criticized their papers in a light but decisive, inimitable style. Steve's name, however, appeared as author only on those papers in which he had done the experiments with his own hands.

MAJOR RESEARCH CONTRIBUTIONS

In the following paragraphs I summarize briefly highlights of Steve's research in roughly chronological order.

Synaptic Transmission-First Studies

Steve's style of research from the outset was to locate the Gordian knot and then cut right through it. By dissecting a single skeletal muscle fiber together with its nerve—an immensely difficult task—Steve could analyze the events occurring at the synapse with greater precision than had hitherto been possible in intact muscles. At a time when intracellular microelectrodes had not yet been invented, his Australia papers on the properties of the end plate potential, on the effects of calcium, and on the changes produced by denervation set new standards for investigating synapses. As a student, I well remember reading each new paper with excitement and admiration. Other experiments with Bernard Katz on crustacean muscles set the stage for later important studies on inhibition.

Slow Muscle Fibers and Muscle Spindles

Steve's initial work in Chicago was on slowly contracting muscle fibers in the frog and this in turn led him to the study of the sensory innervation of mammalian muscle. Although important pioneering studies had been made on sensory muscle spindles by B. H. C. Matthews in the early 1930s and by L. Leksell in the mid-1940s, the literature about the efferent output from the spinal cord to the spindle was abundant but confused and largely incomprehensible. (This was the usual starting point for Steve's generation of a new idea.) At Hopkins, together with Peter Quilliam and Cuy Hunt with whom he was to develop a close friendship and work for several years, he devised an elegant and direct experiment. Electrical recordings were made from a single sensory fiber coming from a muscle spindle receptor in muscle. At the same time an individual motor nerve fiber was stimulated. A large fiber, as expected, caused muscle contractions. When a single small diameter motor fiber was stimulated there was no overt contraction of the muscle, but the stimuli dramatically increased the frequency of the sensory discharge. This was due to activation of small specialized muscle fibers in the muscle spindle. In a series of elegant papers Cuy Hunt and Steve explored in detail the role of this efferent control by the nervous system of the information coming to it.

Retinal Ganglion Cells

In the next series of experiments at Hopkins, Steve turned to signaling in the mammalian retina. In 1952 it was impossible to understand the meaning of signals traveling from the eye to the brain. This was in large part because bright flashes of diffuse white or colored light had been used as stimuli. Through the invention with his friend S. A. Talbot of a new ophthalmoscope, Steve was able to stimulate well-defined discrete areas of retina by small, light, or dark spots. Once again in one series of experiments in which he was sole author, Steve revealed a fundamental mechanism. A key feature was to use natural stimuli to define the receptive field properties of individual ganglion cells and their optic nerve fibers. The major conclusion was that these cells responded primarily to contrast and to moving stimuli rather than diffuse light. These properties in turn depended on the convergence of excitatory and inhibitory inputs arising from cells in preceding layers of the retina.

A story Steve told me shows the impact of these retina papers. Steve had just presented his new findings at a meeting in Cambridge. Lord Adrian, the pioneer in our understanding of sensory signaling whom Steve greatly admired but had never met, was walking along a corridor from the other direction. As he encountered Steve he stopped, cocked his head, and asked simply, "Are they the same in the brain?" David Hubel and Torsten Wiesel have given fascinating descriptions of Steve and the way experiments he made on the retina provided the starting point for their own work in the visual cortex.

Excitation and Inhibition of Crustacean Stretch Receptor

With Carlos Eyzaguirre, Steve made the most elegant and detailed study of the way signals are initiated in mechanoreceptors. He chose the crustacean receptor as the ideal preparation because it could be isolated and explored with microelectrodes. In beautifully clear recordings they defined the properties of the generator potential, the essential intermediary signal between stimulus and conducted action potentials. In the same preparation they provided new insights into inhibitory mechanisms, again demonstrating efferent control by the central nervous system of information coming to it. An important pointer to the future was the study by Steve with Charles Edwards of the effect of gammaaminobutyric acid (GABA), which mimicked the action of inhibitory nerves.

Demonstration of Gaba as an Inhibitory Transmitter

While there were some hints that GABA could mediate inhibition, Steve, Ed Kravitz, David Potter, and their colleagues provided the first definitive proof in lobsters. Comparisons of the actions of GABA with those of the naturally released transmitter revealed a close similarity. In back-breaking experiments, meters (literally!) of single inhibitory and single excitatory axons were dissected day after day from giant lobsters (a fringe benefit for people like me, who joined the lab at this time, were the lobster feasts). Biochemical analysis showed that inhibitory axons contained high concentrations of GABA, approximately a thousand times more than the excitatory axons. These experiments laid the foundation for subsequent work on GABA mechanisms in mammalian brain.

Presynaptic Inhibition

Immediately preceding these GABA experiments Steve together with Josef Dudel had broken new ground by unequivocally demonstrating the mechanism of presynaptic inhibition, hitherto a somewhat ill-defined concept. By picking the right preparation, the nerve muscle junction in crustaceans, it was possible to demonstrate that inhibitory nerves acted in an entirely novel manner. In addition to an inhibitory action on the postsynaptic muscle fiber, impulses in the inhibitory nerve reduced the amount of transmitter released from the excitatory nerve by impulses. Once again, a decisive series of experiments with far-reaching consequences.

Physiology of Glial Cells

By the time I arrived in the laboratory in 1962, Steve and David Potter had already chosen the ideal preparation for studying glia, the central nervous system of the leech. I remember my own initial amazement that anybody would want to study these cells, which were then considered to be the inert connective tissue of the brain. What Steve set out to do was to study their membrane properties and see how they compared to nerve cells. In leech ganglia Steve and David Potter showed that glial cells had higher resting potentials than nerve cells, were electrically coupled, and could not give impulses. Steve and I then went on to determine whether ions and small molecules reached the nerve cells from the vasculature by way of extracellular spaces or through the glial cells. Our results showed that narrow 250-Å extracellular clefts, not glia, acted as the pathway. With Dick Orkand we then used the optic nerves of frogs and mudpuppies to show that the properties of glial cells there resembled those in the leech. We also found a novel interaction: impulses in axons caused potassium to accumulate in extracellular spaces and thereby give rise to a glial depolarization. From this finding came the concept of spatial buffering whereby glial cells could control the extracellular environment of the neurons they surround.

Later experiments by Steve with Monroe Cohen and Hersch Gerschenfeld revealed key properties of the blood brain barrier.

Synaptic Transmission

For the remaining years Steve returned to the study of synaptic transmission, particularly with U. J. McMahan, his close friend and colleague. Their motivation was similar to that of Bernard Katz (although the approach was very different): to understand in detail and quantitatively how nerve cells communicate at synapses. Jack McMahan and Steve took advantage of newly developed optical techniques (differential interference contrast) to observe living synapses between parasympathetic nerve cells in an ideal preparation (again!), the thin transparent septum of the frog heart. Here they and colleagues defined the structure of the synapses at the light and electron microscopic level and demonstrated that nerve-to-nerve synapses resemble physiologically those at the neuromuscular junction. Moreover, as in muscle, acetylcholine receptors spread to cover the surface of the cell after denervation. In other studies on autonomic ganglia with Doju Yoshikami and later with Lily and Yu Nung Jan, Steve made experiments that clarified what was then a confusing and chaotic problem. Considerable heated controversy existed about the properties of slow synaptic potentials in autonomic ganglia and the mechanism by which they arise. Through a combination of pharmacological, biochemical, and electrophysiological approaches, these slow excitatory and inhibitory potentials, which lasted for minutes or hours, were shown to depend in part on actions of acetylcholine on muscarinic as well as nicotinic receptors. In addition they provided the first unequivocal evidence for the release of a peptide (LHRH) from preganglionic terminals and its role in synaptic transmission.

Preceding these studies on ganglia, Steve and Doju Yoshikami published a pivotal paper on synaptic transmission at the nerve muscle junction. In exceedingly difficult experiments they measured the number of acetylcholine molecules in a quantum, the unit of release from motor nerve terminals. The principle was to apply known concentrations of the transmitter in a highly localized manner instantaneously to the receptors at the motor endplate. By comparing quantitatively the artificially and naturally evoked quanta a direct estimate was made of the number of acetylcholine molecules. This was approximately 5,000, a concentration that could be achieved in a single synaptic vesicle.

What made Stephen Kuffler's papers so remarkable was that the point was never in doubt. Even today it is easy to see how each research project decisively took on an issue that was messy, occult, undecided—or not even thought of—and brought it to a new level of understanding. The earlier papers, like the later ones, are easy to read, economical, and written with flair. The clarity of the thinking and the presentation as well as the direct answers usually obviated the need for long discussions.

STEVE IN THE LABORATORY

No one had greater disdain than Steve did for sloppy thinking or sloppy experiments. Yet this attitude was never translated into unkindness at the personal level. A somewhat sharp but subtle wit was his instrument for deflating pomposity or countering aggression. During experiments he *worked;* you would try again and again and again, all day and late at night, and again the next day until you got good recordings you could rely on. Single mindedness and dedication during experiments were in contrast to the relaxed, vague, almost amorphous approach with which long-term projects were discussed in the first place. He used to say that it was silly to do experiments that could take weeks or months without spending a decent time discussing what to do. I believe that he worked by thinking at great length about what was the most interesting project he could solve (that he could undertake himself with his own particular talents). Then came the enjoyment of finding the right preparation. A new project was a time of constant exploration, feeling out different technical approaches, and continually redefining one's objectives. Much the same was true of the way we wrote a book together (*From Neuron to Brain*) every summer for seven years or so. No amount of time was too long to devote to the title (I think we spent three weeks on that), the table of contents, the structure of a chapter, or the esthetics of the figures. At the same time a feature of experiments made with Steve that made them such unending pleasure was the series of jokes, comments, banter, and reminiscences of colleagues. The jokes would flow freely with improvisations, puns, and set-piece jokes. Through his talking with such affection about his previous co-workers, one got to know them. Thus, long before I had met Cuy Hunt or Werner Loewenstein, I looked on them as friends; the same was true even of people who had died, like Joe Lillienthal and Julian Tobias, who had been close friends of Steve's. I never knew him to make a malicious joke or a joke at someone else's expense.

The number of deep and long lasting friendships Steve formed with students and colleagues greatly exceeds the few names mentioned above. The general feeling of excitement in the charmed circle of Steve's department was due to the brilliant students, the extraordinary research being done by the young faculty, and by the infrastructure provided by Marion Kozodoy, Steve's secretary and administrator. None of us ever needed to waste hours of time that could be devoted to experiments on administrative details, which she and Steve somehow handled.

HONORS AND AWARDS

Steve was widely recognized as a truly original and creative neuroscientist. In addition to numerous prizes, honorary degrees, and special lectureships from countries over the world, Steve was elected to the National Academy of Sciences in 1964 and to the Royal Society as Foreign Member in 1971. In 1964 he was named the Robert Winthrop professor of neurophysiology and neuropharmacology. From 1966 to 1974 he was the Robert Winthrop professor of neurobiology, and in 1974 he became John Franklin Enders university professor.

FAMILY

Steve and Phyllis (née Shewcroft) were married in Australia in 1943. She had attended his medical school lectures in physiology. In addition to being a doctor, she was an accomplished painter and educator and received a doctorate from Harvard University. Their oldest daughter Suzanne is a painter. Damien is a well-known neuroscientist in his own right at the University of Puerto Rico. Eugenie is a composer, flautist, and performer in Paris and Julian is a physician in Maine. The four children, Phyllis, and Steve provided warm hospitality and friendship to Steve's "scientific family" from around the world. Towards the end of his life he suffered from diabetes and glaucoma. Nevertheless, he continued to work, swim, travel, and play tennis with uncanny, cat-like ability, enjoying life at home and in the lab to the end. Only his closest friends were aware of the drastic deterioration of his vision or the precariousness of his insulin treatment.

CONCLUSION

Steve's importance for neurobiology was unique. His imaginative experiments have stood the test of time and provided essential pointers for others to take up where he left off. Numerous distinguished molecular biologists and geneticists, such as Gunther Stent and Seymour Benzer to name just two, were attracted to neurobiology by his work.

His immense influence as a teacher was not due to assertiveness or rhetoric but to example. He maintained the highest standards in his students and co-workers by a quizzical look, a mild quip, or—worst of all—boredom. I was always curious about how his philosophy and code of behavior had developed. Coupled with a hatred of extremism, he showed endless sensitivity and consideration in dealing with other people in every walk of life. The only sign I saw of a double standard in his conduct was the contrast between his own lack of consideration for himself and the infinite trouble he would go to for colleagues who were in need of help. It requires poetry or art rather than a standard obituary to convey Steve's joie de vivre, his love of experiments, his love of friends and family, his patience, tolerance, enthusiasm, wit, and wisdom.

A detailed, affectionate, and authoritative account of Stephen Kuffler's life and work has been provided by Sir Bernard Katz (*Biographical Memoirs of Fellows of the Royal Society*, vol. 28, pp. 225-59, 1982) and in a book entitled *Steve, Remembrances of Stephen W. Kuffler,* compiled and introduced by U. J. McMahan (Sunderland, Mass.: Sinauer Associates, 1990).

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Anton Lang

Anton Lang

January 18, 1913-June 24, 1996

By Hans Kende and Jan A. D. Zeevaart

Anton Lang Died on June 24, 1996, in Oxford, Ohio. He belonged to a group of eminent plant physiologists who had a profound influence on the field through their research, writings, and public service. Anton's scientific interest focused on plant development, particularly on photoperiodic regulation of flowering. His writings, especially the scientific reviews, set a standard that is hard to match. His dedicated public service culminated in chairing a committee of the National Academy of Sciences that was appointed to assess, often on dangerous field trips, the effects of herbicide use during the Vietnam War.

Anton was born in St. Petersburg, Russia, on January 18, 1913. His father was a famous cardiologist of German ancestry; his mother was Russian. In the early summer of 1917, he and his mother went on one of their yearly summer vacations to the family dacha near the Finnish border. As the political situation in Russia worsened, the family council decided that Anton and his mother should move to Finland until the conditions in Russia normalized again. After four years of waiting in vain for this to happen, Anton moved with his mother to relatives in Germany, first to a small rural town in eastern Pomerania and then to Berlin.

Anton's early interest in plants was awakened by a school project. He studied the flowers of cucumbers and peas during one summer and at the age of eleven wrote a monograph on the topic. His interest in plants persisted and, after graduation from gymnasium, he enrolled in 1931 at the University of Berlin with botany as his major subject. While studying, he also served as an extra at the Berlin State Opera. For a short while, he even considered becoming an opera singer himself. However, after one audition with an uncle, this plan was dropped. Even though Anton was a very busy man in his later professional life, going to the opera and tending to his flower garden remained his favorite pastimes, as was reading in the three languages in which he was fluent: English, German, and Russian.

Geneticist Elisabeth Schiemann, one of the first women scientists to penetrate the German academic establishment, was Anton's thesis advisor. For his dissertation, he studied evolutionary problems in the genus *Stachys* using genetic and cytological approaches.

Being a stateless person in Germany was not an easy position in the Third Reich and this led to some tense situations for Anton, e.g., the initial refusal of the authorities to admit him to his doctoral exam. His efforts to leave Germany and to continue his studies abroad were unsuccessful. Since Anton did not have German citizenship, he was precluded from obtaining a scholarship and had to earn his way through the university. He did this in part by writing abstracts of papers for various scientific journals. This occupation, which was often tedious and dull, brought two benefits. First, it helped him later in his career to distill the essence of papers for his reviews that became classics in the field. Second, Anton's succinct abstract of a paper by Georg Melchers attracted the attention of the author, who offered him a position as scientific assistant at the Kaiser Wilhelm

Institute in Berlin-Dahlem after Anton's graduation in 1939. Melchers was one of the upcoming developmental plant biologists whose work, together with that of Chailakhyan in the Soviet Union, had led to the hypothesis that flowering is controlled by a hormone. The cooperation between Anton and Melchers proved extremely fruitful and continued at the Max Planck Institute in Tübingen until 1949, when Anton, his wife Lydia, and his mother emigrated to North America. Anton described these ten years with Melchers as one of the most exciting and cherished periods of his career.

Much of our understanding of photoperiodism and vernalization derives from that period of Anton's career. The years of collaboration with Melchers provided a firm physiological basis for the flower hormone (florigen) concept. According to this hypothesis, photoperiodic induction is perceived by the leaves, and a hormonal substance is then transported from the induced leaves to the shoot apex, where it causes a transition from vegetative growth to reproductive development. Using annual and biennial strains of *Hyoscyamus niger* (black henbane, later at Caltech referred to as Russian spinach) and short-day and day-neutral varieties of tobacco and the long-day plant *Nicotiana silvestris,* they showed that the graft-transmissible promoter of flower formation is similar, if not identical, in different species and different photoperiodic response types. However, efforts to extract the flower hormone failed. With self-deprecating humor, Anton used to say that he was a member of that distinguished group of plant physiologists and biochemists who had failed to isolate and identify the elusive floral hormone "florigen." Nevertheless, Anton remained an outspoken proponent of the hypothesis that a specific substance is required for the change from vegetative to reproductive development in plants. Anton and Melchers also studied vernalization in biennial *Hyoscyamus,* i.e., the effect of cold-treatment on subsequent flowering. They showed that vernalization proceeded in two stages, the first reversible and the second not.

In the New World, Anton's first station was Montreal, where he was the recipient of a Lady Davis fellowship in the genetics department of McGill University. As his fellowship at McGill drew to a close, Anton accepted his first job in the United States, a visiting professorship at Texas A&M University. There, Anton was engaged in studies on the flowering of cotton. In the fall of 1950 Anton moved to Caltech, where he became a research fellow with James Bonner. His research with James Liverman addressed the effects of auxin on flowering. Of his two years in Pasadena, Anton said that no one who had passed through Caltech had left it quite the same person, and probably everyone retained a trace of regret at having left.

In 1952 Anton accepted a faculty position in the botany department at UCLA. His seven years at UCLA were among the most productive ones of his career. The growth-promoting effects of the newly recognized plant hormone gibberellin were already known at that time. However, all work had been carried out with plants that had already formed stems. In contrast, Anton applied gibberellin to rosette plants, i.e., plants without a developed stem and found "with boundless delight" that treatment with gibberellin elicited first stem elongation and subsequently flower formation. Although gibberellin was the first chemical to consistently elicit flower formation in vegetative plants, Anton was quick to point out that it was not the elusive florigen. Based on results of earlier grafting experiments, he reasoned that florigen appears to be the same in short- and long-day plants, whereas applied gibberellin can induce flower formation in long-day plants, but not in short-day plants. Therefore, he concluded that gibberellin and florigen could not be the same.

However, he suggested that gibberellin may be involved in the formation of florigen in those plants in which applied gibberellin causes flower formation. Evidence in favor of this idea was obtained later.

Another important discovery at UCLA was that applied kinetin, another newly discovered plant hormone, delayed senescence of detached *Xanthium* leaves. This finding gave rise to the idea, later shown to be correct, that cytokinins fulfill the role of Chibnall's hypothetical root hormone. Such a substance was thought to be produced in the root and transported to the shoot where it would prevent processes associated with senescence, e.g., breakdown of protein and chlorophyll. Cytokinins were already known to regulate cell division when applied in combination with the phytohormone auxin. Anton's discovery that they also retarded leaf senescence opened a whole new field of research. As Anton stated, these various results provided so many leads for further work that it was necessary to decide which ones to follow. Since the effects of gibberellin were closer to the flowering problem, he decided to concentrate on the action of this plant hormone.

In 1959 Anton moved from UCLA back to Caltech, this time as professor of biology and director of the Earhart Plant Research Laboratory, called colloquially the "phytotron." This complex of climate-controlled greenhouses and growth chambers had been designed by Frits Went for the study of plant growth and development under various environmental conditions. In Pasadena, Anton continued his studies on gibberellin action. By that time, the structures of nine gibberellins were known, and Anton compared the effects of these on stem growth and flowering in several plants.

We, the authors of this memoir, were both postdoctoral fellows at Caltech and did at least part of our research with Anton. One important project in which we participated concerned the mode of action of a class of synthetic plant growth regulators, the so-called growth retardants. They were shown to specifically block gibberellin biosynthesis, thereby inhibiting those growth and developmental processes that require this hormone (e.g., flowering in *Samolus* and *Bryophyllum*). Anton was a supervisor who left his coworkers plenty of room for their own initiatives in research but he also instilled in each a sense of high standards regarding the choice and solution of scientific problems. The atmosphere at Caltech was one of continuous stimulation, since these were the days when modern molecular biology was being born. Nevertheless, trips to the desert, the mountains, and the sea were part of our lives, and seminars were sometimes devoted to travelogues, especially after conferences in faraway places. In Pasadena, we also experienced the wonderful hospitality of Anton and Lydia Lang. The Russian Christmas parties, later continued in East Lansing, were a tradition that we all cherished, especially a memorable one held in the Mexican style.

In 1964 the Atomic Energy Commission decided to build the Plant Research Laboratory at Michigan State University, and Anton was named its first director. He moved to East Lansing in 1965 and assembled as staff a group of young faculty members, which included both of us. Anton built an institution that still carries his imprimatur. The esprit de corps of the Plant Research Laboratory is largely a direct result of the philosophy that guided Anton's leadership. He did not have a personal agenda, but he demonstrated in many ways his total commitment to excellence in science. As one example, which director today would take the time to read and edit every paper written in his institute? At the Plant Research Laboratory, Anton initially continued his research on gibberellins, but once he became involved in studying the effects of herbicide use in Vietnam his own research program came to a temporary halt.

After Anton completed his tour of duty in Vietnam, he returned to the bench—or, as he more accurately put it, to the greenhouse. During a sabbatical leave in Moscow with Chailakhyan, he demonstrated that graft-transmissible inhibitors of flower formation (antiflorigens) are formed in non-induced leaves, specifically of the long-day plants *Hyoscyamus niger* and *Nicotiana silvestris.* Following his official retirement in 1983, Anton continued to work on flowering, particularly on in vitro regeneration of flower buds from thin-tissue layers of tobacco. In his last publication, he showed that the potential for formation of flower buds is present in explants from flowering *Nicotiana* plants of all photoperiodic types, not just day-neutral ones.

Anton dedicated much of his time to editing and writing reviews. To many plant physiologists, he was best known for his work as managing editor of *Planta,* an international journal that attained eminence under his uncompromising leadership. The current editors of *Planta* wrote in their obituary that nothing escaped Anton's eagle eye. Authors, editorial board, managing editors, and Springer-Verlag would all receive lengthy letters setting out his comments and criticisms in definitive terms. As editor, Anton provided many young colleagues with meticulous lessons in scientific writing. We all learned, among other things, that results do not suggest, only people do; results indicate. Also, experiments do not reveal; revelations are reserved for the Bible. Anton's reviews were legendary, sometimes longer than the papers themselves. Anton was an intellectual with little flair for technology. He used to type his reviews with two fingers on an old-fashioned typewriter, and Lydia typed the clean copy. One of us offered to teach Anton and Lydia the essentials of word processing, which would have saved both of them a lot of time. Anton gave a stern look and replied, "Lydia and I do not want to save time." As every secretary in the Plant Research Laboratory would confirm, Anton was a democratic man. He insisted on making his own photocopies, just like everybody else did. However, as the photocopiers became more and more sophisticated, his war with this equipment kept escalating. Anton was much better at writing incisive papers than at pushing buttons.

Between 1941 and 1961 Anton also wrote annual reviews on developmental plant physiology in *Fortschritte der Botanik* (now *Progress in Botany*). His writings were never mere compilations of the newest literature but always an integration of new information with earlier work and hypotheses. He also undertook the monumental task of editing volumes XV/1 and XV/2 of the *Encyclopedia of Plant Physiology.* Despite the fact that there were more than fifty contributing authors, these two volumes are among the best edited and integrated ones in this series. Anton's own article "Physiology of Flower Initiation" remains one of the most thorough and comprehensive reviews on flowering. Anton also served on editorial boards of other journals, e.g., of *Plant Physiology, Developmental Biology,* and the *American Journal of Botany.*

Anton's sense of duty was tested to the limits when he was asked to chair the National Academy of Sciences Committee on the Effects of Herbicides in South Vietnam. During the Vietnam War, herbicides were used by American forces to defoliate dense forests, thereby facilitating detection of North Vietnamese and Viet Cong military units. To a lesser extent, herbicides also were used to destroy crops. As the magnitude of this program increased, critical voices were heard, and Congress directed the Secretary of Defense to ask the National Academy of Sciences for a study on the ecological and physiological effects of herbicide use in Vietnam. The Academy accepted this responsibility and in early 1971 Anton agreed to chair the committee despite serious concerns over the feasibility of this task. He did so because he believed in the importance of determining the nature and scale of these effects, and because delaying this assessment lessened the prospects of obtaining meaningful data.

To increase the credibility of the committee, Anton insisted on the inclusion of foreign experts. This request was granted, even though the committee had to deal with classified information at a time when the war was in full progress. Accordingly, the National Academy of Sciences Committee on the Effects of Herbicides in South Vietnam included, besides nine U.S. scientists, three from the United Kingdom, two from Vietnam, and one each from Canada, Sweden, and Taiwan. The obstacles in carrying out the study and writing a report were formidable. They ranged from collecting quantitative field data under combat conditions to dissent among members of the committee on the evaluation of some of the data. Anton and members of the committee came under fire while conducting aerial surveys of herbicide-treated forests and had to endure many physical hardships during their research in the field.

Nevertheless, the committee was able to make estimates of the ecological and health consequences of massive herbicide applications over wide areas. It issued a voluminous report that was submitted by the Academy to Congress and the Secretary of Defense in 1974. This initial study lacks, for obvious reasons, follow-up investigations that would have been necessary to verify the original data and to assess the long-term effects of herbicide use on forest ecology and human health. Anton undertook this assignment because of his deeply felt loyalty to the United States and the National Academy of Sciences. In his recollections published as a prefatory chapter in the 1980 volume of the *Annual*

Review of Plant Physiology, Anton stressed the values of positive patriotism, namely attachment to and pride in one's country. It goes without further elaboration that Anton also served on numerous national committees and panels of the Academy, the National Science Foundation, and other federal agencies.

Anton's scientific and civic contributions were widely recognized, and he received many national and international awards. Among them were in 1976 the Stephen Hales Award and the Charles Barnes Life Membership Award of the American Society of Plant Physiologists. This came as a great surprise to him because nobody before (and since) had received these two highest honors of the Society at the same time. In thanking the selection committees, Anton wrote, "A person adapted to Russian winters, German cuisine, Canadian French, Californian smog, Southeast Asian sniper bullets and other major natural disasters can stand such a stress—but can somebody else, e.g., a plain American?" In 1965, Anton was elected to the German Academy of Natural Scientists (Leopoldina), in 1967 to the National Academy of Sciences, and in 1968 to the American Academy of Arts and Sciences. In 1981 he received an honorary doctorate from the University of Glasgow, and in 1982 he was awarded an honorary membership by the German Botanical Society. He was elected president of the Society for Developmental Biology in 1968 and of the American Society of Plant Physiologists in 1970.

Anton is survived by his wife Lydia who supported him in all of his professional activities. She was a wonderful hostess to all visiting scientists, a true "first lady" when Anton was director of the Plant Research Laboratory, and an efficient editorial assistant. His family includes his two sons

Peter and Michael; his daughter Irene; Irene's husband Howard Kleiman and their children Joshua and Carly.

In conclusion, we want to recognize Anton in the words of a member of the committee that selected him for the Charles Barnes Life Membership Award: "I have a personal affection for Anton Lang because of his selfless contributions to all of us in plant physiology. His personal warmth, kindness and concern for the welfare of young and little-known scientists have helped along many careers, including my own." This is how we and his colleagues will remember him.

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Samuel Colville Lind

June 15, 1879-February 12, 1965

By Keith J. Laidler

Samuel Colville Lind was distinguished for his work on the kinetics of chemical reactions of various kinds, especially reactions induced by ionizing radiation. He is still particularly remembered for his early pioneering study, with Max Bodenstein in Leipzig, of the reaction between hydrogen and bromine. His later research and his books were significant in placing on a firm basis the effects of radiation on the rates of reactions. He had wide experience in government laboratories and universities, and through his engaging personality exerted a strong influence on others. His interests ranged far outside science, and his memoirs, published posthumously in 1972, shed an interesting historical light on the various institutions and people connected to him.

Lind was held in high regard as a scientist, teacher, and administrator. The esteem and affection of his friends and colleagues are evidence of his generosity and helpfulness. He had a lively sense of humor, and enjoyed jokes on himself. His friends ran the gamut from backwoods countrymen he met on his many fishing trips to Nobel laureates.

EARLY LIFE AND EDUCATION

Samuel Lind was born on June 15, 1879, in McMinnville, Tennessee, a town at that time of 2,000 inhabitants on the middle Tennessee plateau just west of the Cumberland Mountains. His father Thomas Christian Lind was born in Sweden and came to the United States at the age of nineteen. He served in the federal army in the Civil War, rose to the rank of captain, and was wounded in the Battle of the Wilderness. He was employed by the Pennsylvania Oil Company and was sent to Tennessee to find oil, but unable to find any, he studied and then practiced law. In May 1878 he married Ida Colville, a native of McMinnville, who belonged to a Scottish-Irish family that had arrived from Maryland and settled in Tennessee several generations earlier. Samuel was the eldest of five children, only three of whom—all boys—survived to maturity.

Lind's education was in the public schools and high school of McMinnville, and he later considered it to have been excellent. In 1895, at the age of sixteen, he enrolled at Washington and Lee University in Lexington, Virginia. This was a traditional, intensely Southern institution that emphasized the classics, but required its students to study mathematics and science. Lind spent most of his first three years studying French, Latin, Greek (which he took for four years), German, and Anglo-Saxon. He then entered his senior year needing six credits in science, and was persuaded that the easiest way to gain them was to take chemistry! He later commented that he had been forced by circumstances into what was to become his life's work.

Previously he had known little chemistry, but thanks to the inspired teaching of Jas Lewis Howe he found the subject entrancing ('Jas' was not an abbreviation). He later wrote that no praise could be too high for the teaching of Howe, who taught all the chemistry courses. After receiving his B. A. degree at the end of his fourth year, Lind decided to return to Washington and Lee for additional courses in chemistry taught by Howe. He also took courses in geology and mineralogy.

In the fall of 1902 Lind entered the Massachusetts Institute of Technology, where he took some courses and acted as a teaching assistant. At the time, MIT gave no graduate degrees, but he carried out some research under the direction of Arthur Amos Noyes and published his first paper. In the following year he was awarded a Dalton traveling fellowship, and decided to go to the Institüt für Physikalische Chemie in Leipzig. In preparation for this he spent three months in a *pension* in Kassel learning German, avoiding as far as possible any contact with English-speaking people.

THE LEIPZIG LABORATORY (1904-1905)

At this time the institute at Leipzig, directed by the eminent Wilhelm Ostwald, was world famous for its contributions to physics and physical chemistry. Students flocked there, particularly from the United Kingdom and the United States. Indeed, the institute was more renowned outside Germany than within, since at the time, German chemists concentrated on organic chemistry and many were hostile to the kind of chemistry done by Ostwald and his associates. Of the American chemists who were at Leipzig at about that time, eleven were later elected to membership in the National Academy of Sciences (Wilder D. Bancroft, William C. Bray, Frederick G. Cottrell, George A. Hulett, Arthur B. Lamb, G. N. Lewis, S. C. Lind, A. A. Noyes, T. W. Richards, E. C. Sullivan, and Willis R. Whitney), and six became presidents of the American Chemical Society (Bancroft, Lamb, Lind, Noyes, Richards, and Whitney).

In 1903, when Lind arrived in Leipzig, Ostwald's personal influence was on the decline, but he had two distinguished associates, Professors Robert Luther and Max Bodenstein. Most of the students went first to Luther for guidance, and he advised Lind to approach Bodenstein as a possible research director. In doing so Lind made a mistake, due to his limited knowledge of the language. He had intended to say, "Herr Dr. Luther hat mich empfohlen Sie zu besuchen (has advised me to see you)," but said instead, "hat mich befohlen (has ordered me)." He at once recognized his mistake and was embarrassed, but Bodenstein was amused, and later enjoyed telling the story. The two had a most profitable association, and got on well except for one unfortunate incident. Lind was boiling chromic acid, a highly corrosive liquid, and Bodenstein entered wearing for the first time a new, elegant spring suit. The beaker cracked, spilling acid over the suit, and Bodenstein only escaped serious injury by quickly dousing himself with water. Bodenstein was at first angry with Lind, but soon realized that he was in no way to blame. Frau Bodenstein later told Lind that the suit had been reduced to shreds.

Lind's first research with Bodenstein was on the kinetics of the reaction between hydrogen and chlorine, but they abandoned the work on finding that there were serious experimental difficulties (which were not to be resolved until some years later). Bodenstein had already made important investigations on the reaction between hydrogen and iodine, which had proved to be much easier to handle; the rate of the reaction is directly proportional to the concentrations of hydrogen and of iodine (in the language of kinetics, the reaction is second-order). Bodenstein and Lind then turned their attention to the reaction between hydrogen and bromine, and found that satisfactory measurements of the rates could be made under a variety of conditions of temperature and reactant concentrations. The reaction was not, however, of the second order. The rate was proportional to the concentration of hydrogen, but not to the concentration of bromine; instead it was proportional to its square root. They also found that the reaction is inhibited (i.e., its rate is reduced) by the product of the reaction, hydrogen bromide. Their work led them to a fairly simple equation for the rate of reaction as a function of concentrations.

Bodenstein and Lind's paper on this reaction, published in 1907 in the *Zeitschrift für physikalische Chemie*, had an important influence on the course of chemical kinetics over the next two decades. Bodenstein was unable to give a detailed explanation for the peculiar kinetic behavior of this reaction, but he did in 1913 make the correct general suggestion that a chain reaction was involved. In 1918 Walther Nernst suggested a specific chain reaction for the more complex reaction between hydrogen and chlorine, but only later did his idea lead to the correct detailed mechanism. In 1919 and 1920 J. A. Christiansen, K. F. Herzfeld, and M. Polanyi independently and almost simultaneously proposed a specific mechanism for the hydrogen-bromine reaction and showed that their mechanism led precisely to the kinetic equation Bodenstein and Lind had obtained for the reaction. Their experimental work was therefore crucial to the evolution of the understanding of chain reactions. The Leipzig Ph.D. degree (magna cum laude) was conferred on Lind in August 1905 on the basis of his work on the hydrogen-bromine reaction. Bodenstein offered him an assistantship in the Leipzig laboratories, but he decided instead to return to the United States.

UNIVERSITY OF MICHIGAN (1905-13)

In September 1905 Lind accepted an instructorship in chemistry at the University of Michigan. He was put in charge of the physical chemistry teaching laboratory, and was able to carry out a little research. An important change in his research career came in 1910, when he spent some months in the laboratories of Professor Marie Curie in Paris. He did not have much personal contact with her, but he at tended her lectures. In the laboratories he gained a proficiency in the handling of radioactive substances and carried out measurements of the charge on the alpha particles. In the spring of 1911 he moved to Vienna to the newly formed Institüt für Radiumforschung, which was under the direction of Professor Stefan Meyer. There he worked on the formation of ozone from the action of alpha particles on oxygen molecules. Within three months he had been able to show that the number of ozone molecules formed is equal to the number of ion pairs produced in oxygen by alpha particles.

On his return to the University of Michigan, Lind wanted to continue this line of experimental research, but he was frustrated by the difficulty of obtaining radium. Instead, he carried out a detailed analysis of results obtained in England by Sir William Ramsay and associates and in 1912 published an important paper in which he clearly enunciated many of the basic principles that apply to chemical reactions induced by ionizing radiation. This was to be his main field of research for the remainder of his career.

U.S. BUREAU OF MINES (1913-25)

In 1912 the U.S. Bureau of Mines began to investigate, as a source of radium, Colorado carnotite, which is a potassium uranium vanadate deposited in sandstone. The work of the bureau was directed in Washington by Charles A. Parsons, chief chemist of the bureau. The fieldwork in Denver, Colorado, was directed by Richard B. Moore, who had studied under Ramsay. Because of his interest in radiation chemistry, Lind accepted in 1913 an appointment with the bureau, and worked at the Denver laboratory. Initially his work was mainly concerned with the extraction and refinement of radium from carnotite, and he published a series of papers on that subject from 1914 to 1920. The work was extremely laborious and was accompanied by some danger from radioactive emissions not fully recognized at the time. The ore contained only a few percent of uranium, and the ratio of radium to uranium was minute. From about 30 tons of uranium oxide obtained from the ore the Colorado institute produced 8.5 grams of radium. Of this, half a gram was entrusted to Lind, and he made good use of it in his experiments over the years, even after his retirement. Being unaware of the dangers of handling radioactive substances, Lind habitually picked up samples with his fingers, and the thumb and index finger of his right hand were burned to half their normal thickness, so that they left no fingerprints. Aside from a decrease in sensitivity in these two digits he suffered no ill effects.

On January 24, 1915, Lind married Marie Holladay of Omaha, Nebraska, and she subsequently accompanied him on all his travels.

After the completion of the separation work, Lind was transferred in 1917 to the Bureau of Mines station in Golden, Colorado, where he devoted much of his time to studying the chemical effects of radiation. For many years much of the work was done in collaboration with D. C. Bardwell. The reaction between hydrogen and oxygen is so slow at ordinary temperatures that no change can be detected, and Lind was the first to observe that reaction occurred at a measurable rate when radon was added as a source of alpha radiation. Lind carried out an important series of investigations on this reaction. He showed that the amount of reaction that occurred was directly proportional to the ionization produced in the gas mixture. He also showed that recoil atoms induce chemical action proportional to the ionization they produce. He also carried out a number of investigations on the influence of inert gases on chemical reactions induced by radiation, finding that reactions are accelerated as a result of the additional amount of ionization produced in the inert gas.

His investigations led him to a number of important conclusions. He was perhaps the first to recognize that chemical reaction is initiated at the centers at which the ions are neutralized. He concluded that in the hydrogen-oxygen reaction, both the hydrogen and oxygen molecules are separately activated by absorption of energy from the alpha rays. He found that about one-half of the energy released by an alpha particle produces a positively charged molecule, by eliminating an electron. Some of the rest of the energy is concerned with the direct formation of atoms or free radicals. The positive molecular ions produced undergo neutralization on encountering other species and are then highly energized and capable of undergoing further reaction.

Lind found that the ion yield—the number of water molecules produced per ion pair in the hydrogen-oxygen reaction—is about four. He interpreted this as due to the clustering of neutral molecules about the ions by electrostatic attraction. During this period he began to write his first book, on the chemical effects of radiation; it was published in 1921.

In 1920 the scientists at the Golden laboratories were transferred to Reno, Nevada, where they first worked in temporary quarters provided by the University of Nevada and later in a building constructed for them on the campus. One of Lind's investigations in Reno was on the coloring of diamonds by radiation, an effect first reported in 1904 by Sir William Crookes. Lind confirmed that diamonds exposed to alpha radiation acquire a brilliant green color and found that the coloration could be conveniently produced by exposure to radon gas. Green diamonds are extremely rare and their prices fabulous. Many diamonds have an unpleasant yellowish tinge, and are worthless as jewelry.

The yellow color was masked by the green tinge produced by radiation, and at first it was thought that diamonds of high value might be produced from worthless ones. It was realized, however, that the artificial green diamonds were very radioactive and therefore unwearable. Something of a stir was created in the jewelry world by the prospect of unscrupulous dealers making fortunes by selling radioactive diamonds. After a famous jewelry firm had treated Lind with some hostility, he decided to carry out no further experiments on this sensitive matter; he did, however, think his results were of scientific interest, and he and Bardwell published an account of them in 1923.

In that year Lind was appointed chief chemist of the Bureau of Mines; this involved a move to Washington, and Bardwell went with him. There the two continued their experimental work in radiation chemistry.

FIXED NITROGEN RESEARCH LABORATORY (1925-26)

In 1925 Lind resigned his position at the Bureau of Mines to become assistant director of the Fixed Nitrogen Research Laboratory of the U.S. Department of Agriculture. The laboratory was in Washington, and the director at the time was F. G. Cottrell, who agreed to appoint Bardwell also.

At this laboratory Lind worked on the radiation chemistry of a number of reactions, including the oxidations of saturated and unsaturated hydrocarbons, the hydrogenation of unsaturated hydrocarbons, and the reaction between carbon monoxide and oxygen. He and his assistants also did further work on the influence of inert gases on radiation-induced reactions.

UNIVERSITY OF MINNESOTA (1926-47)

In 1926 Lind was offered two important academic positions almost simultaneously: to be head of chemistry in two of the most prominent mid-western universities—Michigan and Minnesota. His decision to go to Minnesota was largely because the position offered was head of its School of Chemistry, which was not part of any other faculty and included a very effective department of chemical engineering. Also, the University of Minnesota was a prestigious one; it had a particularly strong administrative structure and had been presided over by men of great distinction.

Lind's twenty-one years at the University of Minnesota were very effective, as he did much to enhance the already high reputation of the school. He soon made a number of wise appointments, one of which was to bring Isaak Kolthoff from the University of Utrecht to be head of analytical chemistry; from 1927 until his retirement in 1962 Kolthoff, by his books and research, made the University of Minnesota a leader in the field. Lind also brought in Frank McDougall to be in charge of physical chemistry, and he in turn appointed a number of people who were to distinguish themselves and the University of Minnesota for the quality of their teaching and research. Among them may be mentioned George Glockler and Robert Livingston (with both of whom Lind later collaborated), Hubert Alyea, and Bryce Crawford. Later Alyea, particularly after going to Princeton, became famous throughout the chemical world for his lecture demonstrations. Bryce Crawford became well known for his pioneering contributions in chemical spectroscopy, and later became dean of the graduate school at the University of Minnesota.

Lind was also active in a number of ways outside his university. He was elected president of the American Electrochemical Society in 1927 and the American Chemical Society in 1940. In 1932 he assumed the editorship of *the Journal of Physical Chemistry*, which had fallen into ill repute as a result of the old-fashioned ideas of its founder and former editor Wilder Dwight Bancroft (A. A. Noyes did not allow the journal in the chemistry library of the California Institute of Technology). Previously the papers published in this journal were in certain narrow and unimportant branches of chemistry and were sometimes written by cranks with outmoded ideas. Lind at once tightened the editorial policies, and the journal soon became a more important medium for the publication of high-quality research.

Because of his many other activities Lind's research output during his years at the University of Minneapolis could not be as great as in previous years. He nevertheless directed an extensive research program in the field of radiation chemistry, and a number of students earned their Ph.D. degrees under his direction. He always maintained an active interest in research, and in 1939 he published, in collaboration with George Glockler, a book on radiation chemistry that exerted a wide influence.

LATER YEARS

Lind retired from the University of Minnesota in 1947 at the age of sixty-eight. In the following year he attended a meeting at Oak Ridge in his native Tennessee, and he and his wife decided to make their home there. In July 1948 he was appointed a consultant to the Union Carbide Corporation, which had the contract under the Atomic Energy Commission to run the plants and laboratories at Oak Ridge. He obtained the necessary security clearance and first worked in the gaseous diffusion plant, which was concerned with the separation of uranium-235—the fissionable isotope of uranium—by gaseous diffusion of its hexafluoride.

For a period Lind served as acting director of the chemistry division at the Oak Ridge National Laboratory and for several years he actively continued his research on the radiation chemistry of gases. In 1957 at the age of seventy-eight he published a significant paper with Philip S. Rudolph on the polymerization of acetylene under the action of alpha radiation. Other radiation-induced reactions studied with Rudolph were the polymerization and dissociation of carbon monoxide.

Lind had a great love of trout fishing, and his memoirs include many stories of fishing incidents and entertaining accounts of several minor brushes with the law, leading to fines as a result of unintentional violation of fishing regulations. It was while trout fishing in the Clinch River below Norris Dam that he met his death on February 12, 1965, at the age of eighty-six. After he had been reported missing, his body was found a mile downstream from where he had been fishing, waters from the dam having carried it there. For some years Lind had been quite deaf and had to wear a hearing aid, which he would often say was a boon to him, as he could turn it off if a conversation became boring. His deafness perhaps contributed to his death, as he may not have heard the rush of the waters unexpectedly released by the dam. He was buried in McMinnville, where he was born; Marie, his wife of just over fifty years, survived him.

Lind was awarded many honors. In 1926 he received the Nichols Medal of the New York section of the American Chemical Society. He was elected a member of the National Academy of Sciences in 1930, the only native Tennessean to have won that distinction. He was elected a member of the Minnesota Academy of Science in 1940 and the American Philosophical Society in 1943. In 1949 he was elected to the Tennessee Academy of Science, and in 1952 the American Chemical Society gave him its highest honor, its Priestley Medal. He was awarded honorary doctor of science degrees by the University of Colorado in 1927, Washington and Lee University in 1939, the University of Michigan in 1940, and the University of Notre Dame in 1963.

I never met Lind, and I prepared this memoir to some extent on the basis of his autobiography, which was published posthumously (1972). It contained an appreciation written by Philip S. Rudolph and a description of the circumstances of Lind's death. The Academy provided me with other information, including an unpublished memoir prepared by Lind in 1943; this carried a complete list of his publications up to that time. Some valuable background material, especially about the Leipzig laboratory, was provided by John W. Servos in his book *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990). I am particularly grateful to Professor Bryce Crawford, who knew Lind at the University of Minnesota, for his unfailing support and helpful suggestions.

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Alfred O C Nier

Alfred Otto Carl Nier

May 28, 1911-May 16, 1994

By John H. Reynolds

In May 1994 the international scientific community lost one of its most distinguished experimentalists. Alfred Nier, regents professor emeritus at the University of Minnesota, used mass spectrometers of his own design and construction in a manner that can only be likened in breadth and importance to Albert Michelson's use of interferometers to answer scientific questions in numerous and widely diverse fields of inquiry. These questions included how old is the earth; which isotope of uranium is responsible for slow neutron fission; how is the atmosphere of Mars composed; what are the details of nuclide stability in the table of isotopes; what nuclides of great rarity remained to be discovered in nature; how effective are various schemes of isotope separation; and how can mass spectrometry be applied in practical ways to chemical analysis and to leak detection in vacuum systems.

EARLY YEARS (1911-36)

Alfred Nier ("Al" to all who knew him) was born in St. Paul, Minnesota, on May 28, 1911. His parents were German immigrants who came to the United States as youngsters. His father eventually owned and operated a small dry cleaning business. Al had one sister, eleven years his senior, and no brothers. His parents had limited education but did have a deep respect for learning; with him they decided early that he would pursue education beyond high school level. The nearness of St. Paul to the University of Minnesota at Minneapolis enabled him to attend despite limited financial resources. There ensued a remarkable relationship between him and that institution; except for two years (1936-38) as a National Research Council fellow at Harvard and two years on the Manhattan Project (1943-45) in New York City, his entire scientific career was spent there. The university in 1966 recognized his great contributions by naming him regents professor of physics, one of the first five faculty members to receive that honor. They further awarded him an honorary doctorate of science in 1980. At a memorial convocation to honor his life, President Nils Hasselmo quoted another regents professor, who said, "Al Nier was the best thing that ever happened to this place."

Al showed an early aptitude for mathematics and science. Courses in shop work and mechanical drawing also attracted him and left him with an aptitude for constructing apparatus with his own hands, which served him well in later years. He and boyhood friends with interests in radio, an emerging technology at the time, followed the latest developments in receiver construction, experimenting with new circuits as they were published. Enrollment in electrical engineering at the university followed naturally from these interests and would have fixed his career in that field had it not been for the lack of engineering jobs upon graduation in 1931. He was encouraged to enter physics by physics professor Henry A. Erikson, who recognized Al's talent early on, as did another mentor, Professor Henry Hartig of electrical engineering.

Professor John T. Tate became Al's advisor when Al switched to graduate study in physics. Tate, well known among physicists partly because of his editorship for many years of the *Physical Review,* had worked with James Franck in Berlin when energy levels for electrons in atoms was an emerging subject, leading Tate at Minnesota into the field of electron impact phenomena. Many of Tate's students studied ionization potentials of atoms and molecules and cross sections for ionization of molecules subjected to electron impact. The required analysis of the resulting fragment ions led naturally to the development of mass spectrometers in Tate's laboratory. Around 1930 three of Tate's students, Walker Bleakney, Philip T. Smith, and Wallace W. Lozier, made notable contributions to this type of research. The focus on nuclear physics in those times motivated Al to select as a thesis topic the use of mass spectrometry to obtain a more precise knowledge of the isotopic composition of the elements. He developed a new instrument having higher resolution than previously available for such studies and undertook the determination of accurate isotopic abundances for five elements, namely argon, potassium, zinc, rubidium, and cadmium. In this work he discovered a rare isotope of potassium, 40K, which later became important in the measurement of geological ages, a topic that reappears later in this memoir.

AT HARVARD (1936-38)

Al's thesis work attracted the attention of Professor K. T. Bainbridge of Harvard, who with E. B. Jordan had designed and constructed a mass spectrograph for precise determination of atomic masses. Bainbridge felt that Al's work on isotopic abundances would complement his own work. The award to Al in those frugal times of one of the coveted National Research Council fellowships and a $5,000 grant from Harvard's Milton Fund—a huge sum for a young researcher during the Depression—stimulated him to accept Bainbridge's invitation to come to Harvard, where he constructed a mass spectrometer superior to any he had used before, and to spend two memorable years there. During his Harvard fellowship Al studied the isotopic composition of nineteen elements and discovered four new isotopes. Of greater importance, however, were his studies of the isotopes of uranium and lead, heavy elements that his improved mass spectrometer could for the first time analyze readily. The work greatly advanced the infant subject of geochronology. His accurate measurements of the isotopic ratio $^{235}U/^{238}U$ bore on the uranium-lead method of dating of minerals where the ratio was of direct importance and indirectly so because it afforded better values of the uranium decay constants. While at Harvard Al began measuring the isotopic composition of various lead ores—so-called common lead as opposed to the isotopically very abnormal leads extracted from uranium and thorium minerals—which had become available to him chiefly from Professor G. P. Baxter of the Harvard chemistry department. These measurements, published in 1938, were used in landmark papers written in the mid-forties by F. G. Houtermans in Switzerland and Arthur Holmes in Great Britain to derive the first conceptually valid if not yet accurate values for the age of the earth. In his final weeks at Cambridge, Al made some of the first carbon isotope measurements, establishing that the ratio $^{13}C/^{12}C$ varies in nature—the basis for an entire field in modern isotopic geochemistry.

PRE-WAR YEARS AT MINNESOTA (1938-40)

Loyalty to his Minnesota mentors and concern for his aging parents led him to return to Minnesota and to join the physics faculty in 1938. "My sister died when I was young and my parents were old enough to be my grandparents," Al once said. "They were alone here." The return to Minneapolis in no way lessened Al's scientific productivity. The years 1938 to 1942, when he was free to pursue basic science, were described by his close friend and colleague Edward P. Ney as a "whirlwind of activity wherein he brought Minnesota to the forefront of isotope research." In this brief span of time a list of his results includes further development of the uranium-lead and thorium-lead methods for dating rocks and minerals; building a thermal diffusion column for the separation of carbon isotopes, thereby establishing collaborations with scientists who could, with him, use ^{13}C tracers to investigate a host of biochemical processes; further studies in neon and methane of thermal diffusion; and demonstration by the instruments constructed in his laboratory that sector magnets could replace, without loss of resolution, the 180-degree magnetic configurations he had used earlier. The resulting practical simplifications of sector-field instruments had much to do with the explosive replication of Nier-type mass spectrometers throughout science and industry worldwide.

Perhaps his most important discovery in those times (certainly the most spectacular, to use his own words) was the isolation of ^{235}U in 1940 and the proof that it was the uranium isotope that underwent fission for neutrons of all energies, including thermal neutrons. A classic paper by Bohr and Wheeler anticipated the result, but Enrico Fermi wanted experimental proof. Fermi and Al met at a meeting of the American Physical Society in 1938. Fermi later wrote a famous "Dear Nier" letter encouraging him to separate a small sample of pure ^{235}U for study of its fission properties. Within a very short time, considering the difficulty of this unprecedented task, Al made the first uranium isotope separation in history, using one of his mass spectrometers. He sent the submicrogram quantities of separated isotopes, fastened by their aluminum backings with Scotch tape in a covering letter, to John Dunning at Columbia. Dunning, Eugene Booth, and Aristid von Grosse repaired to the Columbia cyclotron that very evening and were able to show in a few hours that the 235 sample, and not the hundred-fold more abundant 238 sample, was the source of the slow neutron fission in uranium. The certainty of this result undergirded the remarkable history of the Manhattan Project and the development of nuclear energy. Some might have belittled this experiment because of its simplicity and how quickly it was carried out, but it is doubtful that anyone else in the world could have done the job in anything like his swift and faultless manner. All of Al's talents came together to advance the work. The short account of the work, which appeared in 1940 in the *Physical Review,* essentially brought down the curtain on open publications about nuclear fission until the Smyth report appeared after World War II.

THE WAR YEARS (1942-45)

During the war years, Al's talents were diverted to applied research of great importance to the national effort. In the early development of large-scale separation of uranium isotopes, his laboratory was uniquely able to measure uranium isotopes, and he and a few students like Mark Inghram and Edward Ney were impressed into the task. Eight of the Nier instruments were later dispatched to other sites. Inghram took two instruments to Columbia; Ney took two instruments to the University of Virginia. Later on, for the vast isotope separation effort at Oak Ridge to succeed and to be safe, literally thousands of precise isotopic analyses of uranium samples were required. Al directed the required instrument development for the Kellex Corporation in New York City from 1943 to 1945. A prototype instrument of Nier design was sent to the General Electric Company, where hundreds of them were replicated and sold to the Manhattan Project. Along with the analytical instruments, Al designed a practical and portable helium leak detector of crucial importance in the gaseous diffusion plant.

RETURN TO MINNESOTA (1945-94)

At war's end, Al returned to Minneapolis and to a program of basic research, which flourished without interruption until the last two weeks of his life, when he was tragically hospitalized in almost total paralysis after losing control of his automobile and hitting a tree. He found or made time to chair the Physics Department at Minnesota for twelve years (1953-65), was a stimulating classroom teacher, and served his profession on numerous boards and committees, but his true love was working with research students in his laboratory. At this point in our account, rather than proceed chronologically, we look at some of the main chapters of his research.

GEOCHRONOLOGY AND ISOTOPIC GEOCHEMISTRY (1946-62)

He never lost interest in the subject of geochronology to which he had contributed so importantly during his years at Harvard and immediately thereafter. An increasing understanding of the processes by which the heavier elements formed suggested to C. v. Weizsächer and others that there was a causal relationship between the underabundance of ^{40}K (the rare isotope of potassium discovered by Al in 1935) and the overabundance of ^{40}Ar in the atmosphere. In 1948 with L. T. Aldrich, Al examined the isotopic composition of argon extracted from potassium minerals and discovered an excess there of ^{40}Ar, proving that ^{40}K was weakly radioactive and decaying, at least in part, to ^{40}Ar. With the basis for the potassium-argon dating method thereby conceptually established, its development ensued rapidly in laboratories throughout the world, including Al's, where H. Baadsgard and S. S. Goldich participated in the studies. Around 1950 Aldrich moved to the Department of Terrestrial Magnetism of the Carnegie Institution and established there a geochronology laboratory, which played a prominent role in the development of the rubidium-strontium dating method. Aldrich's colleagues in turn left Washington to establish other sites for geochronological work. In like manner Mark Inghram, whose interactions with Al was mentioned earlier, settled after the war in Chicago and supported work on geochronology, among other research topics. From that laboratory, students Clair Patterson, George Tilton, John Reynolds, Gerald Wasserburg (later a Crawfoord Prize awardee), and George Wetherill moved on to establish their own "shops." In this way one can categorize a large part of the worldwide geochronological effort as a pyramid of workers with Al at its apex. The Nier pyramid would be all encompassing with respect to geochronologists, if using magnetic sector instruments were the defining category, which is much the case.

Al and Aldrich also turned their attention to the scarce ^{3}He isotope. Their first paper on the subject investigated the difference in the ^{3}He/^{4}He ratio between the atmosphere and samples of well helium where ^{3}He is relatively rare because of pure ^{4}He production by alpha-decay of uranium and thorium and their radioactive decay chains. Helium studies of this kind have become another important subfield of isotopic geochemistry. It was discovered by others in 1969 that excess ^{3}He tags material derived preferentially from the earth's mantle so that isotopic studies in helium have since done much to elucidate mantle-crustal processes. Other helium papers by Al and Aldrich describe methods for enrichment of ^{3}He by thermal diffusion and by cryogenic techniques involving liquid helium. The cryogenic papers resulted from a collaboration between Minnesota and J. G. Daunt, Henry A. Fairbank, and others at Yale. A totally different helium investigation arose in Al's laboratory from the discovery elsewhere that iron meteorites contain highly isotopically anomalous helium as the result of spallation reactions induced in the iron by cosmic ray particles and that, because of shielding, depth effects occur. With John Hoffman, and later with Peter Signer, Al developed a program of investigation of helium and other noble gases produced by cosmic rays in meteorites, studies that contributed a valuable chapter in meteoritics. One can surmise that these were the researches that stimulated Al's interest in space physics, to which he made strong contributions later on, and in the Meteoritical Society, whose meetings became the favorite forum for his experimental studies after "retirement."

MASS MEASUREMENTS OF NUCLIDES (1951-63)

Al embarked on this journey in typical Nier fashion by designing a new instrument, namely a double focussing mass spectrometer as opposed to the double focussing mass spectrographs, which came earlier. (The term double focussing refers to the fact that a combination of electric and magnetic analyzers can be arranged so that both the angular and velocity spreads of ions emerging from a source can be brought simultaneously to focus at the final detector. Only double focussing can provide the very high resolution required if precise mass measurements are to be made. The difference between a mass spectrometer and a mass spectrograph refers to the mode of ion detection employed—photographic plates in the case of the spectrograph versus electrical detection in the spectrometer.) In either case the mass of the "unknown" ion is determined from its separation in a doublet occurring at the same mass number with a standard ion, usually a hydrocarbon fragment. Nier's instrument could display these doublets continuously by electrical means, whereas in the spectrographs a "blind" exposure would have to be made and be followed by removal of a photographic plate from the vacuum system for development and measurement. The Nier instrument was highly sophisticated, employing a second, single-focussing spectrometer tube as a means of regulating and, by ingenious circuitry, sweeping the relevant mass region to record the doublet. The final measurement of M/M was reduced to resistance measurements of R/R for a wire-wound potentiometer. Al developed and used this instrument with T. L. Collins, Walter H. Johnson, Jr., Tom Scolman, Karl Quisenberry, Clayton Giese, and others. Typical of Al's ideas about design, the first double-focussing instrument used a 6-in.-radius magnetic sector similar to those employed in his successful isotope mass spectrometers. The success of the first instrument led to other improved designs and finally to an instrument about 2.5 times larger than the original. With a magnetic radius of 16 in. and an ion path of about 10 feet, this was Al's largest instrument construction. Over the period from 1956 to 1979, this instrument was employed to measure the masses of almost all of the stable nuclides in the periodic table.

It is a remarkable fact that the accepted values for the masses of the stable nuclides (and thus their binding energies) now in the tables of nuclear data are predominately those determined by Al, despite the field having been highly competitive in its heyday. An explanation of this has to do with Nier's dislike and avoidance of extremely large construction projects. His taste ran to tabletop apparatus, where the construction resulted from interaction of the researcher with a few artisans from the departmental shops. In principle the resolving power of a mass spectrometer depends almost wholly on the inverse ratio of a slit width to a characteristic radius of the ion orbits in the machine. Most of Nier's competitors chose then to design and construct instruments of very large radius. The problem was that in the large instruments various effects, which had been of negligible disturbance in the table-top apparatus, proved troublesome as the dimensions were scaled up. So, instead of "surfing" through the isotope tables and defining the energy surface for nuclides with new precision, Al's competitors were struggling to build very large pieces of equipment and to solve the new problems that came with their size. Like all generalizations this explanation for Al's success in the field is an oversimplification, but no one can question his good judgement in choosing experimental approaches.

The community of mass measurers, although competitive, was a friendly one. H. E. Duckworth, one such competitor, spoke at Al's memorial colloquium in 1994 and commented on the friendly cooperation he had enjoyed with Al while they served together on the Commission on Atomic Masses of the International Union of Pure and Applied Physics. Duckworth also mentioned that it was Al and A. Olander from Sweden who independently suggested to J. Mattauch in 1956 that the chemical atomic weight scale based on O = 16 and the physical atomic weight scale based on $^{16}O = 16$ be brought together in a unified scale with ^{12}C as the standard, a change that disturbed the chemical atomic weights very little but solved a myriad of problems that followed from the less appropriate (and different) oxygen standards. The international unions for both physics and chemistry adopted this unified standard in 1960.

UPPER ATMOSPHERIC AND SPACE PHYSICS (1964-85)

Al entered space physics with a bang in 1964. His former student J. H. Hoffman had moved on to the E. O. Hurlburt Center for Space Research at the Naval Research Laboratory and their joint interest in applying their skills in mass spectrometry to problems in upper atmospheric physics led to a collaboration between the two laboratories. The first papers from this partnership appeared from 1964 to 1966. By 1985, as part of the growing involvement of the Minnesota department in space physics (e.g., by the work there of Robert Pepin, Jeff Hayden, and Konrad Mauersberger), more than fifty papers in space physics had appeared with Nier as an author.

Reading the first of these papers and knowing Al's enthusiasm for new research topics, one can easily sense his excitement. He was still using familiar instruments of his own construction, but now he had the novelty of hunching down in a bunker "where the clocks ran backwards and all that," while his mass spectrometers went blasting off into new territory. The first successful flight incorporated two mass spectrometers, one of which burned out a filament and simply went along for the ride, but the other worked perfectly. Reading about it, one shares his delight in how the mass spectrum of the neutral species in the upper atmosphere were modulated by the rolling of the rocket, leading to big signals when the motion of the rocket was "toward the air" and small signals when "away from it." With collaborators he was able to analyze this effect in detail and among other things deduce the temperature of the atmosphere as a function of altitude between 120 and 200 kilometers where the data were recorded.

Al directed much of his efforts in space physics to overcoming the problem that chemically active species, such as atomic oxygen and nitrogen, although present in the upper atmospheres of planets in concentrations that are important to know, interact quickly with surfaces of the instruments for their detection. He made the valuable discovery that in his open-source mass spectrometer, where the Nier ion source is exposed directly to the atmospheric particles that stream into the moving spacecraft, minor changes in the voltages applied to the electrodes enabled the instrument to discriminate strongly in favor of particles that had not impacted with surfaces of the instrument before entering the ionizing region. Thus, again his familiar spectrometers could play an important role in studying the neutral particles in planetary atmospheres. As early as 1973 he was reporting results from flying his mass spectrometers in satellites.

Major participation in the Viking missions to Mars provided the capstone of Nier's work with mass spectrometers in space. From the beginning, with Harvard's M. McElroy, he had responsibility for planning and executing the elemental and isotopic measurements of the Martian atmosphere during the descent of the spacecraft. Some of the most important discoveries of the Mars missions came from this work. Most notable was the discovery that nitrogen in the Martian atmosphere is strongly fractionated, such that the rare heavy isotope ^{15}N is enhanced by 62% with respect to the $^{15}N/^{14}N$ ratio in the earth's atmosphere. Two important lines of research stem from this discovery. In the first place, it is a pivotal fact in understanding the history of the Martian atmosphere that the fractionation almost certainly arose from preferential escape of the light isotope ^{14}N and the datum provides an important boundary condition. Second, the anomalous nitrogen Al found on Mars has been a crucial and clinching piece of evidence that a small subset of meteorites in our collections originated on the Martian surface. D. D. Bogard and P. Johnson at the National Aeronautics and Space Administration's (NASA) Johnson Space Center first saw the possible connection through comparing elemental and isotopic abundances of the noble gases in one such meteorite with values found by Nier in the Martian atmosphere. Later analysis of nitrogen and other gases from the putative Martian meteorites by R. O. Pepin, R. H. Becker, and R. C. Wiens in Nier's own department at Minnesota, together with experiments on shock implantation of ambient gases in suitable materials by Bogard and F. Hörz, and by Wiens, proved the hypothesis of Martian origin.

At the request of high-level management at NASA, Al was a late addition to another of the Viking experimental teams, namely the one responsible for the gas chromatograph mass spectrometer on the Viking landers. This highly complex instrument, which included mass spectrometer detection, could analyze volatile products from heating of Martian soil samples, important in the search for organic substances on the planet. It could also build up measurable samples of inert constituents of the atmosphere by processing multiple "gulps" of the predominant CO_2 there. Al's involvement came about because inspectors recognized serious problems with the instrumentation when little time remained for their correction. Al, who was much experienced in miniaturizing instruments for flight, enlisted A. E. Cameron from the Oak Ridge National Laboratory as a coworker and between these "old hands" at mass spectrometry things were fixed. Al with characteristic modesty downplayed their role. Those close to the matter, however, saw their participation as crucial for the program's final success. The most important science from the lander came from the revised instrument. Al went on from his Viking triumphs to participate in measurements of the atmosphere of Venus.

NOBLE GAS MICROANALYSIS (1983-94)

Retirement from research was never an option with Al. Retired only in other respects, he created the perfect program for his last years in the laboratory. His work in space physics had led him to delight in the construction of miniaturized mass spectrometers with high performance. In his space physics years he would come to NASA meetings carrying what looked like an ordinary attaché case. Ordinary it was not; when opened it proved to be a functional mass spectrometer, which he would turn on and proceed to analyze the room air. How better could he convince NASA officials that his instruments should fly on missions to deep space? A refined laboratory version of this spectrometer was the perfect vehicle for Al's late work with a gifted technician, D. J. Schlutter. Needing only modest levels of financial support, they undertook studies of the noble gases in individual interplanetary dust particles, sometimes called Brownlee particles after Don Brownlee, who pioneered in capturing these stratospheric particles and showing which ones were extraterrestrial. His contributions to this new field of study of extraterrestrial material were so important that a major concern at his death was whether others would continue the research. Fortunately, R. Pepin has continued the work with Schlutter and, in tribute to Al's good judgement in choosing research topics, has seen it explode in scope as various other microsamples, such as small lunar and meteoritic mineral grains, come under scrutiny.

NIER AS A PERSON

A frequent description of Al is, "the most refreshing scientist I have ever met." He had not an ounce of pomposity in his makeup. His curiosity and enthusiasm for scientific work were boundless and unmatched. He was competitive but only in the sense that, once setting himself a scientific goal, he applied his characteristic energy and self-confidence to reach that goal as quickly as possible. Once there, he never overstated his results. His measurements invariably stood up within their quoted uncertainties. He freely described and published how he did things. He considered "instrument papers" just as important as those giving results. He had full respect for the talents of the artisans with whom he worked in developing new tools for research. His joint publications with R. B. Thorness, a master machinist with whom he frequently collaborated, are a case in point. The relationship between Al and "Buddy" Thorness was very fruitful and very deep. The respect and consideration that Al had for the "shop" and the skilled and dedicated individuals who worked there was a symbol of the fruitful old system in which experimental physics was greatly advanced by dedicated staff and universities provided facilities as a matter of course.

Al was an enthusiastic traveler. He developed to a fine art the techniques for traveling "light" to a distant meeting and presenting his results. He usually knew about quick-dry clothing and compact slides and cameras (he was never without one) before the rest of us. His picture taking was a good key to his personality. He was usually the most distinguished scientist in a group, one who might fittingly stand aside for others to "shoot." But it was he who carried the camera, organized the group photo, and passed his camera on to others at the end so that the rest of us would be in a picture with him. One travel trick comes to mind. He would carry a set of preaddressed labels with him for the postcards he intended to send. "You can tell by how many stickers are left," he explained, "whether or not you are doing your job." Enjoyment of travel was not a singular appetite. He enjoyed all aspects of his busy life. Travel was just one of the things he relished, which we necessarily witnessed if we went to the same meeting.

Al was married twice and must have felt pain when he and Ruth, the mother of his children, parted company. He was fortunate in that Ardis shared with him his last twenty-five years of exceptional happiness and contentment. Al was very proud of his children, Janet Marx of Springfield, Virginia, and Keith Nier of Madison, New Jersey, and four grandchildren. Ardis has continued a close relationship with them.

Al was awarded many honors for his work. He received the Arthur L. Day Medal of the Geological Society of America, William Bowie Medal of the American Geophysical Union, Victor Goldschmidt Medal of the Geochemistry Society, and the Field and Franklin Award of the American Chemical Society. He was elected to the National Academy of Sciences, American Philosophical Society, and American Academy of Arts and Sciences. His foreign honors included election as foreign scientific member of the Max-Planck Institute for Chemistry in Germany and the Royal Swedish Academy of Science. He was honored by the Atomic Energy Commission and by NASA for contributions to government science programs. We mentioned earlier his honorary doctorate of science from the University of Minnesota. An honor, which he enjoyed repeatedly (and his friends frequently apprised him of), was to have the clue "American physicist" appear in the *New York Times* crossword puzzle. He modestly dismissed this evidence of fame by pointing out that four-letter words with two vowels are much needed in such puzzles.

In addition to honors awarded to him directly, there are many honors that were given to his scientific "descendents," awardees who frequently acknowledged their debt to the facilities Nier invented and the example he set in their use. The respect and affection of those in the Nier pyramid are his most significant and lasting honors.

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Clair Cameron Patterson

June 2, 1922 - December 5, 1995

By George R. Tilton

Clair Patterson was an energetic, innovative, determined scientist whose pioneering work stretched across an unusual number of sub-disciplines, including archeology, meteorology, oceanography, and environmental science—besides chemistry and geology. He is best known for his determination of the age of the Earth. That was possible only after he had spent some five years establishing methods for the separation and isotopic analysis of lead at microgram and sub-microgram levels. His techniques opened a new field in lead isotope geochemistry for terrestrial as well as for planetary studies. Whereas terrestrial lead isotope data had been based entirely on galena ore samples, isotopes could finally be measured on ordinary igneous rocks and sediments, greatly expanding the utility of the technique.

While subsequently applying the methodology to ocean sediments, he came to the conclusion that the input of lead into the oceans was much greater than the removal of lead to sediments, because human activities were polluting the environment with unprecedented, possibly dangerous, levels of lead. Then followed years of study and debate involving him and other investigators and politicians over control of lead in the environment. In the end, his basic views prevailed, resulting in drastic reductions in the amount of lead entering the environment. Thus, in addition to measuring the age of the Earth and significantly expanding the field of lead isotope geochemistry, Patterson applied his scientific expertise to create a healthier environment for society.

Clair Patterson (known as "Pat" to friends) was born and grew up in Mitchellville, Iowa, near Des Moines. His father, whom he describes as "a contentious intellectual Scot," was a postal worker. His mother was interested in education and served on the school board. A chemistry set, which she gave him at an early age, seems to have started a lifelong attraction to chemistry. He attended a small high school with fewer than 100 students, and later graduated from Grinnell College with an A. B. degree in chemistry. There he met his wife-to-be Lorna McCleary. They moved to the University of Iowa for graduate work, where Pat did an M. A. thesis in molecular spectroscopy.

After graduation in 1944 both Pat and Laurie were sent to Chicago to work on the Manhattan (atomic bomb) Project at the University of Chicago at the invitation of Professor George Glockler, for whom Pat had done his M. A. research. After several months there, he decided to enlist in the army, but the draft board rejected him because of his high security rating and sent him back to the University of Chicago. There it was decided that both Pat and Laurie would go to Oak Ridge, Tennessee, to continue work on the Manhattan Project. At Oak Ridge, Patterson worked in the ^{235}U electromagnetic separation plant and became acquainted with mass spectrometers.

After the war it was natural for him to return to the University of Chicago to continue his education. Laurie obtained a position as research infrared spectroscopist at the Illinois Institute of Technology to support him and their family while he pursued his Ph.D. degree.

In those days a large number of scientists had left various wartime activities and had assembled at the University of Chicago. In geochemistry those scientists included Harold Urey, Willard Libby, Harrison Brown, and Anthony Turkevich. Mark Inghram, a mass spectrometer expert in the physics department, also played a critical role in new isotope work that would create new dimensions in geochemistry. The university had created a truly exciting intellectual environment, which probably few, possibly none, of the graduate students recognized at the time.

Harrison Brown had become interested in meteorites, and started a program to measure trace element abundances by the new analytical techniques that were developed during the war years. The meteorite data would serve to define elemental abundances in the solar system, which, among other applications, could be used to develop models for the formation of the elements.

The first project with Edward Goldberg, measuring gallium in iron meteorites by neutron activation, was already well along when Patterson and I came on board. The plan was for Patterson to measure the isotopic composition and concentration of small quantities of lead by developing new mass spectrometric techniques, while I was to measure uranium by alpha counting. (I finally also ended up using the mass spectrometer with isotope dilution instead of alpha counting.) In part, our projects would attempt to verify several trace element abundances then prevalent in the meteorite literature which appeared (and turned out to be) erroneous, but Harrison also had the idea that lead isotope data from iron meteorites might reveal the isotopic composition of lead when the solar system first formed. He reasoned that the uranium concentrations in iron meteorites would probably be negligible compared to lead concentrations, so that the initial lead isotope ratios would be preserved. That was the goal when Patterson began his dissertation project, however attaining it was to take considerably longer than we imagined at the time.

Patterson started lead measurements in 1948 in a very dusty laboratory in Kent Hall, one of the oldest buildings on campus. In retrospect it was an extremely unfavorable environment for lead work. None of the modern techniques, such as laminar flow filtered air, sub-boiling distillation of liquid reagents, and Teflon containers were available in those days. In spite of those handicaps, Patterson was able to attain processing blanks of circa 0.1 microgram, a very impressive achievement at the time, but now approximately equal to the total amount of sample lead commonly used for isotope analyses.

His dissertation in 1951 did not report lead analyses from meteorites; instead it gave lead isotopic compositions for minerals separated from a billion-year-old Precambrian granite. On a visit to the U.S. Geological Survey in Washington D.C., Brown had met Esper S. Larsen, Jr., who was working on a method for dating zircon in granitic rocks by an alpha-lead method. Alpha counting was used as a measure of the uranium and thorium content; lead, which was assumed to be entirely radiogenic (produced by the decay of uranium and thorium), was determined by emission spectroscopy. Despite several obvious disadvantages, the method seemed to give reasonable dates on many rocks. Brown saw that the work of Patterson and me would eliminate those problems, so we arranged to study one of Larsen's rocks. We finally obtained lead and uranium data on all of the major, and several of the accessory, minerals from the rock. Particularly important was the highly radiogenic lead found in zircon, which showed that a common accessory mineral in granites could be used for measuring accurate ages. As it happened, the zircon yielded nearly concordant uranium-lead ages, although that did not turn out later to be true for all zircons. In any case, that promising start opened up a new field of dating for geologists, and has led to hundreds of age determinations on zircon.

In parallel with the lead work, Patterson participated in an experiment to determine the branching ratio for the decay of ^{40}K to ^{40}Ar and ^{40}Ca. Although the decay constant for beta decay to ^{40}Ca was well established, there was much uncertainty in the constant for decay to ^{40}Ar by K electron capture. This led Mark Inghram and Harrison Brown to plan a cooperative study to measure the branching ratio by determining the radiogenic ^{40}Ar and ^{40}Ca in a 100-million-year-old KCl crystal (sylvite). The Inghram group would measure ^{40}Ar while Patterson and Brown would measure ^{40}Ca. They reported a value that came within circa 4% of the finally accepted value.

After graduation, Patterson stayed on with Brown at Chicago in a postdoctoral role to continue the quest toward their still unmet meteorite age goal. He obtained much cleaner laboratory facilities in the new Institute for Nuclear Studies building, where he worked on improvement of analytical techniques. However, after a year this was interrupted when Brown accepted a faculty appointment at the California Institute of Technology. Patterson accompanied him there and built facilities that set new standards for low-level lead work. By 1953 he was finally able to carry out the definitive study, using the troilite (sulfide) phase of the Canyon Diablo iron meteorite to measure the isotopic composition of primordial lead, from which he determined an age for the Earth. The chemical separation was done at CalTech, and the mass spectrometer measurements were still made at the University of Chicago in Mark Inghram's laboratory. Harrison Brown's suspicion was finally confirmed! The answer turned out to be 4.5 billion years, later refined to 4.55 billion years. The new age was substantially older than the commonly quoted age of 3.3 billion years, which was based on tenuous modeling of terrestrial lead evolution from galena deposits.

Patterson's reactions on being the first person to know the age of the Earth are interesting and worthy of note. He wrote,[1]

> True scientific discovery renders the brain incapable at such moments of shouting vigorously to the world "Look at what I've done! Now I will reap the benefits of recognition and wealth." Instead such discovery instinctively forces the brain to thunder "*We* did it" in a voice no one else can hear, within its sacred, but lonely, chapel of scientific thought.

There "we" refers to what Patterson calls "the generations-old community of scientific minds." From my observations, he lived that ethic. To him it must have been an exercise in improving the state of the "community of scientific minds." His attitude recalls the remark of Newton: "If I have seen farther than others, it is because I have stood on the shoulders of giants."

The age that Patterson derived has stood the test of time, and is still the quoted value forty-four years later. In the meantime, there have been small changes in the accepted values for the uranium decay constants, improvements in chemical and mass spectrometric techniques, and a better understanding of the physical processes taking place in the early solar system and Earth formation, but these have not substantially changed the age Patterson first gave to us. Some textbooks have given diagrams showing that the logarithm of the supposed age of the Earth plotted against the year in which the ages appeared approximated a straight line, but Patterson's work has finally capped that trend.

Patterson next focused on dating meteorites directly instead of inferring their ages from the Canyon Diablo troilite initial lead ratios. He did this by measuring lead isotope ratios in two stone meteorites with spherical chondrules (chondrites) and a second stone without chondrules (achondrite).

A colleague, Leon Silver, had recommended the achondrite because of its freshness and evolved petrologic appearance. Coupled with the iron meteorite troilite lead, the complete data yielded a $^{207}Pb/^{206}Pb$ age of 4.55 $\pm$ 0.07 billion years. The achondrite data were especially important because the Pb ratios in the two chondrites were close to those of modern terrestrial lead, raising questions about possible Earth contamination, but the exceptionally high uranium/lead and thorium/lead ratios in the Nuevo Laredo achondrite produced lead with isotope ratios that were unlike any isotopic compositions that have ever been found in terrestrial rocks. They also fit the 4.55 Ga age, which removed any doubts about major errors in the date.

The meteorite work led indirectly to his second major scientific accomplishment. The new ability to isolate microgram quantities of lead from ordinary rocks and determine its isotopic composition had opened for the first time the path for measuring lead isotopes in common geological samples, such as granites, basalts, and sediments. That led him to start lead isotope tracer studies as a tool for unraveling the geochemical evolution of the Earth. As part of that project he set out to obtain better data for the isotopic composition of "modern terrestrial lead" by measuring the isotopic composition of lead in ocean sediments. By 1962 Tsaihwa J. Chow and Patterson reported the first results in an encyclopedic publication that initiated Patterson's concern with anthropogenic lead pollution, which was to occupy much of his attention for the remainder of his scientific career.

The isotope data revealed interesting patterns for Atlantic and Pacific Ocean leads that could be related to the differences in the ages and compositions of the landmasses draining into those oceans. However, in studying the balance between input and removal of lead in the oceans, the authors calculated that the amount of anthropogenic lead presently dispersed into the environment each year was circa eighty times the rate of deposit into ocean sediments. Thus, the geochemical cycle for lead appeared to be badly out of balance. The authors noted that their calculations were provisional; the analytical data were scarce or of poor precision in many cases, however this was the seminal study that started Patterson's investigations into the lead pollution problem.

The limitations in the analytical data on which many of the conclusions in the 1962 paper were based led Patterson to start new investigations to attack the problem. In 1963 he published a report with Mitsunobu Tatsumoto showing that deep ocean water contained 3 to 10 times less lead than surface water, the reverse of the trend for most elements (e.g., barium). This provided new evidence for disturbance in the balance of the natural geochemical cycle for lead by anthropogenic lead input.

In the 1965 paper entitled "Contaminated and Natural Lead Environments of Man,"[2] Patterson made his first attempt to dispel the then prevailing view that industrial lead had increased environmental lead levels by no more than a factor of approximately two over natural levels. He maintained that the belief arose from the poor quality of lead analyses in prehistoric comparison samples in which much of the lead reported was actually due to underestimation of blank contamination. He compiled the amounts of industrial lead entering the environment from gasoline, solder, paint, and pesticides and showed that they involved very substantial quantities of lead compared to the expected natural flux. He estimated the lead concentration in blood for many Americans to be over 100 times that of the natural level, and within about a factor of two of the accepted limit for symptoms of lead poisoning to occur.

R. A. Kehoe, a recognized expert on industrial toxicol ogy[3] accused him of being more of a zealot than a scientist in the warnings he had raised.[4] Another leading toxicologist had just returned from a World Health Organization conference where fifteen nations had agreed that environmental lead contributions to the body burden had not changed in any significant way, either in blood or urinary lead contents, over the last two decades. He called Patterson's conclusions "rabble rousing."[5]

Patterson's reactions are recorded in a letter to editor Katharine Boucot accompanying the revised manuscript:

> The enclosed manuscript does not constitute basic research and it lies within a field that is outside of my interests. This is not a welcome activity to a physical scientist whose interests are inclined to basic research. My efforts have been directed to this matter for the greater part of a year with reluctance and to the detriment of research in geochemistry. In the end they have been greeted with derisive and scornful insults from toxicologists, sanitary engineers and public health officials because their traditional views are challenged. It is a relief to know that this phase of the work is ended and the time will soon come when my participation in this trying situation will stop.[6]

Patterson's participation did not stop; instead on October 27, 1965, he wrote to California Governor Pat Brown restating the points from his 1965 review and emphasizing the dangerously high levels of lead in aerosols, particularly in the Los Angeles area. In it he claimed that the California Department of Public Health was not doing all it should to protect the population from the dangers of lead poisoning. His first request drew a polite rejection. A second letter on March 24, 1966, had better success, perhaps because of a letter from a high state official.[7] On July 6, 1966, Governor Brown signed a bill directing the State Department of Public Health to hold hearings and to establish air quality standards for California by February 1, 1967. Although that deadline was not met, Patterson clearly played a role in advancing concern over California air control standards.

He had simultaneously started parallel actions at the national level as well. On October 7, 1965, he sent a communication similar to the Brown letter to Senator Muskie, chairman of the Subcommittee on Air and Water Pollution. In it he offered to appear before the committee. He was subsequently invited to a hearing held on June 15, 1966, in Washington. There Patterson emphasized that most officials failed to understand the difference between "natural" and "normal" lead body burdens, the former based on incorrect data from pre-industrial humans, the latter on averages in modern populations. In support of that assertion he cited his newer work in Greenland showing the large increases in lead in snow starting with the industrial revolution. He furthermore believed it was wrong for public health agencies to work so closely with lead industries, whom he considered often biased in matters concerning public health.

His views drew support from some of the public (e.g., Ralph Nader), but were once again strongly opposed by others, notably by R. A. Kehoe, the highly regarded authority on industrial poisoning. A battle line was drawn that was to last about two decades.

By 1970 Patterson and his colleagues had completed studies of snow strata from Greenland and Antarctica that showed clearly the increase in atmospheric lead beginning with the industrial revolution in both regions. Modern Greenland snow contained over 100 times the amount of lead in preindustrial snow, with most of the increase occurring over the last 100 years. The effect was about ten times smaller in Antarctic snow, but it was clearly observable. Later work with improved blanks reduced that figure to two.

In 1971 the National Research Council released a report entitled "Airborne Lead in Perspective" to guide the Environmental

Protection Agency's policies on lead pollution. The panel was widely accused of not being forceful enough in interpreting its data and being too heavily weighted toward industrial scientists. [8] Patterson's work was largely ignored, however by December 1973 the EPA did announce a program to reduce lead in gasoline by 60-65% in phased steps. Thus was the beginning of the removal of lead from gasoline.

Meanwhile Patterson continued to work on the lead problem from another perspective by measuring lead, barium, and calcium concentrations in bones from 1600-year-old Peruvian skeletons.[9] The results indicated a 700- to 1200-fold increase in concentrations of lead in modern man, with no change in barium, a good staind-in for lead, and calcium. In a letter Patterson once said, "I have a passionate interest in this paper."[10]

In the late 1970s Patterson turned his attention to lead in food. In 1979 he wrote to the commissioner of food and drugs at the Environmental Protection Agency asserting that "your headquarters laboratory cannot correctly analyze for lead in tuna fish muscle."[11] He maintained that the laboratory blanks were too high to permit accurate analyses for lead concentrations below 1 ppm. When asked if he could cite other laboratories that agreed with his results, Patterson responded that scientific matters are not decided by majority vote.[12] That contact finally led to his participation in a symposium on analytical methods of analyzing for lead in food at the sub-1 ppm level, held October 10, 1981, in Washington. It was attended by both EPA and Bureau of Foods representatives. Patterson made three recommendations for improvements that seem to have been taken seriously.[13] These were (1) to use Bureau of Standards mass spectrometers to permit mass spectrometric lead analyses; (2) to equip EPA field laboratories better; and (3) to promote more contacts between EPA and academic laboratories. A few months later Patterson wrote that he believed the analytical work being done at the headquarters EPA laboratory met his standards.[14]

In 1980 Dorothy M. Settle and Patterson[15] published a warning on the amount of lead entering the food chain due to lead solder used in sealing cans. Although the National Marine Services laboratories had reported only twice as much lead in canned albacore muscle as in fresh tuna (700 versus 400 nanograms per gram), the authors found 0.3 nanogram per gram of lead in fresh and 1400 nanograms/gram in canned muscle. Barium varied by only a factor of two in the samples. A sample of fresh muscle prepared at CalTech and analyzed at the fisheries laboratory gave 20 nanograms per gram for lead, still much higher than the CalTech value. By 1993 lead solder was removed from all food containers in the United States. Patterson's influence is again clearly evident.

Although he was excluded from the earlier 1971 National Research Council panel that produced the report on airborne lead, in 1978 Patterson was appointed to a new twelve-member NRC panel to evaluate the state of knowledge about environmental issues related to lead poisoning. The panel report[16] is noted for containing majority and minority evaluations. The majority report cites the need to reduce lead hazards for urban children; notes that the margin between toxic and typical levels for lead in adults needs better definition; and concedes that typical atmospheric lead concentrations are 10 to 100 times the natural backgrounds for average populations and 1,000 to 10,000 times greater for urban populations. The report asks for further research on these subjects, as well as on relationships between lead ingestion and intellectual ability. The need for improved analytical work was emphasized.

In his lengthy 78-page minority report Patterson argued that the majority report was not forceful enough. Basically he said that the dangers of the prevalent practices were already clearly enough defined and that efforts should start immediately to drastically reduce or completely remove industrial lead from the everyday environment. That included gasoline, food containers, foils, paint, and glazes. He also cited water distribution systems. He urged "investigations into biochemical perturbations within cells caused by lead exposures ranging down from typical to 1/1000 of typical." He had long criticized assigning a sharp limit for lead in air or blood to denote a dividing line between poisonous and non-poisonous levels.

The above items give some, but by no means a complete, indication of the efforts Patterson devoted toward reducing the environmental lead burden. Many others joined the campaign with the passage of time, but he was clearly a principal player, and could be said to have initiated some of the changes that have occurred. Around 1973 lead began to be reduced in gasoline; it was removed completely in 1987. Lead solder has been removed from U. S. food containers a well as from paints and water lines. By 1991 scientists could report that the lead content of Greenland snow had fallen by a factor of 7.5 since 1971.[17]

Patterson will be remembered for having first discovered the differences between "natural" and "common" or "typical" lead abundances in the human population, and for arguing that point until it was universally accepted. That in turn has stimulated considerable medical research to study the effects of lead at below the toxic poisoning level on the human learning ability.[18]

Beginning in the early 1980s, Patterson's interests began to turn toward what I call the third stage of his intellectual career. It involved an introspective, philosophical evaluation of the place of man (H. s. sapiens, as he often stated it) in society. He distinguished between what he termed the engineering versus the scientific modes of thinking. His thoughts are best spelled out in the two articles in the 1994 special issue of *Geochimica et Cosmochimica Acta* in his honor. He sees the scientific mind as the inquiring mind that seeks to uncover the world's secrets, while the engineering mind seeks to control the natural world. This undoubtedly grew out of his experience as a scientist in discovering the age of the Earth, while the engineering mind would be equated with the technology that utilized the large amounts of lead that had polluted the environment. Thus he says,[19] "Most persons cannot see the ills of a culture constructed by 10,000 years of perverted utilitarian rationalizations because they perceive only its material technological forms through the eyes of a diseased *Homo sapiens sapiens* mind." At the end he was working on a book to express his ideas on those and other matters, such as population control. We will never know what it might have contained, but we can guess that it would have been a stimulating, unique, and undoubtedly controversial treatment.

As a person, Patterson was modest about his own accomplishments and generous in acknowledging the contributions of colleagues, especially those of his co-workers. He opened his laboratory to scientists from around the world and trained them in the techniques he had developed. He was self-assured in science and not one to follow the beaten path. Although he was very sensitive to the negative criticisms his work generated, he pursued his beliefs vigorously with what some would (and some did) call a fanatical drive. Perhaps any lesser degree of motivation would have led him to give up the struggle without seeing it through to the finish. He cared deeply about the welfare of society and applied his scientific knowledge toward seeking and making a better future for all. His final efforts on the book he hoped to write were directed toward that goal. His unique personality has been eloquently portrayed in the Saul Bellow novel *The Dean's December,* in which Patterson is the model for Sam Beech. [20] He was truly a one-of-a-kind person.

Patterson's many accomplishments were recognized in 1995 by the award of the Tyler Prize for Environmental Achievement, a most fitting reward for his prolonged efforts on behalf of the environment, the Goldschmidt Medal of the Geochemical Society in 1980, and the J. Lawrence Smith Medal of the National Academy of Sciences in 1973. He was elected to the National Academy of Sciences in 1987, and received honorary doctorates from Grinnell College in 1973 and the University of Paris in 1975, as well as the Professional Achievement Award from the University of Chicago in 1983. An asteroid (2511) and a peak in the Queen Maude Mountains, Antarctica, are named for him.

He is survived by his wife Lorna Jean McCleary Patterson, who resides at The Sea Ranch, California, and children Cameroon Clair Patterson, Claire Mai Keister, Charles Warner Patterson, and Susan McCleary Patterson.

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NOTES

1. Historical changes in integrity and worth of scientific knowledge. *Geochim. Cosmochim. Acta* 58(1994):3141.

2. Contaminated and natural environments of man. *Arch. Environ. Health* 11 (1965):344-60.

3. As an employee of the Ethyl Corporation Kehoe discovered that deaths among workers manufacturing lead tetraethyl in the early 1920s were due to absorption of lead through the skin and

then he developed procedures to protect them from lead poisoning. His studies saved the Ethyl Corporation from having to discontinue production of "Ethyl Fluid."

4. R. A. Kehoe. *Arch. Environ. Health* 11(1965):736-39.

5. Letter of Herbert Stockinger dated October 20, 1965, to Katharine Boucot, the editor who handled Patterson's paper. Patterson file, California Institute of Technology archives.

6. Letter to Katharine Boucot dated February 2, 1965. Patterson file, California Institute of Technology archives.

7. Letter of Rudd (Mrs. Harrison) Brown, chief, Division of Recreation, Calif. Department of Parks and Recreation, to Winslow Christian, executive secretary to the governor, dated May 3, 1966. Brown stated that Patterson was one of her husband's graduate students at the University of Chicago, was a valued staff member at CalTech, and that "Patterson is not a nut." Patterson file, California Institute of Technology archives.

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9. J. E. Ericson, H. Shirahata, and C. C. Patterson. Skeletal concentrations of lead in ancient Peruvians, *N. Engl. J. Med.* 300(1975):949-51.

10. Letter to Harrison Brown dated October 31, 1985. California Institute of Technology archives.

11. Letter to Jere E. Goyan dated December 5, 1979. California Institute of Technology archives.

12. Undated letter to Goyan in Patterson file. California Institute of Technology archives.

13. Statement by Kenneth Boyer in Patterson file. California Institute of Technology archives.

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Berta Scharrer

Berta V. Scharrer

December 1, 1906 -July 23, 1995

By Dominick P. Purpura

Berta Scharrer, co-founder of the discipline that has come to be known as neuroendocrinology, was an exemplar of science and conscience. Her long and prolific life, bracketed by but a few years on either side of the twentieth century, is an inspiring record of scientific achievement. It is also an historical record of the triumphs and horrors and the social and political progress and perversions of this most extraordinary period.

Throughout Scharrer's sixty-five-year scientific career, she faced resistance to her unconventional ideas, prejudice against women, and personal tragedy, along with a few jackbooted thugs posing as politicians. Yet, she persevered in her boundless quest for knowledge, advancing the bold new concept of neurosecretion and reinventing her career many times along the way. Berta's strength, dedication, and enthusiasm touched many during her peripatetic journey through life and lives on in the colleagues, students, and other friends she has left behind.

EARLY LIFE

Berta Vogel Scharrer was born on December 1, 1906, in Munich, Germany, to Johanna Weiss Vogel and Karl Philip Vogel, a prosperous judge who served as vice-president of the federal court of Bavaria. Her childhood with her three siblings was happy—filled with music, art, and all the cultural charms of Munich. Berta received an excellent early education at gymnasium, where she developed an interest in biology that would guide her life's work. As a young girl she set her sights on becoming a research scientist, although she knew the chances for an academic career in biology were slim then for a person who happened to be born with two X chromosomes.

In pursuit of her goal Berta attended the University of Munich, where she became interested in the work of the bee behavioral biologist Professor Karl von Frisch, whose own long life was punctuated with the 1973 Nobel Prize for physiology or medicine. She joined his laboratory for her doctoral work, a comparison of the taste and nutritional quality of various sugars for the honeybee. Berta Vogel received her Ph.D. in 1930 and published her thesis and associated research papers in 1931.

Berta's choice of dissertation laboratory was fortunate, and not simply because Professor von Frisch made a good advisor; it was here that the seeds for her future career in the field of neurosecretion were sown, in the form of her acquaintance with another of von Frisch's students, Ernst Scharrer. These two young scientists soon began a partnership, both professional and personal. From the time of Berta's graduation until Ernst's death in 1965, they would be a team, in research as well as life.

Berta and Ernst's first move together was to the Research Institute of Psychiatry in Munich, headed by Walter Spielmeyer. There Berta took up bacteriology briefly, studying spirochetes and brain infections in birds and amphibians, while Ernst completed an additional degree in medicine. Berta hedged her bets against the prevailing prejudices toward women in research and studied for a certificate to teach at gymnasium during this period, but her heart remained set on a life in science.

BIRTH OF THE NEUROSECRETION HYPOTHESIS

The year 1934 should have been the beginning of halcyon days for Berta. She and Ernst moved to the University of Frankfurt, where Ernst had been appointed director of the Edinger Institute for Brain Research. With this job and a small stipend from Berta's uncle to support them, Berta and Ernst wed. Berta herself worked without pay. Rules against nepotism were strongly enforced at this time, and Ernst took them quite seriously. While Berta did obtain research space at the university, she was given neither academic title nor salary.

This move foreshadowed many years of research and teaching without formal recognition or recompense, but Berta was philosophical about the decision: "An academic career at that time did not look promising at all for a woman. I must say that I could not have done what I have if I had not been married to a biologist who gave me a chance to do my work."[1] Unfortunately, that work would slowly fall into the shadows of the rising influence of Adolf Hitler and the Nazi party.

In 1928, during his thesis studies in von Frisch's lab, Ernst Scharrer had discovered secretory droplets in certain hypothalamic neurons of the European minnow *Phoxinus*. He termed these neurons "nerve-gland cells," and he proposed that they were capable of secreting substances in the same manner that endocrine cells secrete hormones. In Frankfurt, both Scharrers took up further investigation of this controversial hypothesis.

At that time, conventional wisdom held that nerve function was purely an electrical phenomenon. Few scientists were willing to accept that nerve cells secreted any substance, much less hormones. "Remember, synaptic chemical transmission wasn't even known at the time of our first neurosecretion publications. The idea that neurons may be capable of dispatching neurohormonal, or blood-borne signals, an activity previously associated only with endocrine cells, met with powerful resistance."

To bolster Ernst's initial observation in fish, the Scharrers began seeking nerve-gland cells in other organisms. They decided from the start on a comparative approach; Berta would explore the invertebrate animals, while Ernst focused on vertebrates. This strategy proved prescient. Not only did the invertebrate world offer rich diversity, but it revealed parallels with vertebrate biology that would cement and extend the concept of neurosecretion and many years later would inform Berta's novel ideas on the evolution of the neuroendocrine system.

Berta rapidly established the wide distribution of nerve-gland cells in invertebrates. Her classic first papers on the topic, in which she described neurosecretory cells in the Opisthobranch snail *Aplysia* and in the Polychaete worm *Nereis,* appeared in 1935 and 1936. This comparative evidence was essential, as it proved that Ernst Scharrer's initial observation of secretory neurons in fish was not simply an artifact. Still, the theory of neurosecretion remained tenuous; it was based solely on cytological evidence of secretory granules.

During these earliest years of their collaboration, Berta and Ernst Scharrer worked in isolation, outside the mainstream scientific community. "I think it was vital that my husband and I had each other to talk to. We could reassure each other that what we were doing had a future. I don't know how long a single mind could have stuck with the same topic hoping for success." The resistance of their scientific colleagues was to become the least of their problems.

Initially, the Scharrers' time at the Edinger Institute had been a happy and exciting one. They had made lasting friends of individuals like Wolfgang Bargmann and Albrecht Bethe. They summered at the renowned Zoological Station in Naples, collecting scientific samples from the Mediterranean and enjoying the company of the many biologists who flocked there each year. Their research was productive, and political concerns seemed distant. "Because it was a privately endowed research facility not closely connected with university affairs, we thought we would have a chance to work for a few years in a somewhat sheltered situation. But this outlook . . . did not last very long."

On April 1, 1933, the *Reichstag*—Hermann Göring presiding—passed the infamous "Law for the Restoration of the German Civil Service," which mandated the dismissal of all civil servants, including university professors who were defined as Jews under the law. Some saw the resulting vacancies as opportunities; Berta and her husband soon found them intolerable. By 1937 the academic environment had been reduced to parody. "Some professors went into the classroom . . . in full uniform. There were obligatory programs to attend, indoctrination meetings. . . . " The Scharrers experienced increasing pressure to join Nazi organizations and to shun Jewish colleagues. "What the two of us were particularly opposed to was this senseless and immoral philosophy of the Nazis, the idea of racial superiority, anti-Semitism, genocide. . . . We decided that it was impossible for us to be part of this system any longer."

The Scylla of remaining, however, was no less dangerous than was the Charybdis of leaving. They would have to adopt a certain cloak-and-dagger approach to yet another strategic bit of teamwork. As a physician, Ernst was a valuable commodity in a country preparing for war. The springboard for exodus was a one-year Rockefeller fellowship for Ernst at the University of Chicago for which the Scharrers duped the authorities, pretending that they would be returning to Germany. In 1937 Berta and Ernst Scharrer left their full life, their friends, and all of their research materials. They came to the United States with nothing but two suitcases, the four dollars each they were permitted to carry out of Germany, and a united clear conscience.

STARTING OVER

Berta Scharrer thus once again started her research from scratch. Even while fleeing, however, the Scharrers began to rebuild their work on neurosecretion—their eastward passage allowed for stops in Africa, the Philippines, and Japan to collect animals for study. At Chicago, Berta managed to obtain a small laboratory space, and although she was again unsalaried, she was able to resume her studies of neurosecretory cells. With little space and no money to buy supplies, her options for experimental animals were limited. She initially worked with *Drosophila,* but soon, with the help of a friendly custodian, she discovered cockroaches in the basement of her building. She trapped these and studied them, setting the precedent for the extensive work with roaches that lasted into her eighties.

The year at Chicago was difficult. She knew very little English, and visa nuisances required time-consuming travel to Cuba and Switzerland. Yet she managed to publish two papers her first year in the United States, both in English, with help from a retired professor who edited her manuscripts.

In 1938 Ernst took a position as a visiting investigator in the laboratory of Herbert Gasser at the Rockefeller Institute for Medical Research (now Rockefeller University) in New York City. Again, Berta acquired limited lab space and the unpaid position of research associate. During the next few years, Berta described neurosecretory cells in her cockroaches and other arthropods, adding to the ever-expanding menagerie of animals that supported the Scharrers' ideas. And during this time, as the world was plunged into the winter of Hitler's discontent, the sun of reason slowly began to thaw resistance to neurosecretion. At Rockefeller, the Scharrers prepared a landmark paper that they presented in 1940 at a meeting of the Association for Research in Nervous and Mental Diseases. This was the U.S. scientific community's first introduction to the new field of neurosecretion, and the paper was respectfully received.

PHYSIOLOGICAL RELEVANCE OF NEUROSECRETION

Near the end of her stay at Rockefeller, Berta discovered the animal that would eventually help her demonstrate the function and importance of insect nerve-gland cells: the woodroach. Soon after her arrival in New York, she had inquired as to the availability of cockroaches to replenish her stock. For the first time in recorded history, the city had none to offer—at least not in the basement of her building. But in 1940 Berta happened upon some unusual specimens in a shipment of monkeys from South America. These were *Leucophaea maderae,* or woodroaches, and they were particularly well suited to Berta's research. They were larger than American roaches, being about two inches long, and obligingly slower. Furthermore, their brains and nervous systems were amenable to the microsurgery that Berta would soon undertake to demonstrate the importance of neurosecretory bodies.

Berta took these roaches along to her next destination: Western Reserve University (now Case Western Reserve University) in Cleveland, Ohio. Normand Hoerr, an old friend from Chicago and the new chair of the Department of Anatomy, had offered Ernst a position as assistant professor.

From 1940 to 1946, Berta served as instructor and fellow, teaching in the histology laboratory and conducting research—again with no salary.

The Scharrers' life in Ohio was modest. They lived in a small apartment near the school and often worked late into the night alongside their students. Their wanderlust, however, continued to reveal itself. They continued to spend summers on the east coast, at the Marine Biological Laboratory at Woods Hole; they also enjoyed trips to Lake Erie for collection of fish samples, and to Colorado, where they rode horseback and studied English. In 1945 both Berta and Ernst Scharrer became U.S. citizens.

During this time the Scharrers established their lifelong friendship with the anatomist Sanford Palay, then just starting his medical education. Palay has described the prejudices that Berta still faced as a woman scientist, even fifteen years into her career. Initially, she was not even allowed to attend department seminars; this decision was not reversed until she agreed to prepare tea for the faculty. Certain members of the faculty, illustrating the depths of irrationality to which even men of science can fall, blamed Berta for every cockroach they encountered in the hallway, despite the fact that these were roaches of the native variety and clearly not the giant South American species that Berta used in her research.

Berta had established a breeding colony of these woodroaches in her office at Western Reserve and soon set out to elucidate the function of several neuroglandular bodies of the head, including especially the corpus allatum and the corpus cardiacum. Using the microsurgical techniques of cell ablation and nerve section, Berta steadily built up a picture of neurosecretory function in roaches. She found that removal of the corpus allatum had severe effects on the development of the roach due to a resulting hormonal imbalance, nymphs underwent metamorphosis prematurely. Removal of the corpus allatum and the hormones it produced also affected egg development in females. She then went on to show that removal of an associated brain gland, the corpus cardiacum, had no observable effect, for reasons that were quite germane to the neurosecretory hypothesis.

Although the corpus cardiacum did indeed contain important hormones, it served only as a storage depot and transfer station. The hormones were shipped to this body by neurosecretory cells. These neurons continued pumping out hormones even in the absence of the storage point; the sustained flow ensured proper development of eggs and nymphs in animals in which the corpus cardiacum had been removed. Berta was able to demonstrate further that neurosecretory cells conveyed their output of secretory granules to the corpus cardiacum by transport down the nerve axon. These studies provided definitive evidence of the production, transport, and secretion of essential hormones by cells of the nervous system of insects.

Another interesting discovery from this time was the induction of distant tumors by the removal of the corpus allatum and corpus cardiacum. This effect was due not to hormone deficiency, but rather to the unavoidable severing of a nerve during the surgeries. Berta showed that section of the recurrent nerve in roaches led to tumor growth in the stomach and other organs controlled by this nerve, providing a novel insight into potential mechanisms of cancer.

As hoped, Berta's work on invertebrates was complementing Ernst's work marvelously. There were numerous interesting parallels between the corpus allatum-corpus cardiacum system of the insect and the hypothalamic-hypophyseal system of the vertebrates. Indeed, there was growing evidence that the neurosecretory cells of the hypothalamus delivered hormones along nerves to storage points near the brain; Ernst and Berta rightly suspected that one such storage point was the posterior lobe of the pituitary gland. This insight might not have been reached so quickly had it not been for the comparative approach undertaken by the Scharrers.

During the time of their important studies establishing the physiological importance of neurosecretion, Berta and Ernst moved yet again. In 1946 Ernst became associate professor in the University of Colorado Medical School in Denver. Still an unsalaried instructor, Berta had to look beyond her school to finally receive some financial support and some recognition of her accomplishments and international reputation. She first won a prestigious Guggenheim fellowship for the 1947-48 year and then a special fellowship from the U.S. Public Health Service.

Berta finally received her first academic title (but still no salary) in 1950, twenty years after she received her doctoral degree. This acknowledgment came in a roundabout way; she was asked to organize an international meeting in Paris, and had nothing to offer when asked her academic rank. "I felt a little embarrassed for our school and went to the dean. He said he would grant me the listing in the bulletin as assistant professor (research) unsalaried. It seems unusual, but it was what happened at the time."

This was an especially happy and active time for the Scharrers. They loved horseback riding and skiing in Colorado's mountains. They bought a van and learned to drive so they could make expeditions together. As in their research, they divided the labor. "We had an arrangement. I drove up to the mountains, and he drove down."

This was also a productive time. Berta completed the groundbreaking work she had begun in Ohio demonstrating the physiology of the neurosecretory system in insects. Others began advancing the field of neurosecretion as well.

The Scharrer's friend Wolfgang Bargmann and his colleagues learned to stain secretory granules in mammals, enabling new views of the structure of the neurosecretory cell. These studies revealed the architecture of the hypothalamus and the hypophysis, their relationship with one another, and the path taken by secreted granules.

Suddenly, secretion by neurons gained wide acceptance, endorsing the foundation erected by the Scharrers. By the early 1950s the concept of chemical transmission of nerve impulse at the synapse was also widely accepted; neurosecretion became one of the central tenets of nervous system function and the founding principle of the new field of neuroendocrinology.

In 1953 Berta and Ernst visited the site of some of their earliest work, the Zoological Station in Naples. The occasion was the First International Symposium on Neurosecretion, a meeting that put the final stamp of approval on the once-controversial field founded by the Scharrers.

ON TO EINSTEIN

The Scharrers' peregrinations brought them to their final, most important academic destination in 1955. At the newly founded Albert Einstein College of Medicine of Yeshiva University, Berta Scharrer at last came into her own. Ernst had been offered the founding chairmanship of the Department of Anatomy by the dean of the school, Marcus Kogel. "Dean Kogel invited us for an interview. And, to my surprise, without anything being requested on my part, he offered me a full professorship. I finally caught up. By that time I had acquired . . . a certain reputation of my own. He said, 'I know about the nepotism rule, but we are an entirely new school. We can do something a little progressive.'"

And so the Scharrers set off across the United States on a train, bringing crates of noisy woodroaches to their new home in the Bronx. Berta joined her husband's new department as a founding member of the faculty. "[Ernst and I] worked very well together. I worked for half my salary throughout the lifetime of my husband. It was a young school and it needed money, and we felt we were happy with our new appointments. . . . There was only enthusiasm and satisfaction that first year." Berta left aside her own research for several years to develop a course in histology, of which she was course leader. She also advised the medical school's first classes of students, who found her to be a thoughtful and approachable mentor.

The next decade passed with quiet accomplishment. The college, the anatomy department, and the field of neuroendocrinology all matured and flourished. The Scharrers built a small house an easy walk from Einstein and settled in. Together in 1960 they delivered a series of Jesup lectures at Columbia, summarizing the state of neurosecretion research and their comparative studies in vertebrates and invertebrates. These lectures served as the basis for their classic 1963 book *Neuroendocrinology,* which became one of the premier texts in the rapidly expanding field.

In April 1965 the Scharrers went to Miami for the annual anatomy meetings. When Berta returned a few days later, she was alone. During a short and fateful vacation after the meeting, she and Ernst had gone swimming in the Atlantic. A strong undertow swept away Berta's husband and scientific partner of thirty years, and nearly claimed her own life.

Once again Berta was forced to reinvent her career, this time as a solo researcher. "There was a very important turning point after my husband's death. I had to show that I could go it on my own." In fact, shortly before Ernst's death, she had already embarked on a rich new course of study, taking advantage of the new technology of electron microscopy. Berta would pursue microscopic investigation for fifteen years, focusing on secretory vesicles and the structure of neurosecretory cells. She was among the first to detail the fine structure of the insect nervous system.

Berta's work revealed the ultrastructure of the classic neurosecretory cell—these are often constructed much like any other nerve cell, with an extended axon, dendrites, and synapses. She showed that secretory vesicles are elaborated from the endoplasmic reticulum and Golgi bodies of neurosecretory cells, just as they are in other types of secretory cells. These membrane-bound granules are transported down the axon of the cell and released at the axon terminals.

Perhaps Berta's most important contribution in this area was the elucidation of the various targets of these secretory granules. Often the granules are released into blood vessels and reach their targets over a distance through the circulation, just as conventional hormones do. But Berta also showed that other pathways are possible, including secretory granule release from terminals in close contact with target cells, including other neurons, a concept similar to our understanding of chemical transmission at the synapse.

In addition to publishing more than a dozen papers on the fine structure of the insect nervous system, Berta took on new administrative responsibilities following Ernst's death. Although she did not want to be appointed permanent chair of the anatomy department in his place, she did agree to serve as acting chair. She guided the department for the next two years, until my appointment. And, for the first time, she received full salary for her work. She would serve again as acting chair in 1976, when she was influential in the recruitment of the department's present chair, Peter Satir.

She also lent her administrative talents to the world beyond Einstein, serving as president of the American Association of Anatomists in 1978-79, only the second woman to hold this position, and as associate editor of the journals

Cell and Tissue Research and *Advances in Neuroimmunology*. Her humanitarian impulses also informed her activities with the National Academy of Sciences committee on human rights.

Fate's twistings dictated that Berta Scharrer began to receive formal public recognition of her work in neurosecretion just years after her close partner in that work was lost. Ernst Scharrer was not present to share in the validation of the couple's work when Berta was elected to the National Academy of Sciences in 1967, one of only a handful of women members. That same year Berta was elected to the American Academy of Arts and Sciences; in 1972 she became a member of the venerable Deutsche Akademie der Naturforscher Leopoldina.

Berta held honorary degrees from eleven institutions, and received numerous awards, including the Kraepelin Gold Medal (1978), the Fred C. Koch Award of the Endocrine Society (1980), the Henry Gray Award of the American Association of Anatomists (1982), and the Schleiden Medal of the Leopoldina (1983). In 1983 she traveled to the White House to accept the nation's highest scientific honor, the National Medal of Science. And in 1994 she received the Order of Merit from her original home state of Bavaria. A final tribute saw a species of cockroach dubbed *scharrerae*.

LATER CAREER

As these awards and tributes began to flow in, Berta Scharrer scaled back her research to focus on an intellectual synthesis of a lifetime of observations. She became distinguished university professor emerita in 1978, but this standing by no means curtailed her work. During the period from 1975 to 1981 she penned nearly a dozen reviews of the field of neurosecretion and neuroendocrinology, emphasizing the fruits of comparative research in vertebrates and invertebrates. This comparison led her to new theories of the evolution of the nervous and endocrine systems.

As the Scharrers' work made clear, neurosecretory cells are conserved across a wide range of vertebrate and invertebrate animals, as are their varied modes of granule transport and release. In addition, the nature of the neuropeptides that comprise those granules are similar across phylogenetic lines. And while neurosecretory cells are often quite similar structurally to conventional neurons, so they are often similar functionally to glandular secretory cells. Finally, the neuropeptides released by neurosecretory cells are often quite similar to conventional hormones.

These observations led Berta to formulate a comprehensive theory of the evolutionary origins of neurosecretory cells. She proposed that secretory neurons were not a late product of evolution, but were in fact the initial means of intracellular communication of primitive organisms, from which the highly specialized endocrine and synaptic nervous systems evolved. These novel theories are expounded in several of her publications from the late 1970s, showing that even in her seventh decade, Berta Scharrer could still stir up the scientific status quo. And she was not yet finished doing so.

In the early 1980s, at an age when most people would happily retire, Berta leapt back into research. In these "early" years of her new career she studied the nature, action, and evolutionary conservation of neuropeptides, in collaboration with George Stefano and Georg and Bente Hansen. In the late 1980s her attention turned to an entirely new field, that of comparative neuroimmunology. Berta and her colleagues showed that invertebrate neurosecretory cells and neuropeptides participate in regulation of the immune system and vice versa; these interactions bear striking similarities to the behavior of analogous vertebrate systems. Berta

Scharrer continued this work until her death on July 23, 1995. She was eighty-eight years old.

Berta's legacy of influence continues through the numerous scientists she taught and mentored, through her publications and lectures at international symposia, and her worldwide network of friends and colleagues. Her work, and that of Ernst, launched exciting new intellectual enterprises in the face of vigorous opposition. "One couldn't have foreseen the spectacular developments. . . . It has been shown that the early observations were not artifacts or a figment of the imagination. However, we had made bold claims and it is understandable that it took about twenty years of work for most people to accept these concepts." Only a scientist of Berta Scharrer's insight, tenacity, and sheer joy in learning was capable of carrying these ideas through to fruition—ideas from which we benefit today whenever we consider the transformed discipline of neuroscience.

During the preparation of this memoir, I made use of an interview with Berta Scharrer conducted in the preparation of the article "On Journeys Well Traveled."' I also found much useful information in a biography of Berta Scharrer by Birgit and Peter Satir in *Women in the Biological Sciences: A Biobibliographic Sourcebook* (eds. L. S. Grinstein, C. A. Bierman, and R. K. Rose. Greenwood Publishing Group, 1997) and in a speech given in 1982 by Sanford Palay in presentation of the Henry Gray Award to Berta Scharrer at the ninety-fifth meeting of the American Association of Anatomists.

NOTE

1. All unattributed quotes are from an interview with Berta Scharrer that appeared in the article "On Journeys Well Traveled" in *Einstein*, ed. S. K. Millen, Spring/ Summer 1989, pp. 3-6, Bronx, N.Y.: Albert Einstein College of Medicine.

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Frederick Emmons Terman

June 7, 1900-December 19, 1982

By O. G. Villard, Jr.

Frederick Emmons Terman—author, teacher, mentor, university administrator and maker of policy par excellence—was beyond any reasonable doubt responsible for the concentration of economic accomplishment in what has come to be known as California's Silicon Valley, as well as for important innovations in engineering. Son of National Academy of Sciences member the late Lewis Madison Terman, Frederick Terman achieved perhaps as distinguished a reputation for his work in electronics and education as his father—who was credited with development and widespread adoption of the IQ test—had in psychology and education.

Like his father, the younger Terman was gifted with remarkable energy and clearly defined goals. He achieved a lifetime of accomplishment in spite of a setback caused by severe illness (tuberculosis) contracted in 1924. His distinctions included the Presidential Medal for Merit; the IRE (now IEEE) Founder's Award; and Stanford's highest, the Uncommon Man Award. He was a founding member of the National Academy of Engineering. Perhaps more than any other individual since the university's start, he left his mark on Stanford University. Terman served successively as electrical engineering department head, dean of engineering, and provost. His approach to support of graduate education had the effect of winning Stanford University a nation wide reputation, and the approach has been adopted by many other institutions. At one point Stanford, which prior to the war was scarcely known nationally, was graduating more Ph.D.s in electrical engineering than MIT. Terman married in 1928 and fathered three children. Born in 1900, he passed away peacefully in his sleep in 1982.

Frederick Terman had a profound influence on the lives of many others, as well as on his profession, his technical specialty, his university, and indeed his country, as his many awards and prizes make clear. To accomplish all this required phenomenal concentration. If there was a single theme that characterized his life and may in some measure explain his success, it would be his ability to take advantage of opportunities (for example, maintaining contact with former students of unusual skill, keeping in touch with friends in industry, etc.) This theme will appear frequently in this memoir.

The Terman family moved to Stanford University in 1912 and settled in a home on the farm-like campus where Fred grew up. The senior Terman was inventor and co-developer of the Stanford Binet intelligence (or IQ) test, which was widely used in World War I for screening recruits. As part of his research on measuring IQs, he identified a number of individuals having exceptionally high scores, and presumably exceptional intelligence. One of these proved to be his son Frederick. At the time very little of a scientific nature was known about such gifted individuals—in particular it could not be said whether the high intelligence was a help or a hindrance. A study was organized to follow their careers as long as possible. Interim reports (understandably) aroused considerable interest. A finding of one such study was that those with exceptional IQ did considerably better than average career-wise and in their personal lives. This circumstance may well have had an influence in forming Fred Terman's personal philosophy concerning the importance to any organization of truly gifted individuals, who with their followers could be said to form "steeples of excellence."

Since his father believed in progressive education, the younger Terman did not begin his formal schooling until age nine. He graduated from Stanford in 1920 with a major in chemistry. He then switched his field to electrical engineering, receiving a master's degree in 1922. He went to MIT for his doctorate, where he was a student of Academy member Vannevar Bush, another choice that surely helped later. Upon completing his degree in 1924 he was offered an instructorship at MIT, but before he could begin it, he fell victim to a severe form of tuberculosis, which sent him to bed for a year and very nearly took his life. During a protracted convalescence at Palo Alto, he nevertheless managed to teach electrical engineering on a part-time basis in 1925 at Stanford. He then decided to stay on at Stanford and accept a full-time appointment in electrical engineering. During the same period he began work on his first textbook on radio engineering, which was designed to be an improvement on the then leading text in this field authored by Columbia's J. H. Morecroft. The Morecroft text reflected a strong program in radio engineering at Columbia University. For example, its faculty included such well-known early contributors to the art as Edwin H. Armstrong, credited among other things with the invention of FM. Although Stanford had had for several years a distinguished program in electric power engineering under Academy member Harris J. Ryan, there was no formal instruction in radio (or what we now call electronics) until Terman came along. Thus, the decision to compete with Morecroft must have required courage.

Since there were no resources available for building a new program in radio engineering at privately supported Stanford University, Terman had to use every possible source of funds. First were the royalties on textbooks, and in this Terman was successful from the start. In addition, he found that even though it might not be particularly strong, a viable individual patent in a particular field could nevertheless have appreciable value to a company already holding a group of patents in that field. For a second income source, Terman found it possible—at least in the early days—to make patentable improvements to existing inventions, claiming that almost anyone could do it, and that a rate of one or two saleable inventions per month is not unusual. By his own admission, young Fred was not a distinguished inventor like the University of California's Ernest Lawrence, whom he greatly admired. Terman had a remarkable ability to understand complex material and to present it in books, articles, and teaching in such a way that his readers found it easy to grasp. His well-respected textbooks brought in a steady stream of income, much of which he plowed back to support educational enterprise at Stanford. His radio engineering texts were at one time the second most valuable book property of the McGraw-Hill Book Company, being exceeded in popularity only by a standard treatise on engineering drawing.

Terman's own inventions and contributions to the state of the art can be better understood by recalling that in his early days the way vacuum tubes amplified was poorly understood. For example, it was not clear whether residual gas inside the bulb improved results or made them worse. By showing that the tube represented a problem in electrostatics and by deriving a simple but effective equivalent circuit, Terman and his colleagues made the tube amplify so effectively that there was in effect more gain available than needed for the minimum functions. The extra gain could then be used to achieve results not previously contemplated (for example, negative feedback in amplifiers). Since vacuum tubes were costly, a great deal of effort was devoted in those days to cutting down the number needed to perform a given task. One thrust of Terman's work showed not only how to get maximum gain from a given set of tubes, but also what interesting things could be done once that gain was available. Preparation of Terman's textbooks and patent disclosures required visits to manufacturing concerns to establish the state of the art in areas of interest.

When Terman returned to Stanford University in 1946 as dean of engineering, he applied his wartime reputation and experience to augmenting the university's income by encouraging research for the U.S. government, which reimbursed its contractors generously. His success with building the engineering department then led to his appointment as provost, where he was instrumental in building other departments as well.

The success of Terman's books (which had a profound effect on his reputation in electrical engineering) may be traced in part to his choice of subject matter. During the late 1930s most electrical engineering texts were dominated by needs and attitudes of the by then reasonably mature and in some respects standardized electric power industry. Communication, if mentioned at all, was subservient to electric power engineering. Terman's texts reversed this order; radio came first and a-c analysis as needed. In Terman's books mathematical analysis was used when needed and appropriate, and design information was also given. Mathematical derivations primarily for their own sake were avoided. This sometimes gave his texts a deceptively simple appearance, however readers looking for rigor in the mathematical discussions were never disappointed.

Another characteristic of Terman's texts was that they addressed themselves to the user's needs. He always undertook to find out whether a particular design approach described in published literature was actually favored in practice. He would take the trouble to contact the chief engineers of important radio companies to find out which device or approach was widely used. To compensate his informants for their trouble he kept them in touch with the abler Stanford engineering degree candidates. In this way he acted as a sort of one-man employment agency.

In planning his own teaching career at Stanford, Terman must have been influenced by his experience at MIT, where students supplemented theoretical work on campus with practical experience in industry. At Stanford the only such industry contact was incidental to faculty consulting. While arrangements of this sort augmented professors' salaries, they did little to improve the quality of university instruction in the subject field. Financial support was particularly important if students were to be attracted to a privately supported university in those post-depression years. Since there were only a few local manufacturers interested in or able to pay for research at Stanford, it was natural if not inevitable to explore other possibilities, such as the U.S. government.

Still another source of support used in attracting able students was acquisition of discarded equipment from firms contacted by Terman for information needed in his textbooks; he was very skilled at securing gifts of nonstandard but nevertheless entirely workable apparatus.

This activity required that Terman keep in touch with defense research circles in Washington, D.C. It is possible that these contacts—plus those resulting from his textbooks—had more than a little to do with his appointment in 1942 as director of a newly established civilian counter-radar laboratory, a counterpart of the pro-radar MIT Radiation Laboratory in Cambridge, Mass. The new organization was called the Radio Research Laboratory (RRL) and was assigned a very high security level by the military services. This caused some puzzlement at the time, because hardly anybody knew what radar was, much less radar countermeasures.

A further factor making Terman a particularly happy choice was his wide circle of acquaintanceships among radio engineers resulting from his, by then, widely read textbooks plus his professional work for the Institute of Radio Engineers (IRE). (He was the first national president of that organization from west of the Mississippi River.) A complicating factor in staffing RRL was caused by the great many physicists who had already signed up for the radar and atomic bomb efforts; it was expressly forbidden to approach anyone already spoken for.

Located at Harvard University, by the end of the war the RRL staff had grown to about 800 persons. The group included a few atomic physicists, whose mysterious disappearance as the end of the war approached gave rise to some inevitable conjectures. There were also two world-famous astronomers, as well as a remarkable group of radio engineers, many of whom were recruited from prominent industrial laboratories (such as radio broadcasting), which for one reason or another had not previously become involved in war work.

The extent of Terman's previous administrative experience can be surmised from his being head of the Stanford electrical engineering department, which in those days consisted of some five faculty members. At the first official cocktail party he and his wife gave after establishment of the Cambridge laboratory, the Termans found it prudent to seek how-to-give-a-party advice from an eastern U.S. student couple of their acquaintance. There had been no need to acquire this recondite skill at Stanford, because the university's founding grant strictly forbade alcoholic beverages both on campus and in neighboring Palo Alto. Faculty-student socializing at Stanford had traditionally been done at dessert parties.

Radar countermeasures (in case the reader is wondering) consist basically of active jammers (i.e., interfering signal sources), passive reflectors or jammers (also known as "window" and "chaff"), and search receivers for locating the radar to be jammed. Like standard communication sets, these devices were often needed in quantity. In the case of "window" as many as several hundred packages might be required per plane.

In using these devices, enemy counteraction frequently had to be taken into account. For example, given advance warning, the Germans could, to some extent, mitigate the effect of the jammers by changing the operating frequency of their radars. Anticipating this action and providing for it in advance was an important part of jammer design. Getting the right number of jammers to the places where they were needed, and at the right time, was a logistics problem that proved taxing to normal military supply procedures. Civilian assistance proved helpful. Seeing to it that the jamming transmitters were used in the proper fashion was an additional challenge. (For example, jammers do no good if they are tuned to the wrong radio frequency.) Terman's laboratory had the task of finding out which jammers would be important and in what quantities and locations, so they could be manufactured sufficiently far in advance to get to their destinations through the standard military supply channels. It is generally conceded that Terman's group did an outstanding job of dealing with these challenges by following his advice of "keeping your eye on the ball."

One of the sources of undesirable delay was the well-known tendency for able engineers to make a workable device even better. Research engineers tend to build prototype devices, which, however elegant they may have seemed to the designer, could not be manufactured in the available time. It is better to have an inelegant but workable solution delivered on time than a more refined solution that could not be delivered until too late. To speed the supply process RRL followed MIT's example in establishing a transition office whose purpose was to speed up the passage of equipment through prototype design and construction, field test, production design and test, manufacturing, instruction book preparation, packing, field shipment, and finally, user training.

The transition office reported directly to the director, and its job was not considered complete until sufficient of the desired "black boxes" were not only performing in the field as planned, but were producing the desired effect. Other requirements for the black boxes included minimizing space and weight, making adjustment straightforward, and having the device rugged enough to operate under severe accelerations at unconscionably high-altitudes for those times. Many problems of an unusual nature both psychological and technical were encountered, and in most instances, innovative solutions were found. Terman took an active role in supervising the work, dropping in on the various groups (as he did with university students in the laboratory) and making useful suggestions. He believed in the hands-on approach. He was especially good at avoiding related activities, which, however interesting they may have been, did not bring RRL perceptibly closer to its fundamental goal.

As as example of Terman's ability to take advantage of opportunities, one might cite his good fortune in having acquired a wartime home next door to a senior member of the Harvard business staff (William H. Claflin). Chats over the backyard fence on weekends seem to have yielded in valuable insights and information concerning Harvard University customs and practices. An occasional conflict between university customs and military requirements took place. An example of an unexpected situation was the fire accidentally set in the black cloth used to disguise the operating wavelength of a high power jammer called TUBA. Since the antenna was located on the roof, and the firemen had no security clearances to enter the laboratory, they could not get to the fire by conventional access means.

Terman often expressed his gratitude for Claflin's advice and assistance. One of the best indicators of the effectiveness of an organization is whether it stimulates imitation, and RRL qualified on that score. Various military laboratories held both the technical and administrative program of RRL in considerable respect.

Terman's success as director of RRL led to his receipt of various high prestige offers, but both during the war and later he remained intensely loyal to Stanford. He was appointed head of the electrical engineering department during the war, and accepted the post of engineering dean shortly thereafter.

The year 1942 must have been incredibly busy. In addition to assuming directorship of a rather sizeable organization put together at wartime speed, Terman also completed his *Radio Engineers' Handbook,* a volume particularly remarkable because of the coherence of presentation made possible by sole authorship.

Throughout his life, Terman showed great ingenuity in taking advantage of opportunities. His decision to write a series of textbooks intended for a wide audience—rather than specialists—led him to visit regularly a variety of companies in the radio manufacturing field. These visits, whose primary purpose was to inform him of contemporary practices, also helped him identify job opportunities for his students, especially during the depression years. In addition, he could frequently arrange for gifts of equipment to the university—obsolete, perhaps, but nonetheless of value for instructional purposes.

As another example, he published a textbook on measurements in radio engineering, which was in large measure based on experience derived from a measurements laboratory he and his students built as part of the Stanford instructional facility. The book was particularly attractive in its day because of the direct hands-on experience it represented.

Terman also used his students to catch typographical errors in his texts. This was both great fun and part of the instructional process. Some of his books went through several editions, and in this way they were considerably improved each time.

Terman must have received help formally or informally from his psychologist father. Certainly, his procedure of seeking out above-average students, rather than selecting at random from an entire applicant group, suggests that. (Mrs. Terman was a student of Fred Terman's father.)

It is interesting that in the selection process for new appointments the younger Terman did not exclusively rely on IQ scores. While this was useful information, he felt it was important to look at the components of the score, or at the student's detailed academic record. Sometimes, otherwise very able students are turned off by unexciting courses. The trick is to watch for high grades in difficult subjects. A low IQ score in a given subject—or overall—did not necessarily signal a lack of ability.

Another indicator of ability used by F. E. Terman in a manner unusual for his time was extracurricular activity. He found that the most effective individuals were those who, after completing their course work, had time left to do things on the outside, such as athletics, hobbies, or business.

Terman's acquaintance with Vannevar Bush must have had an influence—direct or indirect—on his choice as director of the Radio Research Laboratory at Harvard University. It can be said that the younger Terman had little experience in running a large organization. The professionals among its staff included such specialists as physicists and astronomers, as well as radio engineers with years of industrial experience. In the course of its work the laboratory interacted with a large number of military users, some of whom did not feel particularly pleased to have assistance from a civilian organization. RRL was the lead laboratory of Division 15 of the National Defense Research Committee (NDRC), which in turn was an agency of the Office of Scientific Research and Development (OSRD). OSRD's role in the U.S. war effort was to decide in each instance whether a piece of science-based equipment to aid the military could be developed; to develop it and show that it was indeed useful; and finally, to persuade the military to adopt and use it. The last item was as difficult as it was important, because several of the armed services especially toward the latter part of the war had laboratories of their own in which developments parallel to those of the NDRC were being carried out. While some military service individuals generously aided and accepted the NDRC, others, by insisting on the superiority of their own special approaches, were a source of strain and even programmatic delays.

In spite of—or perhaps because of—wartime pressures, defusing these situations required great tact and skill. Terman deserves credit for his choice of A. Earl Cullum, Jr., as associate director. Cullum was given responsibility for RRL's external relations. A consulting radio engineer having extraordinary tact and originality in human relations and con sensus building, Cullum had a number of years of experience in the ways of officialdom in Washington, D.C. He was a happy choice, and the team of Terman and Cullum proved very effective.

One reason for its effectiveness was what Terman called "keeping one's eye on the ball." This might be defined as deciding at any given time on the most important objectives and moving toward them in spite of the most plausible distractions, and there was never a shortage. It could be said that the technological problems faced by the laboratory were in some respects not as challenging as the human problems, many of which required great ingenuity to solve.

A troublesome item, at least initially, was finding out exactly what countermeasures were needed in a given situation. This required determining what enemy radars might be planned for use, what their characteristics were, and how they were currently being used—all highly sensitive information not normally shared by the military with civilians. One of the first steps taken by NDRC was to devise improved search receivers and procedures for acquiring intelligence of the type needed by RRL. In this connection, invaluable assistance was received from the U.S. Allies, particularly the British. The U.S. mission differed sufficiently (e.g., daylight versus nighttime bombing) to justify an independent search effort.

Later, receivers were initially used to give threat indication and for checking jammer frequency coverage. Next came devising the transmitting electronic jammers themselves plus the passive arrangement code-named "window" and "chaff." This consisted of thin strips of tinfoil a few inches in length. Several of these would create, in falling to the ground, a radar echo equivalent to that of a bomber. These were ejected from the plane to create electronic clouds in which the plane could hide—at least temporarily—to evade ground-based, fire-control radar or airborne fighter attack.

Although the British were among the first to experiment with chaff, Terman made a major contribution to its practicality by arranging for L. J. Chu of MIT (a specialist in electromagnetic theory) to do a complete theoretical analysis so that the design could be optimized. This plus important mechanical innovations made by RRL staff saved, over time, hundreds of tons of aluminum and made any given plane's complement of chaff very much more effective.

Development of the needed electronic jammers called for solution of a large number of individual problems, such as high voltage equipment that could operate at high altitudes without pressurization. Engineers who had spent their civilian careers combating noise suddenly found themselves engaged in trying to produce (and utilize) noise in spectacularly large amounts. Many of the initial RRL devices used the existing state of the art, but methods for generating random noise or energy sources of extremely high RF power required novel approaches.

A most difficult problem was seeing to it that working jammers were not only developed but were also engineered for volume production. It was found necessary to monitor every step of the way from factory to field operation, since roadblocks could and frequently did develop as a result of the sheer size and bulk of the military procurement process. Fortunately, when differences of opinion developed and when it was absolutely necessary, civilians could bypass the military chain of command and straighten out mix-ups that might otherwise have been very troublesome. Of course, this required great tact.

The need for speed in development, procurement, and deployment of military apparatus was never more keenly felt than in the case of radar countermeasures whose use depended on the enemy's disposition and utilization of his radars. In addition, the relative need for some countermeasures depended both on our own frequently changing deployments and the impact on them of enemy activity. The successful use of radar countermeasures by our forces during the Second World War depended in no small measure on the skillful direction of Terman's RRL effort. That effort extended far beyond the walls of the laboratory at Harvard. The military was assisted at every step of the way; for obvious reasons, this assistance had to be low key and largely anonymous, but it was effective.

Terman's personnel challenges were both internal and external to the organization. Inside the laboratory, there was a large staff, many of whom had headed successful industrial laboratories of considerable size. It was unavoidable that laboratory leaders did not see eye to eye on all issues of importance. One of Terman's policies helped him avoid or settle a number of conflicts. In the case of untried individuals, he always waited for signs of natural leadership to emerge before appointing that person to a position of importance. In the end it was Terman's reputation, to which his textbooks greatly contributed, that saw him over the rough spots.

Terman's outside challenges included a few persons and organizations already to some extent in the radar countermeasures field, who understandably felt threatened by the activity at Harvard. This required tact on the part of Terman and Cullum. By including all concerned (even rivals) in the planning and decision-making process in what came to be called "smoke-filled sessions," working at cross purposes was avoided to a considerable extent.

Terman had a remarkable ability to persuade others to adopt the fresh viewpoints he introduced on many issues. This was especially noticeable when he was building up

Stanford University. (For example, see R. S. Lowen, *Creating the Cold War University*, Berkeley, Calif.: University of California Press, 1997.) He used mathematically based arguments when appropriate. If adequate information on a particular issue was unavailable, Terman would arrange to collect it. When at all possible, he would base his value judgements on quantitative considerations, such as classroom attendance, costs of preparing teaching materials, etc. As might be expected, the mathematical approach (for example, the amount of research money a certain department had either spent or brought in during the last year) had the effect of upsetting some of those affected, particularly in the humanities, since some faculty members were unaccustomed to such procedures and in some cases understandably felt threatened. Terman was very skilled in dealing with these reactions. He could foresee them and would come to meetings well prepared with counter arguments. Terman was quite insistent on advance preparation, which was known as "doing one's homework." This procedure caused Terman to be (understandably and perhaps unavoidably) unpopular in certain circles. However, for the most part his proposals represented win-win situations. Once the initial shock wore off, the new procedures usually went smoothly. In preparing his own proposals as provost, Terman took maximum advantage of his own and his father's familiarity both with the campus and the likes and dislikes of the faculty. It is probably fair to say that throughout his life, Terman's enthusiasts and supporters considerably outnumbered his detractors in terms of true influence.

In the postwar years, an important consideration in winning over non-defense sponsors was the generosity of the funding made available when sponsors followed the Defense Department example. It was a pleasant surprise that other parts of the government (such as the U.S. Army Corps of

Engineers) adopted the same generous contracting procedures as those used by the Defense Department when the relatively penurious approach followed by the National Science Foundation was a clear alternative. That Terman so clearly foresaw the generous alternative that was selected is to his credit, since the possibility was by no means obvious at the time.

The success of Terman's wartime radar countermeasures program was not unnoticed by the large backlog of students (and their advisors) in search of university degrees under the GI Bill. Electrical engineering was particularly attractive because of its clear-cut civilian applications. In making appointments, Terman followed his philosophy of strengthening specialties (such as semiconductor devices), which led to additional applications. In addition, to attract attention he made certain landmark appointments of well-known individuals, such as the late William Shockley, co-inventor of the transistor. As a result, there was little difficulty in finding outstanding students—or research support, for that matter. The principal objections at the university to Terman's proposed program of appointments were the faculty members and others who objected to military-sponsored research on general principles; those who felt that support by the government would destroy the unique financial independence of the university; and those who felt that research having a military component was more like development and not sufficiently theoretical for an institution of Stanford's analytical skills.

To these objections some negative perceptions of certain sponsors would normally have to be added. However, Terman's wartime reputation for being friendly and helpful to sponsors and for holding meetings at which information was exchanged on an equal footing overcame them.

Stanford had traditionally followed an appointment procedure whereby each department or area was assigned a fraction of the funds available, and the final decision was made by the department. It was necessary for Terman to circumvent this tradition, which he did by pointing out that if outside financial help could be found for one half of an individual's time, the fraction to be borne by the department would permit two appointments instead of one for the same total amount. By this and other means Terman built up electrical engineering and then the rest of the School of Engineering.

Terman perceived that from the university's point of view a number of useful ends could be served by continuing work for the U.S. government after V-J Day. Of course, strictly military research was expected to taper off postwar to some extent, and it did, but never to the vanishing point. Successful wartime development of the atom bomb conferred great prestige on physicists and on academic research generally. Prior to the war, such research had a reputation for producing results that were interesting but for the most part impractical. The war had shown clearly how academic and government scientists could work together to produce useful, tangible results in a timely fashion. Aided by low-cost air travel, postwar inter-institutional cooperation produced excellent results.

From the sponsor's point of view, to be responsible for an important research program was a great feather in the cap. Provided that the work outcome was successful, the more costly the research the greater the resulting prestige.

From the individual faculty member's point of view, government sponsorship conferred many advantages, not the least of which was independence. From the university admissions point of view, it meant that offers could be made to more and better faculty. A given department budget could be stretched to an extent otherwise infeasible.

However, from the standpoint of the university administrator, direct support of the individual faculty member could be a disadvantage, particularly when the objectives of the faculty member did not coincide with those of the administration. On balance, however, outside support was advantageous in that it could be used to raise the quality of the faculty, thereby making a given department more attractive from the standpoint of all concerned.

Terman can be said to have made major contributions in many directions during his lifetime. His contributions to the state of the electronic arts were a consequence of his textbooks in which he clarified his subject to the point where many readers, who might not otherwise have done so, were encouraged to take up and use electronic devices in their work. His books were translated into a number of foreign languages. This took place even in the Soviet Union during the height of the Cold War.

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V. C. Twitty

Victor Chandler Twitty

November 5, 1901 - March 22, 1967

By Norman K. Wessells

Control of growth of the eye, reasons for the appearance of stripes of pigment cells along the sides of a tadpole, homing to a stretch of stream by a salamander climbing over a dry, thousand-foot-high ridge, searching for the sensory basis of homing in salamanders—two records of distinguished research in the seemingly disparate fields of vertebrate embryology and animal behavior.

Victor Chandler Twitty was a master experimentalist in both venues—the laboratory bench and the mountain terrain and streams of the American West. The unpredictable path of scientific discovery, curiosity about nature, and chance led to this unusual personal history. As Twitty wrote in an autobiographical sketch in 1966, "The role of chance in research and discovery is greater than is generally recognized, and this will be exemplified as the narrative moves from New Haven to Berlin to Stanford, from microsurgery to natural history and back again, and from the study of cell populations in tissue culture to the study of animal populations in the streams and hills of northern California." Telling this story reveals much about the rich intellectual and personal life of one of the mid-century America's leading embryologists.

Victor Chandler Twitty was born near Loogootee in Martin County, Indiana, on November 5, 1901. He was the youngest of five children of John McMahon Twitty and Emma Chandler Twitty. After a boyhood in southern Indiana, he graduated from high school and spent a year in the service of industry and finance. His meteoric rise during that period from factory flunky to filling-station attendant to bank teller transformed Twitty into a painfully earnest young man, and reawakened his father's hopes that there might be some scholarly possibilities in him after all. So, Twitty enrolled in Butler College (now a university) in Indianapolis, where he did exceptionally well from the very start. It was good fortune for him and for science that he took that year off prior to entering Butler, for if he had gone directly to the state university with his high school friends he would have been immersed in basketball, fraternities, and a very different life. Twitty received an A.B. degree in chemistry from Butler in 1925, and then went on to graduate school.

Twitty described his choice of Yale University's Department of Zoology for graduate training as "fortuitous and fortunate." The faculty of the Osborn Zoological Laboratory, headed by Ross Granville Harrison, included several outstanding scientists at the peak of their careers and a group of graduate students who would go on to make significant contributions. Of his choice of embryonic development for graduate training and specialization, he says, "I arrived at Yale with little preparation or bias that would predispose me in choosing among areas of modern research and specialization in the department." Years later Twitty regarded his chance wandering into the Harrison orbit as the single greatest piece of good luck in his life. His choice of Harrison and field of study was surely apt, for it resulted in a career of distinction and honor.

The 1920s were exciting years in embryology. The definitive paper on the discovery of the embryonic organizer was published in 1924. Harrison, who invented cell and tissue culture in 1910 as a means to investigate growth of the nerve fiber, and Hans Spemann in Germany were the two world leaders in embryology. Harrison and his students used microsurgery, transplantation of tissues between amphibian embryos, and tissue and cell culture to investigate problems of growth accommodations and conflicts and the means by which the sizes and the orientations of body parts (eyes, limbs) are regulated as an embryo develops. Experiments using vital dyes (that stained cells and allowed them to be followed in living embryos), among other things, had made mapping the embryo a possibility. Embryonic induction, cell migration, and cell and tissue interactions were being discovered and analyzed for the first time. A new Yukon was opening to which eager miners, in the words of Harrison, were now rushing to dig for gold.

Victor Twitty received his Ph.D. degree from Yale in 1929. His thesis research involved salamander embryos and indeed those sorts of animals would keep his attention for the ensuing forty years in both laboratory and field. The processes by which sets of hair-like cilia on embryos establish regularized patterns of beating, and the relative growth of eyes transplanted between large and small or slow and fast growing species were investigated in the graduate years. Twitty stayed on in New Haven for two years as an instructor and, besides continuing to develop into a superb teacher and investigator, truly struck gold in meeting a talented art student, Florence Eveleth, who was to become his wife on August 3, 1934. But, before then, in 1931, Twitty himself went as a National Research Council fellow to Otto Mangold's laboratory at the Kaiser Wilhelm Institute in Berlin.

Harrison urged the great European metropolitan center of Berlin, perhaps as a means of cultural salvation for his young Indiana Hoosier protege. Mangold, one of Spemann's students, had in fact just completed the line of experiments

Twitty proposed for the fellowship year, namely, to investigate whether the absence of an organ in a species of salamander is due to a hereditary inability to form the organ or whether the inductive stimulus for the organ is absent. So Twitty started another imaginative project on the determination of the initial size of organ rudiments. Though his experiments were fully successful, the results were never immortalized in print, because Twitty learned too late that a student of Spemann (Rotmann) had finished similar investigations. These were "maturing" but not discouraging incidents in Twitty's early scientific life, for they reflected that he was aiming at central questions of interest to the best laboratories in Europe. The Berlin year was not a waste; besides learning new techniques, Twitty met Spemann, Richard Goldschmidt, Johannes Holtfreter, Viktor Hamburger, and Dietrich Bodenstein, who would become lifelong friends or visiting scientists in Twitty's laboratory.

Twitty was well aware in 1932 of the tightening of the job market because of the Great Depression, so he declined a second year in Berlin and became an assistant professor in the Department of Biological Sciences at Stanford University in Palo Alto. The first decade of Twitty's career at Stanford was an exceptional time in the university's history. George Beadle and Edward Tatum began their Nobel Prize-winning work on genes and metabolism. C. B. van Niel at Stanford's Hopkins Marine Station was solving the fundamental chemistry of photosynthesis. And the physics and electrical engineering that was to give birth to the Stanford Industrial Park—and later Silicon Valley—was underway. Twitty's experimental program was diverse and path breaking, his experimental designs elegant, and his science rigorous and self-critical. He investigated the factors that control the proportional size of organs—for example, why an eye with all its complex parts grows to just the right size in its normal host embryo, although that same eye might grow to be much larger if placed in a host embryo of another large-bodied species or smaller if into a smaller-species embryo. He was fascinated by the phenomenon of cell migration, how and why certain pigment cells migrate long distances in embryos and take up residence at just those appropriate sites that yield recognizable color patterns. Such patterns include the stripes along the sides of salamander tadpoles and adults or the striping of zebras and tigers, and of course such patterning has survival value or behavioral roles in various species. Among other discoveries was the effect of a restricted volume, in the form of tissue culture fluid inside narrow capillary tubes, on the ability of primitive migrating cells to mature into blackened mature pigment cells. Such observations led years later to the identification of conditioned medium effects and growth factors on cells. Twitty also had that valuable capacity to recognize the unexpected observation as scientifically significant; he and a student noticed, for example, that embryos of eastern salamanders remained motionless as if anesthetized after they received grafts of tissue from western salamanders. That led to discovery and chemical identification by colleagues in chemistry and medicine of a unique and powerful neurotoxin produced by both embryonic and adult western salamanders.

Perhaps Twitty's greatest strength as an investigator lay in an exceptional ability to cut through the complications of a problem or observation and to frame scientific questions in simple terms, which led to straightforward experiments with a minimum of variables and a maximum of interpretable results.

Twitty's productivity and fine teaching were recognized quickly, and he advanced to the rank of professor in just four years. Soon numerous capable graduate students came to study with Twitty. The result was a stream of scientific papers that brought respect and admiration to Twitty and excellent careers for many budding scientists, who later worked and taught at leading American colleges and universities.

Between the 1930s and the 1950s a number of established experimental embryologists, including Bodenstein (a leading student of insect development from Germany), went to Palo Alto to work with Twitty and his students, thereby enriching the intellectual breadth and experimental approaches of the laboratory. To students and senior visitors alike Twitty gave generously of himself, both as a scientist and as a friendly and compassionate man. He was able to instill his own instinct for and understanding of the experimental approach, his ability to analyze problems into their simpler components, and his appreciation of the role of chance in research and discovery, as well as his unusual talent for accepting scientific or personal disappointment philosophically. His quiet, soft-spoken demeanor put undergraduates, graduates, and professional peers at ease; yet, always present was a sharpness of intellect and penetrating understanding, which taught, served as model, and goaded others to think more precisely and prudently.

Twitty was one of the few twentieth-century American biologists who made very significant contributions in different disciplines of biology. After doing so much in experimental embryology, Twitty was led inadvertently by his experimental material to a new career in animal behavior. While collecting embryos from streams in different parts of California, Twitty and his students noticed differences in egg laying and in pigment patterns of embryos. A long story cut short, this led to identification of different species of the renamed genus *Taricha.* Twitty's analyses of migrating pigment cells in embryos of different *Taricha* species involved at one point creation of hybrid embryos between two species. Difficulty in rearing the hybrids to sexual maturity in the laboratory made it impossible to tell whether the hybrids would have any evolutionary potentiality or consequence. That led fortuitously to a 14,000-acre mountain ranch in northern California, where hybrids were freed in the ideal habitats of the Pepperwood Creek system. Soon new questions about the juveniles and adults arose; would individual hybrids, or members of the separate parental species for that matter, return for breeding to the site on streams where they were embryos or tadpoles or juveniles? Not being willing to wait the four to six years it takes for California newts to reach sexual maturity, Twitty and his "field crew" of associates and students began immediately to mark and release breeding adults. Oscar Anderson, Herbert Little, and David Grant were the core of that talented and observant team.

The results were spectacular. A very high percentage of individuals returned the following year to their breeding sites; indeed the same individuals came back again, year after year, to their particular spots on the creek. In fact, over 20,000 individuals marked over a number of years have yielded only *one* animal that entered one of the other creeks in the drainage! Could animals return home to specific stream sites if moved many yards or a mile upstream or down, or even over thousand-foot-high ridges? The answer was a resounding yes; even in the case of release five miles away over two mountain ridges and with an inviting intervening creek, animals returned with a remarkably high frequency to their original stream sites. In fact, Twitty's traps set along the way revealed that the small amphibians took a beeline route to home. Random wandering or searching down one stream and up another was not the way of homing.

Naturally, questions followed. What senses were used in the extraordinary homing process that Twitty and his students discovered? Vision? Olfaction? What? Understand that no such work had been done on salamanders in the 1950s when this work began, and Hasler's discoveries of the olfactory basis of salmon homing in streams were just being revealed. Twitty's simple and elegant experimental design, used so long in the laboratory, was applied with equal rigor to these field studies.

Twitty's humanity and respect for nature showed through, too. Investigations on the sensory basis of homing involved at one point removing the eyes from embryos, releasing the operated animals various distances downstream and then waiting until the following year. Amazingly, the operated newts began appearing at the original stream site where they had been reared prior to capture and operation. They came overland, not eating normally as they traversed the dry, rocky hillsides. Twitty's reaction (1966) to one animal tells much about him:

> Of the countless displaced newts that I have handled, I think none has made such an impact on me as the first one of these blinded animals to be recaptured. As I examined its empty eye sockets and emaciated body (many blinded newts do not feed at all!), and then looked downstream toward the heavy forest and rugged terrain it had traversed in coming home, my respect for its accomplishment came as near awe and reverence as can be inspired by lowly organisms or possibly even by their highly evolved descendants.

The release and recaptures told Twitty new things about newt life spans; that released hybrids survived and reproduced very well; and that olfaction was likely to be one of the senses involved in homing. (We know today, but did not then, that many vertebrates use magnetic clues to orient during migration or homing.) Unfortunately, Twitty's untimely death ended this fruitful line of observation and experimentation, although there is solace perhaps that even now in the late 1990s some of his marked newts and hybrid populations march each rainy season to remembered breeding sites on Pepperwood Creek. He may be there in spirit to observe in admiration their journey home.

During this field phase of his career, Twitty was not just a field naturalist who goes and watches nature. He was in heart and mind an experimentalist; whether during the early decades at the laboratory bench or during the last two decades in the field in Sonoma County, he could think only in terms of experimental questions and manipulations. Looking and wondering, which he did with such pleasure, was just not enough; his science demanded more than that to reach satisfying answers.

Victor Twitty's distinction as a scientist was recognized in his election to membership in the National Academy of Sciences and the American Academy of Arts and Sciences. He was also a member of numerous other scientific societies in the United States and abroad, and he was a frequent referee of papers submitted to the *Journal of Experimental Zoology, Growth,* and other journals that held the mid-century literature of his science. His string of papers on newt homing and behavior that appeared in the *Proceedings of the National Academy of Sciences* was widely read and quoted as this field of animal behavior unfolded. He was honored in 1964 by being named the Herzstein Professor of Biology at Stanford, that university's oldest, most honorific endowed chair.

Twitty's special ways with people included the ability to lead productively. He was executive head of biological sciences at Stanford from 1948 until 1963. That was a momentous time in biology worldwide, and those departments that hesitated in moving toward new paradigms of research were long in recovering. Twitty's foresight led to the appointment of Charles Yanofsky, Donald Kennedy, Paul Ehrlich, Peter Raven, Clifford Grobstein, Winslow Briggs, the author of this memoir, and many others, just as the move of

Arthur Kornberg and much of the Washington University microbiology department to Stanford Medical School transformed basic medical sciences just a few hundred yards from the Biological Sciences Department. Twitty brought to the chairmanship a sense of direction and purpose, good judgement, tact, an ability to delegate responsibility, and a capacity to compromise the desirable with the possible. He recognized what was coming in biochemistry, the neurosciences, and his own embryology before it metamorphosed into "developmental biology." Although he had no interest in leaving his beloved studies of newt homing, he helped assure that those sorts of evolving disciplines would thrive in Palo Alto.

I met Victor a few days after arriving in Palo Alto as a postdoctoral fellow (with Clifford Grobstein) in August 1960, after a painfully slow drive across the country from New Haven in an old Plymouth. A new Ph.D. in embryology from Yale was a sign to him that I was okay, so off we drove to Rossotti's Alpine Inn in Portola Valley for beer in the warm, dry afternoon of flickering sunlight under the sweet smell of eucalyptus trees. Talk of "the chief," as Harrison was known, and Yale and eastern embryologists was the only agenda, but welcome was the message. Soon Victor couldn't resist describing his Pepperwood newts and his new love in science. That same sort of personal warm welcome went to new Assistant Professor Donald Kennedy, future president of Stanford University, who arrived from Harvard by way of Syracuse that same hot August. This was in a way a benign facet of what has come to be called an old-boy network; institution of graduate training, major professor, and such went with the more objective credentials of quality of scientific work itself. From that first meeting through times of pleasure and trial for the department as geneticists and biochemists from the medical school tried to chart its future until Victor's death in 1967, warmth and friendship are the two dominant memories that Kennedy and I, as well as others, have of Twitty.

Victor Twitty's other personal life was full. Florence and he had five children: John, Eveleth, Sarah Ellen, Edith Ann, and an adopted son Kalevi Holsti (whose brother Ole Holsti is a distinguished political scientist at Duke University). The Twitty home was warm and open, an oasis of friendship for students and visiting scientists, a place for informality and the sharing of a beer or bourbon whiskey and talk about embryos or newt homing or art and water colors. Victor was from boyhood an ardent, successful fly-rod fisherman, especially when it came to rainbow and steelhead trout. He and Florence loved the outdoors, so the many weeks of hard science work at the newt ranch in northern California was a cherished time each year. At home on Alvarado Row on the Stanford campus their annual summer vegetable garden supplied family, friends, and biology department colleagues with sweet corn, zucchini, and the produce of shared, enjoyed labor under the warm California sun. Their homes were also the sites of biology department "high teas," receptions for faculty, graduate students, retirees, and their families, and where laughter, conversation, and a feeling of belonging were generated by vivacious Florence and relaxed Victor. The "high," in fact, was both literal because of Victor's notoriously heavy hand in pouring libations and figurative in the good way we all felt in being part of the extended Twitty family.

It is rare for a person to do really distinguished work in two fields of science so widely different. It is rarer still when that person can write about both of them with the felicity, charm, humor, and wit that characterizes the book *Of Scientists and Salamanders*, which Twitty completed shortly before his death in 1967. He notes how some scientists seem hard put to accept that his change of scientific field "just happened that way"—all the new things followed once the hybrid newts were released. He wrote, "Discriminatory judgements about the importance of different fields or levels of biology are in my opinion intellectually naive." Tolerance for different fields of biology—not arrogance that the study of molecules or cells or genes in developing embryos or ecology is inherently most important—was a Twitty plea. And of him: self-imposed standards of excellence and scientific rigor, clarity in framing scientific questions and experiments, fundamental love and respect for nature, newts, and embryos, and sincere caring for colleagues, students, and family all permeate Twitty's book and make it happy reading. The unusual trail from Harrison and Yale, through Germany and Stanford to newts in Pepperwood Creek is not at all a typical successful science career in the twentieth century. Twitty's history illustrates how unanticipated events, chance, good fortune, and a talented nose for scientific discovery led to his odyssey and to a life of accomplishment, contribution, and satisfaction.

I thank Donald Kennedy for reading and commenting on drafts of this memoir.

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F. W. Went

Frits Warmolt Went

May 18, 1903 - May 1, 1990

By Arthur W. Galston and Thomas D. Sharkey

If ever a scientist could be said to have been born into his profession, it was Frits Went. His father, F. A. F. C. Went, was professor of botany and director of the Botanical Garden at the University of Utrecht in the Netherlands. The director and his family lived in a 300-year-old mansion in the middle of the garden, and Frits was born there in 1903. Because the well-known and respected Professor Went presided over a modern, well-equipped laboratory of botany, he attracted a great many visitors from all over the world, many of them famous in their own right. Young Frits benefited not only from acquaintance with such people, but also from the inquisitive intellectual environment of the laboratory; from witnessing the use of rather sophisticated equipment in experiments on plants; and from being exposed to the approaches used by young investigators to attack a broad range of interesting scientific problems. Frits Went relates, "I greatly profited from the thorough knowledge that my father had of botany in general. He knew the entire plant physiological literature, having read every important paper ever published (and remembering its content). He personally subscribed to most botanical journals. He knew and was friendly with most botanists all over the world, and thus I came to know many of them" (1974).

Such an upbringing certainly provides a formula for success, and young Frits did not fail to benefit from it. Although his father did not press him to become a botanist, Frits was almost automatically drawn to this field, and after graduation from the university, he entered into graduate study in his father's department.

One of his father's students, A. H. Blaauw, had discovered that light inhibited the extension growth of cylindrical plant organs, and on this basis, had attributed all of photo-tropic curvature to differential light growth inhibitions on the shaded rather than the illuminated sides of the organ. When Frits began research in his father's laboratory, he chose to repeat the Blaauw experiments, using an auxanometer invented by another graduate student, V. J. Koningsberger, to measure growth. He quickly confirmed Charles Darwin's finding that the tip of the cylindrical grass coleoptile (a hollow sheath surrounding the first embryonic leaf) is much more sensitive to light than are the basal regions of the organ. After also confirming that removal of the coleoptile tip lowered the growth of the subjacent region and that replacement of the tip restored much of the lost growth, he hypothesized, as had others, that the tip was a source of substances promoting the growth of cells lower down the organ. His creative innovation was to collect such growth hormones by simply allowing them to diffuse from the cut surface of the excised tip into gelatin blocks. When applied symmetrically to the subapical tissue, these gelatin blocks promoted elongation of the decapitated coleoptile, and when applied asymmetrically, they caused curvature in a dose-dependent manner. This experiment, which succeeded in the spring of 1926, was the first unequivocal demonstration of the existence of a growth-promoting hormone, named auxin, in plant tissues. Went used this decapitated oat coleoptile technique to demonstrate that phototropism was linked to and probably caused by a photo-induced movement of auxin from the illuminated to the shaded side of the coleoptile tip without significant destruction of the molecule. His thesis (1928) became an instant classic, and his oat coleoptile technique became the standard procedure for bioassay of auxin.

In the early part of the twentieth century, several European countries, including the Netherlands, had colonies in the underdeveloped world to which young men were sent "to establish their fortunes." Fortified by his new Ph.D. degree and accompanied by his wife Cathrien and ultimately two children, Hans and Anneka, Frits Went served from 1927 to 1933 as plant physiologist at the Royal Botanical Gardens at Buitenzorg (now Bogor) on the island of Java, then part of the Dutch East Indies. He soon discovered that complex equipment did not function well in the oppressive moist heat of Java, and so he moved into simpler applied physiological and ecological work. His experience in the tropics greatly affected his later thinking and research.

In 1933 he moved to the California Institute of Technology to replace another young Dutch plant hormone researcher, Herman E. Dolk, who had died tragically in a traffic accident. At Caltech he quickly became the center of a vigorous group of plant hormone researchers which eventually included Kenneth Thimann, James Bonner, Folke Skoog, and Johannes van Overbeek, among others. Went's work on growth hormones culminated in his publication with Kenneth Thimann of *Phytohormones* (1938) after which his interests gradually shifted to an examination of the effects of environmental factors on plant growth.

With the aid of a generous donor, Lucy Mason Clark, he constructed at Caltech several controlled-condition greenhouses in which plants could be grown under specified regimes of light, temperature, nutrition, humidity, and air quality in an insect-free environment. He soon found that the variability of response of his controlled plant populations to experimental procedures was much lower than that encountered in ordinary greenhouses or in the field. This result encouraged him to venture further into the study of plant growth under stringently controlled conditions, and with the aid of another generous donor, Harry Earhart, he was able to build and dedicate in 1949 a building containing a large new complex of air-conditioned rooms for plant growth. This vast Earhart Plant Research Laboratory, along with its supporting engineering and machinery, was dubbed the "phytotron" by Went's Caltech colleagues. It became the prototype for many similar installations all over the world. In the phytotron Went was able to establish climatic conditions for maximizing productivity of many important plant crops; to find that optimal day and night temperatures were vastly different for each plant and among plants; to study circadian rhythms under specified conditions; and to make significant discoveries about air pollution and desert ecology. Research at the phytotron is well summarized in "The Experimental Control of Plant Growth" (1957). It is paradoxical that the Caltech phytotron, which sparked the construction of other phytotrons all over the world, was destroyed a few years later when it became clear that its prodigious operating costs exceeded the institute's ability to support them.

One of the lasting legacies of work in the phytotron was the analysis of the nature of the air pollution (smog) plaguing the Los Angeles basin, and later, many other major urban centers. Because air drawn into the phytotron was passed over activated charcoal filters, it was free of many components of smog, and the plants thus protected frequently grew much better than outside unprotected plants exposed to smog. It had been assumed that sulfur dioxide was the main toxic component of smog, but Went doubted this, largely because the symptoms of smog damage on plants did not match those produced by SO_2. Aided by his friend and biochemist colleague Arie J. Haagen-Smit, Went began a series of investigations that led to the correct designation of smog as a mixture of the reaction products of unsaturated hydrocarbons (mainly from gasoline) and ozone produced photochemically in the atmosphere. These findings had many practical consequences, including the use of sensitive crops as indicators of smog levels and the rigorous and widely copied air pollution control measures in Los Angeles. Went's interest in air pollution also led to examination of the blue hazes that frequently covered forested areas, as in the Blue Ridge and Great Smoky mountains. His investigations led to quantification of the release of terpenes and other volatile materials from plants and studies of their condensation into the submicroscopic aerial particulates that produce the blue haze through the Tyndall effect.

I (A.W.G.) got to know Frits Went well during my years at Caltech-in 1943-44 and from 1947 to 1955. I conducted some experiments at the phytotron; taught with him in a collaborative graduate course in plant physiology; shared his seminars, informal discussions, and desert field trips; and with my wife enjoyed the hospitality of Frits and Cathrien Went at occasional sumptuous Javanese-style *rijstaffel* dinners at their home. I found him fiercely loyal to his friends and associates, but a vigorous opponent to his antagonists, holding firm convictions and sometimes championing highly unpopular ideas. For example, despite growing evidence that 3-indoleacetic acid is the major native auxin of plants, he continued to believe in the existence of auxin a until his death. In the face of growing skepticism, he never ceased advocating the existence of calines—hypothetical growth substances supposedly interacting with auxin to produce varied specific effects. Against almost unanimous contrary opinion, he seriously argued that petroleum deposits could have arisen from condensation and deposition of volatile emanations (e.g., terpenes) from land plants. None of these hypotheses enjoys much support today, although the discovery of the cytokinins and other plant hormones can be interpreted in terms of the caline theory.

Went decried the growing reductionism in biology and was especially disturbed by the increasing emphasis on DNA, to the virtual exclusion of other subjects. He remained to the end a naturalist, or at least a broad-spectrum biologist. He always advocated simple experiments; was suspicious of any conclusion that had to be bolstered by statistics; insisted that research should be fun; and believed in changing fields from time to time. Because he wrapped these sometimes-unpopular views in a kind of a continental charm, he usually, but not always, escaped offending his opponents.

Nobody who knew Frits Went during his last years at Caltech could have escaped learning of his enthusiasm for the desert flora of the nearby Mojave and Sonoran deserts. He delighted in studying the wildflowers that burgeoned after a spring rain, and in learning the mechanisms involved in their dormancy, germination, and competition with neighbors.

He was fascinated by the tiny annuals of the desert, which he called belly plants, because one had to lie on one's belly to get close enough to study them. To study this flora, he led many legendary field trips, Spartan in nature except for the inevitable strawberry-rhubarb pies he purchased at a bakery en route to Joshua Tree National Monument. Went became an expert in desert flora, establishing a linkage with Israeli botanists through visits to the Negev Desert and with American botanists through work at the Desert Research Institute in Reno, Nevada. In the end he had become a competent systematist and ecologist as well as an outstanding plant physiologist.

In 1958, after a quarter century at Caltech, Went left Pasadena to become director of the famous Missouri (Shaw's) Botanical Garden. He left behind his cherished phytotron and the desert ecosystem he had come to love. What propelled him outward was a growing disenchantment with the burdens of administering and raising grant support for the phytotron and of coexistence with colleagues who were increasingly committed to reductionist molecular genetic approaches to the problems of biology. In St. Louis, he rose to new challenges.

The Missouri Botanical Garden was an ideal place for Went to indulge his taste for studies of the great diversity of plants. However, the garden's infrastructure had become run-down and it was essential to build new plant growth facilities. Drawing on experience in climate control gained at the Pasadena phytotron, Went oversaw design and construction of the Climatron. This impressive geodesic dome is open inside, but through clever use of air movements, it simulates many different climates simultaneously. One of the unusual attractions is a tunnel under Victoria Pool, named after *Victoria regia*, a water lily with six-foot-wide leaves. The tunnel is curved so that at some points visitors see only aquatic life while walking through the tunnel.

Went started many new programs at the Missouri Botanical Gardens. However, he was frustrated by slow progress in some of the building projects and by the large institutional inertia he encountered. He was also being drawn more deeply into atmospheric research. He resigned as director in 1963 to spend the next two years teaching and doing research at Washington University in St. Louis.

The air pollution studies begun at Caltech again occupied Went's attention in St. Louis. Smog and air pollution are obvious in many big cities, but Went measured smog in the remotest parts of the Earth (for example, Pt. Barrow, Alaska, in the 1960s). He was convinced that plants provided the hydrocarbons that formed these particles. Went reminded the world of the experiments of Tyndall, in which the right mix of reduced carbon and catalyst could result in particles formed from carbon compound aggregates. These particles tend to stay small, usually less than one micrometer. Because of their small size, these particles scatter blue light, and Went described this natural blue haze (1960). He believed that many of the "blue" mountain ranges are named for the blue haze developed from terpenoid emissions from trees; Went cited the Blue Ridge Mountains of the Appalachians and the Blue Mountains in Australia as examples. When viewed from a distance, the mountains appeared blue because of the particles arising from condensation of plant-derived terpenoids scattering the light. Thirty years after Went brought attention to the terpenoids emitted from plants, atmospheric chemists are finding that plant terpenoids have a remarkably large effect on the oxidation potential of the atmosphere.

He also used terpenoid plant emissions to explain the origin of anthracite coal and petroleum (1960) and as an explanation of some of the properties of thunderstorms (1962). Unlike the blue haze theory, his explanations of the origin of anthracite and petroleum from volatile emissions of plants are not widely accepted.

In 1965 Went founded the Laboratory of Desert Biology as part of the Desert Research Institute in Reno, Nevada. Now known as the Biological Sciences Center, this group still conducts desert biology and plant-atmosphere research inspired by Went. Upon his arrival in Reno, Went built two air-conditioned greenhouses for his research. While the Climatron had been a huge undertaking, Went built his last phytotron in Reno as economically as possible. In Reno the elegance of simplicity allowed his greenhouses to survive to this day, thirty years after their construction, longer than the existence of the more famous Pasadena phytotron.

In 1967 Went spent six weeks aboard the research vessel *Alpha Helix.* The ship traveled up the Amazon River, but the Brazilian authorities confiscated the gas chromatograph he was intending to use to measure terpenoid emissions from the tropical plants. Unable to do his planned research, Went displayed his breadth of interests and keen ability to observe and explain. He observed masses of fungal hyphae in the soil of the tropical forest and realized that the high rates of fungal metabolism could quickly recycle nutrients to the forest (1968).

It was clear that intensive agriculture, as practiced in temperate ecosystems, would be disastrous in many tropical forests. The fertility of the tropical ecosystems depends on rapid recycling of nutrients by a large fungal community, which is lost under intensive agriculture. Went suggested modifying the tropical forests to that they would produce cash crops without altering the basic ecological relationships that were necessary for high productivity in these environments. Went's seminal work on the root fungi, or mycorrhizae, energized research in this field. Since his initial observations, other scientists have shown that nearly all plants become infected with mycorrhizae and that the same fungus individual can infect more than one tree—even more than one tree species—simultaneously. This work is having a profound influence on forest studies and management.

The maximum growth rate of plants was one of the central questions in plant physiology that fascinated Went. Work on tomato in the Pasadena phytotron was followed up in Reno. Went believed that some factor from decomposing organic matter stimulated plant growth. He grew plants under ideal conditions in his latest phytotron in Reno, placing one group of plants in sand and another in compost taken from his yard. Despite copious fertilizer additions to all of the plants, the plants in compost grew larger. Went suspected that fungi in the soil provided sugar to the plants, but collaborators using stable carbon isotopes as tracers ruled out this possibility. However, given Went's many seminal insights, it may be worth following up these observations to see if compounds besides mineral nutrients are transferred to plants from the mycorrhizae growing on their roots.

Went firmly believed that diffusion processes control the maximum growth rates of plants. He also believed that photosynthesis does not control plant growth; rather, plant growth controls photosynthesis. Given this view, it was easy for Went to accept that some signal from mycorrhizae could substantially alter growth rate. That photosynthesis does not regulate plant growth is not immediately obvious, since nearly all of the mass of a plant comes from photosynthesis. Nevertheless, the plant's use of sugars, which determines the concentration of sugars in leaves, is now known to affect the expression of genes coding for enzymes needed in photosynthesis. Went's ideas on limits to plant growth are not as widely recognized as his work on phytohormones and plant-atmosphere interactions, but some of his insights have been confirmed.

Went's interest in desert plants from his days at Caltech found a new challenge in Reno. He was a charter member of the Northern Nevada Native Plant Society, allowing him to spend many hours observing plants and teaching others the joy of exploring the plant world around them. Despite these broad interests, Went remained primarily a plant physiologist. To him this meant using any and all means to understand plant growth, development, and interactions with the environment. He saw the domination of physiology by molecular biology as an impoverishment of biology. However, he was quick to invoke genetic explanations when appropriate. Went used the term "jumping genes" to describe remarkable similarity in the taxonomy of plants in Australia and New Zealand. At the time this was heresy, and Went intentionally chose a whimsical name to needle his geneticist colleagues. Now moving genes from one organism to another is nearly routine. Went was so often willing to consider what most scientists dismissed, and this permitted him to pioneer new fields of inquiry, rather than to move into fields opened up by others.

Went considered that experiments requiring statistical analysis to tease out the signal from the noise were inadequately controlled. He urged all plant physiologists to work in highly controlled greenhouses or growth rooms so that experimental effects would be clear without resorting to statistics. He felt that ecology was an important tool for physiologists, and that much of what he did was autecology, or the study of the interaction between a single plant and its environment.

In 1985 Went moved to Oregon to be near his daughter. There he discussed atmospheric science with his former graduate student from St. Louis, R. Rasmussen, who also lives in Oregon. In 1990 Went visited the Desert Research Institute in Reno to consult on plants for a large research greenhouse. He died in his sleep while visiting Little Valley, where he had made measurements of air particles twenty years before.

In the last years of his life, Went was working on the formation of particles in the atmosphere from plant emissions. He was frustrated that the interactions between plant hydrocarbons, cloud condensation nuclei, and thunderstorms were not recognized as important problems. He was impatient with the emphasis on rising CO_2 levels in the atmosphere, since increased CO_2 would simply lead to increased growth of plants and the climatic "forcing" by CO_2 is small relative to other gases in the atmosphere. On his last trip to Reno he took along a manuscript of a book on the formation of smog particles. After his death, his daughter and son published his manuscript, which he called *Black Carbon Means Blue Sky*. In the book Went described the formation of cloud condensation nuclei and thunderstorms.

Went was struck by the fact that his father seemed to have read every paper ever published on plant physiology and remembered all of them. Performing research at the very frontiers of knowledge—where there are few enough papers that one can read them all and where new discoveries come easily to those brave enough to follow their curiosity—was what Went recommended (1974). He lived by his advice. He leaves behind a legacy of insights that have fueled many fields. Plant hormones, air pollution, desert ecology, plant-atmosphere interactions, mycorrhizae, and the limits of plant growth all fascinated him at one time or another in his career. However, for many people, Went's greatest legacies are his commitment to science as a human endeavor and his willingness to put forward revolutionary ideas about plants and the world around us.

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Eugene P. Wigner

Eugene Paul Wigner

November 17, 1902-January 1, 1995

By Frederick Seitz, Erich Vogt, and Alvin M. Weinberg

Eugene Wigner was a towering leader of modern physics for more than half of the twentieth century. While his greatest renown was associated with the introduction of symmetry theory to quantum physics and chemistry, for which he was awarded the Nobel Prize in physics for 1963, his scientific work encompassed an astonishing breadth of science, perhaps unparalleled during his time.

In preparing this memoir, we have the impression we are attempting to record the monumental achievements of half a dozen scientists. There is the Wigner who demonstrated that symmetry principles are of great importance in quantum mechanics; who pioneered the application of quantum mechanics in the fields of chemical kinetics and the theory of solids; who was the first nuclear engineer; who formulated many of the most basic ideas in nuclear physics and nuclear chemistry; who was the prophet of quantum chaos; who served as a mathematician and philosopher of science; and the Wigner who was the supervisor and mentor of more than forty Ph.D. students in theoretical physics during his career of over four decades at Princeton University.

The legacy of these contributions exists in two forms. First, there are the papers—in excess of five hundred-now included in eight volumes of his collected works.[1] His legacy also resides in the many concepts and phenomena that bear his name. There is, for example, the Wigner-Eckart theorem for the addition of angular momenta, the Wigner effect in nuclear reactors, the Wigner correlation energy, as well as the Wigner crystal in solids, the Wigner force, the Breit-Wigner formula in nuclear physics, and the Wigner distribution in the quantum theory of chaos.

His collection of essays *Symmetries and Reflections*[2] provides an insightful view of the many intellectual matters that concerned him during a busy career. The recollections of his life[3] recorded by Andrew Szanton when Wigner was in his eighties provide a special insight into the circumstances of his life and the incidents that brought him to the fore.

EARLY HISTORY

Wigner was born in Budapest on November 17, 1902, into an upper middle class, predominantly Jewish family. His father was manager of a leather factory, and clearly hoped that his son eventually would follow him in that post. He had two sisters. The family roots lay in both Austria and Hungary. The two major events that disturbed the tranquil course of his formative years were World War I and the communist regime of Bela Kun, which followed it. Since his father was of the managerial class, the family fled Hungary to Austria during the communist period and returned a number of months later, after the regime of Bela Kun had been deposed.

For his secondary school education, Wigner attended the Lutheran *gimnazium,* which had a dedicated and highly professional teaching staff. Wigner regarded himself an excellent student, but not an outstandingly brilliant one. Throughout his lifetime, he mentioned his debt to two individuals he met through that school. First was his mathematics teacher, Laslo Ratz, who recognized that the young Wigner had exceptional if not rare abilities in mathematics. The second was a somewhat younger student, John von Neumann, who came from a wealthy banking family and who indeed was recognized by Ratz to be a mathematical genius and to whom he provided private coaching. Wigner formed a close friendship with von Neumann that was to endure throughout their lifetimes. As students, they would often walk home together while von Neumann related to Wigner the wonders of advanced mathematics, which the former was absorbing.

TECHNISCHE HOCHSCHULE, BERLIN

While Wigner was strongly attracted to the field of physics, his father, who was of a very practical mind, insisted that instead he attend the Technische Hochschule in Berlin and focus on chemical engineering, so that he might be in a better position to earn a living in Hungary. Wigner followed his father's advice and in 1920 found himself in Berlin. There he spent a substantial part of the day mastering several fields of chemistry, as well as the arts and practice of chemical engineering, which he retained in full force for important use during World War II.

His heart, however, was still devoted to physics, which was in a state of major transition. He spent essentially all of his spare time at the University of Berlin attending seminars and colloquia, where he frequently found himself listening intently to discussions in the presence of the great figures of the time. His interest deepened. It should be added that von Neumann's parents had also insisted that von Neumann focus on chemical engineering, so that he would have a reliable practical background, although his major interest continued to be mathematics.

There was a small but prominent Hungarian community in academic circles in Berlin. Wigner soon formed relationships with its members, which remained close throughout their lifetimes. One of the special links was with Professor Michael Polanyi, a generation older, who gave very generously of his time and attention. He also met Leo Szilard, whom he often referred to as "the general," since Szilard enjoyed making decisions. Other Hungarians Wigner met through the Berlin connections were Dennis Gabor and Egon Orowan. He also renewed there a friendship with Edward Teller, whom he had known as a younger student in Budapest, and who was then working with Heisenberg in Leipzig.

RETURN TO BUDAPEST

Wigner returned to Budapest in 1925 to take a position in his father's leather factory. It was then that he learned of Heisenberg's highly innovative development of the matrix version of quantum mechanics. While he was not entirely happy with his work and circumstances in Budapest, he would have carried through indefinitely in order to be supportive of his family and its wishes.

RETURN TO BERLIN

A year or so after becoming re-established in Budapest, Wigner received an offer of a research assistantship in Berlin from Professor Karl Weissenberg, an X-ray crystallographer at the University of Berlin. When he discussed the matter with his father, the latter was not entirely pleased, although he recognized the intensity of his son's desire to become a professional scientist. Finally, his father decided to let his son return to Berlin, where the latter learned that Michael Polanyi had been instrumental in having the offer extended.

Since Wigner had a very fine command of mathematics, Weissenberg frequently posed problems of a semi-complex nature that had mathematical roots. This led the young novice to explore elementary aspects of symmetry or group theory as he struggled to try to satisfy Weissenberg's curiosity, as well as his own. In the meantime, Heisenbergs's matrix version of quantum mechanics was followed by Schroedinger's wave-like formulation.

Once caught up in symmetry theory, Wigner wondered if it had applications in the field of quantum mechanics. This led him to discuss the issue with von Neumann, who, after pondering the problem briefly, recommended that he read the papers of G. Frobenius and I. Schur on the irreducible representations of symmetry groups. Wigner soon became immersed in the field. He realized it opened a vast new area of mathematical physics for exploitation, the initial applications being to the degenerate states of symmetrical atomic and molecular systems. What many physicists came to call the "group theory disease" was born, with very far-reaching effects.

This initial work of Wigner on group theory and quantum mechanics[4,5] had a profound impact on all of fundamental physics and on Wigner's own subsequent development as a scientist. He understood that the superposition principle of quantum mechanics permitted more far-reaching conclusions concerning invariant quantities than was the case in classical mechanics. With the tools of group theory, Wigner derived many rules concerning atomic spectra that follow from the existence of rotational symmetry.

After a number of months, Weissenberg arranged for Wigner to become a research assistant to Professor Richard Becker, who had been newly appointed to a chair in theoretical physics at the university. Becker was very generous in allowing him to follow his own leads for self-development.

SHIFT TO GÖTTINGEN

In 1927 Richard Becker proposed that Wigner spend a period in Göttingen as an assistant to the very distinguished mathematician David Hilbert. Göttingen was at that time one of the greatest world centers of mathematics, with a continuous history in that field going back to the time of Karl Gauss. Moreover, it was very strong in theoretical physics. Unfortunately, Hilbert had become seriously ill and withdrew essentially permanently from professional work, so that Wigner found himself with a position with no formal responsibilities. He did form, however, friendly links with individuals such as James Franck. He also undertook a cooperative research program with Victor F. Weisskopf, then a student, with whom he published a paper on spectral line shape.

WIGNER'S SOLILOQUY

Having much time to himself in Göttingen because of the special circumstances he encountered there, Wigner decided to come to terms with himself and his new career. After much pondering, he came to three broad conclusions. First, he would devote his life to the further advancement of physics. Second, whenever possible, he would do his best to apply his knowledge of physics to the well being of mankind. Finally, having discovered that the field of group representations opened entirely new vistas in the applications of quantum mechanics, he would follow that area of development as the main lead in his future work.

Just at this point, Leo Szilard earnestly requested that Wigner write a book on group theory and its applications that would be understandable to physicists, particularly members of the younger generation. Soon after Wigner published his first work in the field, the mathematician Hermann Weyl, became interested in the topic and wrote a book on the subject, which was rather inaccessible for most physicists. Thus, Wigner began writing his famous book *Group Theory and Its Application to the Quantum Mechanics of Atomic Spectra,*[6] a continuing classic. In a sense, Wigner reclaimed his birthright while rendering a service.

RETURN TO BERLIN

In 1928 Wigner returned to Berlin and continued his work there. Among his many contributions to the field of quantum mechanics during this period was a paper devoted to the theory of chemical reaction rates that he developed in cooperation with Michael Polanyi and Henry Eyring, a visitor from the United States. The approach used was generalized later by Eyring and applied to many chemical problems. Wigner and Eyring were to become colleagues once again during the 1930s while both were on the Princeton faculty.

PRINCETON BECKONS

In the autumn of 1928 Wigner, again out of the blue, received a most remarkable letter from Princeton University asking if he would be willing to serve for one year as a half-time lecturer in mathematical physics at what for him was an enormous salary. The offer undoubtedly had a complex origin. Oswald Veblen, a distinguished, worldly professor of mathematics at Princeton who hoped to make Princeton the American equivalent of Göttingen in mathematics and mathematical physics, decided that a great advance would be achieved if John von Neumann would join the Princeton faculty on a full-time basis. This idea was by no means far-fetched because von Neumann had decided as early as the mid-1920s that it was very likely that Europe would experience another great war that would be accompanied by a vicious wave of anti-Semitism. He concluded at that time that he would eventually explore possible openings in the United States. When Princeton tried to acquire him on a full-time tenured basis in 1928, he decided he was not yet ready to go that far in terminating his European links and suggested that he and Wigner share the appointment on a half-time basis. Princeton agreed, with the understanding that Wigner's appointment would not carry tenure. In any event, both Wigner and von Neumann found themselves settling in at Princeton on a part-time basis in 1930.

Von Neumann enjoyed his life in the United States immensely from the very beginning. He formed friendships easily, and was soon leading a very stimulating life with his vivacious Hungarian wife, who had joined him. For Wigner, in contrast, the transition was a relatively difficult one. He not only found the informalities of American life strange relative to those in Europe, which suited him so well, but had special difficulty adjusting to Princeton, which had its own somewhat closed social structure. He lived a fairly lonely existence, except for the professional links that grew out of mutual research interests with some members of the faculty.

Not least, Wigner brought with him to the United States the standards of polite social behavior that had developed among the members of the upper middle and professional classes in Europe. There is an almost endless lore of "Wignerisms" that have circulated within the community associated with him. It was essentially impossible not to obey his insistence that you pass through a door before him. Individuals, on wagers, invented ingenious devices, which usually failed, in attempts to reverse the procedure. On one occasion, he encountered an unscrupulous merchant who attempted to cheat him in a too obvious way. Wigner, angry and now somewhat seasoned in vernacular terminology, terminated the negotiation abruptly by saying, "Go to Hell, please!" He often received requests from other individuals to read a research paper written by the latter. If he found many errors, he was very likely to return it with the ambiguous comment, "Your paper contains some very interesting conclusions!"

During that first year, both the mathematics and the physics departments were sufficiently pleased with the arrangement involving von Neumann and Wigner that it was extended on the half-time basis for a five-year period.

As Wigner was preparing to return to Berlin at the end of January 1933, it was announced that President Hindenburg had appointed Adolph Hitler chancellor. Wigner was dismayed, since he knew that his appointments in Berlin would be canceled because of his Jewish background. He returned to Budapest instead of Berlin. During the following year, he decided it would be wise for him to become a U.S. citizen, and citizenship was granted in 1937.

PREWAR YEARS OF RESEARCH

Along with the many other investigations related to physics and chemistry, Wigner initiated advances in three major fields of physics in the prewar years, first at Princeton (1930-36), then during his two years at Wisconsin (1936-38), and after his return to Princeton. He helped open important parts of solid-state physics to applications of quantum mechanics. He was a true pioneer in unraveling the mysteries of nuclear physics, and he derived for practical use the irreducible unitary matrix representations of the continuous group associated with the Lorentz transformation. In each of these three cases, his work opened doorways to areas that were to expand continuously during the next half century as a result of his initial work.

In the field of solid-state physics, he and Seitz, his first graduate student, succeeded in developing an acceptable wave function for the ground state of metallic sodium.[7] When the results associated with it were joined with calculations of the exchange and correlation energies of a gas of free electrons carried out by Wigner, the so-called binding energy or energy of sublimation of the metal could be derived essentially from fundamentals using quantum mechanics. The field was opened further by Wigner in cooperation with several of his students, most notably John Bardeen, who later gained much fame for his primary contributions to the invention of the transistor and the explanation of low-temperature superconductivity. Among other individuals who worked with him in this area at that time was Conyers Herring, who subsequently served as a leading generalist in the field for half a century.

Immediately after the discovery of the neutron in 1932, Wigner studied the early measurements of neutron-proton scattering, the properties of the deuteron, the connection between the saturation property of nuclear binding energies and the short range nature of the inter-nucleon force, and the symmetry properties of the force.

Later in the 1930s, when beta-decay data and energy levels of light nuclei began to emerge, Wigner, together with Gregory Breit, Eugene Feenberg, and others, developed the supermultiplet theory[8] in which spatial symmetry played a key role in the description of nuclear states.

Soon after Fermi found the strong and sharp resonances in the bombardment of nuclei by neutrons, Breit and Wigner developed the very useful Breit-Wigner formula to describe the cross sections in terms of nuclear parameters. Underlying the formula was the concept of a short-lived transition state, somewhat analogous to Bohr's "compound nucleus" and to the transition state appearing in Wigner's conception of a chemical reaction.

In an epochal paper[9] published in 1939, Wigner turned his attention to the inhomogeneous Lorentz group. This group involves time-dependent symmetries, or symmetry groups that include time-translation invariance. The topic had not previously received serious study by mathematicians or physicists. He provided a complete answer to the two major questions he posed: (1) what are the unitary representations of the inhomogeneous Lorentz group and (2) what is their physical significance? In this case, an analysis of its irreducible representations provided a complete classification of all the then known elementary particles. This paper furnished a platform for the further development of relativistic quantum mechanics by Wigner and others in the post-World War II period.

In 1940 Wigner developed the algebra of angular momentum recoupling, using group theoretical methods prior to Racah's algebraic analysis in 1942. The paper,[10] far ahead of its time, had the rather esoteric title of "On the Matrices Which Reduce the Kronecker Products of Simply Reducible Groups." Wigner's friends advised him that the work was too esoteric to merit publication; it did not emerge in published form until twenty-five years later.

Incidentally, Dirac became a frequent visitor to Princeton starting in the early 1930s. Wigner had first met him at Göttingen and developed a strong liking for the very reserved Englishman. The two somewhat lonesome bachelors became close friends, each respecting the other's qualities. Dirac eventually came to meet Wigner's younger sister as a result of this friendship. They were married in 1937.

UNIVERSITY OF WISCONSIN, 1936-38

Although Wigner's non-tenured appointment at Princeton was extended beyond the initial five years, and he was promoted from visiting lecturer to visiting professor, it was not the tenured position he was looking for. He decided he was being rejected. As a result he found it necessary to search for another position during a period in the Great Depression when there were very few tenured vacancies. Fortunately, he succeeded in obtaining such an appointment at the University of Wisconsin with the help of a colleague there, Gregory Breit, who fully appreciated his merits. The warmth of the reception he received at the university made him feel at home very rapidly and he was soon productively at work again. In close cooperation with Breit, he continued to focus attention on nuclear physics. Among other things, they proposed a transition-state picture of nuclear reactions and the previously mentioned Breit-Wigner formula for the scattering and absorption of particles such as neutrons and gamma rays by nuclei. In later years, Wigner strengthened the mathematical foundations on which the relationship was based, using what has come to be termed R-matrix theory.

He also found himself greatly attracted to Amelia Frank, one of the young women members of the faculty. The two were married in December 1936. Unfortunately, she soon developed incurable cancer and died just a few months after their marriage, casting him into a deep depression.

In the meantime, Princeton had come to regret its decision regarding the "dismissal" of Wigner. As a result, he was invited to return to a tenured professorship in 1938. He might have refused under other circumstances, since by this time he felt more than a sense of gratitude to his many friends at the University of Wisconsin. Under the circumstances, however, he decided that it was very important for his own mental health that he leave the surroundings associated with so much grief, and he accepted the appointment.

NUCLEAR FISSION

The return to Princeton brought with it two major developments that rapidly drew Wigner into applied research, this time with feverish energy. It was obvious to him and von Neumann, as a result of the so-called Munich Peace Pact in the autumn of 1938, that the Second World War they had long anticipated was now near at hand and that England, France, and the United States were ill prepared to face it. To protect his parents from the rising power of Hitler, he convinced them to come to the United States, a necessary move to which they never succeeded in adjusting.

A few months later came the announcement of the discovery of nuclear fission by Hahn and Strassmann in Berlin, along with evidence for the large amount of energy released in the process.

In the meantime, Enrico Fermi, who had carried out much of the pioneering work on neutron-induced reactions, had taken the opportunity provided by a Nobel award to leave Italy and accept an appointment at Columbia University in New York City. Moreover, Leo Szilard, who had moved from Berlin to England when Hitler took power, decided to join Fermi in New York, since he also feared that war was imminent.

Leo Szilard, convinced since the 1920s that it would not be long before one would learn to extract an enormous amount of energy from the atomic nucleus, came dramatically alive with the discovery of fission and soon had both Fermi and Wigner deeply immersed in the problem of determining whether a fission-induced chain reaction was possible. By the end of the winter of 1938-39, they decided that the probability of success was high, provided they could obtain the necessary material support. One of the consequences of their conviction was the framing of the letter that Einstein, Szilard, and Wigner sent to President Roosevelt in July 1939 describing the potentialities of a nuclear bomb and warning that, since fission had been discovered in Germany, it was most likely that the Germans would be the first to develop it. It took two and a half years, the start of World War II, and the bombing of Pearl Harbor for the national leadership finally to respond to the need to make adequate resources available.

In the interim, Fermi and a small group working with him at Columbia, along with the cooperation of Szilard and Wigner, succeeded in measuring the various significant parameters, such as the number of neutrons produced per fission, that would determine whether a chain reaction was possible.

In June 1941 Wigner married fellow physicist Mary Wheeler, whom he had met through professional meetings. The two were soon living as happy a domestic life as one could hope for under wartime conditions and were raising two bright, talented children. This union finally freed Wigner from the long periods of loneliness he had experienced since first coming to the United States. The next four decades were happy ones until Mary died of cancer in 1977. Two years later he married Eileen Hamilton, the recently widowed wife of the dean of graduate studies. The two shared close companionship until Wigner's death.

UNIVERSITY OF CHICAGO

By September 1941 there was no doubt about the feasibility in principle of developing a nuclear chain reaction. Moreover, the government decided to concentrate the initial effort of achieving that end at the University of Chicago under the leadership of Arthur H. Compton. Fermi was made director of the experimental research program and Wigner was placed in charge of a theoretical group that would follow developments and explore future possibilities. A strong chemistry group, which could achieve practical means of separating fissionable plutonium from the other by-products of nuclear fission, was also assembled. James Franck was placed in charge of that group, but a team led by Glenn Seaborg was given principal responsibility for carrying through the practical phases of the chemical work. The race was on!

The following few years gave Wigner an opportunity to put to use all of his experience and professional background, not least his careful training as a chemical engineer. While Fermi and his group moved ahead procuring materials of adequate purity and form for the construction of a graphite-moderated natural uranium reactor, Wigner formed a small staff, which, in addition to providing auxiliary help to Fermi, began to design large reactors that could produce practical quantities of plutonium. In his search among individuals not previously known to him, he found two scientists who became main players in his team. The two were Alvin M. Weinberg, a theoretical physicist who had just obtained his Ph.D. at the University of Chicago, and Gale Young, a practical mathematician who had been teaching at Olivet College.

Another major addition to the group was Edward Creutz who had previously joined the junior faculty at Princeton as an experimental nuclear physicist. Creutz realized soon after becoming part of Wigner's team in Chicago that the greatest service he could render was not as a nuclear physicist, but as a highly imaginative and flexible technical innovator. For he solved with speed and ingenuity many urgent problems related to metallurgy and radiation-induced effects that were barriers to progress and were beyond the range of traditionally experienced engineers.

Working with these partners and a small auxiliary staff, Wigner focused his attention on the design of large water-cooled, graphite, natural uranium reactors that would operate in the range of 500 megawatts, producing at optimum about 500 grams of plutonium per day.

By the time Fermi's reactor actually went critical on December 3, 1942, Wigner and his team had completed a task of almost unbelievable proportions, perhaps without equal in the annals of science and engineering. They had emerged with the effectively complete design of the full-scale Hanford production reactors. When the work began, a general outline was agreed on. The basic structure would consist of a lattice of natural uranium rods imbedded in channels extending through a graphite moderator. Some of the major design elements that needed to be determined as the group proceeded were the choice of coolant, dimensions of the lattice and reactor, and disposition of the control rods and cladding and tube materials. They also had to design the uranium fuel rods, determining whether they were to be hollow rods cooled internally or solid slugs cooled externally, all of which was accompanied by detailed analyses of matters such as pressure drops and heat transfer. Beyond this were issues related to the design of the outer shield and the method for loading and unloading. Wigner's personal imprint was on every aspect of the design. When the Dupont company later built the Hanford reactors, Wigner personally reviewed every blueprint.

The path Wigner and his team had to tread to reach their goal was not an easy one. Engineers brought into the program to provide independent advice offered alternative proposals for reactor design. In particular, there was strong support for a reactor that would be cooled by gaseous helium. It was necessary to demonstrate that such alternatives were substantially less desirable than a water-cooled system. Moreover, General Leslie Groves, who was in charge of the overall program, decided that responsibility for the final design and construction of the large plutonium-producing reactors should be given to the Dupont company and not to the staff of the Chicago laboratory.

Wigner felt this decision was wrong on two scores. Many valuable months would be lost while the inexperienced Dupont group became intimately familiar with the science and technology involved; his own team would inevitably be required to serve as frontline advisors, but would be in a completely subservient position. To appreciate the problems he faced and his frustrations, one can do little better than to read Wigner's memoir for the period (pages 24 to 130 of part A, volume V of Wigner's collected works) and the introductory essay by Alvin Weinberg preceding it. The experience left a permanent mark on Wigner, although he did admit later that the reactors built and operated by Dupont at Hanford in Washington state were highly successful.

When it became clear after the testing of the first atomic bomb at Alamogordo in July 1945 that the United States would soon possess an arsenal of nuclear weapons, Wigner joined a group of project scientists who requested that President Truman forego the use of such bombs in Japan. Although he was proud of his contribution to the release of nuclear energy, which he regarded as very important for the future of mankind, he was not comfortable that his work could also contribute to the death of many Japanese civilians. According to his daughter, he later found some solace in the thought that the use of the bombs had also shortened the duration of the war and thereby saved many lives on both sides.

DIRECTOR OF RESEARCH, CLINTON LABORATORIES, OAK RIDGE

Once the basic mission of the Chicago laboratory had been fulfilled and the war was nearing its end, Wigner began to make plans about the best way to explore peaceful uses of nuclear energy in the postwar period. He finally decided to spend a period in Tennessee as director of research at Clinton Laboratories, forerunner of Oak Ridge National Laboratory. A one-megawatt graphite research reactor had been constructed there in 1943 following the success of the Fermi test reactor. Initially, the laboratory at Oak Ridge had been under the management of the University of Chicago, however, it was turned over to the Monsanto Chemical Company at the end of the war.

Wigner planned a two-pronged approach. First, he would establish a training program in which some thirty-five young scientists and engineers could learn the principles involved in nuclear reactors. These individuals would become future leaders in reactor development. Second, he would assemble an expert team to design nuclear reactors that could produce useful power efficiently and as safely as possible, placing much emphasis on the so-called "breeder" reactor. A substantial part of his research team in Chicago, including Weinberg and Young, agreed to join him there and spend the next phase of their professional careers promoting the development of nuclear energy for peaceful purposes. A pithy account of the scientific and technical work carried out under Wigner's guidance during the year or so he was in residence at the laboratory is contained in Weinberg's introductory essay appearing in part A, volume V of the collected work mentioned above.

In the meantime, there was a great deal of legislative activity in Washington about the way the national nuclear energy program should be managed in peacetime. The debate was intense and protracted. The final result was the creation of a new civilian agency, the Atomic Energy Commission, which was put in charge of the operation on January 1, 1947. As the year progressed, Wigner eventually decided he was not really suited to serve as manager of a laboratory in such a complex, politicized environment. Many of the most important technical decisions would be made in Washington rather than in the laboratory. He left Oak Ridge at the end of the summer of 1947 and returned to Princeton to continue his academic career. Alvin Weinberg was eventually selected to be his successor. In the meantime, Wigner was pleased to serve as a valuable consultant to the laboratory.

In parallel with his continuing interest in the technology of nuclear reactors,[11] he became deeply involved with the problems of civil defense and spent much time at Oak Ridge working with a group that was interested in ways of achieving an effective level of defense as inexpensively as possible.

REMAINING ACADEMIC YEARS

On his return to Princeton University from the Clinton Laboratories, Wigner embarked on a long and fruitful period of research and graduate teaching. As mentioned above, he continued with his consulting on reactors and passionate involvement with civil defense. However, his main activity pertained to research, generally with his graduate students and research associates. Of Wigner's more than forty Ph.D. students, the large majority obtained their degrees during this postwar period. While he was perhaps not as venturesome as before the war, his style remained the same and his broad interests continued, particularly in nuclear physics, in the foundations of quantum mechanics, and in relativistic wave equations. He initiated and developed fully the R-matrix theory of nuclear reactions and became a founding father of the quantum theory of chaos. There was also much greater opportunity for him to engage in philosophical reflections and the writing of related essays during the decades of this period.

Wigner's deep interest in the foundations of quantum mechanics, especially the quantum theory of measurement, persisted longer than any of his other interests. It was already present in his "soliloquy" in the 1920s, as well as in his contributions to von Neumann's famous 1932 book on the mathematical foundations of quantum mechanics. It continued in his thoughts and published work until the end of his life. Wigner's monumental work on the representations of the inhomogeneous Lorentz group (1939) led after World War II to his work[12] with Newton on relativistic wave equations. Although this work enjoyed considerable success, important problems remained. Indeed, Wigner remained pessimistic until the end of his life about fully reconciling the present formulation of quantum mechanics with special and general relativity. Limitations on general measurability were pointed out in an important paper with G. S. Wick and A. S. Wightman.[13]

In the postwar years Wigner's interest in nuclear structure gradually waned, but his involvement in nuclear reactions grew and was, perhaps, responsible for more of his published work than any other subject. The various collective models for nuclear structure that gained popularity were not to Wigner's taste. However, he was deeply interested in understanding individual particle motion in nuclei and, with Vogt, used a method very similar to the Wigner-Seitz method for electron correlations in solids to show how the Pauli exclusion principle permitted the persistence of such motion despite the absence of a central field and despite the strength and short range of the nuclear forces.

The R-matrix theory of nuclear reactions arose out of Wigner's prewar work on the Breit-Wigner formula and has remained, for more than half a century, the most successful and widely used method for the description of resonance phenomena in nuclei. It was developed initially with Leonard Eisenbud,[14] but many other students and colleagues were involved in its elaboration. Wigner turned to it and its mathematics frequently.

The mathematics associated with R-matrices and R-functions fascinated Wigner beyond their direct application to resonance reactions. Although he remained a physicist throughout his life, deeply committed to the understanding of nature, he could be beguiled by mathematics. While contemplating the nature of the small random matrix elements involved in the myriad of compound nuclear levels encountered, for example, in the absorption of slow neutrons by uranium to produce slow fission, Wigner introduced an infinite Hermitian matrix that possessed random matrix elements. In this case the random matrix elements were related to the level widths involved in the problem. Using ideas he had gained from von Neumann, he was able to show[15] that a statistical distribution of level spacing still persisted in the midst of utter randomness. This "Wigner distribution" of spacing became a cornerstone of the quantum theory of chaos.

Perhaps because he was the individual who introduced the concept of symmetry into quantum mechanics and had developed well-entrenched concepts of how nature should behave, Wigner was quite taken aback when, in the mid-1950s experimental observations of the details of nuclear beta decay demonstrated that we live in a portion of the universe where inversion symmetry is not valid for the so-called weak interactions involved in such decay.

RETIREMENT

Although he retired as a professor of physics at Princeton University in 1971, Wigner's overall activities did not diminish. In fact, they broadened in important ways, since he was now relieved of some of the routine associated with academic life. Moreover, he was able, with essentially undi minished vigor, to focus as he wished on aspects of physics, philosophy, and technology that were of greatest interest to him personally. He continued his lifelong interest in the mathematical foundations of quantum mechanics with particular reference to the conclusions that could be drawn using the powerful techniques derived from group theory. Moreover, the gradual lightening of responsibilities as he approached retirement gave him the time to prepare the first edition of his collection of philosophical essays *Symmetries and Reflections.* [2] The increased freedom also permitted him to become more deeply involved in international meetings where broad issues related to science were discussed. This included, for example, the annual meetings of Nobel Prize recipients at a private estate on Lake Constance. He also became the leader of free-ranging philosophical discussion groups that met more or less annually under the auspices of the Unification Church.

To retain a link with the teaching side of academic life, he accepted appointments as visiting professor and lecturer at several institutions. Among the most prominent were a series of appointments in the physics department of the State University of Louisiana at Baton Rouge and in the summer school at Erice in Sicily.

He retained close consulting and working relations with his former colleagues at the Oak Ridge National Laboratory with special emphasis on research devoted to means of providing protection to civilians in the event of nuclear war. Linked to this, he devoted much attention to the work of the Federal Emergency Management Agency, which is responsible for preventing and providing emergency aid for national disasters.

Once signs of increasing personal and political freedom began to appear in his native Hungary, he resumed relationships with the cultural and scientific leaders there and encouraged the expansion of freedoms. In the process, he became something in the nature of a Hungarian national hero.

Wigner's vital forces began to display attrition for the first time only when he was well into his eighties, the principal sign being partial, but significant memory loss. He no longer traveled without a companion. Remarkably enough, he retained a fairly complete and detailed memory of matters related to science and technology long after he encountered difficulties in other areas.

In summary, Wigner laid the foundations for the application of symmetry principles to quantum mechanics, an achievement for which he earned the Nobel Prize. Based on these foundations, symmetry has come to play a central role in the development of physics during the second half of this century, granting that the developments have gone considerably beyond Wigner's own work. He was fond of symmetries, such as rotations in which observations remain unchanged when the symmetry transformation is applied uniformly to everything. He usually worked with quantum mechanical systems possessing a finite number of degrees of freedom in which the ground states exhibit the full symmetry of the physical system. In contrast, the ground state can be asymmetric in systems having an infinite number of degrees of freedom (that is, the symmetry is broken spontaneously). Theories involving spontaneously broken symmetries now underlie the description of magnetism, superconductivity, unified electroweak interactions, and many of the concepts employed in attempting to develop theories that will provide further unified understanding of the forces between fundamental particles. Posterity will long remember Wigner for giving powerful new tools to the theoretical physicist, as well as for his comparably basic work on the development of nuclear reactors.

NOTES

1. *The Collected Works of Eugene Paul Wigner.* New York: Springer Verlag:

Part A, The Scientific Papers. Vol. I: Eugene Paul Wigner: A Biographical Sketch; Applied Group Theory; Mathematical Papers. Vol. II: Nuclear Physics. Vol. III: Particles and Fields; Foundations of Quantum Mechanics. Vol. IV: Physical Chemistry; Solid State Physics. Volume V: Nuclear Energy.

Part B: Historical, Philosophical and Socio-Political Papers. Vol. VI: Philosophical Reflections and Syntheses. Vol. VII: Historical and Biographical Reflections and Syntheses. Vol. VIII: Socio-Political Reflections and Civil Defense.

As of December 1997, all part A volumes and vol. VI of part B are available. Volume VII of part B was to be released shortly.

2. E. P. Wigner. *Symmetries and Reflections.* Reprint edition. Woodbridge, Conn.: Ox Bow Press, 1979.

3. A. Szanton. *The Recollections of Eugene P. Wigner.* New York: Plenum Press, 1992.

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15. E. P. Wigner. *Ann. Math.* 67(1958):325.